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ROYAL ARMY MEDICAL
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Siegen — Eucalyptus globulus
Galapin — Bright's disease (Physiopathic Tracings)
Arlidge — Potter diseases
Saundos — Still births
Begbie — Albuminuria
Hardwicke — Slaughter houses
Cobbold — Entozoa
Macdonald — Phronima
Macdonald — Ventilation of Ships
Carter — Calculi
Carter — Mycetoma
Carter — Fatty [illegible]
Curran — Hours of Mortality
Wilkinson — Cubic Space
Morin —

Bequeathed
by Dr. E.A. Parkes.

WH26.
B.26.a.

FIFTH REPORT

OF

THE COMMITTEE

ON

THE TREATMENT AND UTILIZATION OF SEWAGE,

ROYAL ARMY MEDICAL
COLLEGE LIBRARY.

REAPPOINTED AT BRIGHTON, 1872;

AND CONSISTING OF

RICHARD B. GRANTHAM, C.E., F.G.S., *Chairman.*
F. J. BRAMWELL, C.E., F.R.S.
PROFESSOR W. H. CORFIELD, M.A., M.D. (OXON.).
J. BAILEY DENTON, C.E., F.G.S.
J. H. GILBERT, PH.D., F.R.S., F.C.S.
W. HOPE, V.C.
PROFESSOR A. W. WILLIAMSON, PH.D., F.R.S., F.C.S.
PROFESSOR J. T. WAY.

LONDON:
PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.
1874.

Bequeathed
by DR. E. A. PARKES.

Fifth Report of the Committee on the Treatment and Utilization of Sewage, consisting of Richard B. Grantham, *C.E., F.G.S.* (Chairman), F. J. Bramwell, *C.E., F.R.S.*, Professor W. H. Corfield, *M.A., M.D.* (Oxon.), *J. Bailey Denton, *C.E., F.G.S.*, J. H. Gilbert, *Ph.D., F.R.S., F.C.S.*, W. Hope, *V.C.*, Professor A. W. Williamson, *Ph.D., F.R.S., F.C.S.*, and *Professor J. T. Way.

N.B.—Those members whose names have an asterisk prefixed have not attended any meeting of the Committee during the year.

The Committee, in presenting its Fifth Report, has to state that it has continued that part of the inquiry for which it was more particularly reappointed, viz. the examination of the typical case of sewage-farming at Breton's Farm near Romford; and similar Tables to those furnished last year are again supplied, and are described in the portion of this Report referring to this subject.

Another analysis has also been made of the soil of the farm, showing a considerable increase in the amount of nitrogen and of phosphoric acid contained in it.

A further examination has also been made of the sewage-farm at Earlswood, with more satisfactory results than on previous occasions; and Dr. Gilbert has again furnished a note on the dry earth system, which he has made a subject of special investigation.

Whitthread's process, which was described in last year's Report, and of which a short account will be found in the subjoined abstract, has been for a few days at work on a considerable scale at Enfield. A member of the Committee, who recently inspected what was going on there, states that an excellent opportunity for further investigation will now be afforded.

It has been considered advisable at this time, when the Committee has (within a few pounds) exhausted its funds, to prepare and present with this Report an abstract of the four previous Reports made by it to the British Association; this has been done by Professor Corfield on its behalf, and the abstract will be found in another part of this Report.

Since the Committee's last Report the Local Government Board has presented to Parliament a Return moved for in the House of Commons, dated May 13th, 1873, and entitled a "Return of the names of Boroughs, Local Boards, Parishes, and Special Drainage Districts which have, through loans, provided Sewage-Farms or other means for the Disposal of Sewage by Filtration or Precipitation." The various Tables contained in this Return profess to give information, which, so far as it goes, would be valuable if exact. One radical error in the scheme of the Tables is, that there is no separation of the capital expenditure and working expenses of the year, while in the case of sewage-farms the cost of purchasing land is not separated from that of works.

Section I.—*Additional Note on the Dry Earth System.*

In former Reports the Committee has given the results obtained by Dr. Gilbert on the determination of the nitrogen in the soil which had been used in a Moule's earth-closet once, twice, and three times. The same soil, after passing through the closet the fourth time, has been again examined, and the results of the series of determinations are given below:—

	Before used.	After using once.	After using twice.	After using three times.	After using four times.
Percentage of nitrogen in soil dried at 100° C.	0·073	0·240	0·383	0·446	0·540

In the air-dried condition the soil, even after being used four times, contained less than a half per cent. of nitrogen, and, as the Table shows, only 0·54 per cent. in the fully dried condition. Thus, after passing through the closet four times, the soil was but little richer than a good garden-mould; and the Committee must still say, "that such a manure, even if disposed of free of charge, would bear carriage to a very short distance only."

The Committee would refer to former Reports for its opinion of the system in other aspects than that of the mere manurial value of the product; and its conclusions will be found summarized further on.

Section II.—*Earlswood Sewage-Farm.*

The Committee paid another visit to this farm on the 17th May, 1873, and found that nearly the whole of the land was occupied by Italian ryegrass, except about one acre which had been planted with potatoes. There was a very small sale for the ryegrass when green, so that it had been made into hay and stacked; some of last year's stacks still remained on the ground; this shows the necessity of growing crops suited to the neighbouring markets, or else of keeping live stock to consume them, and more particularly cows, for which Italian ryegrass and similar forage crops (grown by means of properly conducted sewage-irrigation, and periodically cut and carried to the stalls) are especially suitable.

At the above date the first crop of ryegrass was only just being cut, whereas the third or fourth crop ought to have been ready, and would have been on a thoroughly drained, properly laid out, and systematically managed sewage-farm.

Samples were collected of the effluent water as it flowed in a ditch, on its way to the river Mole, about half a mile from the farm; and the results of analysis showed that the sewage was much more satisfactorily purified during the dry summer of 1873 than during the wet one of 1871, when the land was supersaturated.

In former Reports of the Committee attention was drawn to this farm, which was then receiving the sewage of Red Hill; it was intended that the sewage of the town of Reigate should also be conducted to this farm, but the works for this purpose are not yet completed.

Analysis.

N.B.—Samples taken twice a day, in the proportion of $\frac{1}{1000}$ of the flow per minute. Results given in parts per 100,000.

Dates (inclusive).	Description of samples.	Average rate of flow per minute.	Solid Matter. In solution. Dried at 100° C.	Solid Matter. In solution. After ignition.	Solid Matter. In suspension. Dried at 120° C.	Solid Matter. In suspension. After ignition.	Chlorine.	Nitrogen. In solution. As ammonia.	Nitrogen. In solution. Organic.	Nitrogen. In solution. As nitrates and nitrites.	Nitrogen. In solution. Total.	Nitrogen. In suspension.	Nitrogen. Total in solution and suspension.
1873. From 16th June, to 8th July.	Effluent water from Earlswood Sewage-farm	gallons. 270	36·10	24·90	...	...	4·93	0·008	0·155	0·96	1·123	...	1·123

Section III.—*Breton's Farm, Romford.*

The systematic observations hitherto carried on with regard to this farm (for a record of which see previous Reports and the following abstract) have been continued during the past year, and the form of last year's Tables has been again adopted to set out the results arrived at.

	tons.
The quantity of sewage received from the town of Romford into the tanks and pumped on to land from March 25th, 1872, to March 24th, 1873 (inclusive), is according to the gaugings..	405,443
The quantity of effluent water repumped on to land during the same period is	38,671
The total quantity of *diluted* sewage *pumped* for distribution is therefore	444,114
The quantity of sewage received from the town of Romford and distributed on to land by gravitation during the above period is	74,499
The quantity of effluent water distributed on to land by gravitation during the same period is	8,980
Therefore the total quantity of sewage, diluted sewage, or effluent water which we have to account for is	527,593

Accounted for thus:—

As appears by the cropping Table the quantity of sewage applied to the land is	523,810 tons.
Supplied to Mr. Gooch (adjoining farmer)	1,548
Applied to garden	2,235
Total	527,593

Tables I. & II. are continuations of the Tables of last year bearing the same numbers, and are records of the observations made with regard to the quantity and composition of the sewage and the effluent water. From the organic nitrogen column in Table II., referring to the effluent water, it will be seen that an improvement has taken place, due probably to the solidifying of the earth over the drains; the proportions of total nitrogen in the effluent water for the two years show a difference of only 0·01 in 100,000 parts.

Table III. shows the absolute quantities of nitrogen contained in the sewage and in the effluent water, as calculated from the details summarized in Tables I. and II.

From this it will be seen that the volume of sewage distributed was considerably greater than in the previous year; but the proportion of nitrogen was smaller, indicating a greater dilution due to the large increase in rainfall.

It would appear that the total amount of nitrogen distributed on the farm was 26·9814 tons, while the previous year's total would appear from the Table to have been only 21·0245 tons; but the explanation is that during the previous year a large quantity of undiluted sewage, namely 83,962 tons, "was run upon a plot of land at the lower part of the farm by gravitation, and simply filtered during periods when it could not be put on the farm, owing to further drainage-works being in progress." The amount of nitrogen which must be added to last year's total to make it comparable with this year's is 6·1964 tons, which makes 27·2209 tons, or practically the same quantity as this year.

The quantity of effluent water measured was 470,552 tons as against only 195,536 tons last year. This is to be accounted for partly by the greater dilution by rain, indicated by a difference of 0·01 of nitrogen per 100,000 parts in the composition of the effluent water, but principally by the fact that the extra drainage alluded to in the last Report has been carried out. Although, therefore, the effluent water this year shows less total nitrogen per 100,000 parts, yet the absolute quantity contained in it amounted to $\frac{1}{3}$ instead of $\frac{1}{10}$ of the absolute quantity distributed over the farm.

Tables IV. to VI. are similar to the corresponding Tables of last year, and are subject to the same qualifications with regard to the quantities of sewage applied to the various crops and plots; that is to say, that the means available for the measurement of the quantities of sewage and effluent water only rendered possible the actual measurement of the total daily quantities, the details professing to show approximately the quantities applied to the individual crops and plots being merely calculated numbers obtained from the daily totals by breaking these up in proportion to the areas irrigated each day. The chief value of these figures is to show the desirability of obtaining such details with precision. This, however, would require a numerous staff of trained chemical and engineering assistants, and also the expenditure of a considerable sum of money in apparatus, and in isolating, by means of sunken barriers of concrete, the individual plots.

By comparing Tables V. and VI. of this year with Tables V. and VI. of last year, it will be found that the total produce *taken off* the farm during the year ending March 24th, 1873, was 1704 tons against 2714 for the preceding year. This was due partly to the fact that the area in standing crop on March 24th, 1873, was 87·62 acres against 40·49 acres on March 24th, 1872 (see Table VII.), and partly to the fact that there were 26·18 acres of cereals in the year now recorded, against 0·9 of an acre in the previous year.

The nitrogen recovered in the crops taken off the land for the year under review is estimated at 15,704 lbs. as against 19,667 lbs. for the preceding year. This smaller quantity recovered out of a larger quantity applied is obviously due to the same causes which affected the weight of crops.

The nitrogen escaping in the effluent water is estimated at 11,973 lbs., as against 5024 lbs. in the previous year. This increase is due to the additional drainage of the farm giving a larger *measured* quantity of effluent water as before explained, namely 470,552 tons as against 195,536 tons.

The amount of nitrogen unaccounted for (that is to say, accumulated in the standing crops and top soil, washed into the subsoil, or lost) is the difference between that applied in the sewage (60,438 lbs.) and the sum of the quantities recovered in the crops (15,704 lbs.) and escaping in the effluent water (11,973 lbs.)—namely, 32,761 lbs., as against 22,404 lbs. unaccounted for in the previous year.

These quantities, expressed in percentages, show that of every 100 parts of nitrogen distributed over the farm in the sewage, 26 were recovered and taken off the farm in crops, 20 escaped in the effluent water, and 54 remained in the standing crops, in the soil, or in the subsoil, or were lost.

This nitrogen balance-sheet shows that the results of an experiment in agricultural chemistry over so extended an area, and with so great a variety of crops, can only give true averages if conducted over a lengthened series of years; for the produce of the farm was in many respects more satisfactory in the year now recorded than in the preceding one, having regard to the amount of cereals grown and the crops left standing, and yet at first sight it appears the reverse.

In the Report of the Committee presented at Liverpool it was stated, on the authority of information furnished by the local authorities, that

(1) The population of Romford was "about 8000;"
(2) That the refuse of about 7000 persons was discharged entirely into the sewers;
(3) That the whole population is within the area provided with underground sewers.

As the Committee had some doubts as to the correctness of these statements, it was thought advisable to have a census of the town, with particulars of sewage connexions, &c. made, and the results will be found in Table VIII.

Samples of soil were very carefully taken on April 30th, 1873, in presence of Messrs. Corfield, Gilbert, Grantham, Hope, and Williamson, at the same part of the farm as on the previous occasion (July 15th, 1870), when no sewage had been applied to that part of the farm. These samples were mixed, and an average sample was analyzed by Dr. Russell with the following results:—

Examination of Soil from Breton's Farm, Sample taken April 30th, 1873.

Soil, after drying by exposure to the air, consists of, in 100 parts:—

Stones too large to pass through holes of a sieve 3·88 millims.	35·77
Moistare driven off at 100° C.	3·40
Soil passing through sieve	60·83
	100·00

In 100 parts of the original soil there is:—

Insoluble in strong hydrochloric acid	55·02
Loss on ignition (includes water driven off at 100° C.)	6·65
Phosphoric Acid (P^2O^5)	0·058
Chlorine	0·002
Ammonia	0·016
Nitrogen as Nitrates &c.	0·00029

The second part of the above Table represents the percentage amounts (calculated from the original soil) of the more important constituents of the 64·23 parts of undried soil. Comparing these results with those given in the Committee's Second Report, it will be seen that the phosphoric acid in the soil has increased from 0·01 to 0·058 per cent., that the loss on ignition of the soil is much greater (leaving water out of the question), that the amount of ammonia has been increased from an inappreciable quantity to 0·016 per cent., and that the amount of nitrates has been also increased. The amount of total nitrogen in the 64·23 parts of soil was estimated by the soda-lime process with the following result:—

Total Nitrogen determined by the Soda-lime Process.

1st experiment	0·191 per cent. Nitrogen.
2nd experiment	0·187 " "
Mean	0·189 per cent. Nitrogen in soil without stones; therefore in original soil (stones included) = 0·121 per cent. Nitrogen.

There is therefore no doubt that the quality of the soil has been considerably improved by the sewaging, and that a good deal, both of the nitrogen and phosphoric acid, is retained in it.

TABLE I.—*Breton's Sewage-Farm.*

Statement of Weekly Quantities of Sewage received on the Farm, with the proportions pumped or flowing by gravitation on to the Land, and of Effluent Water escaping from the Drains, with the proportions repumped, distributed by gravitation, or discharged into the River, and of the Total Liquid applied to the Land.

[Continued from last Report.]

Number of weekly return.	Date (inclusive).	Average monday temperature.	Rainfall during week.	Town sewage.					Effluent water.				Total liquid applied.					Proportion of effluent water to sewage distributed.
				Total delivered on farm.	Average temperature thereof.	Quantity pumped.	Quantity distributed by gravitation.	Total quantity returned from land.	Average temperature thereof.	Quantity repumped on to land.	Quantity distributed by gravitation.	Quantity discharged into river.	Diluted sewage from tank.	Average temperature thereof.	Town sewage distributed by gravitation.	Effluent water distributed by gravitation.	Total.	
	1872.	° F.	in.	galls.	° F.	galls.	galls.	galls.	° F.	galls.	galls.	galls.	galls.	° F.	galls.	galls.	galls.	
110.	July 14 to July 20	67	0·89	1,690,100	65·5	1,383,700	306,400	2,198,200	59	133,700	64,800	1,999,700	1,353,500	65·5	306,400	64,800	1,724,700	1·275
111.	July 20 to July 27	80	0·48	1,653,800	68·7	1,653,800	.....	1,856,900	62	97,700	.....	1,759,300	1,724,300	68·0	.....	.....	1,724,300	1·077
112.	July 28 to Aug. 3	69	0·76	1,677,100	67	1,677,100	.....	1,704,000	62	600,300	.....	1,103,800	2,204,700	66	.....	.....	2,204,700	·773
113.	Aug. 4 to Aug. 10	65	1·64	1,858,700	64	1,060,100	798,600	1,852,100	61	80,700	59,700	1,711,700	1,230,300	64·5	798,600	59,700	2,088,600	·887
114.	Aug. 11 to Aug. 17	67	0·02	1,582,000	65	1,582,000	.....	1,081,000	61	134,400	.....	946,600	1,673,000	65	.....	.....	1,673,000	·646
115.	Aug. 18 to Aug. 24	70	0·00	1,558,400	66	1,358,400	.....	1,348,600	61	525,400	.....	823,200	1,843,600	65·5	.....	.....	1,843,600	·732
116.	Aug. 25 to Aug. 31	67	0·89	1,533,300	64·5	1,533,300	.....	1,896,200	61	192,500	.....	1,703,700	1,752,200	64·	.....	.....	1,752,200	1·083
117.	Sept. 1 to Sept. 7	73	0·47	1,647,500	66	1,647,500	.....	1,663,100	61	15,100	.....	1,648,000	1,682,600	65·5	.....	.....	1,682,600	·988
118.	Sept. 8 to Sept. 14	69	0·03	1,480,500	66	1,480,500	.....	1,664,800	62	574,300	.....	1,090,500	1,977,900	65	.....	.....	1,977,900	·843
119.	Sept. 15 to Sept. 21	60	0·00	1,321,000	65	1,046,800	274,200	1,933,900	61	841,400	97,500	995,000	1,986,400	64	274,200	97,500	2,358,100	·820
120.	Sept. 22 to Sept. 28	54	0·80	1,390,900	62	759,700	631,200	1,638,500	58	169,900	127,100	795,000	1,001,800	61	631,200	127,100	1,760,100	·620
121.	Sept. 29 to Oct. 5	58	0·96	1,626,200	63	1,426,000	220,200	1,809,200	57	331,100	4,800	1,573,300	1,563,600	61	220,200	4,800	1,788,600	1·012
122.	Oct. 6 to Oct. 12	52	0·54	1,347,100	63	1,188,100	159,000	1,653,100	55	151,900	10,500	1,490,700	1,508,400	61	159,000	10,500	1,567,900	1·054
123.	Oct. 13 to Oct. 19	49	0·23	1,447,700	61	1,297,700	150,000	1,647,500	53	159,400	.....	1,488,100	1,368,000	58	150,000	.....	1,518,000	1·085
124.	Oct. 20 to Oct. 26	48	1·88	1,892,300	58	1,892,300	.....	2,803,800	53	280,300	.....	2,523,500	2,010,300	56	.....	.....	2,510,300	1·395
125.	Oct. 27 to Nov. 2	53	0·86	1,625,700	58	1,558,200	67,500	3,064,900	53	34,600	13,500	3,016,800	1,789,500	57	67,500	13,500	1,870,500	1·638
126.	Nov. 3 to Nov. 9	53	0·46	1,673,200	58	1,673,200	.....	2,584,800	53	.....	.....	2,584,800	1,685,000	53	.....	.....	1,685,000	1·534
127.	Nov. 10 to Nov. 16	40	0·99	2,150,100	55	2,150,100	.....	2,153,100	50	.....	.....	2,153,100	1,944,400	52	.....	.....	1,944,400	1·107
128.	Nov. 17 to Nov. 23	46	0·55	2,199,400	56	2,199,400	.....	2,468,100	50	.....	.....	2,468,100	2,371,500	52·5	.....	.....	2,371,500	1·041
129.	Nov. 24 to Nov. 30	49·5	0·75	2,104,800	56	2,104,800	.....	2,141,100	49	.....	.....	2,141,100	2,090,900	54	.....	.....	2,090,900	1·024
130.	Dec. 1 to Dec. 7	42	1·27	2,873,400	53	2,123,400	750,000	2,609,800	49	.....	.....	2,609,800	2,151,100	52	750,000	.....	2,901,100	·900
131.	Dec. 8 to Dec. 15	39	0·60	2,524,100	52	2,524,100	.....	2,482,400	49	.....	.....	2,482,400	2,460,200	50	.....	.....	2,460,200	1·009
132.	Dec. 15 to Dec. 21	42	1·67	3,161,200	50	1,836,100	1,325,000	3,153,100	48	.....	135,000	3,018,100	1,731,800	50	1,325,100	135,000	3,191,900	·988
133.	Dec. 22 to Dec. 28	48·5	0·25	2,591,300	52	2,271,300	320,000	2,482,500	47	.....	61,900	2,426,600	2,326,300	51	320,000	61,900	2,708,200	·919
	1872. 1873.																	
134.	Dec. 29 to Jan. 4	46·5	0·71	2,789,400	52·5	2,789,400	.....	2,674,800	47	.....	.....	2,674,800	2,750,800	51·5	.....	.....	2,750,800	·972
135.	Jan. 5 to Jan. 11	49·5	0·66	3,126,800	52	2,941,900	184,400	2,695,100	47	.....	18,000	2,677,100	2,799,800	51	184,400	18,000	3,002,200	·898
136.	Jan. 12 to Jan. 18	49	0·18	2,836,200	53	2,836,200	.....	2,659,400	48	.....	.....	2,659,400	2,958,800	52	.....	.....	2,958,800	·899
137.	Jan. 19 to Jan. 25	38	0·62	2,535,300	52	1,585,300	950,000	2,334,600	46	.....	271,500	2,063,100	1,842,600	51	950,000	271,500	3,064,100	·762
138.	Jan. 26 to Feb. 1	34	0·00	2,409,100	51·5	2,325,300	83,800	2,519,800	43	.....	.....	2,519,800	2,159,100	49	83,800	.....	2,242,900	·923
139.	Feb. 2 to Feb. 8	34	0·49	2,424,100	50	2,274,100	150,000	2,273,000	43	.....	.....	2,273,000	2,278,100	48	150,000	.....	2,428,100	·936
140.	Feb. 9 to Feb. 15	38	0·11	2,289,500	50	2,289,500	.....	2,324,000	42	.....	.....	2,324,000	2,318,000	49	.....	.....	2,318,000	1·003
141.	Feb. 16 to Feb. 22	35	0·00	2,192,800	35	2,192,800	.....	2,071,600	42	.....	.....	2,071,600	2,228,800	49	.....	.....	2,228,800	·929
142.	Feb. 23 to Mar. 1	37·5	1·46	3,165,500	49	1,814,000	1,351,500	2,308,300	42	.....	208,500	2,099,800	1,767,300	48	1,351,500	208,500	3,327,300	·691
143.	Mar. 2 to Mar. 8	47·5	0·38	2,401,100	49	2,348,600	52,500	2,313,000	43	.....	.....	2,313,000	2,367,300	49	52,500	.....	2,419,800	·956
144.	Mar. 9 to Mar. 15	41·5	0·28	2,510,400	50·5	2,025,400	485,000	2,407,400	43	.....	46,700	2,360,700	2,287,300	48	485,000	46,700	2,819,000	·854
145.	Mar. 16 to Mar. 22	42	0·39	2,426,200	50	1,341,600	1,084,600	2,024,800	43	.....	51,600	1,973,200	1,151,500	49	1,084,600	51,600	2,287,700	·885
146.	Mar. 23 to Mar. 29	52·5	0·02	2,141,100	52	1,343,300	797,800	1,524,800	44	59,300	58,400	1,407,100	1,362,800	52	797,800	58,400	2,219,000	·687

TABLE II.—*Breton's Sewage-Farm.*

Statement showing the Nitrogen according to analysis of Sewage as pumped and of Effluent Drainage-water, from March 1872 to March 1873.

Results given in parts per 100,000.

Analysis number.	Dates.	Sewage as pumped. Nitrogen. In solution. As ammonia.	Organic.	As nitrates and nitrites.	Total.	In suspension.	Total in solution and suspension.	Drains.	Effluent drainage-water. Nitrogen. In solution. As ammonia.	Organic.	As nitrates and nitrited.	Total.	In suspension.	Total in solution and suspension.
	1872.													
20.	Mar. 18 to Mar. 23	4·54	0·88	.....	5·42	1·14	6·56	Average of three.	0·114	0·12	1·46	1·70	.....	1·70
21.	April 9 to April 13	3·43	0·30	.....	3·73	2·67	6·40	"	0·055	0·03	1·74	1·83	.....	1·83
22.	May 13 to May 18	1·62	1·50	.....	3·12	0·91	4·03	"	0·044	0·30	0·26	0·60	.....	0·60
23.	June 10 to June 15	1·88	0·87	.....	2·75	1·27	4·02	"	0·054	0·09	0·77	0·91	.....	0·91
24.	July 8 to July 13	2·52	0·33	.....	2·85	1·11	3·96	"	0·021	0·13	0·85	1·00	.....	1·00
25.	Aug. 5 to Aug. 10	.....	.....	.....	2·87	3·36	6·23	"	0·104	0·24	0·61	0·95	.....	0·95
26.	Sept. 2 to Sept. 7	3·02	0·88	.....	3·90	2·64	6·54	"	0·255	0·28	0·14	0·67	.....	0·67
27.	Sept. 30 to Oct. 5	2·50	1·82	.....	4·32	0·82	5·14	Average of five.	0·075	0·275	1·09	1·44	.....	1·44
28.	Oct. 28 to Nov. 2	3·17	0·97	.....	4·14	0·93	5·07	"	0·019	0·18	1·02	1·22	.....	1·22
29.	Nov. 25 to Nov. 30	3·02	1·49	.....	4·51	2·08	6·59	"	0·063	0·137	1·14	1·34	.....	1·34
	1872, 1873.													
30.	Dec. 30 to Jan. 4	1·79	0·71	.....	2·50	0·75	3·25	"	0·007	0·067	1·116	1·19	.....	1·19
31.	Jan. 27 to Feb. 1	2·75	2·19	.....	4·94	2·38	7·32	"	0·126	0·05	0·86	1·04	.....	1·04
32.	Mar. 3 to Mar. 8	1·45	0·68	.....	2·13	1·21	3·34	"	0·033	0·041	1·21	1·28	.....	1·28
33.	Mar. 24 to Mar. 29	2·01	1·08	.....	3·09	1·25	4·34	"	0·033	0·123	1·00	1·16	.....	1·16

TABLE III.—*Breton's Sewage-Farm.*

Statement showing the Monthly Quantities of Sewage distributed and Nitrogen contained therein, and of Effluent Water discharged and Nitrogen contained therein, for the period from March 25, 1872, to March 24, 1873.

Dates (inclusive).	Sewage or diluted sewage distributed. Quantity.	Nitrogen per 100,000 tons.	Total Nitrogen.	Effluent water. Quantity. Drains A, B, C, D.	Drains E, F.	Total.	Nitrogen per 100,000 tons.	Total Nitrogen.
1872.	tons.	tons.	tons.	tons.	tons.	tons.	tons.	tons.
March 25 to April 24	46,763	6·06	2·8338	16,802	8,111 (computed)	24,913	1·58	·3936
April 25 to May 24	46,695	4·62	2·1573	29,186	14,070 (computed)	43,256	·90	·3893
May 25 to June 24	43,850	4·02	1·7628	24,436	10,945 (computed)	35,381	·91	·3220
June 25 to July 24	38,263	3·96	1·5152	20,839	10,812 (computed)	31,651	1·00	·3165
July 25 to August 24	38,962	6·23	2·4273	20,384	9,891 (computed)	30,275	·95	·2876
August 25 to September 24	37,641	5·84	2·1982	23,675	11,066 (computed)	34,741	1·05	·3648
September 25 to October 24	32,578	5·10	1·6615	24,329	9,513 (partly computed)	33,842	1·33	·4501
October 25 to November 24	38,206	5·83	2·2274	32,201	18,590	50,791	1·27	·6450
November 25 to December 24	53,826	4·92	2·6482	35,801	15,553	51,354	1·26	·6471
1872. 1873.								
December 25 to January 24	56,361	5·28	2·9759	25,080	17,015	42,095	1·11	·4673
January 25 to February 24	45,673	6·33	2·8911	37,327	15,570	52,897	1·10	·5819
February 25 to March 24	43,775	3·84	1·8729	27,335	12,021	39,356	1·22	·4801
Applied to Mr. Gooch's land and to garden	527,593 3,783	5·151	27·1716	317,395	153,157	470,552	1·136	5·3453
Used at Breton's farm	523,810	5·151	26·9814					

The proportion of nitrogen escaping in the effluent water to the quantity applied is thereore ·1981, for about one fifth.

TABLE IV.—*Bretow's Sewage-Farm.*

Statement showing Sewage applied and Crops grown from March 25, 1872, to March 24, 1873.

Description.						Approximate estimate of sewage applied.			Produce.			Remarks.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	
Plot.	No. of beds (inclusive).	Contents.	Crop.	Date when sown or planted.	Date when cut or gathered.	No. of dressings.	Total. *	Per acre. *	Total.	Per acre.	Sewage applied per ton of produce. *	
		acres.					tons.	tons.	tons.	tons.	tons.	
A	1 to 20	6·41	Onions	April 1872	Sept. 1872	17	10,650	1,661	65·9	10·3	162	9·75 tons of this crop ploughed in.
,,	21 and 29 (part)	1·79	Carrots	March ,,	Oct. ,,	5	2,250	1,259	21·3	11·9	106	14·2 tons consumed by cattle on the farm.
,,	21 to 28 (part)	1·59	Peas	June ,,	Sept. ,,	6	2,300	1,447	4·0	2·5	575	Including straw, 3·5 tons consumed by cattle.
,,	1 to 29	9·79	Cabbage	Oct. ,,	,, ,,	26	14,000	1,430	.....	.....	.....	This crop remained March 25, 1873.
Total A	.....	9·79	.....	.....	.....	...	29,200	2,982	91·2	9·3	320	The whole plot was under crop at the end of the year.
B	9 to 26	8·25	Cabbage	Oct. 1871	May to Aug. 1872	31	23,550	2,855	153·6	18·6	153	This crop received 8·577 tons of sewage previous to March 1872. 38·4 tons were consumed by cattle on the farm or ploughed in.
,,	1 ,, 8	3·87	Italian rye-grass	Sept. 1870 and March 1871.	April to Aug. 1872.	22	13,800	3,566	85·0	22·0	162	1208 tons sewage applied in March 1871.
,,	21, 25, & 26	1·33	Hardy green plants	April to June 1872	Sept. 1872	6	1,500	1,128	12·8	9·6	117	One half of the plants were ploughed in.
,,	9 to 16	3·72	Peas	June 1872	Aug. and Sept. 1872	6	6,050	1,626	10·0	2·7	605	Including straw, 3·4 tons consumed by cattle.
,,	17 ,, 20	1·85	Hardy greens	Aug. and Sept. 1872	Dec. 1872 and Jan. 1873.	8	3,600	1,946	7·0	3·8	514	Ploughed in 1·4 ton.
,,	1 ,, 7	3·39	Savoys	July and Aug. ,,	Jan. to March 1873.	10	7,700	2,271	29·6	8·7	260	5·9 tons ploughed in or consumed by cattle.
,,	8 ,, 16	4·20	Cabbage	Sept. 1872	.....	11	7,550	1,798	.....	.....	.....	This crop remained March 25th, 1873.
,,	1 ,, 5	2·43	Oats	March 1873	.....	,,	.....	.....	.....	.....	.....	Crops remained March, 25th, 1873.
,,	6 ,, 7	·96	Wheat	,,	.....	,,	.....	.....	.....	.....	.....	
,,	17 ,, 26	4·54	,,	,,	.....	32	18,450	4,064	.....	.....	.....	
Total B	.....	12·12	.....	.....	.....	...	82,200	6,782	298·0	24·6	276	The whole plot was under crop at the end of the year.
C	All	1·97	Oats	March 1872	Aug. 1872	...	.....	.....	6·8	3·4	.....	4·6 tons straw. 20,328 tons of sewage applied to the fallow previous to sowing the oats.
,,	,,	1·97	Hardy greens	Sept. ,,	Jan. and Feb. 1873.	30	8,850	4,492	6·7	3·4	1,321	3·3 tons ploughed in &c.
,,	,,	1·97	Wheat	March 1873	.....	...	.....	.....	.....	.....	.....	
Total C	.....	1·97	.....	.....	.....	...	8,850 *	4,492 *	13·5	6·8	656 *	Plot all under crop at the end of the year.

The figures in columns marked thus (*) are to be considered only as approximations, for reasons stated in the last Report.

TABLE IV. (*continued*).

Description.						Approximate estimate of sewage applied.			Produce.			Remarks.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	
Plot.	No. of beds (inclusive).	Contents.	Crop.	Date when sown or planted.	Date when cut or gathered.	No. of dressings.	Total. *	Per acre. *	Total.	Per acre.	Sewage applied per ton of produce. *	
		acres.				*	tons.	tons.	tons.	tons.	tons.	
D	All	6·93	Oats	March 1872	Aug. 1872	...	.....	.....	23·3	3·4	.....	16·2 tons straw.
,,	,,	6·93	Italian rye-grass	Aug. ,,	Nov. 1872 to March 1873.	27	17,750	2,561	34·7	5·0	511	The crop remained March 1873.
Total D	.....	6·93	.....	.....	.....	...	17,750	2,561	58·0	8·4	306	Plot all under crop at the end of the year.
E	1 to 13	5·60	Italian rye-grass	March 1872	June to Sept. 1872	6	3,850	1,069	9·1	2·5	423	Fallow received 6670 tons previous to sowing grass.
,,	14 ,, 22	2·17	Strawberries	.....	June to July ,,	2	1,100	507	0·2	0·1	5500	
,,	2 ,, 13	3·30	Peas	June 1872	Sept. 1872	10	6,500	1,969	8·0	2·4	812	Straw 7·4 tons. Used on farm.
,,	1 ,, 22	5·76	Cabbage	Oct. ,,	.....	15	9,350	1,623	.....	.....	.....	Crop remained March 1873.
Total E	.....	5·76	.....	.....	.....	...	20,800	3,611	17·3	3·0	1202	Plot all under crop at end of year.
F	1 to 8	1·70	Carrots	April 1872	Nov. 1872	1	500*	294	30·2	17.8	17	Tops used for fodder, 7·8 tons.
,,	9 and 10	·42	Peas	June ,,	Sept. ,,	1	150	357	1·2	2·8	125	Straw used as fodder, ·94 ton.
,,	11 to 16	1·27	Potatoes	April ,,	,, ,,	...	.....	.....	8·9	7·0	.....	
,,	17 and 18	·42	Strawberries	March ,,	.....	1	200	476	.....	.....	.....	These plants remain. No yield at present.
,,	12 to 16	1·06	,,	Sept. to Nov. 1872	.....	...	.....	.....	.....	.....	.....	,, ,, ,,
Total F	.....	3·82	.....	.....	.....	...	850	223	40·3	10·5	21	Beds 1 to 11 fallow all the winter.
G	13 and 14	·47	Hardy greens	Oct. 1871	May 1872	...	.....	.....	2·3	4·9	.....	Received 1109 tons of sewage previous to March 1872.
,,	1 ,, 2	·47	Mangold	April 1872	Nov. 1872	16	2,850	6,064	13·4	28·5	213	Tops &c. ploughed in 2 tons.
,,	3	·23	Carrots	May ,,	Dec. ,,	11	950	4,130	3·5	15·2	271	·66 ton tops used as fodder.
,,	4 to 9	1·41	Onions	Aug. 1871	May to July 1872	6	3,100	2,199	18·1	12·8	171	This crop received 8·2 tons of sewage previous to March 1872. 12 tons of the onions were ploughed in, there being no sale for them.
,,	10 ,, 12	·70	Cabbage	June 1872	Aug. 1872	5	1,000	1,429	14·5	20·7	69	1·5 ton ploughed in.
,,	13 ,, 15	·70	Brussels sprouts	,, ,,	Nov. 1872 to March 1873.	4	950	1,357	5·8	8·3	164	3·1 tons ploughed in or consumed by cattle.
,,	16	·23	Onions	May 1872	Nov. and Dec. 1872	5	400	1,739	4·2	18·3	95	
,,	17	·23	Potatoes	April ,,	Sept. 1872	2	150	652	1·3	5·6	115	
,,	18 to 22	1·18	Onions	,, ,,	Aug. ,,	5	2,050	1,737	31·0	26·3	66	
,,	4	·23	Kohl rabi	Aug. ,,	Dec. ,,	5	400	1,739	1·5	6·5	267	
,,	9	·23	Cauliflowers	July ,,	Aug. and Sept. 1872	5	450	1,957	5·8	25·2	78	5·2 tons cut for cattle and ploughed in.
,,	5 to 10 & 17 to 22	2·82	Cabbage	Sept. and Oct. 1872	.....	11	3,750	1,329	.....	.....	.....	Crop remained March 1873.
Total G	.....	5·17	.....	.....	.....	...	16,050 *	3,104 *	101·4	19·6	158 *	Part of plot fallow all winter.

The figures in columns marked thus (*) are to be considered only as approximations, for reasons stated in the last Report.

TABLE IV. (*continued*).

Description.						Approximate estimate of sewage applied.			Produce.			Remarks.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	
Plot.	No. of beds (inclusive).	Contents.	Crop.	Date when sown or planted.	Date when cut or gathered.	No. of dressings.	Total. *	Per acre. *	Total.	Per acre.	Sewage applied per ton of produce. *	
		acres.					tons.	tons.	tons.	tons.	tons.	
H	1 to 24	6·40	Cabbage	Sept. 1871	April to July 1872	32	19,950	3,117	101·1	15·8	197	10·6 tons ploughed in. The crop received 6187 tons of sewage previous to March 1872.
,,	1 ,, 17	4·25	Hardy greens	July 1872	Oct. to Dec. 1872	20	6,800	1,600	65·5	15·4	104	6·4 tons ploughed in.
,,	18 ,, 24	2·15	Cabbage	June ,,	Aug. 1872 to March 1873.	14	7,750	3,605	45·3	21·1	171	4·7 tons consumed by cattle or ploughed in.
,,	1 ,, 24	6·40	Onions	Feb. and March 1873		40	53,600	5,250	.....	.....	.....	Crop remained March 1873. Sewage all applied to fallow.
Total H	.....	6·40	.....	.....	.....	...	68,100	10,641	211·9	33·1	321	Plot all under crop at end of year.
I { 1 to 3 and 10 to 18 }		part 4·16	Cabbage and hardy greens.	Sept. and Oct. 1872	April to July 1872	9	5,000	1,202	54·0	13·0	93	10·5 tons consumed by cattle or ploughed in.
,,	4 to 9	2·27	Potatoes	Feb. 1872	July 1872	2	1,250	551	7·3	3·2	171	
,,	1 ,, 3	1·11	Cabbage	May ,,	Aug. 1872 to March 1873.	21	8,250	7,432	23·2	20·9	356	1·5 ton consumed by cattle or ploughed in.
,,	4 ,, 9	2·27	Cabbage-plants	July ,,	Sept. and Oct. 1872	2	650	286	25·8	11·4	25	
,,	10 ,, 15	2·32	Hardy greens	,, ,,	Oct. and Nov. ,,	11	5,650	2,435	30·9	13·3	183	3 tons consumed by cattle or ploughed in.
,,	16 ,, 18	·97	Peas	June ,,	Sept. 1872	7	1,450	1,495	2·6	2·7	558	Straw 2·3 tons.
,,	4 ,, 18	5·56	Fallow	.....	.....	24	19,250	3,462				
Total I	.....	6·67	.....	.....	.....	...	41,500	6,222	143·8	21·6	289	Nearly all this plot was fallow throughout the winter, and the whole clear at the end of the year.
K	All	4·44	Barley	April 1872	Aug. 1872	...	.....	.....	15·3	3·4	.....	10·4 tons straw.
,,	,,	4·44	Italian rye-grass	Sept. ,,	Nov. 1872, 1 cutting	19	10,750	2,421	6·7	1·5	1604	This grass remains.
Total K	.....	4·44	.....	.....	.....	...	10,750	2,421	22·0	4·9	489	Plot all under crop at end of year.
L	All.	2·87	Fallow	.....	.....	...	24,800	8,641				
,,	Part.	·50	Mangold	June 1872	Nov. 1872	4	1,300	2,600	4·1	8·2	317	1·25 consumed by cattle on farm.
,,	,,	2·37	Savoys	June and July 1872	,, ,, to Jan. 1873.	11	5,830	1,616	49·7	21·0	77	Only one sixth of this crop sold; remainder destroyed by floods.
,,	All.	2·87	Fallow	.....	.....	7	6,600	2,300				
Total L	.....	2·87	.....	.....	.....	...	36,530	12,728	53·8	18·7	679	
M	All.	3·17	Cabbage	Oct. 1871	June to Aug. 1872	24	12,000	3,785	65·7	20·7	183	32·75 tons consumed by cattle or ploughed in. The crop received 4394 tons of sewage previous to March 1872.
,,	,,	3·17	Italian rye-grass	Sept. 1872	.....	10	4,950	1,561	.....	.....	.....	Grass not cut till March 1873.
Total M	.....	3·17	.....	.....	.....	...	16,950 *	5,346 *	65·7	20·7	258 *	Standing crop at end of year.

The figures in columns marked thus (*) are to be considered only as approximations, for reasons stated in the last Report.

TABLE IV. (*continued*).

Description.						Approximate estimate of sewage applied.			Produce.			Remarks.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	
Plot.	No. of beds (inclusive).	Contents.	Crop.	Date when sown or planted.	Date when cut or gathered.	No. of dressings.	Total. *	Per acre. *	Total.	Per acre.	Sewage applied per ton of produce. *	
		acres.					tons.	tons.	tons.	tons.	tons.	
N	7 and 8	·52	Broccoli	July 1871	April 1872	...	.....	.....	13·8	26·5	.....	This crop received 2194 tons of sewage previous to March 1872. 11 tons consumed by cattle or ploughed in.
,,	All.	4·15	Italian rye-grass	Mar. and May 1872	July ,, to Jan. 1873.	75	37,950	9,145	187·9	45·3	202	Grass cut 6 times and still remains.
Total N	.....	4·15	.....	.....	.....	...	37,950	9,154	201·7	48·6	188	Standing crop at end of year.
O	All.	5·92	Wheat	Feb. 1872	Aug. 1872	...	.....	.....	20·7	3·5	.....	30 qrs. wheat = 6·75 tons, tail wheat = ·45 tons, straw 13·5 tons.
,,	,,	5·92	Cabbage	Sept. ,,	,, ,,	31	21,100	3,564	.....	.....	.....	This crop remained March 1873.
Total O	.....	5·92	.....	.....	.....	...	21,100	3,564	20·7	3·5	1019	Standing crop at end of year.
P	Part.	2·00	Hardy greens and savoys	April 1872	June 1872 to Mar. 1873.	52	16,300	8,150	22·7	11·3	718	10 tons consumed by cattle or ploughed in.
,,	,,	1·50	Drumhead cabbage	May ,,	Oct. to Dec. 1872.	55	18,450	12,300	45·1	30·1	409	30 tons consumed by cattle or ploughed in.
,,	All.	3·50	Wheat	March 1873	.....	...	.....	.....	.....	.....	.....	The crop remained March 1873.
Total P	.....	3·50	.....	.....	.....	...	34,750	9,927	67·8	19·4	513	Standing crop at end of year.
Q	1 to 10	1·04	Barley	May 1872	Sept. 1872	...	.....	.....	2·6	2·5	.....	2·12 tons straw.
,,	11 to 20	1·07	Savoys	July ,,	Nov. ,, to Jan. 1873.	3	1,150	1,075	23·6	22·1	487	15·7 tons consumed by cattle or ploughed in.
,,	21 and 22	·23	Drumhead cabbage	May ,,	Nov. 1873	1	150	652	3·2	13·9	47	2·1 tons consumed by cattle or ploughed in.
,,	1 to 10	1·04	Cabbage	Oct. 1873	.....	2	250	240	.....	.....	.....	Crop remained March 1873.
,,	11 to 22	1·30	Fallow	.....	.....	2	550	423				
Total Q	.....	2·34	.....	.....	.....	...	2,100	897	29·4	12·6	71	
R	All.	2·52	Mangold	April 1872	Nov. 1872	3	1,450	575	46·6	18·5	31	5 tons small mangolds ploughed in.
,,	Part.	2·40	Fallow	.....	.....	14	8,150	3,396				
,,	,,	·12	Oziers	Jan. 1873	.....	13	680	5,667	.....	.....	.....	Crop remained March 1873.
Total R	.....	2·52	.....	.....	.....	...	10,280	4,079	46·6	18·5	221	
S	All.	·22	Onions	March 1872	July and Aug. 1873.	2	400	1,818	1·8	8·2	222	
,,	,,	·22	Hardy greens	Aug. ,,	Dec. 1872	2	150	682	2·1	9·5	71	·33 ton consumed by cattle or ploughed in.
,,	,,	·22	Rhubarb	Feb. ,,	.....	...	.....	.....	.....	.....	.....	Crop remained March 1873.
Total S	.....	·22	.....	.....	.....	...	550	2,500	3·9	17·7	141	Standing crop at end of year.

The figures in columns marked thus (*) are to be considered only as approximations, for reasons stated in the last Report.

TABLE IV. (*continued.*)

Description.						Approximate estimate of sewage applied.			Produce.			Remarks.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	
Plot.	No. of beds (inclusive).	Contents.	Crop.	Date when sown or planted.	Date when cut or gathered.	No. of dressings.	Total. *	Per acre. *	Total.	Per acre.	Sewage applied per ton of produce. *	
		acres.						tons.	tons.	tons.	tons.	
U	All.	2·53	Sprouting broccoli ...	Oct. 1871	April 1872	...	.....	.....	13·4	5·3	.....	This crop received 5797 tons of sewage previous to March 1872. The greater portion of crop consumed by cattle or ploughed in.
,,	,,	2·53	Dwarf and runner beans.	May 1872	Aug. ,, ...	15	7,900	3,122	7·2	2·8	1097	Three fourths of this crop was ploughed in.
,,	,,	2·53	Wheat	March 1873	.....	37	9,550	3,775	.....	.....	.....	Crop remains; sewage all applied to the fallow.
Total U	.....	2·53	.....	.....	.....	...	17,450	6,897	20·6	8·1	847	Standing crop at end of year.
V	Part.	·50	White broccoli.....	June 1871	April 1872	...	.....	.....	14·5	29·0	.....	Four fifths of this crop ploughed in or consumed by cattle. Received 2126 tons of sewage previous to March 1872.
,,	,,	2·00	Cabbage	Oct. ,, ...	May to Aug. 1872	3	2,950	1,475	35·1	17·5	84	Consumed by cattle and ploughed in 4 tons. Received 2053 tons of sewage previous to March 1872.
,,	,,	2·83	Barley	May 1872	Sept. 1872	...	.....	.....	7·2	2·5	.....	Straw, 5·3 tons.
,,	,,	2·00	Hardy greens	Sept. ,, ...	March 1873	7	2,800	1,400	4·7	2·3	596	Consumed by cattle or ploughed in 2·5 tons.
,,	,,	2·93	Cabbage	Oct. ,, ...	,, ...	5	3,000	1,024	.....	.....	.....	Crop remained March 1873.
Total V	.....	4·93	.....	.....	.....	...	8,750	1,775	61·5	12·5	142	Standing crop on part of plot at end of year.
W	All.	3·0	Oats	March 1872	Aug. 1872.....	...	.....	.....	10·2	3·4	.....	Fallow received 8345 tons sewage previous to March 1872. Straw 6·9 tons.
,,	Part.	2·75	Hardy greens	Sept. ,, ...	Feb. and Mar. 1873	33	9,600	3,491	6·0	2·2	1600	Consumed by cattle or ploughed in 3 tons.
,,	,,	2·75	Wheat	March 1873	.....	...	.....	.....	.....	.....	.....	Crop remained March 1873.
Total W	.....	average 2·87	.....	.....	.....	...	9,600	3,345	16·2	5·6	593	Plot in seed at end of year; quarter acre of plot taken for gravel-pit, &c.
X	All.	3·86	Fallow	.....	.....	7	5,000	1,295				
,,	Part.	3·36	Mangold	May 1872	Nov. 1872.....	7	6,150	1,830	84·0	25·0	73	Waste 8·4 tons.
,,	,,	·50	Savoys	July ,, ...	Nov. ,, to Mar. 1873.	4	650	1,300	10·1	20·2	64	Five sixths of this crop injured by floods and consumed by pigs or ploughed in.
,,	All.	3·86	Wheat	March 1873	.....	14	14,800	3,834	.....	.....	.....	All this sewage applied to the fallow. Crop remained March 1873.
Total X	.....	3·86	.....	.....	.....	...	26,600	6,891	94·1	24·4	283	Plot in seed at end of year.
Y	All.	5·60	Hay	Permanent grass ...	2 cuttings, June and Sept. 1872.	7	5,150	920	24·6	4·4	209	The grass remains; 2400 tons of this sewage applied since the second cutting.
							*	*			*	

The figures in columns marked thus (*) are to be considered only as approximations, for reasons stated in the last Report

TABLE V.—*Breton Sewage Farm.*

Summary for the year ending March 24, 1873, showing the Nitrogen applied to the Land during that period, and its relation to the Produce of the Farm.

Description.			Produce.		Approximate estimate of nitrogen applied.			Approximate estimate of nitrogen.														
								Quantity applied.			Quantity escaping in effluent water.	Difference (in soil, crops, &c.)			Calculated to be in crops.			Not accounted for (in standing crops, soil, and drained away).			Per cent.		
Plot.	Contents.	Crop.	Total.	Per acre.	Total.	Per acre.	Per ton of produce.	Total.	Per acre.	Per ton of produce.		Total.	Per acre.	Per ton of produce.	Total.	Per acre.	Per ton of produce.	Total.	Per acre.	Per ton of produce.	In crop.	In effluent water.	* Not accounted for (in soil).
					*	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*			
	acres.		tons.	tons.	tons.	tons.	tons.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.			
A	9·79	Onions, carrots, and peas	91·2	9·3	29,200	2,982	321	3,369	344	36·9	668	2,701	276	29·6	725	74	7·9	1,976	202	21·7	22	20	58
B	12·12	Cabbage, Italian rye-grass, hardy green plants, peas, and savoys	298·0	24·6	82,200	6,782	276	9,485	783	31·8	1,879	7,606	627	25·5	2,927	242	9·8	4,679	386	15·7	31	20	49
C	1·97	Oats and hardy greens	13·5	6·8	8,850	4,492	655	1,021	518	75·6	202	819	416	60·7	198	101	14·7	621	315	46·0	19	20	61
D	6·93	Oats and Italian rye-grass	58·0	8·4	17,750	2,561	306	2,048	295	35·3	406	1,642	237	28·3	954	138	16·4	688	99	11·9	46	20	34
E	5·76	Italian rye-grass, strawberries, and peas	17·3	3·0	20,800	3,611	1202	2,400	416	138·7	476	1,924	334	111·2	719	125	41·5	1,205	209	69·7	30	20	50
F	3·62	Carrots, peas, and potatoes	40·3	10·5	850	223	21	98	26	2·4	19	79	21	1·9	276	72	6·8	.....	.....	.....	282	20	
G	5·17	Hardy greens, mangold, carrots, onions, cabbage, Brussels sprouts, potatoes, kohl rabi, and cauliflowers.	101·4	19·6	16,050	3,104	158	1,852	358	18·3	367	1,485	287	14·6	533	103	5·3	952	184	9·4	29	20	51
H	6·40	Cabbage and hardy greens	211·9	33·1	63,100	10,641	321	7,858	1228	37·1	1,557	6,301	985	29·7	1,187	185	5·6	5,114	800	24·1	15	20	65
I	6·67	Cabbage, hardy greens, potatoes, and peas	143·8	21·6	41,500	6,222	289	4,789	718	33·3	949	3,840	576	26·7	989	148	6·9	2,851	427	19·8	21	20	59
K	4·44	Barley and Italian rye-grass	22·0	4·9	10,750	2,421	488	1,240	279	56·4	246	994	224	45·2	373	84	17·0	621	140	28·0	30	20	50
L	2·87	Mangold and savoys	53·8	18·7	36,530	12,728	679	4,215	1473	78·3	835	3,380	117	62·8	301	105	5·6	3,079	1073	57·2	7	20	73
M	3·17	Cabbage	65·7	20·7	16,950	5,346	258	1,956	617	29·8	387	1,569	495	23·9	368	116	5·6	1,201	383	18·3	19	20	61
N	4·15	Broccoli and Italian rye-grass	201·7	48·6	37,950	9,145	188	4,379	1055	21·7	867	3,512	846	17·4	2,350	566	11·7	1,162	280	5·7	54	20	26
O	5·92	Wheat	20·7	3·5	21,100	3,564	1021	2,435	411	117·6	482	1,953	330	94·3	472	80	22·8	1,481	250	71·5	19	20	51
P	3·50	Hardy greens, savoys, and cabbage	67·8	19·4	34,750	9,927	512	4,010	1146	59·1	794	3,216	919	47·4	380	109	5·6	2,836	810	41·8	9	20	71
Q	2·34	Barley, savoys, and cabbage	29·4	12·6	2,100	897	71	242	103	8·2	43	194	83	6·6	191	82	6·5	3	1	·1	79	20	1
R	2·52	Mangold	46·6	18·5	10,280	4,079	221	1,186	471	25·5	235	951	377	20·4	261	104	5·6	690	273	14·8	21	20	59
S	·22	Onions and hardy greens	3·9	17·7	550	2,500	141	63	286	16·2	12	51	232	13·1	21	95	5·4	30	137	7·7	33	20	47
U	2·53	Dwarf and runner beans and sprouting broccoli	20·6	8·1	17,450	6,897	847	2,013	796	97·7	399	1,614	638	78·3	156	62	7·6	1,458	576	70·7	8	20	72
V	4·93	White broccoli, cabbage, hardy greens, and barley	61·5	12·5	8,750	1,775	142	1,009	205	16·4	200	809	164	13·2	419	85	6·8	390	79	6·4	41	20	39
W	2·87	Oats and hardy greens	16·2	5·6	9,600	3,345	593	1,107	386	68·3	219	888	310	54·8	275	96	17·0	613	214	37·8	20	20	60
X	3·86	Mangold and savoys	94·1	24·4	26,600	6,891	283	3,06	795	32·6	608	2,461	638	26·2	527	137	5·6	1,934	501	20·6	19	20	61
Y	5·60	Hay (equal to four and a half times this quantity when green)	24·6	4·4	5,150	920	209	594	106	24·1	118	476	85	19·3	1,102	197	44·8	.....	.....	.....	136	20	
	107·55		1704·0	15·85	523,810	4,870	307	60,438	562	35·5	11,973	48,465	451	28·5	15704	146	9·2	32,761	350	19·2	26	20	54

The figures in columns marked thus (*) are to be considered only as approximate (for reasons stated in the last Report), with the exception of the grand totals.

TABLE VI.—*Breton's Sewage-Farm.*

Summary from Crops gathered during the period from March 25, 1872, to March 24, 1873, showing the quantity of each kind of Produce and the Sewage applied thereto.

[N.B.—The Sewage here stated is only that applied during the above period. In some cases, therefore, it does not represent the total quantity applied to the Crops.]

Crop.	Total acreage of each description of crop.	Produce of each crop.		Sewage applied to crops.		Sewage applied per ton of produce.	Nitrogen.				
		Total.	Per acre.	Total. *	Per acre. *		Quantity applied in sewage. *	Quantity escaping in effluent water. *	Quantity estimated in crops.		Not accounted for (in standing crops, soil, &c.). *
									Per acre.	Total.	
	acres.	tons.	tons.	tons.	tons.	tons.	lbs.	lbs.		lbs.	lbs.
Italian rye-grass	22·99	323·4	14·1	84,100	3658	26	9,702	1,923	0·54	3,912	3,867
Hay (meadow)	5·60	24·6	4·4	5,150	920	209	594	118	2·00	1,102	.....
Cabbage	27·78	512·6	18·5	94,700	341	185	10,924	2,164	0·25	2,871	5,889
Hardy greens	17·16	138·0	8·0	38,950	2269	282	4,493	890	0·25	773	2,830
Savoys	7·33	113·0	15·4	13,330	1819	118	1,538	305	0·25	633	600
Brussels sprouts	·70	5·8	8·3	950	1357	164	110	22	0·25	32	56
Broccoli	3·55	41·7	11·7	.....	.....	.....	.....	.....	0·25	234	.....
Cauliflowers	·23	5·8	25·2	450	1957	78	52	10	0·25	32	10
Kohl rabi	·23	1·5	6·5	400	1739	267	46	9	0·375	13	24
Beans (dwarf and runner)	2·53	7·2	2·8	7,900	3123	1097	911	181	0·50	81	649
Peas	10·00	25·8	2·6	16,450	1645	638	1,898	376	3·40	1,965	.....
Carrots	3·72	55·0	14·8	3,700	995	67	426	84	0·20	246	96
Mangold	6·65	148·1	21·6	11,750	1715	79	1,356	269	0·25	829	258
Onions	9·45	121·0	12·8	16,600	1756	137	1,915	379	0·22	596	940
Potatoes	3·77	17·5	4·6	1,400	371	80	162	32	0·25	98	32
Oats	11·90	grain 12·6 straw 27·7	1·1 2·3	.....	.....	.....	.....	.....	grain 2·0 straw 0·6	937	.....
Barley	8·36	grain 6·8 straw 18·3	0·8 2·2	.....	.....	.....	.....	.....	grain 1·6 straw 0·5	448	.....
Wheat	5·92	grain 7·2 straw 13·5	1·2 2·3	.....	.....	.....	.....	.....	grain 1·8 straw 0·6	472	.....
Strawberries	2·17	0·2	0·1	1,100	507	5500	127	25	0·1	.....	102
Mixed crops—cabbage and hardy greens and savoys ...	6·16	76·7	12·4	21,300	3458	278	2,456	486	0·25	430	1,540
To fallow land and to land under crop March 25, 1873	85·25	.....	.....	205,580	2412	.....	23,728	4,700	.....	.....	19,028
Total	241·65	1704·0	.....	523,810	.....	307	60,438	11,973	.....	15,704	32,761
						per cent. ...	100	20	.....	26	54

The figures in columns marked thus (*) are to be considered as approximations (for reasons stated in the last Report), with the exception of the grand totals.

TABLE VII.—*Breton's Sewage-Farm.*

Comparative Statement of Crops on Land and Land lying fallow on March 24, 1872, and March 24, 1873 respectively.

Plot.	March 24, 1872.			March 24, 1873.		
	Acreage.	Area in crop.	Area fallow.	Acreage.	Area in crop.	Area fallow.
	acres.	acres.	acres.	acres.	acres.	acres.
A	9·8	.....	9·8	9·79	9·79	.....
B	12·1	12·1	.....	12·12	12·12	.....
C	2·0	.....	2·0	1·97	1·97	.....
D	6·9	.....	6·9	6·93	6·93	.....
E	5·8	2·2	3·6	5·76	5·66	.....
F	3·82	.....	3·82	3·82	1·48	2·34
G	5·17	1·88	3·29	5·17	2·82	2·35
H	6·4	6·4	.....	6·4	6·5	.....
I	6·67	3·40	3·27	6·67	.....	6·67
K	4·03	.....	4·03	4·44	4·44	.....
L	.....	.....	.....	New plot. 2·87	.....	2·87
M	3·36	3·36	.....	3·17	3·17	.....
N	4·15	0·52	3·63	4·15	4·15	.....
O	5·92	.....	5·92	5·92	5·92	.....
P	3·50	.....	3·50	3·50	3·50	.....
Q	1·60	.....	1·60	2·34	1·04	1·0
R	2·52	.....	2·52	2·52	·12	2·40
S	0·33	.....	0·33	0·22	·22	.....
T	0·34	.....	0·34	Plot converted into a pig-run.		
U	2·53	2·53	.....	2·53	2·53	.....
V	4·48	2·50	1·98	4·93	2·93	2·00
W	3·00	.....	3·00	2·87	2·87	.....
X	3·86	.....	3·86	3·86	3·86	.....
Y	5·60	5·60	.....	5·60	5·60	.....
	103·88	40·49	63·39	107·55	87·62	19·93

TABLE VIII.

A Statement showing the population in the Town Ward of Romford, with and without connexion with the main sewer running to Breton's Farm.

Detail.	Houses.		Water closets.		Other closets.		Slops.		Number of inhabitants.	Water-supply.		Remarks.
	Occupied.	Empty.	Connected.	Not connected.	Connected.	Not connected.	Connected.	Not connected.		Water company.	Private.	
Fully connected	951	59	235	...	786	...	850	..	4438	328	683	All connected except the 15 closets.
Partly connected	15	...	9	...	10	15	15	...	112	7	8	
Slops only connected	40	...	...	23	...	58	26	...	747	5	35	Including the Union 328 inhabitants.
Without any connexion	182	12	...	9	...	205	...	191	890	44	149	
Without any connexion, that stand on the outskirts of the Town Ward	30	1	...	7	...	36	...	31	151	...	31	Being Marshalls Park, Pelhs Lane, and Rush Green.
General Total	1218	72	242	39	796	314	891	222	6338	384	906	

(Signed) HY. CULLEN, May 4, 1873.

An Abstract of the Four Reports already presented to the Association by the Committee. Prepared for the Committee by Professor CORFIELD.

In the following Abstract of the four Reports already presented to the Association by the Committee, I have thought it best to bring together the results of the Committee's investigations under a few heads; so that each division of the subject may appear in the Abstract complete in itself, and not split up into portions, as would have been the case had each Report been abstracted separately.

I. *Conservancy Plans.*

A series of Reports "from foreign countries respecting the practices prevailing abroad for disposing of the refuse of towns, villages, public institutions, factories, dwellings, &c., and having reference to the sanitary condition of the districts in which they are situated, the state of rivers, or the support and increase of the produce of the soil," was obtained by the Committee from Her Majesty's Secretary of State for the Home Department; and from these it appeared "that in most cases (both in town and country places) the use of privies is very general, water-closets being rare even in large towns, and that the usual method of dealing with human excreta is to allow them to collect in pits (Abtrittsgruben, fosses), which are sometimes drained either naturally by the permeable character of the soil, or artificially, so that most or all of the liquid portion of the contents of the pits flows away or infiltrates the surrounding soil." (Report I. 1869, pp. 318–321.)

Information was also obtained from 107 places in the United Kingdom, having an aggregate population of more than four millions. It was found that in 42 of these the privy and ash-pit system was general, and in 25 partial; while in 71 places out of the 107 the liquid refuse of the town was discharged into the adjoining stream or river, and in two instances into pools of water. (Report I. p. 325.)

In the Second Report the returns from 200 towns, "recording the existing arrangements of water-supply, sewerage, scavengering, and disposal of refuse," are tabulated—the result being that there were 70 of these towns where "privies very greatly exceed water-closets in number," and 75 in which "privies are still much used." (Report II. 1870, p. 53.)

Privies, both in England and abroad, were found to be frequently built over rivers (Report I. pp. 318–321, and Report II. p. 59); and in some towns many houses are without any provision whatever for the removal of the excremental matters.

It was found that in only two instances in England, and one in Scotland, was any profit derived "from the sale of ashes and excretal and other solid refuse," the losses of some towns being considerable (Report II. p. 55). The Committee specially investigated the ash-pit system as carried out at the town of Bury in Lancashire, for two reasons—"because it is a town where it may be said there are no water-closets," and because "the almost total absence of water-closets" would enable the Committee, by examining the liquid escape into, and discharge from, the sewers, to judge whether any of the proposed methods of intercepting fæcal matter from the sewers (such, for instance, as the earth-closet) would in themselves be either a solution of the great sewage question, or even one considerable step towards it.

It was found that the privy accommodation of the lower classes of houses was very insufficient—that the removal of the mixed night-soil was found to be difficult and expensive, so that the quantity obtained from the whole town only realized £100 per annum—and that, in spite of the fact that so much refuse matter was kept out of the sewers, the sewage during the greater part of the day was, as shown by chemical analysis, only a little weaker than that of a water-closeted town usually is, while during the forenoon it was invariably "very thick, black, and greasy," and "smelt very bad;" and the Subcommittee appointed to consider the matter reported, "that although the sewage from a town managed on the Bury system is weaker, and therefore less valuable, and proportionately more difficult to deal with than the sewage from a water-closeted town, yet that its purification is just as imperatively necessary." The Subcommittee considered that "the figures obtained in Bury, of the ash-pit system as carried out there, prove that financially it is, so far as Bury is concerned, a total and complete failure, as the gross return is only a little over one halfpenny per head of the population annually."

In many towns, especially abroad, portable or fixed reservoirs (fosses) for the collection of excretal matters, unmixed with other substances, are in general use. Sometimes they are drained into sewers, and sometimes so constructed as to collect both liquid and solid refuse, contrivances to separate the solid from the liquid excreta being sometimes employed. These reservoirs are "frequently ventilated by means of shafts rising above the house-tops." The fixed reservoirs are emptied periodically, their contents either being "simply dipped out," or "removed either by pumping into closed tank-carts with lift-pumps, or by means of a vacuum previously produced in the tank-cart." The portable reservoirs are removed bodily with their contents, and replaced by empty ones. At some towns a profit is realized, while at others a loss is entailed; but the communications received from foreign countries afforded "abundant evidence that, wherever the subject has been considered, there is a strong though vague sense of the injury to health resulting from the accumulation of excretal materials in pits &c. within populous districts, by the impregnation of the soil, by the pollution of rivers and well-water with drainage from such accumulations, or from the discharge of excretal materials into watercourses directly or indirectly." (Report I. pp. 321 & 322.)

Dry Earth System.

In the 200 scheduled towns before referred to, only 446 earth-closets were reported to exist. The Committee inquired into the results of this system at several places, but only obtained a return from Lancaster, "the only place where an attempt has been made to carry out the system on a large scale," where it appeared that the system was not thoroughly carried out, some of the essential conditions to its success being entirely neglected. Thus, instead of the dried earth being used *in detail*, "a quantity of soil is thrown once a day on the matters collected; and the result is that the product is removed in a very offensive condition." When it is stated that about 2½ lbs. of soil were used per head per day, and that the manure was afterwards mixed with other town refuse, it is not surprising that it only fetched 5*s.* a ton, and that its analysis showed "that it did not contain more nitrogen than good garden-mould," and that, on being applied to grass land at the rate of about six tons per acre, "the produce of hay was by no means large."

Dr. Gilbert conducted, on behalf of the Committee, some experiments with Moule's earth system. The result showed that earth which had been used even *three times* in the closet could only be considered to be a rich garden-mould; and the Committee remarked "that such a manure, even if disposed of free of charge, would bear carriage to a very short distance only."

The following Table shows the results of these analyses, as far as the nitrogen is concerned :—

	Before used.	After using once.	After using twice.	After using three times.
Percentage of nitrogen in soil dried at 100° C.	0·073	0·240	0·383	0·446

While the Committee considered that any such system was impracticable for large populations, on account of the amount of earth that would be required to be carted in and out daily, they added, "It may readily be admitted that it would be a great advantage, in a sanitary point of view, in the cases of sick rooms, detached houses, or even villages, and that it might be even economical where the earth for preparation and absorption and the land for utilization are in close proximity." (Report III. pp. 187 & 188, and Report IV. p. 143.)

II. *Water-Carriage System.*

This is only carried out in a few foreign towns (Report I. pp. 321–323). Of the 107 places reported on by the Committee in their First Report, there were only 11 without any system of sewerage at all; 48 were completely sewered, and 48 partially so. In 42 places water-closets were found to be general, and in 25 adopted partially. In the 200 towns scheduled in the Second Report, there were 44 in which water-closets were found to be general, and 75 in which they existed in considerable number; but there were only 11 of these 200 towns also which were totally unprovided with sewers, and in which the liquid refuse of various sorts found its way into surface-streams or was absorbed by the subsoil—a sufficient proof that Conservancy plans do not get rid of the necessity of having sewerage arrangements in towns.

As to water-supply, the sources appear to be exceedingly various. "In the 200 scheduled towns there are 90 wholly dependent on a public or general supply, 22 on private sources only, and 88 partly on private sources in addition to a public supply." The quantity supplied varied from 10 to 60 gallons per head per day, a large number of towns having a supply varying from 20 to 30 gallons per head. Storm and surface-waters are, as a rule, received into the sewers; but sometimes, "where new systems of sewerage have been adopted, the old sewers are entirely devoted to the discharge of surface-waters; in one instance special sewers are appropriated to the same purpose, while in 11 other instances the old surface-channels are used."

It was found that in about 100 out of the 200 towns the sewers drained the subsoil. The 200 towns were arranged in three classes as follows :—

	Towns.	Population.
I. Towns having a complete system of underground sewerage, a general water-supply, and a general adoption of water-closets discharging into the sewers	44	1,154,600
*II. Towns having a system of underground sewerage with water-supply, and only a partial adoption of water-closets	145	5,785,840
III. Towns with no system of underground sewerage	11	218,800
Total	200	7,159,240

* In this class there are some towns with as few as six water-closets only.

Only vague information was obtained about the ventilation of the sewers, "owing to the fact that very few instances exist in which any thing has been systematically done." The Committee in their First Report stated that this was a most important matter for consideration, and "that it would be in the highest degree desirable to institute an inquiry into the nature of the gaseous emanations from the sewers in various places" (Report I. p. 330). They consequently instituted some experiments, both chemical and microscopical, on air collected from some sewers in Paddington. Dr. Russell's analyses showed that the carbonic acid varied from 0·12 to 0·51 per cent., and the oxygen from 20·7 to 20·91 per cent.; while no combustible gases were detected, and only a trace of ammonia could be discovered in water through which a large quantity of sewer air had been passed. The remark is made that "these experiments must be looked upon as simply tentative, but certainly indicate a purer air in these sewers than might have been anticipated." The microscopical examination conducted by Mr. Cooke showed that the suspended substances which were collected by passing air through tubes containing plugs of cotton-wool were very various, and consisted of inorganic matters, a few starch-granules, and spores of various sizes, together with fragments of cellular tissue, woody fibre, fibrils of feathers, &c. The general results, however, indicated "comparative freedom from organic bodies." (Report II. pp. 72–75.)

The Committee made a special investigation into the sewerage arrangements of the town of Cambridge, where water-closets are general, though not universal. The outlets of all the sewers were found to be under the level of the surface-water in the Cam, so that "the sewage is backed up in the sewers for a considerable distance; and the subsoil is constantly saturated with both water and sewage in the lowest parts of the town." As many of the sewers are old and of irregular shape, much escape into the subsoil takes place. "Inquiries were made into the state of some of the wells belonging to private houses, and it was found that they were all contaminated by sewage, owing to their proximity to the sewers in the streets and to the drains on the premises, so much so, that the water cannot be used for drinking but only for washing." The remarks made on the subject by the Subcommittee, consisting of Messrs. Grantham (Chairman), Corfield, Hope, and Williamson, finish as follows :—

"The chief general importance of the inquiry into the conditions of Cambridge is the proof thus obtained of the pollution of wells, and therefore of subsoil, by the agency of previous street- or house-sewers constructed in their vicinity; and the Subcommittee desires to give expression to the conviction forced upon it in the course of its inquiries, that all sewers, properly so called (that is to say, drains into which refuse from human habitations is admitted), ought to be constructed of materials which are altogether impervious, and that a separate system of pervious drains, similar to agricultural drains, should be constructed where necessary to dry the subsoil. The Subcommittee is of opinion that the further construction of pervious sewers should be prohibited by Parliamentary enactment." (Report II. p. 61.)

The amount of sewage discharged varied in the 200 towns scheduled, partly with the amount of the water-supply, and partly with the amount of surface or subsoil waters admitted into the sewers—the largest quantity being in the case of the town of Hertford, where the discharge per head per diem amounted to 257 gallons, the water-supply being only 61½ gallons; and the smallest, amounting to only six gallons per head per diem, being recorded in one or two instances.

Treatment of Sewage.

"Of the 189 towns and districts having systems of sewerage, 143 discharge their sewage without any treatment whatever; in 17 instances the sewage is simply filtered before discharge, in 7 instances it is chemically treated, and in 17 cases recourse is had to irrigation, whilst in 5 instances the system of disposal includes more than one of these methods." By "simple filtration" is generally meant mere straining, a method obviously insufficient for the purification of sewage.

Certain processes for precipitating the valuable materials contained in sewage were investigated by the Committee with the following results:—

I. *The Phosphate Process* of Messrs. Forbes and Price, which consists in the addition to the sewage of (*a*) a mixture of native phosphate of alumina and sulphuric acid, and (*b*) sufficient milk of lime to neutralize the sewage. The result was that during its passage through a large tank "the suspended matters were very completely deposited, and the supernatant water ran over the sloping edge of the tank at its extreme end bright and clear and almost odourless." It was found that the water did not putrefy, even after the lapse of four months, that it contained only the merest trace of phosphoric acid, no sulphuretted hydrogen, nor any nitrates nor nitrites, but that it contained "as much actual ammonia as ordinary dilute London sewage, and also a certain "amount of albumenoid ammonia." The precipitate had no offensive smell. The valuable constituents of sewage, with the exception of the suspended matters and the phosphoric acid, are not precipitated by this process, and cannot be utilized unless the effluent water be afterwards used for irrigation, in which case the milk of lime would not be added, and the clarified sewage would still contain a quantity of phosphoric acid.

"The advantage of this use of it, if it were found to answer from an economical point of view, would be the deodorization of the deposit in the tanks and of the sewage itself, which is certainly at present a great desideratum, especially as regards the tanks." (Report III. pp. 185–187.)

II. *Whitthread's Patent.*—Experiment was made on 100 gallons of Romford sewage with one pound of the mixture used in this process—a mixture which was stated to consist of dicalcic and monocalcic phosphate, two equivalents of the former to one of the latter, a little milk of lime being afterwards added. The result was a very rapid precipitation, the supernatant water remaining nearly clear and quite inoffensive. The precipitate, dried at 100° C., contained as much as 3 per cent. of ammonia and a considerable quantity of phosphate of lime. The supernatant water contained rather more actual ammonia than the original sewage, but scarcely any organic nitrogen, showing that the organic matters in solution, as well as those in suspension, had been almost entirely removed by the process. This water contained, however, a considerable quantity of phosphoric acid, which would be valuable if the water were afterwards used to irrigate land; "but, unless means are devised for separating it, it would constitute a serious loss if the water were thrown away." It must be added that this was regarded merely as a preliminary experiment.

III. *General Scott's Process.*—This was investigated at Ealing. It consists in the addition to the sewage, while in the sewers, of a mixture of lime and clay, in the proportion of about 10 cwt. of the former and 5 cwt. of the latter to 400,000 gallons of sewage. The result was a very complete precipitation of the suspended matters, which were collected in tanks, the supernatant water being passed upwards through filter-beds, and discharged into the river. The sludge from the tanks is drawn off from time to time, partially dried by an hydraulic press, and then burnt in a kiln, no additional fuel being necessary after the fire is once started, as the dried sludge contains sufficient organic matter to burn the deposit. The result is the production of cement. It was found that the sewage was rendered inodorous while in the sewers, and that the whole process was inoffensive.

The Committee considered that "on the whole this process, when perfected, promises well as a means of treating one of the difficulties of the sewage question—the disposal of the sludge precipitated from sewage. It appears not only possible to destroy the solid matters by fire, but also to secure some return from their use in the manufacture of cement."

They found, however, that the effluent water contained organic matters in solution as well as ammonia; so that this process cannot be considered as sufficient of itself for the purification of sewage, nor for its utilization, but only as one for satisfactorily getting rid of the offensive sludge which otherwise accumulates in the tanks.

Filtration.

Upward Filtration.—The process of upward filtration through gravel was examined at Ealing when General Scott's process was in abeyance. It was found that this process, whether accompanied or not by the addition of a deodorizing mixture to the sewage in the sewers in the town, "effected only a very slight purification of the sewage, which left the filter still a sewage of average strength. It was not even clarified." This observation thus confirmed the results of experiments previously carried out by the Rivers' Pollution Commissioners.

Weare's Process.—This process, which is employed at the Workhouse at Stoke-upon-Trent, where the water-supply is very scanty and the sewage consequently remarkably strong, consists in the filtration of the sewage through coarse ashes and charcoal contained in the tanks through which it passes successively. It appeared to be considerably purified; but still the effluent water after passing through the deodorizing tanks is described by Dr. Russell as having a strong smell of sewage. It is also to be observed that no nitrates were found in this water, thus showing that no oxidation had taken place. From the fact that the flow of effluent water was only about 2000 gallons as against 5000 gallons of sewage in the 24 hours, and that the chlorine was reduced to nearly half its original amount, the reduction taking place almost entirely in the first or so-called fæcal tank, it would appear that a considerable dilution must in some way have taken place, accompanied by a very considerable and unexplained escape, which amounted, even supposing there were no dilution, to three fifths of the total amount.

Intermittent downward Filtration.

This process was examined at Troedyrhyw, near Merthyr Tidfyl, where an area of about 20 acres has been converted into a filter-bed for the purification of the sewage of the town of Merthyr Tidfyl. The soil consists chiefly of gravel and sand, having a vegetable-mould on the surface. It is extremely porous. The land is drained at a depth of less than 7 feet, the drains being brought together at the lowest corner, where the effluent water is discharged into an open drain leading to the river Taff. "The area is laid out in square beds, intersected with roads and paths, along which are constructed the main carriers which receive the sewage from the outfall-sewer, and distribute it over the beds."

The sewage, after being screened through a bed of "slag," in which the larger suspended matters are arrested, is turned on to one of the four plots into which the area is divided, and allowed to run on this plot for six hours, when it is turned on to another one. Thus each of these four plots has 18 hours for rest and aëration of the soil. The surface of the area is laid up in ridges, and cabbages and other vegetables planted along them, the sewage running in furrows between.

The main results of the examinations which took place in January and in July, extending over seven and eight days respectively, were:—that the effluent water discharged was very largely diluted with subsoil-water which had percolated through from the river-bed (this was proved both by the gaugings and by the analyses, and had been already observed by the Rivers' Pollution Commissioners); that the effluent water was very satisfactorily purified, the nitrogen in solution appearing in the form of nitrates and nitrites—a sufficient proof that a considerable amount of oxidation goes on in the filter-beds.

Upon a comparison of the total nitrogen in solution in the sewage, in the effluent water, and in the subsoil-water (which was also analysed), it was found that the amount in the effluent water was almost exactly the amount that would be present in the sewage if diluted with the amount of subsoil-water (rather more than its own volume) with which the analyses and the gaugings showed it to have been diluted; that is to say, that a quantity of nitrogen equal to the amount in solution in the sewage escaped in the effluent water, and was lost (escaping, however, almost entirely in the oxidized and innocuous form of nitrates, &c.), the amount retained in the soil and by the plants being, therefore, equal to the amount in the suspended matters of the sewage. The effluent water was not quite so pure in the summer as in the winter: in the former case four fifths, and in the latter twelve thirteenths of the nitrogen contained in it was in the form of nitrates and nitrites.

The sewage was cooled by its percolation through the soil; in the winter from 48° F. to 46° F. (the temperature of the subsoil-water being 42° F.), and in the summer from 60° F. to 55° F.

The crops grown on the surface of the filter-beds were successful, and realized very good prices.

Irrigation.

In the First Report of the Committee a list of fifteen places where irrigation was practised was given, and a list of twelve more where it was contemplated; and it was stated that the areas used for irrigation varied from 0·4 of an acre to ten or twelve acres per thousand of the population, the distance of the land from the lowest outfall sewer of the town varying from 100 yards to upwards of a mile.

The general result was reported to be as follows:—"At most places the application of the sewage to land has been found to exercise a most beneficial influence on the condition of the streams and rivers receiving the drainage of the district."

"Generally speaking no objections appear to have been made to the application of sewage for irrigation; and where such objections have been urged on the ground that the application was offensive and injurious, they do not appear to have been supported by medical authority, and in several instances they have ceased. As regards the sanitary condition of these districts, it appears that in most cases the application of sewage for irrigation has not been attended with any apparent change; but there is said to be a marked improvement at Braintree."

"It is probable that . . . the application of liquid sewage to land would become a source of revenue to towns only under specially favourable circumstances, and that, in opposition to the opinions which have been somewhat hastily formed in certain cases, it will more frequently entail some amount of expenditure on the towns themselves. At the same time the benefit to land, and the improvement in the condition of rivers, to be realized by the mode of dealing with liquid sewage, can scarcely be matter of doubt or uncertainty any longer."

Of the 200 towns tabulated in the Second Report, 19 had recourse to irrigation either wholly or partially or in connexion with some precipitation process; and in one case, that of Leamington, irrigation was intended, and has since been carried out.

Owing to the fact that one of its members is the lessee of Breton's Farm, near Romford, in Essex, the Committee has had the advantage of making continuous investigations of the results of irrigation with this particular farm for the past three years, results which are detailed in the Annual Reports. Special investigations have also been made with the following results:—At the farms at Tunbridge Wells, where the sewage is applied to the surface of the land on the Catch-water System, and where under-drainage has not been systematically carried out (the drains which already exist having, in fact, been brought up to the surface to empty into the carriers), the purification of the sewage cannot be said to be satisfactory; for although a considerable dilution with subsoil-water takes place, the water which has passed over the land is still impure, and, moreover, contains scarcely any nitrates, thus showing that very little oxidizing action takes place.

The same result was found at the Reigate Farm at Earlswood, where the state of the effluent water was still more unsatisfactory; in fact, in one instance, it was found that sewage which had passed over the fields was actually stronger, except as regards actual ammonia (*i. e.* it contained more of the total solids in solution with more nitrogenous organic matters), than it was after passing over only the first of these fields—thus showing that the ground was so saturated with sewage, that any additional sewage passed on to it could "only concentrate itself by evaporation or by solution of matters in the upper layer of the soil." (Report III. pp. 181 to 185.)

These farms were again inspected in the following year. It was found that the effluent water was running clear and free from smell. No analyses were, however, made at this time. The crops included oats, beans, and wheat, as well as meadow-grass and Italian rye-grass, and seemed to be in a satisfactory condition; but no general system of subsoil-drainage had been commenced. A comparison was made in January 1871, during severe frost, of the results obtained in the purification of sewage at the three following farms:—Breton's Farm, near Romford, Beddington Farm, Croydon, and Norwood Farm. It was found that in the latter two cases, where the sewage was passed over the land on the Catch-water System, it was not satisfactorily purified, the nitrogen escaping in the effluent water being only partially in the state of nitrates and nitrites; while in Breton's Farm, where the sewage passes through the soil, the farm being in effect a large filter-bed, "(1) oxidation goes on in winter as well as in summer, and almost all nitrogen lost is lost in an oxidized and inoffensive form, and (2) this loss is very slightly greater in winter with a very strong sewage than in summer with a weaker one; so that sewaging in the winter would appear to entail no extra loss of manure."

It was also observed that while in summer sewage is cooled by percola-

tion through the soil, and almost always heated (sometimes considerably so) by surface-flow, as was observed both at Tunbridge Wells and at Earlswood (the temperature of the effluent water in the latter case being actually 5° F. higher than that of the sewage), in winter, on the other hand, the cooling which takes place is less with percolation through the soil than with surface-flow in both instances; so that "these results are favourable to percolation through the soil, as opposed to mere surface-flow, both in summer and winter. Percolation causes a considerable cooling in the summer, while in winter it does not cool the effluent water so much as surface-flow does."

These results induced the Committee to make the following distinct statement in their Third Report, p. 185:—"It may seem almost superfluous for the Committee, after so many years of general experience throughout the country, to argue in favour of the subsoil drainage of naturally heavy or naturally wet land with impervious subsoil for purposes of ordinary agriculture; but some persons have strongly and repeatedly called in question the necessity of draining land when irrigated with sewage; and the two farms at Tunbridge Wells, to a great extent, and more especially the Reigate Farm at Earlswood, have been actually laid out for sewage-irrigation on what may be called the 'saturation principle;' so that it appears to the Committee desirable to call attention to the fact, that if drainage is necessary where no water is artificially supplied to the soil, it cannot be less necessary after an addition to the rainfall of 100 or 200 per cent. But a comparison of the analyses of different samples of effluent waters which have been taken by the Committee from open ditches into which effluent water was overflowing off saturated land, and from subsoil-drains into which effluent water was intermittently percolating through several feet of soil, suggests grave doubts whether effluent water ought ever to be permitted to escape before it has percolated through the soil."

At Breton's Farm, where the sewage of the town of Romford, with a population of 6338 (a little more than two thirds of which only discharge their refuse into the sewers, the previous estimates having been all too high), is utilized upon 121 acres of land, there are special advantages for accurate investigation. The soil, which was very poor, consisting in many parts almost entirely of gravel (as will be seen by the analysis already quoted from the Committee's Second Report), was laid out in rectangular beds on the Ridge-and-Furrow System, the "beds" or "lands," each 30 feet in width, running at right angles to the main carriers which distribute the sewage. The sewage, when it arrives on the farm, is received in one of two tanks, where a deposit takes place and a scum forms on the surface. The liquid is run out between these into the pumping-well, and is raised by a pump "to a height of about 25 feet into iron troughs supported on wooden tressels, which convey the sewage to all parts of the farm, by discharging it either directly into the gutters or grips formed on the ridges of the 'lands,' and out of which the sewage is distributed right and left down the slightly inclined slopes of the lands, or, in the first instance, into concrete carriers, raised by earth banks to a height intermediate between the height of the iron troughs and the level of the ground." (Report II. p. 62.) "About 85 acres of the farm, which are above the level of the tank, have been underdrained by pipe-drains 50 yards apart, and from 5 to 6 feet in depth, in such a manner that the water from the drains can be discharged into the sewage-tank if required in dry weather, or at pleasure into the river Rom."

This arrangement afforded excellent opportunities for the gauging of the effluent water.

In the Second Report will be found a detailed account of the crops grown and the prices obtained.

Some of the earliest experiments made by the Committee related to "the capacity of earth laid out in beds of 30 feet wide for the absorption of liquid." Three different kinds of gauges were used, and a time was chosen "when the land was in what may be considered an average state of moisture." From these experiments "it resulted that land in the state of moisture which existed on the 19th March [1870] and laid out in beds of 30 feet wide would only absorb, when consolidated, on the surface about 40 tons of liquid per acre, and when stirred to a depth of 9 inches on the previous day, about 90 to 110 tons per acre. By the word 'absorb' is meant that no more than the above quantities could be applied without the formation of puddles at the sides of the beds." (Report II. p. 69.) It was considered that 400 tons per acre was probably the largest quantity that had ever been applied in any one dressing, and that the assumption that the first dressing all over the farm was at the rate of 400 tons per acre, the second at that of 200, and the subsequent ones at that of 100 was probably not far from the truth.

In the Third Report, p. 175, will be found a summary of the results of the gaugings of the sewage and effluent water from June 12th, 1870, to July 15, 1871 (a period of 399 days). It appears that the average quantity of sewage received from the town per day was 1029 tons, to which something must be added for night-sewage which was allowed to run on to the meadows between the farm and the town. After the 15th of April, when the new tanks were completed and all the sewage received on the farm, the total amount was found to be 1262½ tons in the 24 hours, 621½ tons of which came during the working day of ten hours, and the remaining 641½ during the night of 14 hours. These quantities, when computed for a day and night of 12 hours each, give day-sewage 729 tons, night-sewage 533½.

The sewage as pumped contains a certain amount of effluent water that has been brought back into the tanks. The average amount of this diluted sewage pumped was 1182 tons per day. The effluent water discharged, as far as could be estimated, was about 513½ tons per day. The rainfall during the 399 days was 22·64 inches, equal to 2287 tons per acre.

The experiments on the temperature of the sewage and effluent water are very important. The temperature of these liquids is very uniform when compared with that of the air, "being lower during extreme heat, and higher during extreme cold." "The ranges and variation over the total period have been:—

"Atmosphere	28°·5	to	76°	= 47°·5 F.
Town-sewage	43	„	66	= 23
Sewage pumped	43	„	67	= 24
Effluent water	41	„	64	= 23"

In one week during a severe frost, "when the mean noonday temperature was 28°·5 F., that of the sewage pumped and effluent water was 43° F." (Report III. p. 176.)

The Fourth Report gave the results of the observations carried on from March 25, 1871, to March 24, 1872, both days inclusive; and gave a more special account of the analyses of the sewage and effluent water during that period. The analyses were made of average samples—that is to say, of samples taken in proportion to the rate of flow of the sewage at the times as indicated by the gaugings.

The general results were:—

Sewage from the town	416,787	tons.
Effluent water returned to the tanks	52,466	,,
Therefore Diluted sewage	469,253	,,
Of which, Amount utilized	385,291	,,
Amount merely filtered	83,962	,,

As to the composition of the sewage and effluent water, the average amount of nitrogen for 100,000 tons in the diluted sewage pumped was 5·529 tons; that in the effluent water 1·147.

*As the total amount of diluted sewage was	380,277	tons,
And the total effluent water	195,536	,,

it follows that "the proportion of nitrogen escaping in the effluent water to the total quantity applied is therefore ·1067, or about one tenth."

An estimate was also made of the amount of nitrogen recovered in the crops; the general result of the whole being that of 100 parts of nitrogen in the sewage pumped, 42 were recovered in the crops, 11 lost in the effluent water, and 47 not accounted for—that is to say, remaining in the soil or escaping into deeper subsoil-waters. (See accompanying Report.)

Some experiments were also made with the view of inquiring into the possibility of the distribution of entozoic disease by means of sewage-irrigation. Some "slime and mud" from the bottom and sides of carriers at Earlswood Farm was examined by Mr. M. C. Cooke, who found that it contained life of various kinds, especially Annelida, but did not detect any entozoic larvæ. The existence of this slime at the bottom of the carriers here was attributed by the Committee "to the fact that the subsoil is kept in a saturated condition by the want of underdraining;" and they were of opinion "that when land is thus saturated with sewage, certain atmospheric conditions exist which may be attended by malaria more or less injurious to health." (Report III. p. 182.)

Dr. Cobbold was requested by the Committee to examine, in conjunction with Professor Marshall and the writer, the carcass of an ox fed for two years on sewage-grown grass. It was found to be, as he reports, free from internal parasites of any kind. All the viscera, together with portions of numerous muscles, "with their associated areolar and aponeurotic coverings," were carefully examined. He observed that the conditions were favourable to this result, inasmuch as (1) the grass &c. was cut and carried, and the animal was not grazed on the farm; (2) the soil is very porous; (3) mollusca, so often the intermediary bearers of entozoal larvæ, were scarce; (4) the only mollusks found (a species of *Limnæa*) contained no cercarian larvæ; (5) the "flaky vegetable tufts" collected from the sides of the furrows contained "numerous active free *Nematodes*, but no ova of any true entozoon; (6) the sewage probably contained sufficient alcohol to destroy the larvæ. The Committee agreed with all these observations except the last.

The absence of mollusca is most remarkable, and with it must be associated the observations recorded by the Committee of the destruction of wireworms &c. by the sewage. Thus a crop of American oats was seriously damaged and in danger of being destroyed by the ravages of the *Oscinis vastator*, one of the smallest but most destructive of those grubs and wireworms which at times cause such injury to cereal crops in this country. Two heavy dressings of sewage were applied to this bed during two successive days, the result being that the grubs were entirely destroyed and the greater part of the crop saved. (Report II. p. 65.)

Again, at Tonbridge Wells "it was stated that a large field of turnips, being infested with the fly, was flooded with sewage, which drowned the fly and saved the crop, which is expected to turn out well but rather late."

So far, then, as actual facts at present show, "there is no evidence that entozoal forms of life are to be found on the farm at all in any stage of their existence, or in the flesh of an animal fed exclusively for 22 months on sewaged produce grown on the farm." (Report III. p. 189.) As far as the sanitary influence of sewage-farming is concerned, the Committee have returns from eight places where it is at work. In no instance has any disease whatever been traced, either among the labourers on the farm or among the inhabitants in the vicinity, or among the cattle, to the sewage-farm. In two instances it is reported that the health of the neighbourhood has improved, and in several that the land has very much improved in value, and the production of crops is much more certain. The note from Aldershot is, "Sanitary state of Camp and Barracks vastly improved. The land produces fair crops under sewage, which before produced nothing whatever."

Conclusions arrived at by the Committee.

I. All conservancy plans, including midden-heap and cesspool systems, dry ash- and dry earth-closets, pail-closets, &c., are quite incompetent as solutions of the general question of the removal of the refuse matters of a population.

Such plans deal with only a small part of the liquid manure; towns which resort to one of them require, therefore, to be sewered, and the sewage requires to be purified.

The manure produced is in all cases (except in that of simple pails or tubs where no extraneous materials are added) poor, and will only bear the cost of carriage to a short distance, taking into consideration the cost of collection. That produced by the dry earth system is, even after the earth has been used four times over, but little better than a good garden-mould. Such plans, moreover, all violate one of the most important of sanitary laws, which is that all refuse matters which are liable to become injurious to health should be removed instantly and be dealt with afterwards. With all these plans it is an obvious advantage on the score of economy to keep the refuse about the premises as long as possible; and the use of deodorants of various sorts, or even of disinfectants, proves that this is the case, and that these systems all depend upon a fallacious principle. They should therefore be discouraged as much as possible, and only resorted to as temporary expedients, or with small populations under exceptional circumstances.

II. The water-carriage system, on the other hand, is based upon a sound principle, that of removing all the refuse matters at once, and in the cheapest possible manner, by gravitation, and ought to be resorted to in all but the most exceptional cases.

The opinion of the Committee, that all sewers should be made of impervious materials, and that separate drains to dry the subsoil should be constructed where necessary, has already been most emphatically expressed.

The freest possible ventilation of sewers, house-drains, and soil-pipes, in order to prevent accumulations of foul air, is also essential.

With regard to the utilization of sewage, the Committee has come to the conclusion that the precipitation-processes that it has examined are all in-

competent, and necessarily so, to effect more than a separation of a small part of the valuable ingredients of sewage, and that only a partial purification is effected by them. Some of them may, however, be useful as methods of effecting a more rapid and complete separation of the sewage-sludge.

The upward-filtration process only effects a clarification of the sewage, and is therefore no solution of the question.

Weare's charcoal-filtration process, as carried on at Stoke-upon-Trent Workhouse, did not give satisfactory results, the effluent water being in effect weak sewage; an opportunity will, however, soon be given for an examination of this process in a modified form on a much larger scale at Bradford, and under more favourable conditions.

Intermittent downward filtration through soil has been shown at Merthyr Tydfil to afford a means of purifying the sewage under favourable conditions; but it cannot be said to be a method of utilization except to a very partial extent, as the investigations made by the Committee showed that the effluent water contained as much nitrogen as was originally in solution in the sewage, but mainly as nitric acid instead of as ammonia and organic nitrogen. There can be no doubt that the process would prove useful as an adjunct to irrigation, or where a sufficient amount of land for irrigation cannot conveniently be got.

By properly conducted sewage-irrigation a solution is afforded to the question of sewage utilization; it has already been stated that a precipitation-process, or some clarifying process, may be found useful. If such process, however, removes the phosphates from the sewage, it will, if employed for irrigation, require to be supplemented either by the use of the precipitate produced in the settling-tanks, or by that of some other manure supplying phosphoric acid.

In all instances it is essential that the land should be well underdrained, and that the sewage should all pass through the soil and not merely over it; otherwise, as has been shown, it will only occasionally be satisfactorily purified.

The catchwater, or, as the Committee has termed it, the supersaturation principle, is not defensible either on agricultural, chemical, or sanitary principles.

An irrigation-farm should therefore carry out intermittent downward filtration on a large scale, so that the sewage may be always thoroughly purified, while at the same time the maximum of utilization is obtained.

It is certain that all kinds of crops may be grown with sewage, so that the farmer can grow such as he can best sell; nevertheless, the staple crops must be cattle-food, such as grass, roots &c., with occasional crops of kitchen vegetables and of corn.

And it is also certain, from the analysis of the soil, that it becomes very much richer under sewage-irrigation, and that some of the manurial constituents of the sewage accumulate in it.

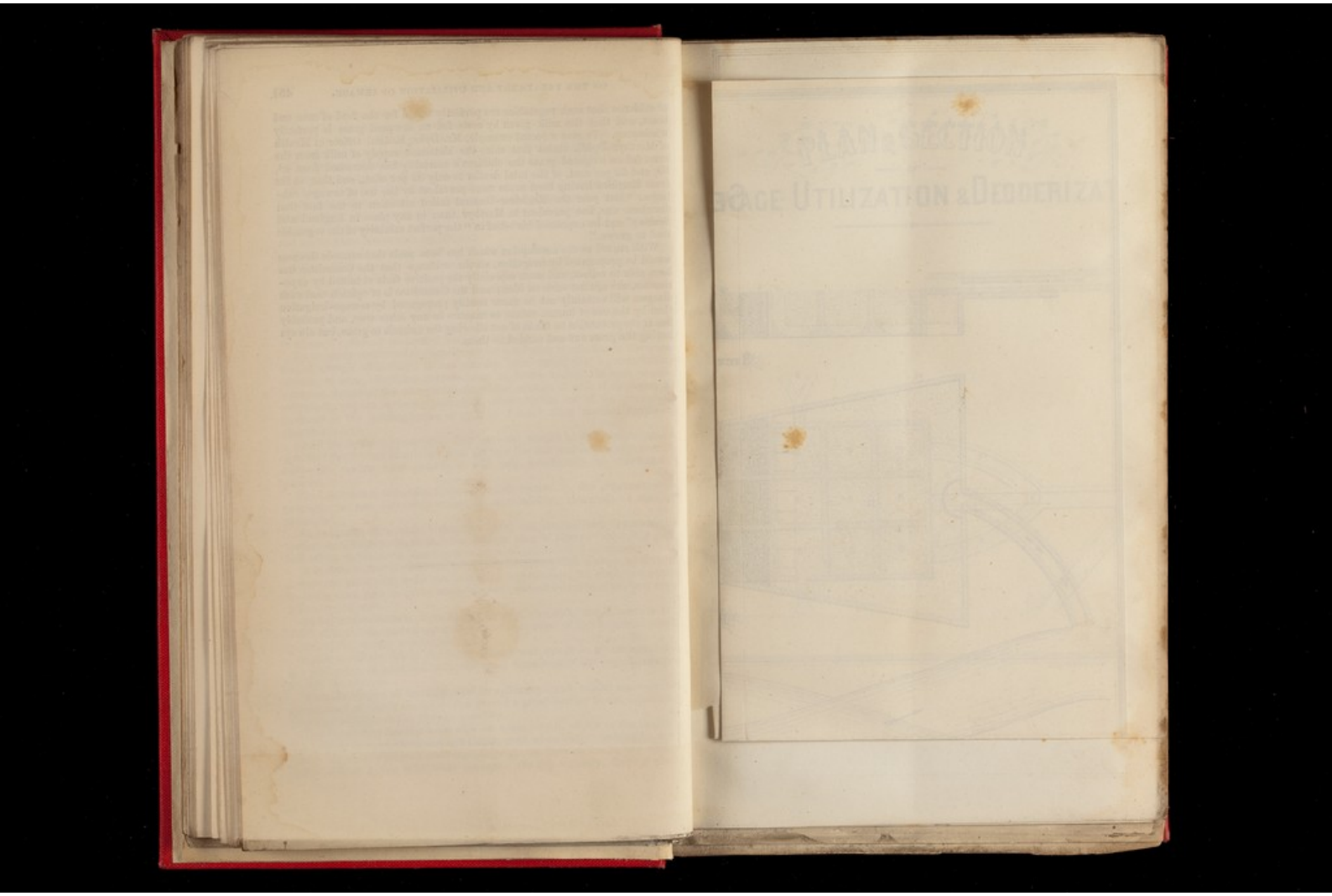
Cattle should be fed on the farm. The result would be a vast increase in the production of meat and milk, the great desiderata of the populations producing the sewage.

Thus the system of farming must be specialized and capital concentrated, the absence of which conditions has proved a great barrier to the satisfactory practical solution of the sewage question.

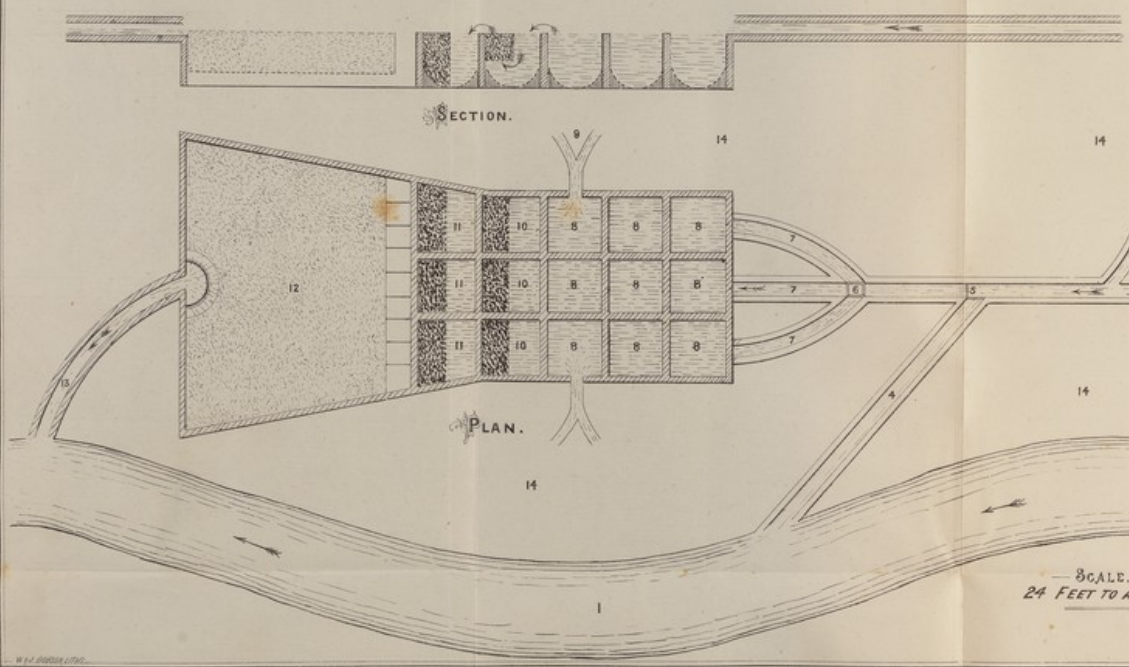
The Committee has not been able to trace any ill effects to the health of the persons living around sewage-farms, even when badly conducted; nor is there any proof whatever that vegetables grown thereon are in any way inferior to those grown with other manure. On the contrary, there is plenty

of evidence that such vegetables are perfectly suited for the food of man and beast, and that the milk given by cows fed on sewaged grass is perfectly wholesome. To give a recent example, Mr. Dyke, Medical Officer of Health of Merthyr Tydfil, states that since the abundant supply of milk from the cows fed on irrigated grass the children's mortality has decreased from 48, 50, and 52 per cent. of the total deaths to only 39 per cent., and that, so far from diarrhœa having been made more prevalent by the use of sewaged cabbages, "last year the Registrar-General called attention to the fact that diarrhœa was less prevalent in Merthyr than in any place in England and Wales;" and he expressed his belief in "the perfect salubrity of the vegetable food so grown."

With regard to the assumption which has been made that entozoic diseases would be propagated by irrigation, all the evidence that the Committee has been able to collect, and more especially the positive facts obtained by experiments, are against such an idea; and the Committee is of opinion that such diseases will certainly not be more readily propagated by sewage-irrigation than by the use of human refuse as manure in any other way, and probably less if the precaution be taken of not allowing the animals to graze, but always having the grass cut and carried to them.



PLAN & SECTION
— OF —
SEWAGE UTILIZATION & DEODERIZATION.



REFERENCE.

1.—*The River.*
2.—*Main Drain to the town.*
3.—*Tributary Drains.*
4.—*Storm Water Drain.*
5.—*Storm Water Basin.*
6.—*Junction Sluice Chamber*
7.—*Drains to each row of tanks.*
8.—*Precipitating Tanks.*
9.—*Irrigation Drains on to the land.*
10.—*Horizontal Charcoal Deoderizing Chamber*
11.—*Vertical do. do.*
12.—*Gravel Filter Bed.*
13.—*Outlet Drain.*
14.—*Meadow Land whereon to utilize the liquid sewage.*

SCALE.
24 FEET TO AN INCH.

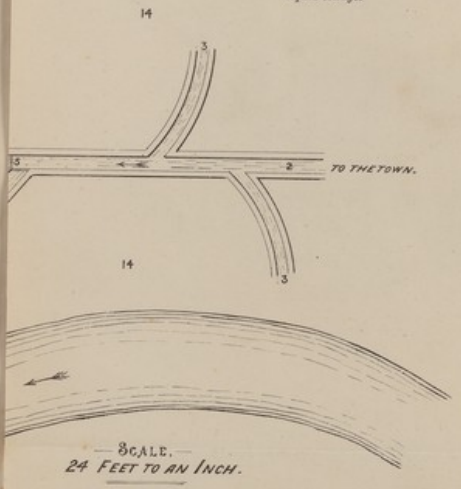
J. DUTHIE, PRESTON.

PLAN No 1.

ION.

REFERENCE.

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13.—*Outlet Drain.*
14.—*Meadow Land whereon to utilize the liquid sewage.*



SCALE.
24 FEET TO AN INCH.

J. DUTHIE, PRESTON.

A TREATISE

ON THE

UTILIZATION OF TOWNS' SEWAGE,

PARTICULARLY WITH REFERENCE TO ITS APPLICATION TO PRESTON.

ILLUSTRATED WITH PLANS AND SECTIONS.

BY

JAMES DUTHIE,

CLERK OF WORKS TO THE PRESTON LOCAL BOARD OF HEALTH.

[ENTERED AT STATIONERS' HALL.]

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PREFACE.

Having frequently been asked to publish in pamphlet form my views on the Sewage Utilization Question, particularly with reference to its application to Preston; and this request having been more frequent of late, I have at length consented to do so, with the hope that some good will result therefrom.

Should this hope be realized in ever so small a degree, I shall consider my leisure time well spent in writing this somewhat imperfect treatise.

My object in undertaking this work is solely to do good to every town and district, if possible, and to Preston in particular.

I therefore trust that the public will receive this treatise in the same good spirit in which it has been conceived and written.

J. DUTHIE.

4, Peel Terrace,
Preston, July, 1870.

A TREATISE
ON THE
UTILIZATION OF TOWNS' SEWAGE.

Chapter I.

CLEANLINESS IS NEXT TO GODLINESS.

The disposal or utilization of towns' sewage, night soil, street sweepings, &c., as well also as the refuse made at the various manufactories, such as chemical, dye, paper, soap, and other works, which now turn their refuse into the rivers and streams, is a question which is now commanding special attention all over the country. Royal Commissioners have visited nearly all the counties in England, making enquiries respecting this important question, with a view to some general remedy. Medical Gentlemen, Civil Engineers, Sanitary Reformers, and Local Boards of Health are also turning their attention to this important matter. Such being the case, we venture to say a few words upon the subject with a view of assisting to solve this great sanitary problem. In approaching this important question, we shall do so from four points of view, all of which bear a close connection with each other, and are links in the same sanitary chain.

The first point is to improve the health, vigour, and happiness of the people, and thereby greatly reduce the death rate.

The second point to look at is the purification of our rivers and streams, again making them as the Great Almighty originally made them—pure, so that the fish may again sport about in their pure native element as they did in the days of our forefathers.

In the third place we must look at this subject from an agricultural point of view, that is, returning to the land those elements that have been extracted from it in the food which the land yields for the sustenance of man and beast.

In the fourth place we must look at this question from a financial or commercial point of view. Each of these links in the sanitary chain is of some public importance, and when considered as a whole they become of the greatest importance to all the human family.

The first of the above propositions, that of improving health and thereby lengthening life is within the reach of all, by attending to the rules or laws of cleanliness and temperance. Cleanliness is only second to Godliness, and those who neglect this divine sanitary law will most assuredly suffer the penalty of their guilt to the extent of their negligence. It is therefore of the greatest importance that we should, in the first place, be clean in our own persons, attending well to the soap and water process, and also to baths and bathing, with a proper change of clean clothing.

With temperate habits and a proper amount of physical exercise, along with cleanliness, we should enjoy life, and no doubt live to a good round age, very probably to the three score years and ten which is allotted to man.

The next thing to be attended to is cleanliness at home in all our household affairs. At home our wives, daughters, and sisters reign supreme, and it is a blessing for man that they do. It is a pleasing fact to know that the ladies as a rule are both house

proud and clean in their various domestic arrangements; but, it is too true that there are hundreds, nay thousands of exceptions to this great and good rule of putting the house in order. It is of great importance that those house-wives who neglect their homes, through the want of a proper knowledge of household matters, should be educated how to do their work. The sanitary associations which are formed in most of our large towns are now rendering good service in this particular, by the instructions given by their visitors.

The construction of the dwellings of the people has a great deal to do with the health of the masses. In this particular, great improvements have of late years taken place; more attention is now paid to ventilation, draining, and other sanitary improvements than was the case some years ago. This arises from the fact that the law now compels all who intend to build to first submit their plans to the Local Boards of Health, or other authorities in their district. This plan has worked well in bringing into existence a better class of cottage property as dwellings for our labouring classes in cities and thickly populated towns.

In connection with house construction, the closets or petties ought to be well considered with a view to improving them; also the best mode of removing the excrement with despatch and without creating a sanitary evil. This is an important part of our subject, and is now occupying the attention of scientific and other gentlemen, and there is no wonder at this when we look at the old open cesspool and ashpit system, where household refuse, viz.: excrement, slops, ashes, vegetable and other refuse, are all, from time to time, collected together and allowed to decompose, giving off those poisonous gases which pollute the air we breathe, surround and enter our dwellings, producing disease and bringing premature death to thousands. We may

take as a safe and sure guide that all accumulations of refuse, which give off smells of a disagreeable kind, are most injurious and dangerous to health, if allowed to remain about our dwellings.

Nature, by endowing us with the sense of smell, has provided man with the means of detecting anything that may do him harm, and, should we neglect these warnings, nature will most assuredly inflict upon us the penalties of such negligence. There is also the demoralizing and degrading effects of the open petty system in our large towns, and these are likely to lead to a disregard of all precautions for the preservation of public and also of private health. The sense of sight becomes offended, and all our better feelings are to some extent blunted by the unseemly sight of the old open petty system. Some years ago the water closet system was introduced to remedy these evils, and this it most certainly did so far as the excrement and the slops were concerned, but, in removing this much of the refuse from the dwellings of those who adopted the water closet system, there was being produced another huge evil, viz.:—the pollution of our beautiful rivers and streams, making them into large open cesspools in and near the towns drained into them.

It must be here stated, that town sewage does not do all the evil in polluting the rivers, for the various manufactories which turn their refuse, both liquid and solid, into the rivers or streams, do more to pollute the streams than town sewage does, and when we consider that these works have been, and still are, on the increase, this must necessarily be so. When this water closet pollution of rivers became so bad, sanitary reformers began to look about for a remedy. A few years since a company called the "Patent Eureka Sanitary Company" was formed in Hyde, a small town a few miles from Manchester. Its mode of collecting the excrement matter, ashes, and other refuse made in the dwellings of the people,

was by supplying two boxes to each house, one for the excrement and one for ashes and other refuse made. As these boxes became filled the company's servants called for them with horse and cart, leaving empty boxes in place of the full ones taken away. These boxes were fitted up with close lids, and deoderizing powder was used to prevent the effluvium escaping.

These were taken to the Company's works, and the contents underwent a process of drying and mixing, which when finished became a first-rate guano, good for agricultural purposes. This found a ready sale to the farmers in the neighbourhood, the company realising a good per centage for their capital invested.

Now, out of this plan of collecting, mixing, and distributing the refuse made by the inhabitants of Hyde, has sprung all the dry earth systems which are now more or less in operation and being experimented upon in various towns.

The Goux system of collecting the excrement in barrels lined with an absorbent matter, considered from a sanitary point of view, as well also as entailing too much labour in its workings, will never allow of its coming into general use.

The dry earth system recommended by the Rev. Mr. Moule is also impracticable from the fact that it would be difficult, and also very expensive, in procuring, drying, and carting the necessary quantity and quality of earth into the towns for use, and again carting out of the towns after being used.

The best of these dry closet systems which has come under our notice is that of Mr. Morrell's dry sanitary scheme. This consists of a very ingenious self-acting closet which is fed with the fine ashes made in every house where a fire is kept. It is a fact that should not be lost sight of, that these dry ashes are an excellent deoderizer. Each time the closet is used, on getting up from the seat, just

sufficient ashes are admitted to cover the excrement; thus all smell and nuisance are avoided. Portable square boxes are placed to receive all, and when filled are taken away and replaced with empty ones. But, unfortunately, the difficuties which all these dry systems have to contend against are great, and some of them are so great as not to be overcome.

In the first place they are very liable to be tampered with, more especially by children, and thereby knocked out of working order. Secondly, there is every probability of their being neglected and not regularly or properly fed with the fine dry ashes. In this case, they would become a greater nuisance than our present open petties. In the third place it would be very expensive, and also very inconvenient at times to collect these boxes and convey them to the various depôts; this would be the case more particularly in large cities and towns.

There is little or no doubt but Mr. Morrell's system, as well as other dry systems, could be carried out in hospitals, barracks, camps, union workhouses and other places where there is discipline, order, and a proper attention paid to their various workings; also in mills and workshops if the proprietors would employ persons to look properly after them. But in the dwellings of the masses in large towns it would be found almost impossible to carry out the system in an effectual and efficient manner.

In taking an impartial view of the various systems for removing excrement and slops from the dwellings of the people, we are obliged to say that the water closet system is decidedly the most practical. The obections urged against the water closet are few, and these are easy to remedy. The greatest of these objections is the pollution of our rivers and streams. This can be entirely remedied by intercepting the sewage, and thereby preventing it from entering the rivers, and convey it through the

intercepting drain outside the town, and there erect tanks for its reception and precipitation.

The second objection urged is the large consumption of water used in flushing the excrement down the drains; this supply being drawn from the towns waterworks supply.

Now, if we take the Liverpool estimate of water consumed for closet purposes, which is five gallons per head per day, it will be somewhat enormous and expensive; for, suppose each of the 17,300 houses in Preston had water closets, with our 100,000 inhabitants, the water consumed in those closets at the above rate would be about one hundred and fifty million gallons per year. This at sixpence per thousand gallons would amount to the sum of £3750 per annum. Almost the whole of this large consumption of water could be provided for without drawing upon the water supply of the town by providing large cisterns over each yard closet wherein to convey the rain water from the top of each house, and utilize it for closet purposes; only the closets in the interior of the houses would require water from the town's supply. By thus utilizing the rain water, the sewage would be less diluted, and would, therefore, be of more value as a futilizer. The simplest constructed closets are, as a matter of course, the best. Most of the patent self-acting closets are too complicated, and are easy of being knocked out of gear by children tampering with them. Those that we should recommend are the fast, stationary seats with lid, the common closet pan or pot, with the syphon pipe attached underneath, emptying into the drain. These to be fed with water by means of a pipe from bottom of cistern above, to the top of pan or pot, worked with chain and pully. If every two houses had their closets back to back at the far end of the yard with the ashpit between them, and the cistern covering over all, there would be a better storeage for water, and

the ashes and vegetable refuse would be kept completely dry, and therefore could not become offensive and injurious to health. The atmosphere in and around the dwellings of the people would be much purer than at present under the open petty system. Health would be improved and life enjoyed for a longer period of time than at present. In the water closet system we have an improved social institution based on the best and truest principles, and we may rest assured that all other systems will bear no comparison with it for removing excrement from the midst of largely populated towns. It is only in those places where the water closet system does not exist that we must look for the presence of those gases which are so injurious to health.

Chapter II.

"Dirt is a useful thing at present put in the wrong place."—*The late Lord Palmerston.*

It is now universally admitted that the great sanitary question of the day is the utilization of our town sewage. The improvements which have been made of late years in the drainage of towns have rendered it absolutely necessary that something should be done with the sewage of our towns—both liquid and solid—instead of allowing it to empty into our rivers and streams, and thus polluting and making them into a huge open cesspool, charging the atmosphere with poisonous gases, thereby spreading disease and premature death to thousands of our fellow creatures.

The present mode of getting rid of town sewage and refuse from manufactories cannot be permitted much longer. The idea is fast spreading that our beautiful rivers and streams were intended by an allwise Creator to refresh and beautify the country.

Previous to the passing of the Public Health Act in 1848, house and town drainage were very deficent all over the country; and the drainage, such as then was in existence, was only intended for surface and slop water, but after the passing of the above Act, efficient drainage works were undertaken in many towns throughout the country, and when these became completed, along with the adoption of the water closet system, then the evil of polluting the rivers became manifest, and for which we have

now to seek a remedy. From whatever point of view we may regard this important question —either sanitary, commercial, or otherwise, its importance is becoming apparent.

By our present improved system of house and town drainage—large quantities of solid refuse is carried by water into and through the sewers before fermentation takes place; by this means the atmosphere in and around the dwellings of the people are greatly purified.

But this solid matter becomes deposited in the beds of the rivers and streams in which it enters, and there decomposes and performs its deadly work, polluting the streams and poisoning the fish, and also the cattle that drink of their waters. We would here ask, has not this river pollution had something to do with the origin of the cattle plague?

It is a fact that fish forms an important part of human food, and finds employment to many in catching and selling them; whilst the wholesale destruction of river fish by this river pollution lessens the amount of human food, and frequently makes it much dearer.

It has been decided in the law courts more than once, that the authorities of a city or town have no right whatever to turn their sewage into a public river, thereby transferring the nuisance to their neighbours who are situated below them.

When we consider that a many public water works have to pump their water supply for the domestic purposes of thousands of people from rivers which receive the sewage of populous towns, the liquid refuse from the various manufactories which are situated on their banks, and which also contain the putrid carcases of drowned animals, the odour and substance of which are imparted to the water; when we consider that the inhabitants of some of the metropolitan and other districts are compelled to consume this water, we cannot for a moment

wonder at cholera, fevers, and other contagious diseases making such ravages amongst the people, as is too often the case.

Mr. Rawlinson, a few years ago, stated before a committee of the House of Commons on the sewage question, "that the excrement matter of some 800,000 persons emptied into the river Thames above a part at which the water is taken for the domestic purposes of the inhabitants of the western, north-western, and south-western portions of the metropolis." This is something for the people of those districts to think about when using this water for eating, cooking, &c. Looking at these and various other facts bearing upon and connected with this important subject, there is no wonder that the interception and utilization of town sewage should have forced itself upon the attention of the public.

And it is evident that something will have to be soon done to remedy this gigantic evil, the result of which will be to prevent the great sacrifice of life and lessen the amount of human suffering; to preserve the fish in the rivers for human food, and to give back to the land those elements that have hitherto been worse than thrown away in return for the food and raiment which the land yields for the sustenance of man. The great question now is, how is this to be accomplished. How are we to collect, utilize, and distribute this refuse matter, both liquid and solid for agricultural purposes, without at the same time creating a greater evil than we now experience by allowing it to run to waste.

This is the most important sanitary question of the present day, and one that deserves the serious attention of all sanitary reformers, with a view to restoring our beautiful rivers and streams to the use our beneficient Creator intended them, and also with a view to the adoption of some approved general utilizing system which can be made applicable to almost every town or district throughout the country.

For the accomplishment of this very desirable object, various schemes have been from time to time suggested.

One of these schemes has been to separate the solid matter from the liquid by precipitation, and making the solid sewage thus obtained into a portable guano or manure.

This mode of dealing with town sewage has been acted upon in many towns. The largest works constructed for this purpose is at Leicester, where the authorities expended £40,000. Plymouth, Cheltenham, Worksop, Stroud, Coventry, and other places, have also adopted the precipitating system.

Looking at these experiments from a strictly commercial point of view, they have almost all, more or less, failed to remunerate those who invested their capital in them; but looking at them again from a sanitary point of view, they have each and all accomplished much good.

At Leicester and other places this process of precipitating is effected with lime; the quantity of lime used is about fifteen grains per gallon of sewage. The sewage flows into a large tank, the lime is then added, and the whole is agitated briskly; it is then allowed to flow into subsiding tanks, the solid matter held in suspension is here precipitated to the bottom of the tanks, and the water is run off through a weir a little below the surface.

At some of these works the water is afterwards filtered before it enters the outfall, and a small quantity of chloride of lime is used to check putrefaction.

At Leicester, where the above plan is acted upon, a great change has taken place with respect to the purification of the river Soar, where the sewage enters into. Before these works were established the river was so polluted with the sewage that fish could not live in it, and illness prevailed amongst

those who were compelled to inhale the effluvia from it; but now the fish can be seen sporting about near the present outfall, and those who live on the banks enjoy comparatively good health.

In Leamington the A B C process of Mr. Sillar is in operation. In this process the sewage by its own gravitation flows to the works, where it is mixed with clay, bone ashes, sulphate of magnesia, blood, alum, and charcoal. The proportions are about one gallon to 200 gallons of sewage; the whole is well agitated, and it then flows into the subsiding tanks where the solid matter which was held in suspension is precipitated to the bottom, and the clear water flows off; these tanks are emptied as often as required; the solid sewage matter is then allowed to dry, and is sold for manure at a fair profit. This is perhaps the only place where the precipitating process is remunerative.

There are also other plans in operation in various parts of the country somewhat similar to the above, none of which are remunerative in a money point of view, yet they are doing much sanitary good.

The works at Leamington are likely to undergo a severe test as to their efficiency.

These works were visited a few days ago by Sir John Thwaites and twenty-four members of the Metropolitan Board of Works, accompanied by their chemist. The various processes were closely inspected by all. Arrangements are in progress for the Metropolitan Board to take possession of the works for a fixed period, and to send an independent chemist to strictly test the efficiency of the works. These experiments will be looked to with some interest by the public.

Chapter III.

"The downfall of the Roman Empire was due to the waste of the reproductive element drawn from the soil, and never returned to it in order to perform its functions."—*Baron Liebig.*

We shall now proceed to examine this sewage question from an agricultural point of view, and in doing so we shall dwell more particularly upon the irrigating principle, and its results.

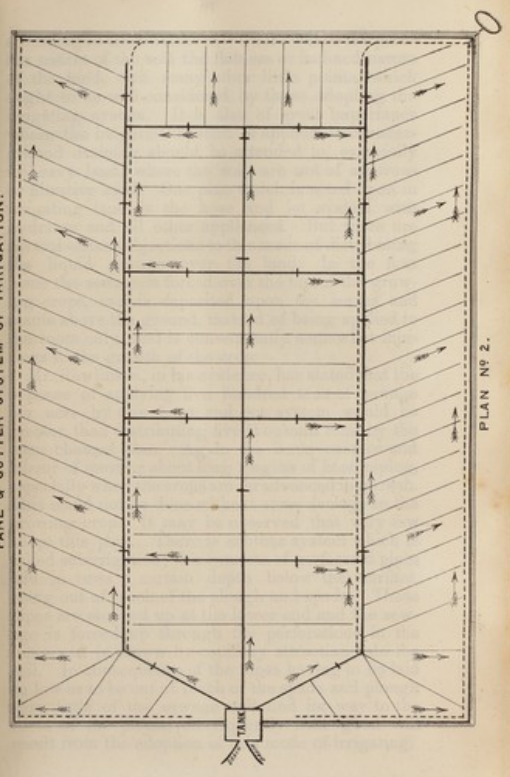
Sewage irrigation consists in conveying the sewage direct upon the land in a liquid state as it flows from the sewers. In many localities, where the inclination of the land is suitable, the sewage would flow by its own gravitation to where wanted, but in other districts less favourably situated pumping would have to be resorted to. There is not the least doubt but that the land is the place intended by the order of nature whereon the sewage and refuse of our towns should be deposited—it having there an important work to perform.

Town sewage contains all those elements which are continually being extracted from the soil, and therefore ought to be again returned to it, so that the soil can perform its reproductive functions. Soils and the roots of plants have a great power of abstracting the poisonous impurities from sewage water, and thus making it again almost pure, in fact no chemical process has yet been discovered that can do this so efficiently as the roots of plants.

The general principles which ought to be observed by farmers and others in the application of sewage are not of a difficult nature; they are easy to be carried out. It is true they will have to be modified

PLAN

PANE & GUTTER SYSTEM OF IRRIGATION.



PLAN Nº 2.

according to circumstances. There will be the peculiar nature of the soil, the flatness or inclined nature of the land, with many other little points, which ought to be well considered by those adopting the irrigating system. It is also of great importance where the irrigation system is applied that underground draining should be attended to, especially in heavy land, where the soils are not of a porous or filtrative kind. One plan which is acted upon in irrigating land is the hose and jet system with hydrants and all other appliances. But there are several serious objections to this mode of distributing the liquid sewage over the land. In the first place the sewage is forced over the top of the growing crops, and is deposited upon the leaves and plants above the ground, instead of being applied to the roots only, and is consequently somewhat injurious to the growth of the crop.

Mr. Rawlinson, in his evidence, has stated that the expense of applying five hundred tons of sewage per acre by this hose and jet system would be greater than distributing five thousand tons by the open channel plan. Again the inconvenience and labour of moving about long lengths of hose piping, especially when the crops are far advanced in growth. This could not be done without some damage to the growing crop. It may be observed that very few adopt this plan. There is another system which is called subirrigation; this consists of perforated pipes laid in rows a certain depth below the surface, being out of reach of the plough and spade. These pipes are stopped up at the lower end and the sewage is forced up through the perforations in the pipes and is drawn by capillary attraction into the soil. In consequence of the pipes having to be laid so low as to be out of reach of the spade and plough very little of the sewage will find its way to the roots of the plants, consequently little good will result from the adoption of this mode of irrigating.

The last system that we shall notice is the open carrier or channel system. This plan may be subdivided into three different modes of working, known by the names of the pane and gutter plan, the ridge and furrow plan, and the catch-water plan; each of these is to suit the different levels and inclinations to be met with on various farms. The catch-water plan is perhaps the cheapest of the three; this is only applicable to hilly grounds or steep inclines.

The pane and gutter system is the one most acted upon, and is best adapted for ground with an easy inclination.

The main open gutters or carriers are cut along the land parallel with its fall or inclination; other gutters branching out of these boards are placed across all these gutters at intervals, which stop and back up the sewage and cause it to overflow the land.

There are also what are called discharging gutters, which receive the water after percolating through the land, leaving its impurities in the soil to nourish the plants; it is then conveyed into the outlet drain, and through this to the outfall into the river; by the time it reaches this point it is in a comparatively pure state, and fit for fish to gambol and sport in.

The simplicity of this system of irrigation is its greatest recommendation. It is acted upon at Edinburgh, Croydon, Norwood, and other places. The cost of laying out land for irrigation purposes differs greatly according to local circumstances. Some portions of the Edinburgh farms cost between £12 and £20 per acre, which is rather a high price. The quality of town sewage will greatly depend upon the efficiency of house drainage. It will be more valuable as a manure in those towns where the water closet system is in general use. The quantity of solid matter contained in town

sewage will vary in accordance with the different circumstances under which it is produced. It will alter with the seasons of the year, with the number of manufactories which turn their refuse into the sewers, and the extent to which the water closet system is adopted, as well as other minor considerations; but we may rest perfectly assured that all descriptions of sewage can be profitably applied to the land. Soils of all kinds will be improved by a judicious application, and the crops greatly increased by its use.

It is generally considered that sewage is only applicable to grass land, but this idea is an erroneous one. The experiments which have for some years past been carried on at Barking and other places fully demonstrate that almost all crops thrive under a judicious use of sewage. At Barking the results have been satisfactory with wheat, oats, rye, red cabbage, onions, mangolds, strawberries, parsnips, &c., &c. With respect to the quantity of sewage to be applied to a given area of land, this would vary according to the peculiar nature of the soil, and also the kind of crop grown upon it. Porous sandy soil will take a great deal more sewage than stiff clay or loamy soil.

It is thought from experiments already made that from four to five thousand tons per acre per annum for grass land is the quantity which will be found the most profitable to apply; yet at Edinburgh as much as eight thousand tons are applied, this will no doubt arise from the fact that the sewage supply of Edinburgh is greatly in excess of the land at their disposal, and they are consequently compelled at certain times throughout the year to allow the sewage to flow into the sea.

It has been ascertained by repeated experiments that after a given quantity of sewage has been applied to grass land—say five thousand tons per acre—the result of increasing that supply is not at all in proportion to the increased quantity of sewage

applied. The experiments at Rugby during the years 1861, 2, and 3, give the following average results:—without sewage, 9¼ tons of grass per acre; with 2000 tons of sewage per acre, 22¼ tons of grass per acre; with 5200 tons of sewage, 30¼ tons of grass per acre; with 8000 tons of sewage, 32¼ tons of grass per acre. From these figures it will be perceived that the increased quantity of produce is not in proportion to the increased quantity of sewage applied; but it rather goes to prove that a moderate and judicious application will ensure the best and most profitable results. These figures also prove what has before been stated, viz.:—that between 4000 and 5000 tons per acre per annum, and on some soils less than this, is the quantity best adapted to the wants of the soil to enable it to perform its productive functions.

Milk farming is generally found most profitable when near to large towns. Milk is one of the most essential articles of food and will always command a ready sale, and that crop which adds most to the production of milk will most certainly find a market for its disposal, whether it be grazed on the land or cut for fodder.

At Rugby experiments have been tried with sewaged and unsewaged grass as food for milk cows, with a view to ascertain the relative quantity of milk produced. The results were found to be slightly in favor of the unsewaged grass, but there were circumstances that would account for this slight difference. There were three cows fed on unsewaged grass, whilst there were ten fed on the grass irrigated with sewage, some of these giving more milk than the three fed on unsewaged grass. The time of calving also differed, those fed on the sewaged grass being 12 days earlier than the average of the others. If all circumstances had been taken into due consideration it is probable that the yield would have been found about equal.

It has also been ascertained that one acre of Italian rye grass irrigated with 5000 tons of sewage will yield 1000 gallons of milk per annum.

Now, taking the price of milk at one penny per pint, or eightpence per gallon, this in money value would amount to £33 6s. 8d.

The following figures will show the relative annual value of an acre of land, (with the different quantities of sewage applied, and also without sewage,) estimated as milk, the price being one penny per pint:—

	£	s.	d.
Without sewage - -	15	0	0
3000 tons of sewage -	25	7	0
6000 „ „ -	33	6	10
9000 „ „ -	36	1	4

If we view this important question of sewage application on a grand extensive scale, that is to crops of all kinds, there is little or no doubt but that land which is now poor and exhausted would become again restored to fertility, and by these means the wealth and prosperity of our country would be increased; and we hope the time is not far distant when full justice will be done to the land by sewage irrigation; when every farmer and tiller of the land will have sewage pipes laid on to his farm for irrigation purposes just the same as in towns we have gas and water pipes laid into our houses for domestic use and comfort.

The plan in front of this chapter shows the pane and gutter system of irrigation. There will be seen two drains conveying the sewage into a square tank at the entrance to the land, where the solid matter is precipitated; the liquid running off through the main feeders, which are shown by the thick lines. The cross pieces in these lines are boards, which can be placed in the gutters and taken out at pleasure; when in, the sewage is backed up and overflows the land. The thin lines are the general distributors of the sewage. The dotted lines show the discharging drains; these receive the water after it has percolated through the soil, and convey it to the outfall drain O; and the arrows show the course taken by the sewage.

Chapter IV.

"Muck is money."—*The late Earl of Derby.*

We shall now proceed to examine this important subject from a commercial point of view. This is a very interesting part of the question: there are some who will no doubt think that it is the most important. Many of these would not for a moment think of utilizing their sewage unless they were insured of realizing a handsome profit by so doing. This would be a narrow and selfish view to take of so important a subject. We believe that health, comfort, and long life to the masses of our people are of far greater importance, and of more value, than mere gain of money; but at the same time, where both these advantages can be accomplished together, then by all means let it be done. There are a many towns and districts that would be greatly benefited both in a money and sanitary point of view by adopting the utilization system; and there are many districts where engineering and other local difficulties exist, all of which can be overcome, yet these difficulties would prevent the undertaking from being a paying one, but they would have the consolation of knowing that they were being benefited in a sanitary point of view, and this would be a great blessing and worthy the undertaking.

We shall now endeavour to ascertain what has been the result of the sewage irrigation scheme adopted at Edinburgh. The quantity of land under irrigation there is about 347 acres. Some of these

meadows have been under irrigation 50 years, some 40, and some 20 years. It will be thus seen that the irrigation scheme has been well tested in Edinburgh. The population contributing to the sewage connected with these works is about 125,000. The quantity of sewage used in irrigating these meadows is about 8000 tons per acre per annum. Nearly the whole of these meadows are irrigated by gravitation, upon the pane and gutter system, with several patches of the catch-water plan. The sewage water which runs from the west side of Edinburgh, and supplies some of these meadows, is very good for irrigation purposes, and part of the land is laid out in four levels in succcession, so planned that the sewage put on the top level will run over each of the others in succession before it reaches the river. It may be here stated that the quantity of sewage is greater than can be used, the area of the land being too small for the purpose, hence a large quantity of the sewage is run direct into the sea. The Craigentinny meadows are situated on the east side of Edinburgh, and consist of about 250 acres; the original cost of forming these meadows for irrigation purposes was £4500, averaging about £21 per acre—which was very high. Included in the above is £1250, the cost of a steam engine, boiler, pumps, &c., to lift the sewage for irrigating that portion of the farm called Engine Meadows.

Some portion of these meadows previous to 1826, when they were first laid out for irrigation, were drifted sand banks producing nothing, and consequently worth scarcely anything; land in the neighbourhood now lets at about £5 per acre per annum— this is without the sewage.

We will now take a summary of what these 347 acres of land which are under irrigation produce in money value per annum:—

	£	s.	d.	
Craigentinny Meadows -	29	2	10	per acre.
Do. Engine Meadows	16	0	0	,,
Lochend Meadows - -	33	2	2	,,
Grange Meadows - -	27	10	0	,,
Dalroy Meadows - -	31	10	0	,,
Quarry Holes Meadows -	30	0	0	,,
Bonington Road Meadows -	24	0	0	,,

The average of the above summary will be about £27 10s. per acre per annum.

The total quantity of land under irrigation being 347 acres, it will be seen from the foregoing summary that the total average of money made by the sales of grass amounts to £9542 10s. per annum, and that the average per acre upon all the meadows is £27 10s. per annum. Taking these sums at 30 years purchase, the average value of the land per acre would be £825, and the total amount of capital here represented is £286,275.

And now taking the value of the land without sewage irrigation at £5 per acre, this at 30 years purchase will give £150 per acre, and will represent in capital £52,050, which, taken from the former sum, leaves £234,225 as the amount of capital which is represented by the sewage irrigation.

These figures plainly show that nearly a quarter of a million of money has been made during the last 30 years of only a portion of the sewage of Edinburgh, and that every acre of land under sewage irrigation produces six times as much as the average of agricultural land in the country, and is capable of still further extension. Again, looking at the results recorded in the above summary as the ascertained rental which was for 1860, the average of the whole being £27 10s. per acre, thus representing for the 347 acres under irrigation a revenue of £9542 10s., and deducting from this £5 per acre or £1735 as the value of the land without sewage, and taking an additional sum of £1 10s. per acre or £520 10s. as interest and working expenses, we

shall still have as the result the sum of £21 per acre or £7287 per annum as the value of that portion of the sewage of Edinburgh which is now utilized. But as the land is insufficient for the sewage—this sum might be multiplied by 3 thus giving £21,861 that this portion of the sewage of Edinburgh might realize, were it all judiciously utilized.

We will now take a brief glance at Rugby, where the Royal Commission carried out some important experiments. Rugby contains upwards of 7000 of a population, and is one of the best sewered towns in England, the water closet system being attached to almost every house. A gentleman of the name of G. H. Walker, Esq., leases the sewage. 470 acres of meadow and pasture land are under irrigation—the sewage being pumped by a 12-horse power engine.

The Royal Commissioners commenced their experiments in 1861, and completed them in 1864; they were greatly assisted by the arrangements made by Mr. Walker, the lessee, and also by his tenant, a Mr. Campbell.

The Royal Commissioners issued their third report in 1865, from which the following facts are taken:—two fields, which were called A and B, were experimented upon. These fields were divided into four equal plots, three of which were irrigated with sewage in various quantities, and the other had no sewage applied during these experiments. We shall condense the figures relating to these experiments by giving only the total average for the three years 1861, 2, and 3, this being sufficient for our present purpose:—

AVERAGE FOR 1861–2–3.

PLOT 1.					PLOT 2.				
Sewage Applied.	Grass Produced.				Sewage Applied.	Grass Produced.			
Tons.	T.	C.	Q.	lbs.	Tons.	T.	C.	Q.	lbs.
0	9	6	0	24	2575	22	5	2	9

PLOT 3.				PLOT 4.			
Sewage Applied.	Grass Produced.			Sewage Applied.	Grass Produced.		
Tons.	T.	C.	Q. lbs.	Tons.	T.	C.	Q. lbs.
5203	30	6	2 6	7728	32	8	3 5

It will be seen from the above table that the results are not in direct proportion to the quantity of sewage applied, but rather that a moderate use of it gives larger proportional results.

We shall now show in like manner the increased money value due to sewage applied in the different quantities, taking the same three years, 1861–2–3. Green grass is here calculated at 13s. 4d. per ton.

PLOT 1.				PLOT 2.			
Sewage Applied.	~~Increased~~ value ~~due to Sewage~~.			Sewage Applied.	Increased value due to Sewage.		
Tons.	£	s.	d.	Tons.	£	s.	d.
0	6	4	1	2575	8	12	10

PLOT 3.				PLOT 4.			
Sewage Applied.	Increased value due to Sewage.			Sewage Applied.	Increased value due to Sewage.		
Tons.	£	s.	d.	Tons.	£	s.	d.
5203	14	0	2	7728	15	10	7

The Commissioners experimented in 1863 by applying sewage to Italian rye grass, and for this purpose selected a field of three acres, and divided it into three equal plots; one was unsewaged, to the second was applied 787 tons of sewage, and the third received 1522 tons. The result of this was, that plot one, without sewage, produced 16¾ tons of grass; plot two, with 787 tons of sewage, gave 20¾ tons of grass; and plot three, having 1522 tons of sewage applied, produced 25¼ tons of grass.

Now, these experiments at Rugby go to prove that when liquid sewage is judiciously applied to grass land it is worth about one penny per ton. They also demonstrate that the land is permanently improved by a judicious application of sewage.

From the experiments made by Professor Way in 1861, 2, and 3, upon the Rugby sewage, it appears that the ammonia, or fertilizing qualities, averages 6½ grains per gallon; three-fourths are held in solution, and one-fourth in suspension. This is rather small, considering that the excrement of all the 7000 inhabitants enters into the sewage; but the water supply and the rainfall very much dilutes it. If the rain water was utilized for closet purposes, the present supply might be almost dispensed with, and the sewage would be made comparatively richer as a fertilizer.

At Croydon, the Local Board of Health, being threatened with legal proceedings for polluting the River Wandle, were obliged to intercept their sewage and prevent it from flowing into the river in its impure state. To accomplish this, 250 acres of land were taken upon a fourteen years' lease at a rental of £4 per acre per annum. The land had previously been let at less than half this sum, but the Local Board being compelled at once to intercept their sewage and prevent it from running into the river, and this being the most suitable land whereon to utilize their sewage, the owner obtained a higher rental for it. This land was sub-let by the Local Board to a Mr. Merriage, a gentleman with some practical abilities, at £5 per acre, the Local Board undertaking to pay £3 per acre towards the laying out of the land for irrigation purposes. This was done upon the pane and gutter system. The ground is so well laid out that the sewage flows over it in a veryeven thin stream, and about thirty acres are irrigated at a time, the whole being laid out in sections.

The population of Croydon is upwards of 16,000, the town is well sewered, and the water closet system generally in use.

The water supply is very large, being estimated at 40 gallons per head per day; the quantity of water used by the water closets and the rainfall goes to dilute the sewage very much. The estimated quantity of sewage applied to the land is upwards of 6000 tons per acre per annum. The kind of crops produced are Italian rye grass and meadow grass, the average quantity produced is about 32 tons per acre per annum, which is taken in four or five cuttings. The estimated value of the produce from the land under sewage irrigation is £27 per acre, being nearly the same as at Edinburgh, though the sewage applied is a great deal less. These figures again prove that the increased value of the land by the application of sewage is upwards of £20 per acre. There is no nuisance whatever at the Croydon irrigation works, the sewage percolates through the soil and enters the river in almost a pure state, so much so that, during a recent visit of the Mayor of Preston, J. Rawcliffe, Esq., with some of the officials, one of those gentlemen drank heartily of it as it flowed into the river, and at this outlet trout and other fish may be seen sporting about.

The sewage utilization works at Croydon are considered a great success, and have gone a great way along with the previous works named to solve this great question.

There are other places throughout the country where the irrigation system is being carried out on a small scale with equally good results, but we think that the three examples given are sufficient to prove the truth of what the late Earl of Derby said at the Royal North Lancashire Agricultural dinner in 1862, when held at Preston in the Guild week, viz.:—"That muck is money."

Chapter V.

SEWAGE IRRIGATION APPLIED TO PRESTON.

If such results as those described in the preceding chapter can be realised at Edinburgh, Croyden, Rugby, and other places, what is there to prevent similar results from being obtained in Preston? All things being equal, similar advantages could be attained anywhere. It is true that most towns are not so favourably situated for utilizing their sewage as the towns above named; yet Preston is most fortunate in having a natural advantage over many towns in this respect, and it ought to be amongst the foremost towns in adopting the utilization of its sewage.

The population of Preston is now about 100,000, and the number of dwelling-houses is something like 17,300. There are about 1900 water closets in the borough. It is also one of the best sewered towns in England; the only thing wanting to complete its sewage arrangements is the intercepting of the sewage, and thus prevent it from polluting the river, and convey it to some suitable place, and there utilize and filter it before it enters the river. It will very naturally be asked how and where is this to be done.

The land most suitable, and which appears to have been formed by nature for this purpose, is Ashton, Newton, Clifton, and Freckleton marshes. These marshes are situated on the north side of the Ribble, and contain between 800 and 900 acres; and

with the exception of Freckleton, which has for several years been farmed by a company, this land is at present of very little value. Should ever the Ribble scheme be carried out, there will be an additional quantity of land which will be added to these marshes, which will be suitable in every respect for irrigation purposes.

All this marsh land, which is now producing little or nothing, could, by the application of the sewage of Preston, be made into very valuable meadow and pasture land

The quantity of sewage now flowing into the river, in a liquid state, is about three and a half millions of tons per annum, included in this is some ten thousand tons of solid matter which is carried and held in mechanical suspension, and this is deposited in the bed of the river and contributes towards silting it up. Now taking this three and a half million tons at one penny per ton as given by the Rugby experiments, it would amount to £14,583 6s. 8d. per annum. It will be thus seen that we are annually casting into the river Ribble nearly £15,000, and by so doing polluting it, poisoning and preventing the salmon and other fish from coming up, thereby making it scarce and dear.

The plan that we should recommend to be adopted is to intercept the sewage at the marsh end, at the junction of the two sewers which now drains the whole of the town. From this point should be built a brick sewer three feet diameter on to Ashton marsh, taking the easiest and best route through the ship yard and Water lane. The present outlet in the ship yard to still remain and to be used as a overflow drain in thunder storms and continuous heavy rains. At the most convenient spot on the Ashton marsh should be built three rows of precipitating tanks similar to those shown in Plan No. 1. These tanks to be fit up with deodorising chamber filters and other requisite appliances. At

these tank works, a twelve horse-power steam engine, boiler, pumps, &c., should be erected for pumping the sewage from the tanks on to the land, for emptying the solid matter from the tanks, and for other work which may be required.

These marsh lands to be levelled and laid out on the pane and gutter system, similar to plan No. 2. From these tank works should be laid 12 inch sewage pipes along the higher part of the land right on to Freckleton; these pipes should be laid on the surface of the ground, and stations or basins constructed at intervals from which the sewage could be conducted over the land in open carriers by gravitation when wanted. Thus the whole of the land would be laid out in sections, and each section irrigated at its proper stated time and with the proper quantity of sewage. In long frosts, wet seasons, or at other times when the sewage is not wanted on the land, then instead of pumping it on the land it should be allowed to pass through the deoderizing chambers and gravel filter beds into the outlet sewer, and empty into the river, which it would do in almost a pure state.

The night soil and street sweepings should be carted to this tank depôt and there mixed with the solid sewage matter; and if found necessary works should be erected for drying, mixing, and making the whole into guano, easy of removal and good for agricultural purposes, or it could be sold in its present state to farmers for its marketable value. The removal of night soil is troublesome to deal with in most boroughs, especially where contracted for. It is more satisfactorily performed where the authorities employ their own night workmen with proper superintendence. Horses, carts, and drivers, might be contracted for to take the refuse to the depôts out of the town; this refuse would almost pay for its removal. We will now give the probable cost of these works, also the annual expenditure in conducting the works:—

ESTIMATED COST.

850 acres of land @ £4 per acre at 20 years purchase		£68000
Forming and laying out the land, farm buildings, roads, fences, embankments, &c.	£8300	
Brick culvert and sewage pipes	2350	
Engine, boiler, pumps, and tank works complete	2600	
Contingencies on cost of works, 7½ per cent.	1000	
		14250
		£82250

ANNUAL EXPENSES.

Interest—5 per cent. on purchase of land, cost of works, &c.	£4122
Superintendence, repairs, charcoal, coal, management, incidental expenses, &c.	1150

It will
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ERRATA.

Page 34, line 15, for £4122 read £4112 10s.
Page 34, line 18, for £5272 read £5262 10s.
Page 35, line 13, for £18 4s. 1d. read £18 14s. 1d.

sewage can not be applied to the land. We shall still have three million tons per annum for irrigation purposes; this will give 3529 tons per acre per annum.

From the experiments instituted at Rugby it would appear that between 3000 and 4000 tons of liquid sewage per acre per annum, is the most profitable quantity to apply to meadow or pasture land for irrigation. Then taking the estimated value of sewage as given at Rugby, Croydon, and Edinburgh, viz.:—1d. per ton, this will give for the three million tons available for Preston the sum of £12,500.

There will also be at least 30 thousand tons of solid sewage, street sweepings, and night soil, this would sell—say for 2s. per ton and would realize £3000, making an income arising from utilizing the sewage of £15,500 per annum.

The probable gain clear of all expenses likely to accrue from this undertaking would be upwards of £10,600, which will be better seen from the following figures:—

		PER ACRE PER ANNUM.
Value of land without sewage	£3400—	4 0 0
Produce due to sewage, viz.:— 3,000,000 tons per annum @ 1d. per ton	12500—	14 14 1
	£15900—	18 4 1
Deduct interest and working expenses	5272—	6 4 0
Total Gain	£10628—	12 10 0

These figures speak for themselves and therefore need no comment. But should the produce from the land increase in value similar to what it has done at Edinburgh, and there is no reason why it should not do so, then instead of £18 14s. 1d. per acre or £15,900 per annum as given in the above table, it would be £27 10s. per acre or £23,375 per annum; deduct from this £6 4s. per acre or £5272 per annum as interest on capital and working expenses, and we shall have the sum of £18,103 per annum or £21 6s. per acre as the result of the application of sewage irrigation.

The above sum would be equal to a reduction in the rates of Preston of upwards of 2s. in the pound, or taking the lesser sum as given in the table above, viz.:—£10,628 per annum, this would be equal to nearly 1s. 6d. in the pound of a reduction in our rates, and either of these results are well worthy of an effort being made to accomplish.

ESTIMATED COST.

850 acres of land @ £4 per acre at 20 years purchase		£68000
Forming and laying out the land, farm buildings, roads, fences, embankments, &c.	£8300	
Brick culvert and sewage pipes	2350	
Engine, boiler, pumps, and tank works complete	2600	
Contingencies on cost of works, 7½ per cent.	1000	
		14250
		£82250

ANNUAL EXPENSES.

Interest—5 per cent. on purchase of land, cost of works, &c.	£4122
Superintendence, repairs, charcoal, coal, management, incidental expenses, &c.	1150
	£5272

It will be seen from these estimates that the annual expense per acre will be £6 4s. 0d.

We have previously stated that the quantity of sewage is three and a half million tons per annum; deduct from this half a million tons for frosty weather, heavy rains, and at other times when the sewage can not be applied to the land. We shall still have three million tons per annum for irrigation purposes; this will give 3529 tons per acre per annum.

From the experiments instituted at Rugby it would appear that between 3000 and 4000 tons of liquid sewage per acre per annum, is the most profitable quantity to apply to meadow or pasture land for irrigation. Then taking the estimated value of sewage as given at Rugby, Croydon, and Edinburgh, viz.:—1d. per ton, this will give for the three million tons available for Preston the sum of £12,500.

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		PER ACRE PER ANNUM.		
Value of land without sewage	£3400—	4	0	0
Produce due to sewage, viz.:—3,000,000 tons per annum @ 1d. per ton	12500—	14	14	1
	£15900—	18	4	1
Deduct interest and working expenses	5272—	6	4	0
	—[illegible]	12	10	0

[illegible] and therefore [illegible] produce from [illegible] what it has [illegible] eason why it [illegible] 14s. 1d. per [illegible] in the above [illegible] 10s. per acre or £23,375 per annum; deduct from this £6 4s. per acre or £5272 per annum as interest on capital and working expenses, and we shall have the sum of £18,103 per annum or £21 6s. per acre as the result of the application of sewage irrigation.

The above sum would be equal to a reduction in the rates of Preston of upwards of 2s. in the pound, or taking the lesser sum as given in the table above, viz.:—£10,628 per annum, this would be equal to nearly 1s. 6d. in the pound of a reduction in our rates, and either of these results are well worthy of an effort being made to accomplish.

Chapter VI.

DESCRIPTION OF AUTHOR'S PLAN.

If there be one subject more than another that deserves the serious attention of all who are interested in sanitary improvements, it is the subject of house and town drainage—considered with a view of suggesting some approved general scheme for dealing with the sewage and refuse of large towns. Such is the spirit in which the author of this treatise has entered upon this interesting and important subject.

We will now briefly describe the system shown in Plan and Section No. 1:—Instead of turning the sewage into the rivers at the nearest or most convenient point, as is the case at the present time, it ought to be conveyed by means of an intercepting sewer to some convenient spot near the banks of the river or stream; or if found more advantageous, near a canal below and some distance from the town. In the latter case the surplus sewage water, when deoderized and filtered, would become an excellent feeder to such canal; but in either case the works here recommended should be constructed on or near land whereon to utilize the liquid sewage by irrigation. On this selected spot should be be built three rows of brick or stone tanks, the number and size of which to be regulated by the size of the town, or area of the district drained into them.

The end tank in each row to be fitted up as a vertical deoderizing chamber, and the last tank but one also fit up as a horizontal deoderizing chamber.

These chambers would be in cages or sectional parts, and filled with charcoal or engine cinders, as a deoderizer. In some one of each of the outside rows of tanks must be constructed the irrigation drains, by means of which the liquid sewage would be conveyed on to the land. Should the tops of the tanks be above the level of the land, the sewage will by its own gravitation flow on to the land wherever required for irrigation purposes. But, if the tanks are below the level of the land, then pumping will have to be resorted to.

Below and adjoining the tanks is shown a large gravel filter bed from which runs an outlet drain into the river, brook, or canal.

Some little distance above the tanks are shown a junction sluice chamber, this is where the sewage can be turned into any of the three rows of tanks as desired by means of iron sluice gates. Above this again will be seen the storm water basin from which runs the storm water sewer into the river; and above this again is the main drain to the town with its tributary drains running into it. The use of the storm water basin and drain is that in thunderstorms, or when a continual fall of rain causes the water in the main sewer to rise above a certain level it will run over into the storm water drain and enter the river instead of passing through the tanks, and if found desirable after the sewers have become thoroughly flushed out, the whole of the storm water could be turned into the river at this point; such storm water would contain little or no sewage and would therefore do the river no harm in the way of polluting it.

At all times when the liquid sewage would not be wanted on the land, or during a long frost, or in harvest time, the sewage would flow through the tanks, the solid matter precipitating to the bottom assisted by the use of a little lime. When the sewage came to the horizontal deoderizing chamber it

would enter it at the bottom, percolate upwards and flow into the vertical chamber. This upward filter was originally designed by the late Prince Consort. The water would then flow through the vertical deoderizing chamber and empty into the gravel filter bed and will percolate upwards through the different stratas of gravel and pass out by the outlet sewer into the river, brook, or canal, in almost a pure state. By these means the rivers would again become pure as they were in the days of our forefathers. The solid matter deposited in the tanks would be emptied as required by means of the endless chain and buckets similar to the dredging machinery, and worked by the stationary engine. When the front section of the charcoal deoderizing chamber became choked up with the sediment from the sewage it would be taken out; the other cages removed forward to the front, and another cage with new charcoal placed at the end.

The gravel filter bed could never become choked up because all the sediment is deposited in the tanks and deoderizing chambers; but this filter bed is laid out in two sections, so that if any repairs were wanted one half of the filter could be stopped working for that purpose, whilst the other half could be performing its ordinary work. There are many other little matters of detail connected with this plan too numerous to mention.

It will be seen that this scheme combines the irrigating, precipitating, deoderizing, and filtering principles; and the author flatters himself that in this simple combination of principles is what constitutes its superior advantages over all other plans yet suggested.

These principles, in parts, or as a whole, are applicable to all towns or districts, as well also as on a small scale to all works that turn their liquid refuse into the rivers and streams.

This plan has for its object the sanitary improvements of our towns, the purification of our rivers

and streams, and the progress of agricultural science.

There is no doubt whatever but that the labours of the Royal Rivers' Pollution Commission who are now engaged in the consideration of this most important subject, will result in the recommendation of some general scheme for towns to utilize and deoderize their sewage before it enters the rivers. As well, also, a plan for the various manufactories to filter their liquid refuse; and we feel persuaded that such a scheme will be something like the above.

ERRATA.

Page 25, end of 24th line, read £18 for £21.

The following letter appeared in many of the daily papers and periodicals in January last:—

A SUGGESTION TO AGRICULTURAL SOCIETIES.

SIR,—The Agricultural Societies of England have now become a very important and powerful "institution" in this country. During the last thirty years they have, by their various excellent shows of cattle and improved implements, done a wonderful amount of good, the benefits of which are now felt by all classes of society in town and country.

The breed and quality of cattle have been greatly improved; the progress made in agricultural implements has been astonishing; the laying out and manuring of lands, and the rotation of crops, have also been greatly improved upon. Larger and better crops have been the result, and all this has been either entirely or partly brought about by agricultural societies and their great annual competitive shows throughout the country.

But there is yet one great problem connected with agriculture to be solved. At the present, and for some time past, civil engineers and sanitary reformers have been endeavouring to solve it; but I feel convinced that our more advanced Agricultural Societies will have to step in with their able aid and assistance in this important matter. I allude to the utilization of town sewage. The late Earl of Derby offered £100 to any town in North Lancashire that would adopt a system of utilizing sewage similar to that carried out at Wellington College, Sandhurst, by which the sewage matter of the district is intercepted, and made available for agricultural purposes. The prize has not yet been awarded—not even competed for—and from all appearances, is not likely to be.

As the various Agricultural Societies are now about fixing upon the towns wherein they intend holding their next shows, also fixing the prizes they intend awarding, I beg respectfully to suggest to all Agricultural Societies the great necessity of offering prizes for the first, second, and third best systems of utilizing town sewage for agricultural purposes, such plans or systems to be shown by models and drawings at the forthcoming shows. The judges in this department ought to be gentlemen possessing a practical knowledge of agriculture, as well also of civil engineering. I feel certain that if this suggestion were acted upon, it would greatly assist in solving this most important question, and by so doing the science of agriculture would be benefited, and our large cities and towns improved in a sanitary as well as in a pecuniary point of view.

Yours, &c.,

J. DUTHIE.

Preston, Jan. 26th, 1870.

W. AND J. DOBSON, PRINTERS AND LITHOGRAPHERS, FISHERGATE, PRESTON.

INTERMITTENT DOWNWARD FILTRATION.

INTERMITTENT downward filtration through natural soil is not only applicable (1) when carried out by itself, in cases where suitable land cannot be obtained in sufficient quantity for surface sewage irrigation; but it may be adopted (2) with great advantage in combination with surface irrigation, as a means of enabling the sanitary authorities of towns to utilize and cleanse the sewage they have to dispose of, with the greatest chance of profit, and upon any quantity of land—be it much or be it little—that may be obtainable at a fair price; or, to use a common phrase, it enables them, in the disposition of sewage, to 'cut their coats according to their cloth.' But this is not all: intermittent downward filtration is pre-eminently valuable (3) when used in combination with surface irrigation, as a means of positively securing to the latter a profit which it cannot command by itself.

The origination of intermittent downward filtration as a means of purifying sewage, is due to Dr. Edward Frankland, of the present Rivers Pollution Commission, and was first brought before the public, as a suggestion, in the Report of that Commission, dated 18th February 1870. The Commissioners therein explained the very valuable experiments carried on by them in their laboratory, which enabled them to announce that, by a properly conducted system of intermittent filtration, natural soil, effectively under-drained, might be

used as a filter to cleanse town sewage to a degree of purity sufficient for all but domestic purposes. When recommending this process as a means of purification, which might well be tried in exceptional cases, viz. where surface irrigation would not be applicable, the Commissioners thought it their duty, however, to state that there were 'three formidable objections' to it. They say: 'First, it is entirely unremunerative, the amount of sewage applied to a given area of land being probably in such a case too great to permit of the growth of any ordinary agricultural crop. Second, the whole of the manure ingredients of the sewage would be absolutely wasted. And third, the collection of solid fæcal matters upon the surface of the soil, with no vegetation to make use of them, would probably give rise to a formidable nuisance, especially in hot weather.'

In the month of June of the same year (1870), I was appointed by the Lords Justices of Appeal in Chancery to devise a means of preventing the pollution of the River Taff, in South Wales, by the sewage of the town of Merthyr-Tydvil, which had been the subject of complaint and litigation. Having made sundry experiments on the percolation of water through different soils (see *Journal of the Royal Agricultural Society of England*, Vol. xx. Part ii.), I had obtained a confidence in the process of intermittent downward filtration which I should not otherwise have possessed, while, as an engineer extensively engaged in drainage works, and practising steam-cultivation, I had become satisfied that, with land specially and deeply under-drained and deeply cultivated, with the surface properly prepared for the purpose, not only could the sewage be cleansed of its impurities, as advanced by Dr. Frankland, but that the process could be rendered free from

all nuisance, and that vegetation could be grown on the surface of the land at the same time. Accordingly, I recommended to the Lords Justices of Appeal the adoption of intermittent downward filtration through natural soil for the purification of the sewage of Merthyr-Tydvil, and was ordered to see that the necessary works were carried out there.

As my name has consequently been associated with intermittent filtration, I have thought it right thus to explain my first participation in the matter; for I have no wish to take upon myself any credit with respect to the process, beyond that of having successfully overcome the objections the Commissioners had anticipated.

The results of intermittent filtration at Merthyr show—

First, That crops of an ordinary agricultural character can be grown on the surface of the filtration areas at the same time that the sewage is applied to them.

Second, That the return by the sale of crops, *calculated on the total amount of money expended in the necessary works*, exceeds that derived from any instance of surface irrigation yet recorded.

And third, That the process may be carried out without nuisance, or, in fact, without the escape of any objectionable odour recognisable twenty yards from the place of application.

Hence the objections anticipated by the Rivers Pollution Commissioners have been severally met and refuted.

I should also state that the Merthyr filtration areas have now been in use about two years, and that the purifying power of the soil remains unimpaired.

As to the application of the process of intermittent down-

ward filtration, in cases where suitable land of a sufficient quantity cannot be obtained for surface irrigation.

The Rivers Pollution Commissioners state, in the Report referred to, that an acre of suitable land, drained six feet deep (affording thereby two cubic yards of filtering material for every square yard of surface), will cleanse the sewage of 3300 people up to the standard suggested by them. Abstractedly this is correct; but, to secure permanency of effect and constant freedom from such objections as were anticipated by them and others, I have altered the proportion from 3300 persons to an acre, to 1000 persons per acre; the arrangement being—

I. That, instead of one area, three areas of an equal extent should be provided, each capable of cleansing the whole of the sewage, and of growing crops at the same time.

II. That each area should be used for the purification of the sewage for one year only at a time, so that two out of the three areas may be for two years in succession devoted to full plant growth.

In this way a considerable return per acre will be obtained from the two areas not in use for sewage filtration, as they would receive only just as much sewage as would produce the most abundant yield; while the occasional relief from sewage thus afforded to the area devoted to filtration, would favour an increased production from that area also.

It need hardly be pointed out, that in those cases where the depth of drainage is perforce of physical circumstances reduced to less than 6 feet, the superficial extent must be increased in proportion as the depth of the under-drainage is diminished, in order to secure the necessary quantity of filtering material for purification.

If clay soils be used, special precautions will be necessary to prevent the cracking of the land, and the passage of the sewage by the cracks to the under-drains; and it may also be observed that land may be too porous as well as too dense,—not because of any incapability of percolation, but because of the fact that, while the one will crack, the other will be too coarse, and let the sewage down in an impure condition to the under-drains, and so fail in the character of a purifying filter.

The preparation of land for intermittent downward filtration is, in truth, essentially the work of an engineer, as the quantity of sewage to be cleaned must be apportioned to the character and quantity of soil used as a filter, and proper precautions must be taken to prevent the sewage passing to the under-drains without adequate filtration.

As to the use of intermittent downward filtration in combination with surface irrigation.

Though the advantage of being able to resort to a process allowing of the permanent cleansing of the sewage of 1000 persons through a single acre of land, without nuisance, and attended with a large acreage yield of crops, will be found immense in the case of manufacturing towns in close proximity to each other, or when situated in narrow valleys deficient in land available for irrigation, *the most frequent application of intermittent filtration, when better understood, will take place in connection with surface irrigation as a safety valve*, and as a means of securing a profit not otherwise obtainable from that mode of utilization alone. It enables sanitary authorities to utilize nearly any quantity of land obtainable—*i.e.* to 'cut their coats according to their cloth,'—as, for every acre applied to inter-

mittent filtration, nine acres which would otherwise be required for irrigation may be saved. With such a combination, the greater the quantity of land that can be purchased at a moderate cost and devoted to sewage farming, the greater will be the return. Hitherto sewage farming—which is synonymous with sewage irrigation—has not been profitable, and has not been readily taken up by the practical farmer and market gardener, because both are disinclined to take upon themselves the obligation of disposing of any quantity of diluted sewage that may be discharged from a town, though they may each appreciate the fertilizing elements it contains, as well as the advantages of a plentiful supply of water. In fact, they decline dealing with sewage that may be discharged at all times and under all circumstances—in wet weather and in dry weather, in winter and in summer, at night as well as in the day, and when the land is in crop as well as when it is in fallow—*simply because they cannot be masters of their own proceedings.* To modify these disadvantages, intermittent downward filtration can be brought into use in a very simple and distinct manner. Take, for example, a town of 50,000 inhabitants, ordinarily requiring for surface sewage irrigation 500 acres of land. I should suggest that 50 acres out of the 500 should be prepared for intermittent filtration, and divided into three areas, as already described. These 50 acres would be sufficient of themselves for the permanent purification of the sewage free from all objection; while, to secure the utmost return from the remaining 450 acres, which would be laid out for ordinary surface irrigation, the sewage would be delivered when, and only when, it may be advantageous to apply it. By such means the maximum yield would be obtained not only from the 450 acres

under surface irrigation, but from two-thirds of the 50 acres prepared for intermittent filtration.

With the power on the part of the farmer, or market gardener, to treat land laid out for surface irrigation exactly as he pleases, and to take the sewage when and in what quantity he wishes, it is clear that he can afford to pay, in addition to the actual value of the land, a fair amount for the manure ingredients of the sewage, as well as for the water by which it is diluted, and which at certain times of the year is even more valuable than manure itself. By a 'fair amount,' I do not mean such estimates as chemists have put upon the intrinsic value of the manurial elements of sewage, but such a price as practical horticulturists and agriculturists would put upon an equally productive dressing of ordinary manure with the extra advantage of water.

Intermittent downward filtration must not be considered and treated as 'intensified irrigation,' as it has been called; it must be designed and carried into operation as a distinct work, to be utilized at all times when it would prove the *safety valve* to irrigation. And it is, as I have said, the work of an engineer, because it involves an accuracy of treatment which does not apply to ordinary surface irrigation. It does not necessarily follow, however, that the preparation of land for intermittent filtration is much more expensive than for surface irrigation. Where the land is suitable and the surface flat, it will only be in the increased extent of permanent carriers and the greater capacity of the under-drainage pipes that the cost will be greater.

Comparing *intermittent downward filtration through natural soil, per se,* as herein described, with *surface sewage irrigation, per se,* as ordinarily adopted, the

extent of land required for permanent success will be found to be as 1 in the former to 10 in the latter case; while the return *on the money expended* will, in the majority of cases, be much greater from intermittent filtration than from surface irrigation.

Comparing *ordinary surface sewage irrigation, per se*, with a *combined system of irrigation* and *intermittent filtration*, in the way herein explained, the original outlay will be found to be somewhat greater in the latter case than when either process is adopted by itself; but the return per acre in the case of the combined system will be found to exceed very largely that from irrigation or from filtration alone.

The only real difficulty in intermittent downward filtration is experienced in cases where the population contributing the sewage is limited, and where the discharge of the sewage is very irregular,—at some period of the day the volume being little or nothing, and at times considerable. In such cases it is difficult to deliver to the land the precise quantity of sewage it is designed to absorb and purify. The same thing, but in a less degree, occurs in surface irrigation, where the object should be to give to vegetation only that quantity of sewage which it can appropriate with advantage. But this difficulty has now been overcome by the use of the patent 'automatic sewage meter,' of which I enclose particulars* for those persons attending your meeting who may be interested in the subject.

J. Bailey Denton.

22 Whitehall Place,
London, *January* 14, 1873.

* The Automatic Sewage Meter is particularly suitable for small towns, isolated institutions and mansions, and villages. It gets rid of cess-pits altogether, and enables the inhabitants to utilize the liquid refuse of dwellings economically and profitably, independently of the rate of outflow.

THE

SEWAGE QUESTION;

WITH

REMARKS AND EXPERIMENTS

SHOWING THE

INEFFICIENCY OF WATER TRAPS,

AND THE

LIABILITY OF LEAD PIPES TO DECAY AND CAUSE DISEASE.

BY

A. FERGUS, M.D., M.R.C.S. Eng.,

Visitor (formerly Examiner in Chemistry), Faculty of Physicians and Surgeons, Glasgow; Pres. of Social and Sanitary Economy Section, Glasgow Philosophical Society; Vice-President of Glasgow Medico-Chirurgical Society; Member of Council of National Association for the Promotion of Social Science; Associate Member of the Metropolitan Association of Medical Officers of Health; Member of Health Committee of the City of Glasgow," etc., etc.

GLASGOW:
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PREFACE.

I HAVE to apologize for presenting the following matter to the public in such a fragmentary form, and the only excuse I can offer is that my address was not intended for publication. Having been asked, however, that it might be so, I have endeavoured to render it more worthy of acceptance by adding some Appendices. The first of these produces proof as to the production of diarrhœal disease from excremental pollution. The second enters into details as to the decay of lead soil-pipes, first observed several years ago by myself, of which an illustration is given. The third Appendix is devoted to proof of the views first brought before the public, so far as I am aware, in my own Address on the passage of gases through water. The experiments then exhibited in proof of this have been repeated and revised, and the result has always been the same, as stated in the text and in Mr Mactear's remarks. In confirmation of my opinion that water traps are unfit to exclude gases, I am told by plumbers that in the space between the bend or trap of a w.c. pipe and the pan of the closet, there is generally a quantity of sewer-air, which must have found its way there by passing through the water.

Appendix No. IV. refers to the question whether any injurious effect is produced by the pollution of our river on the health of the people living near it; and No. V. is an extract from the Report of Messrs Anderson and Bateman, condemning the present system of water-carriage, to which condemnation I cordially assent.[1]

Our present system is wasteful in an economic point of view, and hurtful to health by allowing excretal matters to decompose in the sewers. The only true mode of purifying a river is to keep all putrescible matter out of the sewers flowing into it.

Since the annexed pamphlet was written, a return, which has been a considerable time in preparation, has been completed, and some of the facts brought out by it may be usefully stated here. In the first place, we are struck with the small number of public works, within the municipal bounds of Glasgow, discharging refuse into the sewers—viz., 55 discharging organic, and 54 chemical refuse. We do not see any great difficulty in dealing with these, and in so purifying the water before it is discharged into the sewers as to bring it up to the standard of purity recommended by the River

[1] As my paper is passing through the press, I have added a sixth Appendix, containing remarks on Messrs Bateman and Bazalgette's scheme.

Pollution Commission. As regards w.c.'s, we find a total of 31,927, distributed as follows:—In warehouses, counting-houses, etc., 2550; in shops, 1709; in houses of more than two apartments, 21,512; in houses of two apartments, 5702; and in houses of one apartment, 454. Any one can imagine with how little attention to decency they can be used in the last, and possibly also in the second last, of such dwellings. Again, if we look at the limited air space, compared with the number of inmates, as well as the imperfect way in which in cheap tenements the w.c.'s are got up, we can readily understand their injurious effect on the health, so emphatically stated by the Medical Officer of Health for Edinburgh (*Edinburgh Medical Journal*, 1872, page 1129). If there is any reliance to be placed on the experiments as to gases getting through water, sinks must play an important part in the causation of diseases arising from sewer-gas; and we find that in Glasgow there are no fewer than 11,185 of these in houses of one apartment; in houses of two apartments, 30,502; and in houses of more than two, 29,604. Another mode by which communication exists with the sewers is the modern luxury of fixed basins, and of these the number is 3865. If we sum up the w.c.'s, sinks, and fixed basins, we shall find that in Glasgow there are within the houses, exclusive of the baths, 107,083 openings into the sewers.

The mode in which the cisterns are placed immediately below the w.c. of the flat above, and the objectionable practice of drawing the domestic supply from a cistern so placed, has often been denounced as a sanitary evil. From the return to which I have alluded, we find that the number of cisterns for both w.c.'s and domestic supply is 8940; the number of cisterns for w.c.'s only, is 14,957; and the number of houses supplied directly from the main is 55,394.

The only other point in the return which we shall notice as bearing on the public health, is the soil-pipes, of which there are still 17,701 which are not open to the air at the top. There are 31,802 which *are* open. I do not use the term *ventilated;* it would be a misnomer to do so; inasmuch as ventilation means the replacing of foul air by fresh. The opening of soil-pipes at the top is certainly good in a sanitary point of view, as it allows the foul air to escape; *but it cannot be called ventilation, because the foul air in the sewers and pipes merely escapes, not to be replaced by fresh air, but by equally foul air from the sewers.* I must be allowed strongly to insist on this; because we are so frequently shown plans to ventilate sewers, none of which really do so; they merely allow egress for foul air, often with very praiseworthy plans for disinfecting it; but do not replace the foul air by fresh, as all ventilation properly so called ought to do.

THE SEWAGE QUESTION;

WITH

REMARKS AND EXPERIMENTS SHOWING THE INEFFICIENCY OF OUR PRESENT SYSTEM OF TRAPPING.

By ANDREW FERGUS, M.D., Glasgow.

(*The Opening Address, Session* 1873-74, *of the Sanitary and Social Economy Section of the Glasgow Philosophical Society.*)

REPRINTED FROM THE EDINBURGH MEDICAL JOURNAL FOR FEBRUARY 1874.

THERE are few subjects on which the public mind is more bewildered than that of the sewage question; and to such an extent does this prevail, that some men who formerly felt interested in it have sunk into a state of apathy, and have become persuaded that there is no satisfactory solution of the matter. Perhaps this is not to be wondered at, if we consider the variety of plans suggested, and the diversity of opinions expressed on the subject. One great cause of this diversity, we think, is that the question has not been broadly looked at. It is many-sided; but writers have not recognised this, and have only looked at it in one aspect. An engineer looks at it from his own point of view, and a grand engineering scheme is the result; an enthusiastic agriculturist does the same, and sewage irrigation is to solve all our difficulties and enrich the country; the sanitarian looks to the public health, and tells us to keep all excretal matters out of the sewers, and so prevent the generation of sewer-gas; while the general public, offended by the unpleasant odours of our rivers, urgently demand their purification. Now, the question is certainly an engineering and mechanical one; it is also agricultural, economic, and chemical; but it is, above all, sanitary; and, to arrive at anything like a satisfactory solution of it, we must consider it under all these aspects, without allowing any of them unduly to influence us, or having our minds warped in any one direction. To-night we must chiefly confine our attention to the sanitary aspect of our subject.

Our first question, then, is, What is sewage? We reply, It is

a mixture of rainfall (including also subsoil drainage), household slops, the excreta of man and animals, and the organic and inorganic refuse of public works. As regards the first of these,—the rainfall,—it will, of course, vary in different localities, but must in all cases be regarded as beneficial, inasmuch as it will assist in flushing the sewers. In addition to the rainfall, we have often the contents of various small streams or burns which have been converted into sewers by having been arched over. This is a most pernicious practice, because the beds of these streams are always irregular, retarding the flow of the sewage, and affording facilities for matter to accumulate and decompose in their crevices and irregularities, and thereby occasioning much evil. The entrance of subsoil water into our present system of sewers is also an evil, because, where it can do so, the contents of the sewers and the gases generated in them can also escape, polluting the soil and any water that may be drawn from it for domestic use.

Of course this evil will be primarily felt in small towns and villages, where the inhabitants depend upon wells for their domestic supply; yet, even in cities, the constant pollution of the earth by the escape of sewage must in the long-run become deleterious.

As regards the other matters in sewage, we must enter a little more into detail, by saying that, with very rare exceptions, the really injurious ingredients are the organic, or, in simpler language, those which are capable of decaying. To estimate, therefore, the amount of what is hurtful, we must inquire how much is organic in the other constituents of sewage.

As given above, let us take house slops, and we shall find, 1st, of polluting ingredients, the soap with which we wash, and the dirt from our clothes and bodies. Now, a large portion of the soap consists of soda and potash, salts which cannot under any circumstances become deleterious or polluting. 2d, We have the organic matter derived from the cooking of food and the washing of cooking utensils and dishes. Chamber slops come under the next head, viz., excreta; but I do not think it impossible to keep these separate from other house slops, as will be shown when we come to treat of remedial measures.

We have dwelt thus prominently on house slops, because we have repeatedly heard it objected to all dry systems that they do not deal with these, and they are spoken of in such terms as might lead the unlearned to believe them equally injurious with the excretal matters.

We think that the sources of organic contamination from house slops are confined to, 1st, Soap, mixed more or less with animal matter from our bodies, and the washing of our clothes; 2d, Water containing organic matter from the washing of pans, dishes, etc.; 3d, Water in which vegetables have been cooked. We need not particularize water in which meat has been cooked, as it is to be hoped it may generally be utilized for food.

If the foregoing be a correct account of the organic ingredients in house slops, it will be seen that *they* cannot be charged with any considerable amount of organic pollution, and might, as far as Glasgow is concerned, be passed with impunity into the rivers—our large domestic water-supply and great rainfall so diluting them, as to render them innoxious. The water of such rivers would not be more impure than the standard allowed by the River Pollution Commissioners, which is as under.[1]

The same cannot be said of the next ingredient that enters into the composition of sewage—viz., the excreta of man and animals. We are inclined to charge these with being, in a high degree, the cause of our present difficulties, and believe that ignorance has had much to do with converting our streams and rivers into gigantic cesspools. We use the term "ignorance" advisedly, for we are sure that when water-carriage was first introduced, the results that would flow from it were not anticipated—in fact, were never investigated. The idea acted upon was, that if these offensive matters were once out of sight, there was an end of them, and no evil consequences were ever dreaded. It was known that water, or rather the air dissolved in water, would oxidize excretal matter, but bitter experience has very sadly proved that no river in the kingdom can oxidize the excreta from the cities on its banks. Yet water-carriage has been adhered to with the most wonderful tenacity, and schemes without number have been devised to purify the water, but none of them, so far as we are aware, has ever been completely successful.

The last ingredient of sewage is the organic and inorganic refuse from chemical and other works—all of which, we hold, should be kept out of the sewers; and if a law were passed enforcing this, we have little doubt but that in the majority of cases the saved material would recoup the expenses of the process.

[1] Any liquid containing, *in suspension*, more than three parts by weight of dry mineral matter, or one part by weight of dry organic matter, in 100,000 parts by weight of the liquid. Any liquid containing, *in solution*, more than two parts by weight of organic carbon, or three parts by weight of organic nitrogen, in 100,000 parts by weight. Any liquid which shall exhibit by daylight a distinct colour when a stratum of it, one inch deep, is placed in a white porcelain or earthenware vessel. Any liquid which contains, in solution, in 100,000 parts by weight, more than two parts by weight of any metal except calcium, magnesium, potassium, and sodium. Any liquid which, in 100,000 parts by weight, contains, whether in solution or suspension, in chemical combination or otherwise, more than ·05 part by weight of metallic arsenic. Any liquid which, after acidification with sulphuric acid, contains, in 100,000 parts by weight, more than one part by weight of free chlorine. Any liquid which contains, in 100,000 parts by weight, more than one part by weight of sulphur, in the condition either of sulphuretted hydrogen or of a soluble sulphuret. Any liquid possessing an acidity greater than that which is produced by adding two parts by weight of real muriatic acid to 1000 parts by weight of distilled water. Any liquid possessing an alkalinity greater than that produced by adding one part by weight of dry caustic soda to 1000 parts by weight of distilled water.

If an Act be passed compelling all such refuse to be purified in some way or other, we shall be quite prepared for a great outcry against its hardships, and expect to be told that it will be utterly ruinous to the manufacturers.

The same thing happened at the passing of the Alkali Act, yet we believe that few will deny that the saving of the material that polluted the air has resulted in profit.

Organic matter may be rendered valuable by Mr Chapman's process; but if it will not pay to do so, it ought to be removed by downward intermittent filtration, before the water from any public work is allowed to enter the sewers.

Having thus briefly considered the composition of sewage, let us now turn our attention to the mode in which, by drainage, it is conveyed away.

The primary objects of drainage being to remove surface water and dry the subsoil, that system must be the most perfect which will both receive and freely carry off the surface water, while at the same time the joints of the system must be so formed that they will allow the gradual drainage into the sewers of the water lodged in the subsoil.

Wherever this is carried out, there is at once a marked improvement in the health of the inhabitants, as illustrated by the following from Mr Simon's Report:—In nine towns out of twenty-five where there were drainage works, there was a reduction of more than fifty per cent. in the cases of typhoid; in ten, a reduction from one-half to one-third; while in four there was either a slight decrease or an absolute increase. In these four the sewage was pumped. It was quite expected that as drainage works were carried out there would be a diminution in zymotics, but experience proves that it has also a favourable influence as regards phthisis.

If the sewers were kept only for the rainfall, no injury could happen from them to the community; but from mistaken notions, and by a bad habit, which has perhaps grown upon us insensibly, we pour all sorts of refuse into the sewers, and they again pass them on to our rivers.

If we are ever to solve the problem of the sewage question, it will be by a return to nature's own laws—namely, pass the rainfall as speedily as possible into the water-courses and rivers, and apply the excreta to the soil.

If the question only involved the purification of rivers, I would not interfere with it; but I believe our present system so highly injurious to public health that I am most anxious to bring it prominently before you to-night.

If the urine and fæces are kept separate, decomposition is retarded, but it is facilitated by their admixture, and still more so by the addition of water. I hope the advocates of our present system will keep this in mind, as they often dwell on the importance of at once getting quit of refuse matter. If we did so, there would be no sewer-gas, and no need of sewer ventilation.

Fresh excreta, except perhaps as regards one disease, are not injurious to health, but become so as soon as they decompose and give off gas.

1st. By vitiating the air in general, and depriving it of oxygen gas, as all decomposition of organic matter must of necessity do.

2d. Excreta from diseased patients produce like diseases in persons previously healthy, and this in some diseases is intensified enormously by the decomposition of the excreta.

3d. Sewer air contains a putrid organic vapour, which is highly injurious to health.

4th. Decomposition of excreta is very much facilitated by their being shut up in limited spaces, with a moderate supply of air and water; therefore, our modern house and service sewers and cesspools are ingeniously-contrived chemical laboratories for the production of sewer-gas.

5th. When sewage matter in a state of decomposition is exposed freely to the air, it ceases to be injurious to health.

I now proceed to make a few remarks on some of the above heads.

1st. All decomposition is simply a burning up or combustion of the oxygen of the air with the organic matter; therefore where this goes on there must be a diminution of the vital constituent of the atmosphere, and Dr Angus Smith, in his excellent work, has shown the diminution of oxygen in the air of towns.

Possibly further investigation will show how much of this is owing to the breathing of the population, and how much to the decomposition of organic matter in middens and sewers.

Another phenomenon attending decomposition is the evolution of heat. Sewage is generally hotter than the atmosphere, and it is yet to be decided how much of this is caused by hot water from houses and works passing into the sewers, and how much is owing to the decomposition. I incline to believe that the latter plays an important part in it.

The excreta of even healthy people are injurious when decomposed, inasmuch as they diminish the amount of oxygen in the air, and we find that breathing air with a diminished percentage of oxygen has a lowering effect on the general health.

My second head is of much greater importance, and must be dwelt upon at some length.

No one who has paid the least attention to sanitary matters will deny that excretal pollution is a powerful agent in the promotion of preventible disease; but I do not think that sufficient attention has been paid to the fact that in some diseases which are generally ascribed to this source there has been a very large increase since the introduction of water-closets.

In order to show this, I have here a table, for the working out of the percentage of which I am indebted to Mr Scott—the mention of whose name at this Section will be a sufficient guarantee for accuracy.

TABLE *showing the Number of Deaths per annum in England and Wales, from certain Zymotic Diseases separately, and from other Causes taken together, for each Million of Living Population*—1838-71.

Year.	Population.	Deaths per annum for each Million of Living Population.								
		Diphtheria.	Diarrhœa, Dysentery, and Cholera.	Scarlatina.	Fevers.	Measles.	Hooping-cough.	Smallpox.	Total from Zymotic Diseases.	Total from other Causes.	Total from all Causes.
1838	15,287,699	...	225	380	1228	426	596	1064	3919	18,502	22,421
1839	15,514,255	...	225	665	1010	705	526	589	3720	18,130	21,850
1840	15,730,813	...	205	1260	1092	593	390	663	4303	18,562	22,865
1841	15,929,492	...	264	889	932	433	508	400	3426	18,166	21,592
1842	16,130,326	...	472	794	1004	542	502	168	3482	18,186	21,668
*											
1847	17,150,018	...	911	857	1807	507	540	246	4868	19,814	24,682
1848	17,356,882	...	890	1181	1270	396	395	398	4539	18,497	23,036
1849	17,564,656	...	†4222	746	1045	311	547	263	7136	17,962	25,098
1850	17,773,324	...	810	752	865	398	437	263	3525	17,236	20,761
1851	17,982,849	2	1004	756	907	521	440	389	4109	17,879	21,988
1852	18,193,206	4	1196	1034	1025	321	441	402	4423	17,955	22,378
1853	18,404,368	7	1114	851	1008	266	609	171	4021	18,859	22,880
1854	18,616,310	11	†2261	984	1015	498	525	151	5445	18,078	23,523
1855	18,829,000	10	799	910	875	390	541	134	3659	18,950	22,609
1856	19,042,412	12	836	732	850	374	484	119	3407	17,100	20,507
1857	19,256,516	16	1248	723	988	310	526	294	4015	17,786	21,801
1858	19,471,291	248	822	1369	918	476	588	332	4703	18,300	23,003
1859	19,686,701	487	1046	1011	807	485	456	193	4487	17,903	22,390
1860	19,902,713	262	562	486	654	480	430	138	3012	18,227	21,239
1861	20,119,314	225	1044	451	767	450	612	66	3615	18,012	21,627
1862	20,352,140	241	622	729	920	482	603	80	3677	17,774	21,451
1863	20,590,356	316	816	1480	875	551	548	290	4876	18,137	23,013
1864	20,834,496	262	881	1426	965	400	411	369	4714	19,070	23,784
1865	21,085,139	197	1228	839	1092	406	410	304	4476	18,806	23,282
1866	21,342,864	141	†1529	547	989	513	739	143	4600	18,860	23,460
1867	21,608,286	120	1006	562	781	305	550	116	3447	18,354	21,801
1868	21,882,059	138	1482	1001	960	531	422	94	4568	17,396	21,964
1869	22,164,847	118	969	1247	829	465	495	71	4194	18,131	22,325
1870	22,457,366	120	1211	1449	798	336	530	116	4560	18,387	22,947
1871	22,712,266	111	1136	818	695	409	456	1018	4643	18,026	22,669
Mean of 5 years, 1838-42		...	298	798	1053	540	504	577	3770	18,308	22,078
*											
,, ,,	1847-51	...	†1569	858	1197	427	472	313	4835	18,278	23,113
,, ,,	1852-56	8	†1241	902	955	370	520	195	4191	18,188	22,379
,, ,,	1857-61	248	944	796	877	440	524	187	3966	18,064	22,030
,, ,,	1862-66	231	†1015	1004	968	471	542	237	4468	18,530	22,998
,, ,,	1867-71	121	1161	1017	809	409	491	283	4282	18,059	22,341

I have the deaths from the principal zymotics in England and Wales from 1838.

There is a gap from 1842 to 1847, but this is not our fault, the figures are not to be found in the Registrar-General's return. Instead of dealing with gross numbers, I shall draw your attention to the deaths per annum of one million of the population. I take as

* Details not published in official returns for the four years, 1843-46.

† Including an epidemic of Asiatic Cholera, general in 1849 and 1854, partial in 1866.

types of disease produced by excremental pollution, diphtheria, and the diarrhœal group, and compare them with the others.

Of the former there is not a single death recorded from 1838 till 1851, when it is credited with two per million. In a few years—viz., 1859—it amounted to 487, and since then it has fluctuated very much, but has never been under 100. The diarrhœal diseases varied before 1847 from 225 to 472 per million; it then suddenly rose to 911, and it is, in 1871, 1136. In some cholera years it was much higher.

Let us now turn to some of the zymotics which are not so clearly traceable to excremental pollution. Measles, from 1838 to 1842, varies from 426 to 705, while in the last five years it varies from 305 to 531. Hooping-cough in the first five years was from 502 to 596; in the last five years from 422 to 739.

Fevers, from 932 to 1228 in the first five years; for the last five, from 695 to 989. Scarlet fever, during the first five years, from 380 to 1260; in the last five years, from 547 to 1449. Smallpox, for the first five years, varied from 1064 to 168; for the last, from 1018 to 71.

At the beginning of the period, the public were not fully protected by the compulsory vaccination. As regards fevers, it is to be regretted that they are all in one group. It would have been very interesting if, as is now the case, typhus and typhoid had been in separate groups.[1]

We may affirm that sanitary science had no existence during the earlier years of this return, and I hope we may assume that the diminution in the percentage of deaths in all the zymotics in our table, except three, is attributable to the labours of the sanitarian.

But how are we to account for the slight increase in scarlet fever, the striking increase in the diarrhœal group, and the addition to the list of a most deadly zymotic previously almost unknown? What changes have been made in the habits of the people which could in any way lead to so sad a result, after all the legislation and patriotic efforts to ameliorate the condition of the people, and to lessen the enormous amount of preventible death? Certainly the only great change has been the mode by which we get quit of our excreta and refuse.

[1] In addition to looking at individual years, let us examine the average of five years as given in the lower part of the table. As regards diphtheria, the average in the first five years after its appearance is 8, during the next five years 248, for the third five years 231, and for the last five years 121.

The diarrhœal group gives an average of 298 for the first five years; 1569 for the next, including the severe cholera epidemic of 1849; for the third five years, 1852 to 1856 inclusive, 1241, also with the cholera epidemic of 1854; from 1857 to 1861, 944. From 1862 to 1866, including the slighter cholera epidemic of the latter year, 1015. During the last five years, the mean has been 1161, which, contrasted with the first five years, shows us that while there has been no cholera in either period, the deaths in the diarrhœal group have increased almost fourfold.

If any one had hinted to me ten or a dozen years ago that our present system of water-carriage would prove a source of disease, I would have scouted the idea; and even when I began the study of this great question, I had perfect faith in the system, and have only been driven from it step by step as new light gradually dawned on me, and the question was sifted to the bottom.

An Act was passed in 1847 compelling all the privies of London to drain into the sewers, and we all know how soon the river became foul. The pollution of the Thames should be studied by those who attribute the foul state of our rivers to chemical and other works. There is no great amount of works on its banks, neither was there any very marked increase of these after 1847; making it, we apprehend, very clear that the Acts of 1847 and 1851, which compelled all excretal matters to be drained into the sewers, passing of course into the rivers, must have been the cause of the very foul state of the Thames in after years.

It is impossible to trace when the same system was adopted in other parts of the country; but if excretal pollution gives rise to certain diseases, and if we find these diseases increase after a change more or less complete in our mode of dealing with these matters, it is but a fair inference that the change, no matter how apparently clean, comfortable, and luxurious, must be the cause of the increase.

The subject is so very important, that I hope you will bear with me if I go somewhat into matters of detail and proof. You observe by the table that not a single case of diphtheria is to be met with till 1851; and Mr Simon asks why this disease, which overran Europe in the sixteenth century, and since then has scarcely been spoken of, should recently have become a formidable disease in England. I believe the answer to be simply this, that we have been careless in the disposal of our excreta, and have been drinking water and breathing air contaminated by excretal matter in a state of decomposition. For details I refer to the second and eighth report of the Medical Officer of Health of the Privy Council.

Lately, I was asked to see a case of diphtheria in a house where a fatal case had already occurred, and I found sewer-gas getting into it through perforated pipes. In a still more recent case I found it gaining entrance through the sink.

The production of scarlet fever from excremental pollution is probably caused by the stools of patients suffering from that disease getting into the wells, or decomposing in the sewers, and infecting the sewer-gas. Dr Littlejohn, the Medical Officer of Health for Edinburgh, says, "If they took a range of houses where this had been done (viz., water-closets introduced), in the course of one year there were double the number of deaths from typhoid and scarlet fever; and any epidemic fever occurring in these houses was intensified, and assumed a character of malignant mortality."[1]

In the report for 1870 of the Medical Officer of Health to the

[1] Edinburgh Daily Review, 18th January 1872.

Privy Council, I find a table of the places visited by the Inspectors owing to the increased death-rates.

The total number of the places so visited is 66, but as 6 of these are double entries, the number is reduced to 60. In 50 of these, the increased death-rate was traced to excremental pollution, producing typhoid fever in 49 of them, and scarlet fever in 16.

The table I exhibit fully bears out Dr Greenhow's opinion as to the increase of diarrhœa. (See Appendix, No. I.)

Typhoid fever, killing from 15,000 to 18,000 every year, is another disease arising from excremental pollution.

I think I need not quote authorities to prove this; but I may refer you to the researches of all modern investigators of this disease, and especially to the reports of Mr Simon, yearly prepared for the Privy Council. The origin of typhoid fever from excremental pollution is clearly proved in almost every volume of these valuable reports, either in the reports themselves, or in the appendices written by the various official inspectors.

Having thus, as I hope, proved that certain diseases are owing to excremental pollution, let us now turn our attention to the manner in which this pollution acts.

In many cases it does so by coming in contact with wells, and polluting the water; but this, as we all well know, cannot be the case in Glasgow, where the public wells have been shut up. There is, however, another way in which it acts most injuriously—viz., contaminating the air we breathe with the results of excremental putrefaction.

Before considering this, however, we may glance at our third head, the composition of the air in sewers.

It differs very much in different circumstances, but the gases given off by the decomposition of sewage are carbonic acid, carburetted hydrogen, sulphuretted hydrogen, and ammonium sulphide, and a putrid organic vapour, which is carbo-ammoniacal, giving rise, in all probability, to the offensive smell, and possibly also an active agent in producing disease. I do not enter upon the question of whether disease is produced by the putrid organic vapour arising from the decomposition of *all* excreta, or whether, in order to do so, there must also be the excreta from diseased patients. This has already been ably discussed, and is very interesting as a purely medical question; but time will not allow us to enter upon it to-night.

Generally the amount of oxygen is diminished, and there is always more or less of the putrid organic vapour.

In one case, Dr A. Smith found that 62 cubic feet of air from a cesspool destroyed as much potassium permanganate as 176,000 cubic feet of pure air.

But you will inquire, "How can this gas get into our houses through well-trapped drains?"

I answer that it is not easy even to trap drains.

Some people imagine that, if they have a few inches of water in a bent tube, dignified by the name of a syphon-trap, no air can pass into their houses from the sewers, and that they can ensure safety by trapping and ventilation. At one time I thought it might be possible to keep the results of the decomposition going on in the sewers out of our houses, and have frequently recommended, as the best method of ventilation, that the sewers should be connected with the furnaces of the large public works. This was quite a cherished idea, and I abandoned it with great reluctance; but experiment, observation, and reflection compelled me to do so.

If we were to connect the sewers with furnaces, or build ventilating shafts, and create a current by fire, no doubt we would have an up-cast draught of air; but I fear very little of it would come from the vitiated air in the smaller drains, but from the nearest rain-water pipe, or from the open gratings in the streets.

To illustrate how little effect trapping has in keeping sewer-air out of house drains, I show you these decayed lead soil-pipes, which, on a former occasion, I dwelt upon here, and brought forward proof to show that these perforations were caused by sewer-gas.[1] These house soil-pipes were all trapped before they entered the drains,[2] and yet you see as much gas got through the trap as to destroy the lead.

Of course, we all know that, if there is great tension in a sewer, either from its entering a tidal river or from any other cause, there will be a temporary failure of the trap, and a rush of sewer-air past it. Still I do not think such an occasional occurrence sufficient to account for what we see in these pipes. My impression is, that the cause must be one of pretty constant action, and that the following is the process going on without much cessation—viz., *the sewer-air is absorbed by the water on the sewer side of the trap, and discharged from the house end of it.*

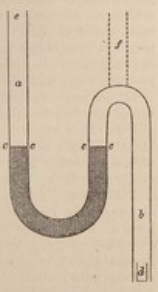
I have not hastily arrived at this conclusion; it has been forced on me as the only solution of all the facts of the case. If the destruction of good, well-made lead pipes is occasioned by sewer-air, and if this takes place in well-trapped house soil-pipes, we are shut up to one of three conclusions—1*st*, That the trap must be defective; 2*d*, That decomposition goes on in the trap itself to such an extent as to produce sufficient vitiated air to perforate these pipes; or, 3*d*, The conclusion at which I have arrived, and have stated above—the passage of sewer air *through the water* in the trap.

[1] At page 733 of this Journal for February 1872, there is a lithograph of these decayed pipes, and at pages 722 to 724 there is detailed account of how the lead pipes become destroyed, and proof is adduced to show that sewer-gas destroys unventilated (*i.e.*, not open at the top) pipes in from twelve to twenty years, and ventilated pipes in from twenty to thirty. Since writing on the subject in 1872, I have repeatedly traced typhoid and diphtheria to this source. (See Appendix, No. II.)

[2] And several of them ventilated by being open at the top.

As regards the first point, I may say that almost all the decayed pipes on the table were from houses where the drains were trapped between the house and the sewer. In one case I detected this state of pipes as a cause both of typhoid and diphtheria in an establishment where the party in charge specially congratulated himself on the security of his traps. He showed me them with no little satisfaction, and I assure you they were the most effective traps I have seen. Yet in that very establishment, four years ago, it cost upwards of £100 to renew the plumber-work thus corroded; and I am happy to mention that there has not since then been a single case either of typhoid fever or diphtheria.

As regards the second supposition, I need hardly indicate how little probability there is of decomposition going on in the trap, to such an extent as to produce sufficient gas to occasion the destruction of lead pipes. We are, therefore, shut up to the last alternative—viz., that however well drains may be trapped, sewer-gas will find its way from them into our houses;[1] and any one who is acquainted with Graham's investigations as to the diffusion of gases, will readily understand how this will happen. At the suggestion of my friend Mr Scott, this has been illustrated as follows:—



A bent tube such as the annexed was used, *a* representing house side of trap or soil-pipe; *b*, sewer side of same pipe; *c*, *c*, *c*, *c*, level of water in trap; *d*, small vessel or test-tube, from which the gas was evolved; *e*, position of the test employed; *f*, position of tube subsequently added to ventilate the trap.

In the first experiment a small glass tube was placed at *d*, containing solution of ammonia; in fifteen minutes it passed up the tube, through the water, and discharged the acid, with which litmus paper had been reddened, suspended at *e*.

In another experiment, instead of litmus paper at *e*, a small test-tube containing a little Nessler solution was introduced; in half an hour the reaction of ammonia was very distinctly visible.

The tube was left suspended by metal wire all night, and in the morning the wire was eaten through, and the test-tube had fallen to the bottom.

An objection was taken to these experiments because ammonia is lighter than air, and has a strong affinity for water. Other gases, heavier than air, were then tried; the results were much the same, as will be seen from the table now given:—

[1] Dr Angus Smith found that 8000 cubic feet of the air of a house, into which sewer-air had penetrated, destroyed more than twenty times as much potassium permanganate as the same quantity of pure air.

Gas.	Sp. gr.	Source.	Test.	Time for reaction to show.
Ammonia.....	·50	Solution	Litmus.....	15 minutes.
Do.,	do.,	do.,	Nessler.....	30 minutes. Eat through a small wire in less than 24 hours.
Sulphurous acid.....	2·25	do.,	Litmus.....	1 hour.
Sulphuretted hydrogen ...	1·25	do.,	Lead paper.....	3 to 4 hours.
Chlorine.....	2·50	do.,	Iodide of starch paper,	4 hours.
Do.,	do.,	do.,	Litmus water in trap,	Began to show in a few minutes. In half an hour the whole was bleached.
Carbonic acid,	1·50	Generated	Lime water in trap.....	1¼ hours.
Do.,	do.,	do.,	Litmus suspended at *e*,	3 hours.

Against these experiments, it was urged that the result would be different if the trap were ventilated. To meet this objection, they were repeated with the trap, somewhat modified by the introduction of a ventilating-pipe, which is represented by the dotted lines in the figure. The results were much the same, except that the reaction was a little longer in showing itself.

Mr M'Tear, F.C.S., repeated and varied the experiments, and the results will be found in the Appendix to this paper, No. III.

Possibly, some may think that I am dwelling on this subject at greater length than its importance demands; but we must all be roused from the false security of thinking that we can purchase safety by well-trapped drains.

Under my fifth head, I now remark that there has never been any evidence brought forward to prove that the state of the river Clyde, though often offensive, is in reality injurious to health. Diseases referable to excremental pollution do not prevail to a greater extent on its banks than in other parts of the city. (See Appendix, No. IV.)

I wish I had time to read to you an extract from Dr Rennie's work, on "Pekin and the Pekinese," in which he clearly proves that decomposing matter, when left freely exposed to the air, ceases to be injurious to health. When in possession of Pekin, we began to *improve* its sanitary condition, but the results were so unsatisfactory, that we were obliged to return to the ways of the "HEATHEN CHINEE." If this is correct, we should pause before we expend a vast sum of money on the execution of some grand engineering scheme, such as that of Messrs Bateman and Bazalgette, which might purify the river, but, as the sewage would be pumped, would also, in all probability, be as injurious to the public health as the same process has been in other towns.

I have a great respect for Mr Bateman, and am sorry to find

that in the 1868 report there is a departure from the sound views contained in the joint report of Dr Anderson and himself, published in 1858 (Appendix V). Possibly, in 1868, his better judgment was warped by his coadjutor, or overshadowed by the main drainage scheme for London; the success of which, in a sanitary point of view, may be tested by the following quotation from the last edition of Dr Murchison's work on Fevers, published this year. He says, "It is not a little remarkable that the increase of enteric fever in the metropolis has been contemporaneous with the completion of the main drainage scheme."

If I have succeeded in proving to you that sewer-air is injurious to health, and that it cannot be kept out of our houses, you will naturally ask me what is to be done. Are we, with Oriental fatalism, to sit down with our hands folded, and say God wills it? If such had been my conclusion, I would not have wasted your time here to-night.

It appears to me intolerable that health should be lowered and lives wasted when there may be a remedy; and I often wonder if the community will ever recognise the fact, that every case of typhoid (enteric or gastric) fever is owing to our taking into our systems the results of excremental decomposition, by breathing air or drinking water tainted by it. Besides the positive injury proceeding from our present system, there is its wastefulness, Mr Smith having shown us last year, in a very vivid manner, that its agricultural waste is equivalent to sending thousands of loaves down the Clyde.[1]

Let us now turn to what we know of remedial measures. If a future Mr Buckle shall ever write the history of human folly, as was projected by Mr Caxton, the various sewage patents that have been taken out will afford him ample material for an interesting chapter.

Sewage has been tortured in every possible way, and all kinds of materials have been employed to precipitate it, each new scheme being heralded by a vast flourish of trumpets as the solution of the difficulty, and as a perfect mine of wealth to the fortunate shareholders. I need not tell you that most of them have, of necessity, ended in failure as to the results *quoad* sewage, and disappointment as regards the shareholders. I use the term "necessity" advisedly, because most of the projectors were ignorant of the very complicated nature of the fluid they had to deal with, and fancied that, if they could precipitate the suspended and part of the soluble matters, they had solved the difficulty. Time will not permit me to enter into details as to these schemes; and it is not neces-

[1] If we are shocked by the waste of bread, how much more should our feelings be roused by the consideration that our present sanitary arrangements occasion directly the death of hundreds of our population just as effectually (but more leisurely and cruelly) as if we administered to them a dose of some potent poison.

sary, because they all propose to deal with sewage at the outfall, after the mischief has been done, involving all the evil effects that are produced by the decomposition of sewage.

No plan can at all meet the exigencies of our case which does not arrange that all decomposition in the sewers shall be prevented.

No doubt we shall meet with many difficulties in so great an undertaking, but I know of no subject for the sake of which we should face them more determinedly; and I am persuaded, if we did so, our persevering efforts would be crowned with success. Many of you are aware that some time ago the Board of Police sold the urine from the public urinals to Mr Chapman, and that he has been collecting it for some time by pneumatic pressure, and subjecting it to chemical action. To do so with the whole of the excreta of Glasgow would be a vast undertaking indeed, but I am thoroughly convinced that in some such direction we must look for the solution of our problem. Experiments have been made already as to the practicability of doing so; and leaving the water-closets to all appearance just as they are, the Board of Police fitted up what are called Hoey's closets in a set of workmen's houses to test this. The closets *appear* exactly as at present, the difference affecting only their interior arrangements. The flush-pipe is not attached to the general cistern, but to one holding only one-fifth of a gallon of water, which takes two minutes to fill. When the bar of the water-closet is raised, the supply to this small cistern is cut off, so that it is almost impossible to waste water; it could only be so by some one raising the bar every two minutes. Then the waste-pipe from the closet flows into a reservoir underground, from which the excreta are removed by pneumatic pressure, without the slightest offence, and then taken to the country, to be evaporated into portable manure.[1]

To this system it is sometimes objected that one-fifth of a gallon will not keep a closet clean; but in reply I may mention that, more than four years ago, I had a closet fitted up on this principle, and it has kept quite as clean as other closets in the same house where the supply is unlimited. Another objection urged is, that the sewage matter in the tank will decompose. This, however, has not been found to be the case.

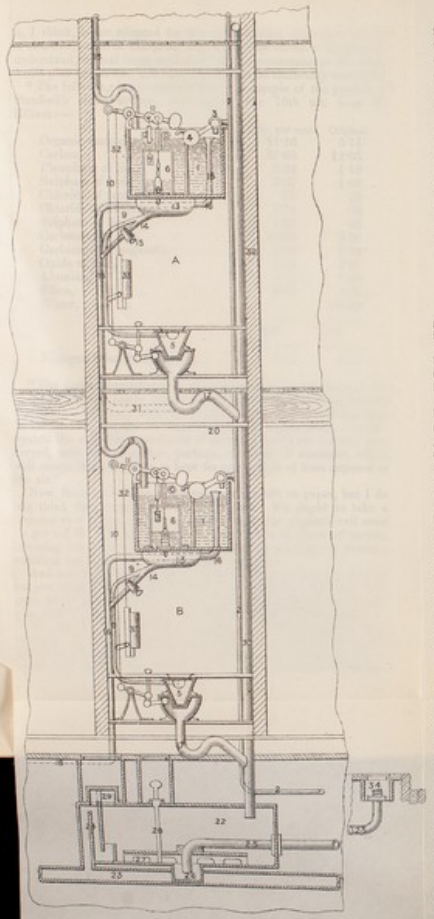
The Board of Police requested Dr Wallace to make an exhaustive analysis of this and other processes, and I shall take leave to quote what he says about Stanford's carbon process:—

"The process of Mr Edward C. C. Stanford, of the British Seaweed Company, for the utilization of excrementitious matters by means of seaweed charcoal, has been before the public for some time, and is now well known. This variety of charcoal has a wonderful power of absorbing gases—in other words, deodorizing—and

[1] The lithograph attached to the paper fully explains the mechanism of Mr Hoey's system.

HOEY'S WATER-CLOSET.

VERTICAL SECTION.



DR. WILLIAM WALLACE, PH.D., F.R.S.E., F.C.S., Consulting and Analytical Chemist, in his "Report to the Members of the Sewage Committee of the Town Council of Glasgow," writes as follows:—"This is an invention of Mr. Hoey of this city, and is intended to effect three objects:—1st. The saving of the greater portion of water usually employed in W.C.; 2d. The exclusion of "the sewage from the ordinary drains; and 3d. The utilization of the whole product, urine and feculent matter, by evaporating to dryness after adding sulphuric acid or sulphate of lime. The "following analysis[1] shows that the product is obtained in a tolerably concentrated form, and that it is of considerable value as a manure if treated in the manner above described. The use "of such a system as this will entirely prevent the formation of sewer gases and the evolution of those from water-closets; it will save a very large quantity of water now wasted; it will produce "a substance that will at least pay the cost of collection in the tanks and removal; and, above all, it will enable us to retain the use of the water-closets to which we have become habituated, and "which we will not readily part with. The system of Mr. Hoey, therefore, is well worthy of consideration."

DR. ANDREW FERGUS, M.D., M.R.C.S., in his remarks on said Report, printed by order of the Committee as an appendix thereto, says:—"The analysis of Chapman's fluid (Report, page 4) "from the public urinals is interesting as bearing on Hoey's system—which is intended to fulfil two conditions, viz. to save water and keep all excreta out of the sewers and rivers. It comes out "of the ordeal of chemical research better than could have been expected; when we consider that one-fifth of a gallon of water is used in the closet, and also that in many cases the chamber slops "will be reduced by the addition of water, we cannot expect a very valuable fluid, yet the analysis shows that it is fully equal to one-half the value of the fluid from the public urinals, as shown in "the annexed table from Dr. Wallace's analysis.[2] In a word, the fluid from the urinals is not so valuable as might be expected—possibly from the accidental addition of rain water; whereas that "from Hoey's closet is of greater value than could have been hoped for. Hoey's system meets every sanitary and economic requirement, inasmuch as it, 1st. Saves water; 2d. Prevents decomposition "of the excreta, and therefore prevents the formation of sewer gas and all the numerous ills arising from it; 3d. By its use the excreta of diseased patients cannot pass into the sewers and produce "like diseases elsewhere; 4th. It can be easily worked. It possesses this special recommendation for Glasgow, where we already have so many water-closets, that it leaves these just as they "are, and does not in the least interfere with domestic cleanliness or privacy."

DESCRIPTION OF SECTION.

A.—Vertical section of a W.C. in the first floor of a tenement originally fitted with the ordinary piping and apparatus where a pan W.C. is used.

B.—Section of a similar W.C. on ground floor of same tenement. In both of which the cistern (1) is supplied from the main by the pipe (2) through the stop-cock (3), the water in cistern being regulated in height by the ball-float (4).

The limitation and consequent amount in saving of water is effected by inclosing the flushing valve (8) by a cylindrical chamber or meter (6) of any required dimensions, the lower end of which has a flange jointed on and fixed with screws to the bottom of water cistern (1) so as to be perfectly water-tight. Centrical on the top of the meter or measuring chamber a lead pipe (o) is soldered, of such length that the top of the pipe is on a level with top of the cistern sides. The wire for lifting the flushing valve (8) passes down through the pipe (o), which also serves as an air pipe to the meter (6) when being discharged. There is a small hole through the top of the meter as an inlet for water, which serves also to allow of a wire passing down through the same; this wire has a small india-rubber valve and lead weight (7)—within the meter (6)—attached thereto, opening downwards; the top end of the wire is connected to a small india-rubber band (12), and through it raised and lowered by the action of the lever (11), which also, by a wire, lifts the flushing valve (8). During the slipping action of the slack links at the bottom of this wire for lifting the flushing valve (8) the india-rubber band (12) draws up the small india-rubber valve (7), thereby shutting the inlet before the flushing valve begins to lift. The subsequent stretching of the india-rubber band (12) permits the flushing valve to open sufficiently to allow the water to escape at once from the meter (6) into the service-box (13); the contractile force of the india-rubber band (12), when the action of the lever (11) is reversed, insures the flushing valve being shut before the inlet valve begins to open, and the further motion of the lever (11) opens the inlet valve (7), so as to allow a free ingress of water to the meter. The ordinary overflow pipe (16) shows by the dotted part (31) the usual connection with the soil pipe at W.C. cesspool, that portion of the overflow pipe (31) is separated from its connection with the soil pipe, and turned downwards so as to discharge the overflow into the W.C. cistern of the house below, having a trap (19) formed thereon to prevent vitiated air or air containing elementary contagious matter ascending through it to the houses above. The leakage intercepting pipe (14) forms a connection between the flushing pipe (9) and the overflow pipe (16), and has a regulating stop-cock (15), which is so adjusted that if the flushing valve gets leaky to the extent of the whole water entering the meter through the inlet valve, all such water would pass through the leakage intercepting pipe (14) directly into the overflow pipe (16), and through it be delivered into the cistern of the house below, and there become available for W.C. flushing; thus preventing the usual, and at present unavoidable waste of water, and at the same time preventing the further dilution of human refuse collected in the intercepting tank (22), which is placed at, or as near as possible to, the bottom of the soil pipe (30). In the ground floor W.C. the overflow pipe is continued down to the level of the street gutter, where it is allowed to escape in such a manner that the inspector, in his ordinary rounds, cannot fail observing it, if there be such in any of those connected W.C.'s from the top to the bottom of a tenement, whether caused by float-cock, flushing apparatus, or from any other leakage whatever.

The ordinary soil pipe (30) is made to terminate within the intercepting tank (22), so that the human refuse—which now pollutes our drains and sewers, discharging the poisonous gases generated therein into our street atmosphere, if not directly into our houses—drops innocuously into a close intercepting tank (22), which is emptied of its accumulated contents once a week by atmospheric pressure acting on its surface discharging it through the pipe (25), which has a screwed coupling (34) at the end terminating in the street, for the purpose of forming a connection with an exhausted tank placed on a carriage, into which it is received and therein conveyed to the depôt. The end of the discharging pipe in the tank dips into a well (26), which is covered by an inverted trough having side apertures (37), to insure the tank as near as possible being completely emptied at each discharge into the exhausted carriage tank, the exhaustion of which is effected by the action of two air-pumps while on its return journey when empty. At the further end of the tank (22) there is a trapped overflow (24), which in case of neglect or accident would allow any redundancy of liquid to escape into the ordinary drain-pipe (23). This trap prevents air from passing into or out of the tank except through the wire cage (29), which is filled loosely with sea-weed charcoal, and fixed on the top of and communicating with the overflow pipe (24), through which the air required for discharging the contents of the tank must pass. The dipping rod (28) is used for ascertaining the depth of the tank's contents at any time, but especially immediately before emptying, for the purpose of computing the quantity of human refuse and water to be discharged therefrom. The disinfecting apparatus (33) is made to act, each time the closet is flushed, by the action of the wire (32), and discharges a given quantity of acid along with each flushing, which in its descent is mixed with the flushing water; the displacing plunger is moved by the suspending wire (32) attached to the lever (11), which is moved by the wire (10), the lower end of said wire being connected to a second lever moved by a third lever which gives motion to the closet-pan when the handle is pulled up.

[1] Analysis.	Parts in 100.	Grains in a Gallon.
"Carbonate of Ammonia,	0·7483	524·16
Organic Matter	0·5862	410·34
Phosphate of Lime,	0·0199	13·93
Carbonate of Lime,	0·0120	8·40
Carbonate of Magnesia,	0·0103	7·21
Sulphate of Potash,	0·0804	56·28
Chloride of Potassium,	0·0528	36·96
Chloride of Sodium,	0·3239	226·73
Oxide of Iron and Alumina,	0·0226	15·82
Silica,	0·0155	10·85
	1·8724	1310·68
Ammonia ready formed,	0·2652	185·64
Do. producible,	0·0652	45·64
Total Ammonia,	0·3304	231·28"

[2] Fluid from Urinals (Chapman).

	Parts in 100.	Grains in a Gallon.
Solids	3·6053	2523·85
Total Ammonia	0·3705	259·35

Fluid from Hoey's Closet.

	Parts in 100.	Grains in a Gallon.
Solids	1·8724	1310·68
Total Ammonia	0·3304	231·28

Since Whitsunday, 1872, there have been seven of these closets in uninterrupted use, as an experimental test at the instance of Glasgow Police Board.

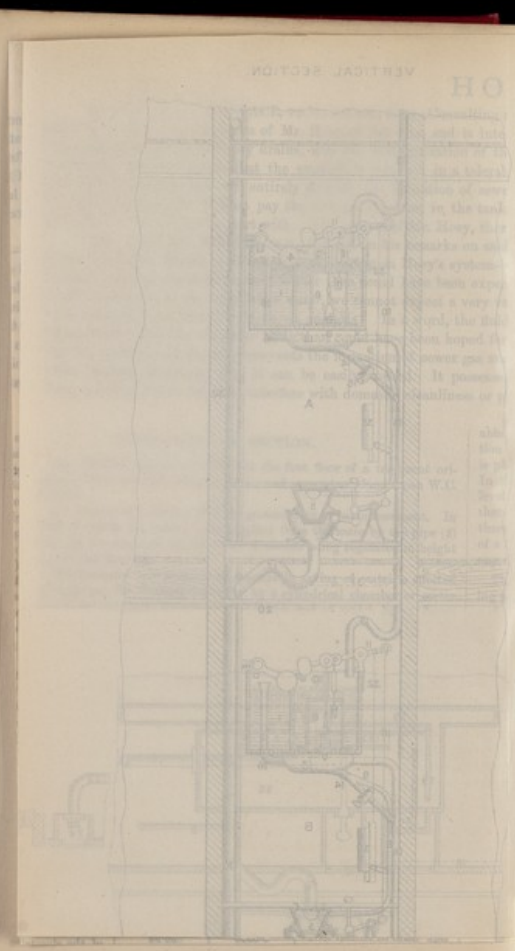
Several members of the Board having expressed a doubt that although these closets might do where each family has its own closet, they might not suit where two, three, or four families used the same closet—to test this opinion, a tenement was selected consisting of four stories with four houses in each flat—the occupants of the said four houses using one common closet erected on the stairhead. These closets were altered to the limited flushing plan, and have been in operation since Whitsunday, 1873, with perfect success, requiring no more attention from the persons using them than when working on the unlimited flushing plan, with this advantage, that there is now no smell in any of the closets, whereas formerly there was.

The disinfecting apparatus (33) has not been introduced into any of these closets, not having been found necessary; the attachment to and removal of such from any closet is an operation of the simplest kind, and could be accomplished at any time.

The tanks are emptied once a week; the contents may be mixed with the city ashes, or, as at present, be delivered at the Sewage Company's Works, and operated upon in the same manner as the urine collected by the company from the city urinals. The specific gravity of the urine is 1030, while the products of these closets have a specific gravity of 1011.

In all these closets the quantity of water used for flushing is about one-fifth of a gallon. The process of flushing can only be repeated every three minutes. The handle of the closet cannot be fixed so as to make a continuous run.

Any ordinary water-closet can be converted into Hoey's water-closet at a moderate expense, and the alteration makes no change in the mode of using the closet or in its outward appearance.



is, I think, better adapted for this purpose than any other variety of charcoal, not even excepting that made from bones. There are, I understand, several large workshops where necessaries on Mr Stanford's plan are in use, and they are said to be eminently successful.

"The following is the composition of a sample of the product of Stanford's closet system, received on the 16th ult. from Mr Ellison:—

	Dry, per cent.	Original.
Organic matter,	11·16	3·74
Carbon,	37·68	12·65
Phosphate of lime,	3·54	1·19
Sulphate of potash,	3·07	1·03
Chloride of potassium,	·70	·23
Chloride of sodium,	1·74	·58
Sulphate of lime,	1·43	·48
Carbonate of lime,	19·35	6·50
Carbonate of magnesia,	3·92	1·32
Oxide of iron,	3·61	1·21
Alumina,	4·12	1·38
Silica,	9·68	3·25
Water,	...	66·44
	100	100
Nitrogen equal to ammonia,	3·70	1·24

"The various ingredients in this mixture are partly those of the charcoal, and are partly due to the urine and fæcal matter, but the composition will vary with the length of time the charcoal has been in use, and the number of times it has been regenerated. In this system the valuable ingredients of the excrements are entirely preserved, with the exception, perhaps, of traces of ammonia, which will escape if the mixture is kept for any length of time exposed to the air."

Now, these and other schemes show a profit on paper, but I do not think that should be our first object. We ought to take a broader view of the question, and feel that this gigantic evil *must* be got rid of. We must not go on contrary to the laws of nature, lowering our health by breathing impure air, wasting our manure, swelling our death-rates, and robbing posterity.

And now I have to apologize for detaining you so long, but the subject is really all-important; and I am most anxious that Glasgow, which has set such a noble example as regards the water-supply, shall not make a mistake in this equally important matter.

If it is true that organic poisons producing disease may pass from sewage; if it is true that cholera, diphtheria, typhoid fever, and diarrhœa are traceable to the taking into our systems, by air or water, the results of decomposition of human excreta; if it is true

that these diseases and others from the same cause swell our death-rate, and carry off some of the most valuable of our population,—then, Gentlemen, I affirm that the only true sanitary solution of our difficulties is, that all excreta shall either be returned to the earth or subjected to chemical action, rendering decomposition impossible; and I am, furthermore, sure that if a tithe of the time, skill, and ingenuity, and one-thousandth part of the money, that have been devoted to water-carriage had been spent in investigations in this direction, the problem of the sewage question would have been solved long ago.

APPENDIX.

No. I.

PRODUCTION OF DIARRHŒAL DISEASES FROM EXCREMENTAL POLLUTION.

Another group of diseases arising from excremental pollution are the diarrhœal, including cholera. I need not, I think, insist that this last disease is the direct consequence of taking into our system the results of excremental pollution. Instead of going into any proof of my own as to this, I shall quote from the admirable and vigorous language of Mr Simon. In the Eighth Report, page 39, he thus writes:—"When cholera is epidemic in any place, persons who are suffering from the epidemic influence, though perhaps with only the slightest degree of diarrhœa, may, if they migrate, be the means of conveying to other places an infection of indefinite severity; but the quality of infectiveness belongs particularly, if not exclusively, to the matters which the patient discharges by purging and vomiting from his intestinal canal; that these matters are comparatively non-infective at the moment when they are discharged, but subsequently, while undergoing decomposition, acquire their maximum of infective power; that choleraic discharges, if cast away without previous disinfection, impart their own infective quality to the excremental matters with which they mingle in drains or cesspools, or wherever else they flow or soak, and to the effluvia which these matters evolve; that if the cholera contagium, by leakage or soakage from drains or cesspools, or otherwise, get access, even in small quantity, to wells or other sources of drinking water, it infects in the most dangerous manner very large volumes of fluid; that in the above described ways, even a single patient with slight choleraic diarrhœa may exert a powerful infective influence on masses of population, among whom, perhaps, his presence is unsuspected; that things such as bedding and clothing, which have been imbued with choleraic discharges, and not afterwards fully disinfected, may long retain their infectious properties, and be the means of exciting choleraic outbreaks wherever they are sent for washing or other purposes."

Again, in Report Ninth, pages 33 and 34, Mr Simon thus powerfully writes:—"It cannot be too distinctly understood, that the person who contracts cholera in this country is, *ipso facto*, demonstrated with almost absolute certainty to have been exposed to excremental pollution; that what g ve him cholera was (mediately or immediately) cholera-contagium — discharged from another's bowels; that, in short, the diffusion of cholera among us depends entirely upon the numberless filthy facilities which are let exist, and specially in our larger towns, for the fouling of earth, and air, and water, and thus secondarily for the infection of man, with whatever contagium may be contained in the miscellaneous outflowings of the population. Excrement-sodden earth, excrement-reeking air, excrement-tainted water—these are for us the causes of cholera. That they respectively act only in so far as the excrement is cholera-excrement, and that cholera-excrement again only acts in so far as it contains certain microscopical fungi, may be the truest of all propositions; but whatever be their abstract truth, their separate application is impossible. Nowhere out of Laputa could there be serious thought of differentiating excremental performances into groups of diarhœal and healthy, or of using the highest powers of the microscope to identify the cylindro-tænium for extermination. It is excrement indiscriminatley, which must be kept from fouling us with its decay.

"The local conditions of safety are, above all, these two—(1) that, by appropriate structural works, all the excremental produce of the population shall be so promptly and so thoroughly removed, that the inabited place, in its air and soil, shall be absolutely without fæcal impurities; and (2) that the water-supply of the population shall be derived from such sources, and conveyed in such channels, that its contamination by excrement is impossible."

"It is to be hoped that, as the education of the country advances, this sort of thing will come to an end; that so much preventable death will not be always accepted as a fate; that for a population to be thus poisoned by its own excrement, will some day be deemed ignominious and intolerable."

As regards diarrhœa, Dr Greenhow reports, and Mr Simon endorses his opinion, that the deaths from this cause vary from 4 in every 100,000 of the population, to 663 in the same number. He calculates that if the diarhœal death-rate could be restricted in the whole of England to ten times the minimum death-rate of 4, there would be a saving of 20,000 lives annually, and there would be a great reduction in the deaths from typhoid fever also. Both Mr Simon and Dr Greenhow are of opinion, that the excess of mortality from these diseases has in all places been coincident with one or other of two definite local circumstances: (*a*) the tainting of the atmosphere with the products of organic decomposition, especially of human excrement; or (*b*) the habitual drinking of impure water.

No. II.

DECAYED PIPES AS A CAUSE OF DISEASE.

About fifteen years ago I first detected perforated soil-pipes. At first I merely looked upon them as a nuisance, because offensive, and did not connect them with disease; neither did I then know how these perforations were produced.

In the first pipes I inspected, as the perforations were on the upper side, I imagined that pieces of lime might have dropped down on the pipes and eaten their way through. This opinion I abandoned, as I found the perforations were from within. I also found that they were generally on the upper surface of the pipe, and that, therefore, the destruction could not be caused by fluid conveyed through it. I also observed that the pipe usually affected in this manner was the cross one, leading from the closet to the main descending soil-pipe; and that if there was a bend or arch in the pipe, the upper surface of the bend or arch would become perforated. Another element to be taken into consideration is, that these perforations are most frequently found in the upper flats of houses, and, occasionally, in the sides of the descending soil-pipes.

The question now arises, How is this destruction of good well-made lead pipes effected?

If we remember the position of this destructive action in the pipes, and take into consideration the results of chemical analysis,[1] as well as the increased rapidity with which this action takes place in pipes that are not ventilated, I think we are justified in coming to the conclusion that these results are owing to the action of sewer-gas. If you will carefully notice the specimens you will perceive that the destructive action is from within, and that in some places the pipes are more or less corroded even where the action has not gone the length of complete perforation (fig. 5).

Of course, my own discovery of such pipes in houses has only been where I have been attending professionally, and where the diseases were of such a type as are known to be produced by breathing the results of the decomposition of excreta. The diseases I have observed as resulting from this state of pipes are typhoid fever, diphi-

[1] By Mr Stanford's kind permission, I am enabled to give the analysis of the inner coating of these pipes:—

Carbonate of lead	2·70	86·00	91·00	92·90
Carbonate of lime	80·63	2·50	2·10	2·90
Water	0·45	1·20	1·00	0·50
Lead insoluble as oxide	—	3·50	1·50	1·45
Silica	1·35	2·80	1·00	1·60
Organic matter	14·87	4·00	3·40	0·65
	100·00	100·00	100·00	100·00

The analysis No. 1, indicates that probably chloride of lime had recently been used to destroy the effluvia from the already perforated pipes.

theria, scarlet fever, diarrhœa, etc.; and in one or two cases I have had to order the removal of families who were suffering from ill-health from this poison, without exhibiting symptoms of any well-defined disease. As the investgation of the causation of disease is carried out, I believe that the list already given of those diseases produced or conveyed by sewage will be increased. In two cases of phthisis in people long past middle life, I could not attribute the disease to any other cause. The family history was good, the mode of living correct, and the general health satisfactory up to the time of the soil-pipes becoming corroded (fig 3).

About four years ago I was asked to visit the child of the superintendent of a large public institution. I at once suspected typhoid fever, which proved to be the case. I was, moreover, told that it had twice already been in this gentleman's family; and that the year before the whole family had suffered from diphtheria. On making inquiry as to the state of the drainage, water-supply, etc., I was told that the water was Loch Katrine, and so beyond suspicion. The master of works for the institution was a most intelligent man, who had paid special attention to the drainage. He had soil-pipes with extra depth of traps, indeed the deepest I have seen, and it was difficult to account for the manner of infection, the pipes being pronounced free from any defect; but, in a short time, another child was seized, as well as several of the officials of the institution. In passing a particular spot in the superintendent's house, however, I perceived a faint odour of sewer-gas; and on mentioning this to him, he most promptly had the pipes uncovered, and perforations were at once discovered in the vertical soil-pipe.[1] This led to a complete overhaul of all the pipes in the house, and the removal of those that were defective; and since then I have not heard of a single case of tyhoid or diphtheria in the establishment. Plumbers do not readily detect this defective state of pipes, as they look for a liquid leakage. Some time ago, in visiting a case of typhoid, I asked the mother of my patient as to the state of the closet and pipes. She replied, "Oh, Doctor, you are wrong this time; all these things were renewed only two months ago." Of course, I was extinguished; but still ventured to inquire what might be the cause of a very perceptible, and by no means agreeable, odour. A dead rat was suggested as its possible origin. However, as it was no matter to trifle with, I requested that the plumber might be sent for, and also that he should be told that the soil-pipe close to the cross-pipe was eaten away (fig. 1).

At my next visit the lady told me that the plumber had been very much astonished to find the pipe exactly in the state I had predicted. I now exhibit the pipe.[2] I have been told that this state of pipes must be peculiar to Glasgow, and must be produced by some chemicals getting into the sewers; but I have made many

[1] I would by no means be understood to say that smell, merely as such, produces disease; it is simply a quality of what in this case does so.
[2] Fig. 1.

inquiries, and find the same thing existing in every water-closet town in reference to which I have had information. This, the worst specimen on the table, is from Edinburgh, where there are few chemical works (fig. 6). Lead has generally been used as the material for soil-pipes, and as we have seen how capable it is of corrosion, it becomes a very imporant sanitary question, to inquire how long a good lead soil-pipe will hold out. I have been studying this question for several years, and it is now about seven since I first exhibited decayed pipes in public, yet I would not wish to dogmatize on the subject, but rather give approximations, and would remark that the time will vary under the various circumstances according to the strength and rapidity of flow of the sewage, as well as the original thickness of the pipe. But after allowing for this, we must broadly distinguish between soil-pipes which are ventilated and those which are not. By the former, I mean when the pipe is carried up to the roof of the house and open to the external air; by the latter, I mean when the pipes are closed up. Of these last mentioned, the duration may be stated to be about twelve years, the extremes of variation being from a minimum of eight to a maximum of twenty years.

In ventilated pipes the duration may be stated to be nearly double, running from twenty-one to twenty-three years, the extremes of variation being from eighteen to thirty or even more years. The practical sanitary conclusion which it concerns us all to keep in mind is, that any house, no matter how carefully or well built, may become unhealthy from this source, and that when cases of typhoid fever, diphtheria, etc., occur, the pipes should be thoroughly inspected—especially their upper surface—and the whole of the soil-pipe uncovered.

I must strongly insist on this, as in many cases the plumbers have declared the pipes to be all right, which turned out to be very defective when uncovered. For some years back, I have insisted on a careful examination of the soil-pipes wherever I have cases of typhoid or diphtheria, and in every case where I could get this carefully carried out, I have detected these perforated pipes, or sewer-air getting into the houses in some other way.

What first led me to connect this state of soil-pipes with disease was the fact that, as regards water-supply, Glasgow should not be exposed to diseases resulting from excremental pollution; and if we have these diseases, we must attribute them to some other source.

Another way in which sewer-gas may act injuriously, is by its passing up the waste-pipe of the cistern and becoming absorbed by the water. In houses that have been shut up for a few months, this should be guarded against by running off all the water, and clearing out the mud at the bottom of the cistern, before the houses are re-inhabited. I also propose to insert into the waste-pipe a long wire cage with a circular hood, and to keep this filled with seaweed or other charcoal, which should be replenished from time to time

(fig. 4). Of course, it would be much better if all water used for cooking or drinking were drawn direct from the main.

It would prolong the duration of the pipe, and be an important sanitary improvement, if the soil-pipes were in every case carried up to the roof, and left quite open to the air. The expense of this would be more than compensated to the landlords by the greatly-increased duration of the lead pipes.

DESCRIPTION OF PLATE.

FIG. 1.—Pipe removed from a house where there was typhoid. Two months before it was taken out, the water-closet had been renewed, but the plumber had failed to detect the state of this pipe.

FIG. 2.—A piece of lead soil-pipe slightly corroded, which entered an iron pipe below a water-closet. The iron pipe much perforated, allowing an escape of sewer-gas, which produced fatal diphtheria.

FIG. 3.—Perforated soil-pipe from house where party died of phthisis.

FIG. 4.—Wire cage, filled with seaweed or other charcoal, to put into the overflow pipe of cistern.

FIG. 5.—Soil-pipe laid open, in which, so numerous were the perforations that very little of the lead required to be cut. The sides *a a*, *a a*, are irregular owing to these perforations, and they may also be seen over the whitish part. The spaces between *a a* and *d d* are the parts of the soil-pipe covered with the whitish deposit spoken of in the paper. The part within the *b's* is the lower segment of the pipe over which the sewage flowed. It is quite sound—not in the least decayed or worn.

FIG. 6.—Pipe from Edinburgh. It will be noticed that the solder has not decayed, *a*.

FIG. 7.—Main descending soil-pipe, showing the perforations.

No. III.

MR MACTEAR'S EXPERIMENTS, READ BEFORE THE PHILOSOPHICAL SOCIETY OF GLASGOW.

The experiments shown here at our last meeting by Dr Fergus on the ease with which sewer-gases would pass trapped pipes, were most interesting; but I am afraid, from remarks made by various gentlemen, both at the meeting and afterwards, that they were not sufficiently conclusive. It was said at once, these traps are not ventilated, and so you cannot expect any other result than that the gases will pass, more especially as they are under pressure. Now, in the first place, very few traps are ventilated (although, no doubt, they should be), and in most cases we have actually a pressure in the soil-pipes. It occurred to me, after seeing at last meeting the experiments, that, as it was evidently owing to diffusion that the gases passed, no trap would keep them back, even if it was ventilated, given a sufficient length of time; and I had a series of experiments made, which I believe to be quite conclusive on this point.

The first series was made in glass tubes; and it having been suggested to me that it would be much more convincing to unscientific minds were I to repeat them with actual lead traps, I had another series carried out with the traps before you.

The usual size of a ventilating pipe on a 3 or 4 inch trap is from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch, being in the ratio of from $\frac{1}{4}$ to $\frac{1}{8}$. In first series

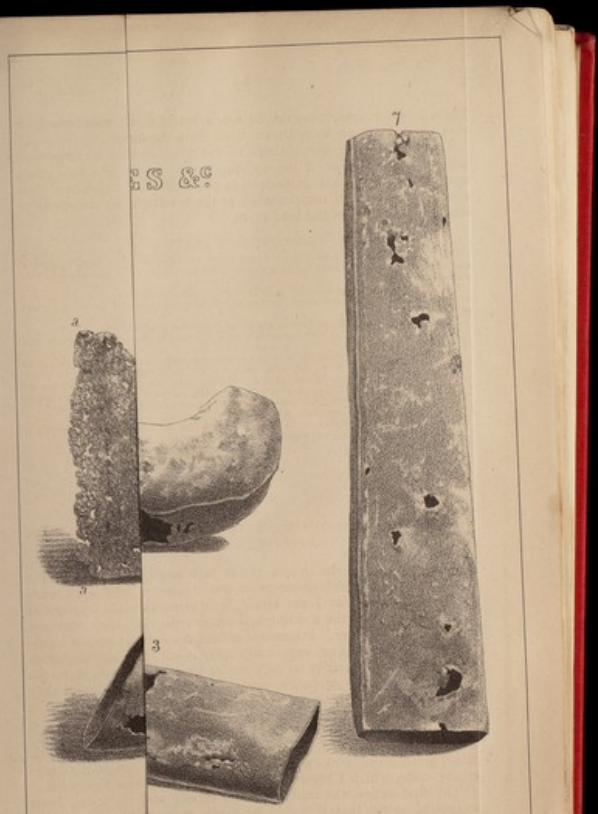
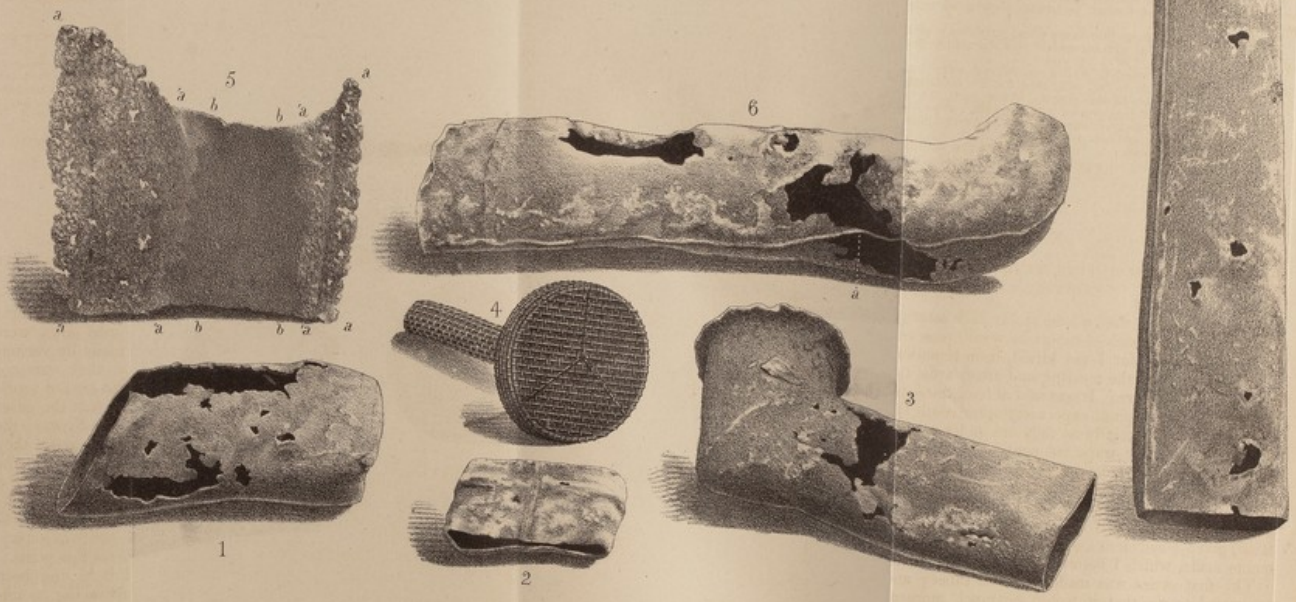


Plate of Decayed Soil Pipes &c

REFERRED TO IN Dr FERGUS' PAPER.



WILMOUR & CLAN LITHO GLASGOW

of experiments it was from $\frac{1}{7}$ to $\frac{1}{8}$ nearly. In second series of experiments it was from $\frac{1}{2}$ to $\frac{1}{4}$. In all of them much larger in proportion than is usual when ventilation is applied, which, however, is rarely the case.

A number of experiments were made and various gases were used, the last tried being coal-gas, which bears some resemblance, in general composition, to sewer-gas. Besides these, putrid beef-juice, egg, and urine were used, with the hope of finding that the minute organisms seen in these decomposing substances would pass through the trap. In this, so far, I have been unsuccessful; but I quite believe that they will so pass through, if time and favourable conditions are given. The odour of putrid matter passes most readily through these traps, and is quite distinct to the sense of smell.

I have here some diagrams which show the passage of the gases through the traps. A curious fact, and yet one quite to be expected, is here shown; viz., that light gases pass through by the top of the bends, and heavy gases at the bottom; also, that the gas does not saturate the water in the trap by any means, but that it first saturates the surface, next the vapour, then the gas seems to sink down in a fine stream and gradually travels through the liquid to the other side, when it again spreads out and begins to diffuse, both into the atmosphere above it, and downwards through the water in the trap; in fact, the most apt illustration of the general appearance, is the gradual dispersion of a so-called smoke ring from the funnel of a locomotive.

1*st*. I have, in first place, diagram showing passage of NH_3 (ammonia). This in glass tube, ventilated as you see, showed on surface in 1″, began to move downwards in 5″, reached surface at other side of trap in 15″, and rendered litmus paper, suspended in tube, blue in 20″. With the wide tube open, it began in 1″, and coloured litmus paper in 100″.

2*d*. Cl (chlorine), diagram shown, began to show in 2″, completely through in 2 hours; with wide tube in $2\frac{1}{2}$ hours. The movement in the case of Cl and other heavy gases is comparatively rapid while passing downwards, but very slow in passing upwards in the other limb (Indigo solution in water).

3*d*. Co_2 (carbonic acid) passed through in 60″ with small tube.
" " " 80″ " wide "
(with litmus.)

4*th*. H_2S (sulphuretted hydrogen) passed through in 80″ with wide tube.
" " " 100″ "
small tube.

This experiment showed that it is not necessary that the liquid should be saturated, as it took $2\frac{1}{2}$ hours after the gas had passed through to destroy the remaining blue colour in iodide of starch.

5*th*. Coal-gas, with small tube, through in 2 hours.

We had to depend on the small amount of sulphur compound

present to help us to trace the movement in this case through solution of iodide of starch, and the reaction is very faint, but the smell was most distinct.

Second Series.

4 inch large lead trap, ventilating tube 1 in. dia., 2 ft. long
1*st.* NH_3 passed through 3 inches of water in 3′ 20″.
2*d.* Coal-gas " " " 4′ 35″.
3*d.* Putrid egg and urine; smell passed through in 3 days.

Third Series.

Small lead trap 3 in. dia.; ventilating tube 1 in. dia., 2 ft. long.
1*st.* NH_3 passed through in 2′ 55″; 2 inches water in trap.
2*d.* Coal-gas " " 1′ 25″ " "
3*d.* Putrid beef-juice; smell through in 3 days.

I submit, Gentlemen, that these facts prove conclusively that, ventilation or no ventilation, gases do pass through, if we only give them time; and it is quite certain, that if we were to extend these experiments further, we would find that after the water in the trap has become saturated, the rate at which the gas would pass would be greatly increased, for we have in nearly all cases a considerable difference in the temperature of the outside sewer-pipe and the house, conducing greatly in favour of the flow of gases inwards.

I think most of you will admit that these sewer-gases will and do pass into our houses; and it seems to me that we cannot, in the face of such evidence as has been laid before us from time to time, lay the flattering unction to our soul, that these gases are quite harmless. No doubt you have heard it said, as I have, about our friend Dr Fergus, that he had sewer-gas on the brain; but, Gentlemen, if this sewer-gas mania (if you like to call it so) should lead to the saving of even *one* human life, should we not have cause for thankfulness? I think that we should, before the close of this discussion, ascertain the feeling of the Society on the question of danger, or no danger, from sewer-gas.

As to ventilating pipes, merely one word in closing. In ventilating any space, it is necessary to have a supply of fresh air as well as to remove the foul. In trapped pipes we have no provision for this, and so our ventilating pipe acts mainly by diffusion. If, instead of one, we apply two pipes, one a good deal higher than the other, we may then do some good—the fresh outside heavier air descending the one tube, and the impure lighter air ascending the other; but I am thoroughly convinced, from the experiments I have described, that no ventilation will altogether remove the danger of sewer-gas getting into our houses.

I may mention that in these experiments, the gases were not given off under pressure, and that the large trap contains, of water, 228 cubic inches or 132 oz., and the small trap, 45 cubic inches or 27 oz.

No. IV.

EFFECT OF THE CLYDE ON THE HEALTH OF THE PEOPLE.

There can be no doubt in any one's mind that the purification of rivers is of great importance; but before spending vast sums of the public money on such an object, I think it may be well to consider whether such rivers exercise any hurtful influence on the health of the inhabitants on their banks.

As regards the effect of the river Clyde on the health of the city of Glasgow, I think I may be allowed to speak with some little authority, inasmuch as I acted as District Surgeon of a strip of the city close to the river, including the shipping on the north side of the harbour.[1]

During all the time I had charge of this district (nearly 20 years), there was no prevalence of diseases arising from excremental pollution, neither did I find the health of the people in the least degree affected by living in close proximity to the river. I shall, however, adduce other evidence than my own on a subject of such importance, and therefore quote the opinions of the late and the present Medical Officers of Health for this city.

Dr Gairdner says "that there would be no doubt that the state of the river involved an enormous nuisance, and that it was possibly injurious to the health of certain people in its vicinity; but he held that, judging simply from the facts, there was not the slightest proof that the death-rate of Glasgow was appreciably affected by the state of the river. And the reason probably was, that the sewage material, when it gets rolled about and oxygenated in the bed of the river, ceases to possess the peculiar property of producing distinct diseases. Neither enteric fever, nor any other fever, was demonstrably produced in this way" (*Glasgow Medical Journal*, November 1868).

Dr Russell says, "There is no indication of any deleterious influence exercised by the Clyde. During these three years only two cases of enteric fever came from vessels in the harbour, one in September 1867, one in January 1868.[2]

[1] Bounded on the east by the centre line of Dunlop Street; on the west by a burn (now a sewer) situated between M'Alpine and Washington Streets; on the north by Argyle Street; and on the south by the centre line of the bed of the river.

[2] As any fact bearing on the relation of the Clyde in the harbour to health is of peculiar value at present, I append the following from Dr Dunlop, Assistant Medical Officer of Health, who has, in his official capacity, paid particular attention to the question. He writes, in reply to my inquiries—"From an inquiry among the men themselves, but more especially from a long and interesting conversation with the gentleman who has the superintendence of the ferrymen, and whose experience of them extends over a long period, say 20 years, I learned, firstly, that the ferrymen are all strong healthy men when

"It is also remarkable that from York Street to New Wynd, a strip of the city lying along the north side of the harbour, not a case was admitted. A still worse instance of a polluted stream is the Molendinar Burn—which runs open through crowded localities for short lengths. There is almost an entire absence of enteric fever in those localities. This illustrates well a grand sanitary principle, that it is not so much the presence of decomposing matter as the prevention or limitation of the great natural processes of disinfection by air, earth, and water, which determines a deleterious action on the inhabitants. The Molendinar is harmless until it gets into the sewer, and so is the Clyde while it flows open to the air, though both are sufficiently disgusting.

"A deficient trap to a drain, or a badly-flushed water-closet, will certainly do more injury than all the vapours of the Clyde in the harbour or the Molendinar at the foot of Drygate or Blackfriars Street."—*Glasgow Med. Journal, Nov.* 1868.

No. V

EXTRACT FROM MESSRS ANDERSON AND BATEMAN'S REPORT, 1858.

We have taken the liberty of italicizing some portions which we think specially important.

"Looking at the question in its widest aspect, it is doubtful whether any of these plans reach the root of the evil; and we are inclined to go much further, and to express *our belief that the principle on which the refuse matters of towns are now universally got rid of is radically wrong.* The system of sweeping them away by means of an abundant supply of water is no doubt very simple, and has certain advantages, provided it could be effectually carried out. But the imperfect success attending it in practice is sufficiently attested by the present state of the sewers in all our large towns. Nor can we fail to see that a great part of this want of success is the natural and unavoidable consequence of a system which, by the facilities it affords for getting rid of refuse, renders individual sanitary precautions less a duty than they were under the older

appointed; secondly, that they are seldom off work from illness, and when they are so, it is usually in the winter season, and the ailment is cold; and, lastly, that, as a rule, they continued all healthy strong men, and none of them, so far as he knew, or I could learn, have ever suffered in their health from the putrid condition of the river during the summer. I carried the inquiry among bargeman—Clyde-deepeners—river pilots—seamen on board steamers, coasters, etc., and did not discover any facts, though I was anxious to find them, pointing to the morbid influences of the river."

arrangements. The sewers are open to all without let or hindrance, and every description of waste is indiscriminately thrown into them in its most offensive condition; and provided an individual gets rid of his own refuse, he considers himself at liberty to disregard the convenience and comfort of his neighbour, or at all events to throw upon the public the burden of getting rid of a nuisance which he might have abated at a less cost than the public can, and it may be in some instances with absolute profit to himself. The cure for all this lies in the fact that there is really a large amount of preventable contamination of our sewers, which should never be allowed to flow into them, but should be preserved apart, and submitted to some process for the preparation of manures, which it would then be possible to produce with much greater prospect of success than after they have been diluted with the enormous quantities of water passing through the sewers. Many manufacturers are in this predicament, and as a remarkable instance of the practical application of the principle now actually in use, Teal's patent for the recovery of the fat from the soap-liquor of wool-scouring may be mentioned, which process is successfully in use in Glasgow and many other places, with the effect of converting a very filthy matter into a clear though not colourless fluid. We would not by any means have it supposed that similar results are to be obtained from the refuse of all manufacturing processes, but no doubt there are others to which some mode of purification might be applied.

"These observations apply with equal force to excrementitious matters; and we are fully persuaded that if these substances are to be converted into a manure with any prospect of profit, it must be by excluding them from the sewers, and preserving them in an inoffensive state by means of a dry deodorizer. If this could be successfully done, the advantage to the sewers is sufficiently obvious, for it is these excrementitious matters which are the most offensive part of their contents, and it would have the effect also of doing away with the existing system of water-closets, which, in a sanitary point of view, are quite behind the age. In expressing this opinion, we are well aware that we run the risk of being misrepresented, and described as the advocates of an obsolete system which has been properly abandoned, and the offensive and noxious character of the old plan will be contrasted with the many advantages of the water-closet. We by no means advise a return to the old system in its primitive form, for that cannot for a moment be supported; but it appears to *us, that had the ingenuity which for a long series of years has been devoted to the contrivance of ingenious water-closets been turned to plans for the effectual application of dry deodorizers, that the result in a sanitary point of view would have been quite as good, probably better,* and the product might be converted by a simple process into a manure which would probably sell at about £5 per ton. We refrain from entering into details regarding this plan, but its importance must not be underrated. At present but a small

proportion, not above one-fourth, of the houses in Glasgow are furnished with water-closets, and their rapid increase must tend to magnify very greatly the evils at present experienced. The plans proposed for precipitating and deodorizing the sewers are no more than palliatives, and with the extension of the city, and the increase in the number of water-closets, these palliative measures must become day by day more costly, so that it is really a matter for grave consideration whether the attention of the public should not be directed to the contrivance of some substitute for the conveniences now in use, which shall fulfil sanitary requirements without contaminating the sewers, and yield a marketable manure.

No. VI.

EXTRACTS FROM PAPER, READ IN 1868, ON MESSES BATEMAN AND BAZALGETTE'S SCHEME OF 1868.

Messrs Bateman and Bazalgette being simply engineers, when the subject was remitted to them, it was natural to suppose that we should have an engineering scheme—gigantic works, deep tunnelling, large sewers, and expensive pumping stations. I have the most perfect faith in a British engineer being able to execute any work; and as I read over their report, I was reminded of an ancient engineer, who said, if he had a lever long enough and a fulcrum strong enough, with his own weight he would move the world. Modern engineers are not quite so ambitious, but they do execute grand works, their leverage being unlimited taxation, and their fulcrum a docile people, able and willing to pay it. . . .

If we adopt their scheme, what are the results? First, we dilute the excreta with an enormous amount of water, and then pass it into the sewers to putrefy, where it loses its strength and produces noxious gases. It is afterwards carried to a station and pumped to a sufficient height, and finally conveyed through a costly tunnel to the Ayrshire coast, there to be applied to the land, or run into the sea.

No doubt, if such a scheme were carried out, it would improve the river, but would not improve our sanitary state or cleanse the sewers; indeed, I believe it would increase the quantity of gas poured into our houses, and at the pumping stations the fluid would be lifted, but the gas would remain in the sewers. . . .

Messrs Bateman and Bazalgette do not fully provide for storm water, but dwell on the impounding capacity of their sewers to meet the requirements of heavy rain. But let us suppose that before heavy rain the sewers are but one-fourth filled with sewage, and three-fourths with sewage air. As the sewers fill, the air must

be forced out. The hydraulic pressure put on the sewer air by the filling of the sewers with water, no trap yet invented could resist. Hence every heavy fall of rain (and we have plenty) would pollute our atmosphere with sewer-air. . . . I suppose it would be presumptuous in me, not being an engineer, to make any remark except on the sanitary aspect of the question, still I think £40,000, which is all these gentlemen allow, a very small sum for land. We are told that the tunnel would be easily made, because it is in the coal-measures, and will avoid all workings except in the last two miles. But may there not be workings at some future time? We all know how the ground sinks, and we might find some fine morning that our tunnel had sunk a few feet and become useless. . . . Before the authorities execute any large work to pour sewage matters on to the shore, they should look to the legal consequences. This subject has been before the Courts in England, and Vice-Chancellor Wood said that the notion of collecting all the sewage of a large town and pouring it into a river, without the slightest attempt to clean it of its grossest materials, is simply monstrous. In another case, the Court said, it would not balance the convenience of a town against the legal rights of an individual complaint; the latter must be respected.

It would be awkward for our authorities if, after undertaking all these works, some town in Ayrshire, or some individual, either there or where the works were open, should obtain an interdict preventing them passing any sewage matter through them.

Note, 1874.—In 1868, Messrs Bateman and Bazalgette estimated that their scheme would cost one million and a quarter. If we take into consideration the great rise that has taken place in land, materials, and wages, and also that most works cost more than the original estimate, we may conclude that these works could not now be completed much, if at all, under £2,000,000, the interest of which at 4 per cent. is £80,000. As one penny in the pound yields a little over £8000 a year, we would require a tax of tenpence in the pound to pay for the interest of construction, in addition to which the pumping, maintenance of conduit, superintendence, etc., etc., must be provided for. Some enthusiastic people propose to convert the Ayrshire sands into blooming gardens and cultivated fields by sewage irrigation; but this, so far as I am aware, has never succeeded in purifying the sewage of a large city. A serious objection to sewage irrigation is the fact, that farmers will only take the liquid when they require it; and as continuous application to land is necessary to get quit of the sewage, any town adopting this process must become proprietors of the land; so that, in addition to all our existing municipal trusts, we shall be obliged to form a farm trust for our Town-Council to manage!!!

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Compliments.

ON CHROMIC ACID

AS AN ANTISEPTIC, DISINFECTANT, ETC.

MORE ESPECIALLY AS

COMPARED WITH CARBOLIC ACID.

BY

JOHN DOUGALL, M.D.,

GLASGOW.

Reprinted from "Lancet" of 16th and 30th December, 1871.

ON CHROMIC ACID AS AN ANTISEPTIC, DISINFECTANT, ETC.,

MORE ESPECIALLY AS COMPARED WITH CARBOLIC ACID.

CHROMIC ACID occurs in brilliant crimson needles, which deliquesce by exposure to the air. When pure, it is almost odourless. Its aqueous solution has a sour metallic taste, and a rich amber or reddish-brown colour. It is very soluble in water, sparingly soluble in chloroform, insoluble in the fixed oils and fats. Contact with carbolic acid, mucilage, glycerine, alcohol, ether, and with organic matter generally, instantly decomposes it with more or less violence, oxygen being evolved and heat generated.

ANTISEPTIC PROPERTIES.

As an antiseptic, disinfectant, and preventive of germ growth, chromic acid stands "second to none." Several varied experiments were made in order to ascertain its antiseptic powers in contrast with those of carbolic acid.

I.—One ounce of ox muscle was immersed for twenty-four hours in four ounces of an aqueous solution of chromic acid (1-2000), then suspended in the air. In two days it was quite black, and in six days as hard as wood, in which condition it still remains (three months after), without mould or taint.

II.—One ounce of ox muscle was immersed for twenty-four hours in four ounces of aqueous solution of carbolic acid (1-1000), then suspended in air. In six days it was much hardened, brownish-black, speckled with mould, and distinctly tainted.

III.—Three wide-mouthed bottles were filled respectively with one ounce ox muscle and four ounces of a solution of chromic acid

1-1000, 1-2000, 1-3000. They were kept at about 60° Fahr. in a moderate light, and quite open. At the end of three weeks the 1-3000 solution was putrid; the others were unaltered.

IV.—Two glass tubes were two-thirds filled with ¼ oz. ox muscle and one ounce aqueous solution of carbolic acid, 1-1000 and 1-1500. Their mouths were immediately hermetically sealed by fusion. They were kept at about 60° Fahr. for fourteen days, when the portions of muscle evinced putrefaction by floating. The solutions, at first of a transparent pink, became hazy and nearly colourless. At the end of three weeks, on the tubes being opened, there was a sudden outrush of volatile products of putrefaction.

V.—¼ oz. ox muscle was immersed for twenty-four hours in one ounce of a solution of carbolic acid, 1-1000; then allowed to drain for six hours. ¼ oz. ox muscle was immersed for twenty-four hours in one ounce of a solution of chromic acid, 1-1000; then allowed to drain for six hours. On weighing the two portions of muscle, that from the chromic acid solution was twenty-one grains lighter than the other. The chromic acid had extracted seventeen per cent. more albumen than the carbolic acid.

VI.—¼ oz. cartilage from the trachea of a cow was immersed for six weeks in two ounces of a solution of carbolic acid, five grains per ounce, then dried. ¼ oz. cartilage from the trachea of a cow was immersed for six weeks in two ounces of a solution of chromic acid, five grains per ounce, then dried. On weighing the two portions, that from the chromic acid solution was two grains lighter than the other.

Experiments I.-IV. are sufficient not only to demonstrate the very superior antiseptic powers of chromic acid, but also to shew that these greatly surpass those of carbolic acid.

It appears that chromic acid acts as an antiseptic by coagulating protean compounds, a property which it possesses in the highest degree, and to which I am not aware that attention has been hitherto directed, although used for a considerable time in hardening animal tissues for microscopical examination. The coagulating

power of chromic acid in albuminous solutions has been compared with that of most metallic salts, acids, &c., and found in a greater or less degree to exceed them all; *e.g.*, it has about ten times the coagulating power of carbolic acid, fifteen times that of nitric acid, twenty times that of bichloride of mercury, and one hundred and fifty times that of chloralum, &c.* Shortly after a piece of muscle is immersed in a solution of chromic acid there forms in considerable quantity a greyish, opaque, granular sediment—coagulated albumen; while the deep-red of the muscle is changed to brownish yellow. Ultimately it looks as if it had been boiled, and is composed almost exclusively of fibrous stroma, which, on drying, becomes extremely hard and tough. The supernatant fluid, at first reddish brown, is nearly colourless or pale amber. A portion of muscle immersed in a solution of carbolic acid retains a light-pinkish, somewhat blanched aspect; while the solution, remaining void of sediment, is reddish and hazy, but translucent. If to a portion of this fluid there be added a little chromic acid solution, not stronger than 1-1000, a voluminous precipitate of albumen is at once obtained, clearly demonstrating the superior coagulating power of chromic acid over that of carbolic acid.

As regards the exact coagulating power of chromic acid, it is found that a solution of two grains per ounce at once indicates the presence of albumen in a solution consisting of one part of a saturated solution of beef juice in twenty of water; while one of beef juice in thirty of water may be detected by allowing the mixture to stand for twelve hours. Chromic acid is admirably adapted for determining volumetrically the percentage of albumen in a fluid. An albumenometer may be constructed as follows:—Fill a wide-mouthed burette to a multiple of 100 with albuminous urine or an albuminous fluid; add solution of chromic acid, about four grains per ounce, in slight excess; shake the mixture; set

*These results were ascertained by first adding to a burette uniform portions of an albuminous solution, and then the substance to be tested, till coagulation was produced.

aside for twenty-four hours; read off the precipitate, and multiply to per cent. No heating is required. Chromic acid also coagulates mucus, saliva, chondrin, and gelatine. With the latter two it forms, in excess, canary-yellow fluids and flocculent precipitates coloured like chromate of lead. The reaction with gelatine is as delicate as that with tannin, giving a response with 1 to 5000. Chromic acid is therefore a test for gelatine. An aqueous solution of carbolic acid (1 to 20) produces only slight haziness in gelatinous solutions.

DISINFECTING PROPERTIES.

Chromic acid coagulates, hardens, and oxidises decomposing organic matter. It combines simultaneously with ammoniacal products and with nascent sulphuretted hydrogen, reducing the latter to water and free sulphur ($2\,CrO_3 \times 3\,H_2S = Cr_2O_3 \times 3\,H_2O + 3\,S$). Added to putrid blood, flesh, pus, urine, or faecal matter, the offensive odour is soon absolutely removed, the mixture remaining fresh for an indefinite time. Dr. R. A. Smith found that bichromate of potassium surpassed thirteen other of the most energetic antiseptics, including carbolic acid, in preventing the evolution of sulphuretted hydrogen in a mixture of equal parts of blood and water.* This salt has a coagulating power near that of nitric acid—*i.e.*, fifteen times weaker than that of chromic acid. Hoppe Seyler has shown that, "while in disinfection it is highly necessary to destroy the products of fermentation and putrefaction, yet the destruction of sulphuretted hydrogen and ammonia can have no more influence on the fermentative changes involved in cholera and typhus than the removal of carbonic acid can have upon the progress of alcoholic fermentation; and therefore the ferment itself must be attacked, which, as pointed out, is more resistant than the living organisms in decomposing solutions."† Now chromic acid, as already shown, fulfils admirably these requirements. Carbolic acid does not combine with ammonia, nor decompose sulphuretted hydrogen. This is

* Disinfectants and Disinfection, pp. 89-91.
† *The Lancet*, Aug. 26, 1871.

surely of importance in a sanitary sense, when it is remembered that almost to these properties alone are due the marked disinfecting powers of sulphurous acid, nitrous acid, permanganate of potassium, bisulphite of lime, protosulphate of iron, chloride of zinc, chloride of aluminium, chlorine, bromine, iodine, &c. Carbolic acid seems to act as an antiseptic solely by coagulating albumen. It does not preserve by absorbing and retaining moisture, like chloride of sodium, alcohol, &c., as, practically, it has no affinity for water. Chromic acid is the reverse.

In Dr. A. E. Sansom's book, page 28,* the following passage occurs:—"It has been shown, however, that carbolic acid has the faculty of coagulating albumen. Is it on account of this faculty that it prevents fermentation and putrefaction? On this point a comparative experiment throws some light. Let a solution of the albumen of egg be precipitated, in one case by heat or by an ordinary chemical reagent, and in the other by a solution of carbolic acid, and let the resultant precipitate be kept a considerable time in contact with the air. It will be observed that whereas in the one case the albumen will become decomposed in the ordinary manner, that precipitated by carbolic acid entirely resists putrefactive change. It is therefore obvious that carbolic acid has an action over and above its action as a mere precipitant of albumen." The fact is here ignored that in general chemical precipitates of albumen are soluble in pure water, specially carbolico-albuminoid precipitates; and as the latter cannot exist without the presence of carbolic acid, the difference between the "carbolic" and "heat" precipitates is simply that the one contains an antiseptic, while the other does not. As regards the other precipitates, the alleged results only show that carbolic acid is a more powerful antiseptic than any of the ordinary chemical reagents referred to. Furthermore, it may be urged that carbolic acid is volatile, whereas chromic acid is fixed; therefore the former can arrest putrefaction by coagulating floating particles of organic débris. But in a series of experiments by Dr. R.

* The Antiseptic System. By A. E. Sansom, M.D., London.

A. Smith,* "to determine the efficiency of strong gases and volatile substances in preventing putrefaction," it is shown that pieces of fresh meat suspended in bottles containing chlorine, bromine, iodine, hydrochloric acid, ammonia, protoxide of nitrogen, nitrous acid, and sulphurous acid, were fresh at the end of twenty-eight days; while a piece suspended in a bottle containing heavy oil of tar, and a piece in a bottle containing M'Dougall's Powder, of which the chief ingredient is crude carbolic acid, grew slimy and putrid in seven days. I have also made the following experiment:—A piece of ox muscle, a portion of beef juice, of urine, and of infusion of hay—the three latter in separate phials—were suspended in a gallon bottle, which contained fully a pound of pure carbolic acid, about six inches from its surface. The bottle was kept open at about 60° Fahr. On the fourth day the beef juice was putrid and swarming with bacteria, vibriones, &c. A portion of the same juice exposed to the air was not more putrid on the fourth day than that in the bottle. On the fifth day both the urine and infusion of hay teemed with life; while on the surface of the latter there was an abundant development of penicillium. On the sixth day the piece of muscle was putrid and slimy. These facts prove that the vapour of carbolic acid, even when most concentrated, fails to arrest putrefaction, and to prevent the appearance of germs; indeed, as seen with the beef juice, it does not even delay those phenomena, the portion suspended in the carbolised atmosphere putrefying as quickly as that kept in ordinary air.† "Here is a beautiful hypothesis slain by an ugly fact." After these results it is impossible to conceive, as it would be absurd to expect, the infinitely-minute quantity of carbolic vapour which can be tolerated in the wards of an hospital, or is capable of diffusing spontaneously in an alleged infected medium, competent to destroy floating unseen germs and organic particles, seeing that in the highest state of

* Disinfectants and Disinfection, p. 109.

† A German observer, Von Ankum, has performed an almost identical, though less severe experiment, with the same result.—*Medical Times and Gazette*, 16th December, 1871.

concentration, and all the conditions in its favour, it failed to affect visible germs and tangible organic particles.*

POWER IN PREVENTING ANIMALCULES.

As a preventive of germ life, chromic acid surpasses sixty-six other chemical bodies, consisting of irritant, narcotic, and narcotico-irritant poisons, including all the known antiseptics and disinfectants, except two or three substances with which it has not yet been compared. In this respect it greatly excels carbolic acid, the average preventive strength of which, in three aqueous solutions of hay, urine, beef-juice, and egg albumen, is only 1 to 400, while that of chromic acid is 1 to 3300.†

PHYSIOLOGICAL AND TOXICOLOGICAL EFFECTS.

These are so intimately associated in the administration of chromic acid, that it is doubtful where the first end and the second begin. The following experiments will partly shew the above:‡—

I.—At 5 P.M. gave by mouth to a pigeon two grains chromic acid in one dram of water. Till 6 it remained perfectly quiet (except giving an occasional pur), when it vomited a few undigested grains of wheat. Continued without change till 10 P.M., when it was left for the night. 9 A.M., following day, found dead in *rigor mortis*. *Autopsy*—Fauces and œsophagus deeply stained brownish-yellow and considerably corroded. The crop contained a quantity of peas, barley, and wheat, also coloured by the poison. Its inner coating was quite disorganized; the smallest amount of friction with the finger converting it into minute shreds; while the whole organ had lost its natural elasticity and was easily torn. The contents and inner coating of the stomach were deep green. The latter was much softened, and parted more easily than natural from the thick external muscle, which seemed unaffected.

* See, also, "On the Relative Powers of various Substances in the Destruction of Microscopic Organisms," by author. *Lancet*, Aug. 6th, 1870.

† "On the Relative Powers of various Substances in preventing Animalcules," by author. Churchill.

‡ These experiments are here detailed for the first time.

Lungs—slightly congested. Heart—left ventricle firmly contracted; right, full of dark fluid blood.

II.—5 P.M., Gave to a medium-sized rabbit, by stomach, three grains chromic acid in ½ oz. water. It remained quiet till 10, when it was left for the night. 9 A.M., following morning, found dead, but warm and flaccid. *Autopsy*—Lungs slightly congested at their free margins. Heart—all the chambers filled with dark fluid blood. Stomach—full of pulp, the upper portion of which was stained brownish yellow. On turning out its contents and washing carefully, the inner gastric surface from a short distance up the œsophagus, along the cardiac orifice, round the greater cul de sac, on half-way to the pylorus, in nearly the whole circumference, was found in the following remarkable condition:—It was of an ashy hue, and in the immediate vicinity of the cardiac orifice, much corrugated and shrivelled into mammillary protuberances, the whole being profusely interspersed with small red fissures, all running transversely to the longer gastric axis. On closer inspection it was easily seen that the red hue of the fissures was that of the corroded muscular coat; while the slightest friction caused it to peel off and break up into small friable particles. The external peritoneal or serous tunic seemed, however, quite intact, as it could be stretched considerably without tearing. Shading gradually off from these conditions to near the pylorus was a great tract of inflamed surface ready to slough, as it was easily detached and broken up. At the pylorus the surface had a pale and normal aspect, it being quite evident that the poison had not reached it. The remaining organs were not examined.

III.—11 A.M., Introduced beneath the skin of a medium-sized rabbit two grains chromic acid. 2-20 P.M., No effect; introduced by same opening an additional six grains chromic acid. 2-30, Position prone; rapid movement of head in an antero-posterior direction with each respiration; respirations uncountable; ears erect; anxious and watchful countenance; evidently suffering much pain. 2-45, Stretched on side, with head leaning on wall; eyes

closed; a tremulous movement over the whole body; extremely rapid antero-posterior motion of head; an occasional abrupt cough; restless; pupils normal. 3, Respirations continue uncountable; is unable to stand; rapid motion of head. 3-30, No change. 3-45, Quieter, tendency to relax side (left), where the acid was inserted; coughing as before; tips of ears cold. 3-55, Defecation; micturition; great depression; lying with nose touching the ground; limbs nearly powerless; respiration very slow and weak. 4-5, Position prone; depression increasing; coughing at short intervals. 4-15, Considerable diarrhœa; extreme prostration; irregular cardiac action; complete paralysis of limbs. 4-25, Lying on side; respiration faint; eyes nearly closed. 4-40, Faint twitchings; death quietly. 8, *Autopsy*—Extreme *rigor mortis*. The animal had lain on its side since it died, and it was now observed that the pupil of the eye which had been turned to the ground was greatly dilated, while that of the eye which had been turned towards the light was extremely contracted. On opening up the part where the poison had been introduced, a considerable portion of the skin was found to be of a dirty yellow hue and much hardened. The lumbar fascia and muscles, with a great portion of the abdominal muscles of the left side, were much corroded, and looked as they had been boiled. In a small muscular hollow immediately opposite the opening where the acid had been inserted was observed a dark brown glistening object, which on further examination proved to be the kidney. It seemed quite normal. The poison here had eaten through the whole thickness of the erector-spinæ muscles. Lungs—slightly reddened at free margins. Heart—left ventricle contained a large dark clot; right ventricle filled with black coagula and dark liquid blood. No other appearances worthy of note observed.

The results of these experiments show that chromic acid, in concentrated solutions, is a pure and powerful corrosive of animal textures, effecting speedy and complete local disorganization. So actively does it destroy the vascular tunics, gelatinising their fluid

contents, that absorption is rendered impossible—these by the merest contact with the acid being converted into consolidated emboli, which choke the capillary passages and preclude further ingress. This view is confirmed by, or may be inferred from, the fact of the poison not being found in the blood or urine. If equal portions of muscular tissue and chromic acid be left in contact for about one hour, the whole is converted into a mass like burnt sugar, which is freely soluble in water, rendering it yellowish-brown.

CHROME SORES.

Through the kindness of Messrs. White, of Shawfield, near Rutherglen, the largest makers of bichromate of potassium in this country, I had an opportunity lately of examining some of the "chrome sores" on the bodies of several workmen. These occur chiefly on the hands and exposed parts, and are said to arise from the salt coming in contact with a denuded cuticular surface. The first symptom is pain, succeeded by redness; and latterly the affected part assumes a papular or furunculoid form. After the lapse of some days, a cylindrical slough or core exudes from the centre of the swelling, leaving a deep pit with nearly vertical walls, the bottom of which generally extends through the cutis vera, and not unfrequently into or through some of the muscles. The sores are long of healing, doubtless from loss of structure, the salt acting like its acid, though with less intensity.

Chromic acid in this form has been said to have a strong propensity for the destruction of cartilage, inasmuch as the nasal alæ and septum, and even the larynx of workmen employed in the manufacture of bichrome are frequently greatly corroded by it. The ulceration has been considered similar to that of tertiary syphilis, and bichloride of mercury recommended as an antidote, and, withal, this absurdity has been promulgated by a standard work on chemistry.* From the experiments with the acid on

* Miller's Inorganic Chemistry (page 636, Ed. 1864). In Meadow's Prescriber's Companion (page 152) potassæ bichromas is said to have been recommended as an antisyphilitic.

portions of the trachea of a cow, the reverse is proved to be the case. Indeed, chromic acid might rather be said to have a specific action on gelatine or muscle, because breaches of surface are here the rule, whereas ulceration of cartilage is the exception. The cause of the nasal and laryngeal affection is clearly the same as that of the other sores—a cuticular hiatus; and while there seems an additional reason why the nasal walls should be attacked more than other parts, still that is fully counterbalanced by the material of which they are composed resisting the action of the acid more; hence it is seldomer affected.

During respiration in an atmosphere contaminated with floating particles of the chromic salt, these are constantly brought in contact with the anterior nares, &c.; and, dissolving in the nasal secretion, set up irritation, which, if prolonged, results in abrasion of the mucous membrane, What follows is plain: each inspiration deposits a variable quantity of salt upon the affected part, resulting in ulceration.

AS A TEST FOR STRYCHNIA.

Chromic acid elicits the coloured reaction in a solution containing $\frac{1}{1000}$ of strychnia. The *modus operandi* is to put two or three minims of the strychnia fluid on a white non-porous surface; then add a few needles of chromic acid, which instantly dissolve, imparting to the liquid their characteristic tint. One or two drops of *concentrated sulphuric acid* are now added, and the play of colour is at once evolved. With stronger solutions the result is very distinct, and in any case unfailing. In applying this test the above method must be strictly adhered to; because if sulphuric acid be added to the strychnia solution before the chromic acid, the latter will not dissolve, and the result is negative. Chromic acid does not give the coloured reaction with dilute sulphuric acid.

It is well known that bichromate of potassium, so commonly used for this test, does not always succeed. I have seen it fail in the

hands of two professors—one a celebrated toxicologist—when attempting to show it to their class; and am not aware that any explanation of the cause has been given, though it seems to me as follows:—It is obvious from the fact of chromic acid giving the coloured test at all, that to it alone the reaction is due when bichrome is used; that neither the potash in the latter, nor the sulphate of potash resulting from the addition of sulphuric acid, are indispensable agents in developing the reaction. Furthermore, as strong sulphuric acid is necessary both when using bichrome and chromic acid, *per se*, it is evident that when added to the first, the result is the formation of sulphate of potassium, water, and free chromic acid; indeed this is exactly the method by which the latter is prepared, as shown in the following equation:—$K_2O\ 2CrO_4 + H_2SO_4 = K_2SO_4 + H_2O \times 2CrO_3$. The chromic acid, being thus liberated, oxidises the strychnia, and the coloured reaction is afterwards developed by the strong sulphuric acid. But it may happen that the sulphuric acid, which might have proved strong enough for isolated chromic acid, is rendered too weak for bichrome in consequence of being partially neutralised by the base of the latter, and further diluted by the water, which, as seen in the formula, is generated by the hydrogen of the sulphuric acid combining with the oxygen of the potash; and this, I believe, is the most frequent cause of non-success of the test. With isolated chromic acid failure is of course impossible from such a source; and when used as described, the reaction is not only constant, but greatly more delicate. Another cause of lapsus with the bichrome, also applying in part to chromic acid, is seen in the following table of results:—

Bichrome added to solution of strychnia, then sulphuric acid added = much colour.

Sulphuric acid added to solution of strychnia, then bichrome added = considerable colour.

Sulphuric acid added to bichrome, then solution of strychnia added = almost no colour.

Chromic acid added to solution of strychnia, then sulphuric acid added = much colour.

Sulphuric acid added to solution of strychnia, then chromic acid added = a little colour.

Sulphuric acid added to chromic acid, then solution of strychnia added = almost no colour.

These results are just what might be expected. It will be seen, as with chromic acid, that when sulphuric acid is added to bichrome, or *vice versâ*, succeeded by strychnia, the reaction is almost *nil*. Doubtless, from the loose and empirical manner in which this test is usually applied, its failure is frequently due to the above cause; and indeed, from notes in my possession, I find this very method recommended.

ANTIDOTE.

In poisoning with chromic acid the best antidote is beef-tea, raw egg, milk, or any albuminous or gelatinous substance. A very handy and efficient antidote is a solution of ordinary painters' size.

THERAPEUTICAL PROPERTIES.

I am not aware what effect on the system prolonged minute doses of chromic acid (which may be absorbed) would produce, though, from its strong coagulating properties, a general viscidity of the blood might be anticipated, whatever further consequences should arise. In the absence of such knowledge, it seems to me that it might be used with a fair prospect of success in the following cases, for some of which it is already recommended in works on Materia Medica:—As a hæmostatic; as a caustic to cancerous tissues, chancres, condylomata, hospital gangrene, phagedænic ulcers, bites of rabid animals, poisoned wounds, warts, hæmorrhoids, &c.; as a wash to allay fetid discharges; as a gargle in diphtheria; as an injection in ozena, uterine catarrh, leucorrhœa, and gonorrhœa; to disinfect cholera and fever stools, for which it is admirably adapted; as a preventive of suppuration putrefaction, &c.

In every case it must be used dissolved in water; and, except where a strong caustic effect is desired, in medical or surgical cases, the solution should not be stronger than half a grain to the ounce, and be succeeded immediately after gargling or injection by the application of pure water. I have used chromic acid to a limited extent in the five latter affections above specified with a fair measure of success; in gonorrhœa and ozæna the results are especially gratifying. Finally, I have no doubt that its remarkable antiseptic, disinfectant, and coagulating powers will suggest many applications in medicine, surgery, and hygiene not above enumerated.

AIRD AND COGHILL, PRINTERS, GLASGOW.

ON THE

RELATIVE POWERS OF VARIOUS SUBSTANCES

IN

PREVENTING THE GENERATION OF ANIMALCULÆ

OR THE DEVELOPMENT OF THEIR GERMS;

WITH

SPECIAL REFERENCE TO THE GERM THEORY OF PUTREFACTION.

By JOHN DOUGALL, M.D.,
GLASGOW.

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Aird and Coghill, Printers, Glasgow.

ON THE

RELATIVE POWERS OF VARIOUS SUBSTANCES

IN PREVENTING THE GENERATION OF ANIMALCULÆ OR THE DEVELOPMENT OF THEIR GERMS.

The words GENERATION and DEVELOPMENT are here used so as to embrace the two opposite views held by the Panspermatists and the Nonpanspermatists; or, to employ the terms adopted by Professor Huxley last year, the words are meant to embrace the two conflicting theories of Biogenisis and of Abiogenisis; the Panspermatists, or Biogenists, believing that all life is derived from pre-existing life—"*omne vivum ex vivo*"—while the Nonpanspermatists, or Abiogenists, hold that certain combinations of dead matter are capable, in favourable circumstances, of originating many of those functions which are termed *vital*. In other words, they assert that spontaneous generation is not only possible but demonstrable.

It is not fairly within the scope of my subject to discuss the question at issue, nor does it appear that any modifications are required in the brilliant array of facts which Professor Huxley marshalled last year in defence of Biogenisis, nor that any serious doubts have been entertained regarding the accuracy of the very interesting and apparently practically exhaustive experiments of Dr. C. Bastian, which seem to prove Abiogenisis.

My object was not to inquire whether those amazingly minute animated specks, called in general terms Animalculæ, are generated or developed, but how their propagation could be retarded or altogether prevented.

As certain substances are inimical to the existence of the higher

organic structures, so also are they, to a considerable extent, adverse to animalcular vitality. Hence the assumed latent principle of life in the hypothetical germ of the Biogenist may be enfeebled or stifled, and the supposed subtle essence which is ready to permeate and vivify—to endue with perception and appetite the protean molecules of the Abiogenist—arrested or destroyed. In either view the result is the same—the non-appearance of life in solutions otherwise favourable to its production, caused by the addition of certain chemical substances of given strengths, and diffusible in the Animalculæ producing media.

Three series of experiments were made—one with vegetable and two with animal matter—of which the following are a few of the more important details:—

Sixty-seven substances were selected for trial, consisting of irritant, narcotic, and narcotico-irritant poisons; also several others whose sensible properties, or alleged antiseptic powers, recommended them as worthy of experiment. Nitrate of ammonium, and Spirits of nitric ether were added for contrast. Sulphite of soda, Perchloride of iron, and one or two other such bodies, were unintentionally omitted.

To three drams of a solution, consisting of one part of the substance to be tested, in 500 parts of water, was added one dram of a filtered infusion of hay, of the strength of half a dram of dried hay to a fluid ounce. This mixture was put into a phial. Where the substance was volatile the phial was kept closed, otherwise it was left open.

The *modus operandi* with the animal substances was similar, the only difference being that human urine was substituted for infusion of hay in one set of experiments, and a mixture of beef juice and egg albumen in another; while only half a dram of the latter mixture was added to three drams and a half of the test solutions. Three blank experiments were first made, *i.e.*, three phials were filled respectively with water and infusion of hay, water and urine, and water with juice of flesh and egg albumen only, in order that

any contrast in the growth of animalculæ, in the simple, and supposed preventive solutions might be noted. All the phials when filled were placed in a medium temperature of about 60° Fahrenheit, and exposed to a moderate light. In two to six days the microscopical examination of each series, with a magnifying power of 700 diameters, was begun and concluded. The simple solutions were found teeming with Bacteria, Vibriones, Monads, Amœbæ, Torulæ, &c. The results of the examination of the various test solutions were necessarily of two kinds—*life*, or *no life*; the one or other being inferred from the presence or absence of moving bodies only. No notice was taken of fungi, which were generally more or less present, especially in the vegetable solutions.

In those cases where motion was perceived, the phials were refilled with stronger solutions; conversely, weaker solutions were tried, and after being allowed to stand as before, were again examined microscopically and the process repeated till a point was gained where none, or only the faintest movement was perceptible. Here the strength of the solution was ascertained, and its preventive power noted, *i.e.*, the quantity of the foreign substance in the mixture in which no animalculæ were observed after the lapse of six clear days.

When the experiments had been concluded in the manner stated, they were repeated as follows, in order to confirm or correct previous results:—

Separate solutions were prepared of the various substances, each of its recorded preventive strength, with the same proportions of each of the organic fluids added as hitherto. They were then set aside under the same circumstances, for the same length of time, and again examined microscopically, and the results finally adopted.

It may be stated that most of the solutions had to be examined from three to eight times in each of the three series of experiments; the strength of the separate mixtures requiring to be either augmented or diminished at every inspection, before their preventive powers could be ascertained.

TABLE *shewing the amount of various substances which prevents the appearance of Animalculæ in six days at about 60° Fahrenheit, when added to Infusion of Hay, to Human Urine, and to a mixture of Beef Juice and Egg Albumen.*

	Hay.	Urine.	Beef Juice & Albumen.	Average.
I. METALLIC SALTS.				
Sulphate of Copper,	1-1000	1-1000	1-800	1-933·3
Bichloride of Mercury,	1-700	1-700 f.p.*	1-500	1-633·3
Protosulphate of Iron,	1-500	1-500	1-100	1-366·6
Arsenious Acid,	1-750	1-150	1-50	1-316·6
Sulphate of Iron and Potassium (Iron Alum),	1-300	1-300	1-300	1-300
Acetate of Lead,	1-300	1-300	1-300	1-300
Tartrate of Antimony,	1-300	1-300 f.p.	1-300	1-300
Chloride of Zinc,	1-300	1-300 f.p.	1-300	1-300
Sulphate of Zinc,	1-300	1-300 f.p.	1-200	1-266·6
Bichloride of Platinum,	1-250	1-250	1-150	1-216·6
Nitrate of Silver,	1-300	1-100	1-50	1-150
Average,	1-454·54	1-381·8	1-277·27	†1-371·18
II. ORGANIC ACIDS.				
Benzoic,	1-700	1-700	1-200	1-533·3
Hydrocyanic (B.P.), **	1-500	1-500 f.p.	1-500 f.p.	1-500
Picric,	1-350	1-350	1-350	1-350
Oxalic,	1-350	1-350	1-200	1-300
Carbolic,	1-300	1-300	1-200	1-266·6
Tartaric,	1-300	1-100 f.p.	‡ ,,	1-133·3
Acetic (B.P.),	1-350	1-25	,,	1-125
Average,	1-467·1	1-332·1	1-207	1-315·45
III. Salts of the Alkaline Earths.				
Chloride of Aluminum,	1-2000	1-500 f.p.	1-300	1-933·3
Sulphate of Aluminum and Potassium (Alum),	1-800	1-500 f.p.	1-100	1-466·6
Solution of Chloride of Calcium,	1-200	1-200	1-25	1-141·6
Chloralum (Commercial),	1-300	1-50	1-50	1-133·3
Sulphate of Magnesia,	1-250	1-50	,,	1-100
Bisulphite of Lime,	1-100	1-50	1-25	1-58·3
Average,	1-608·3	1-225	1-83·3	1-305·5
IV. INORGANIC ACIDS.				
Sulphuric, B.P.,	1-800	1-500	1-100	1-466·6
Nitric (B.P.),	1-400	1-400	1-200	1-333·3
Hydrochloric (B.P.),	1-500	1-400 f.p.	1-100	1-333·3
Nitro-Hydrochloric (B.P.),	1-250	1-250	1-100	1-200
Sulphurous (B.P.),	1-250	1-50	1-50	1-116·6
Average,	1-440	1-320	1-110	1-289·96
V. ALCOHOL and its DERIVATIVES				
Bromal Hydrate,	1-500	1-500	1-500	1-500
Chloral Hydrate,	1-600	1-600 f.p.	1-300	1-500
Perchloride of Formyle (Chloroform)	1-500	1-400 f.p.	1-25	1-308·3
Alcohol,	1-350	1-50	1-20	1-140
Spirits of Nitrous Ether,	1-350	,,	,,	1-116·6
Acetone,	1-100	,,	,,	1-33·3
Average,	1-400	1-258·3	1-140·83	1-266·33
VI. INORGANIC BASES.				
Tincture of Iodine,	1-400	1-400	1-50	1-283·3
Caustic Potash,	1-300	1-50	1-10 very putrid.	1-120
Average,	1-350	1-225	1-30	1-201·6

	Hay.	Urine.	Beef Juice & Albumen.	Average.
VII. ORGANIC SALTS.				
Hydrochlorate of Strychnia,	1-450	1-450	1-300	1-400
Sulphate of Bebeeria,	1-500	1-25	1-25	1-183·3
Acetate of Morphia,	1-500	1-25	,,	1-175
Stearate of Soda (Hard Soap),	1-25	1-25 f.p.	1-25	1-25
Average,	1-368·75	1-131·3	1-87·5	1-195·8
VIII. Inorganic Alkaline Salts.				
Bichromate of Potassium,	1-900	1-900	1-900	1-900
Permanganate of Do.	1-500	1-200	1-125	1-275
Chlorate of Do.	1-500	,,	,,	1-166·6
Hyposulphite of Soda,	1-250	,,	,,	1-83·3
Borate of Soda,	1-200	,,	,,	1-66·6
Condy's Fluid,	1-200	,,	,,	1-66·6
Sulphate of Soda,	1-75	,,	,,	1-25
Chloride of Do.,	1-25	,,	,,	1-8·3
Nitrate of Ammonium,	1-25	,,	,,	1-8·3
Average,	1-297·2	1-122·2	1-113·88	1-177·76
IX. VOLATILE GUMS.				
Camphor,	1-300	1-150 f.p.	1-50	1-166·6
X. AROMATIC OILS.				
Oil of Caraway,	1-400	,,	,,	1 133·3
Oil of Cassia,	1-400	,,	,,	1-133·3
Oil of Peppermint,	1-250	,,	,,	1-83·3
Average,	1-350			1-116·6
XI. ORGANIC BASES.				
Glycerine,	1-300	,,	,,	1-100
XII. BITTER EXTRACTS.				
Aqueous Extract of Quassia,	1-50	,,	,,	1-16·6
XIII. ANIMAL SUBSTANCES.				
Tincture of Musk,	1-50	,,	,,	1-16·6
Spirituous Extract of Cantharides,	,,	,,	,,	,,
XIV. AROMATIC EXTRACTS.				
Spirituous Extract of Capsicum (Cayenne),	1-25	,,	,,	1-8·3
XV. Poisonous Vegetable Extracts, Spirituous and Aqueous.§				
Extract of Aconite,	,,	,,	,,	,,
Extract of Belladonna,	,,	,,	,,	,,
Extract of Calabar Bean,	,,	,,	,,	,,
Extract of Digitalis,	,,	,,	,,	,,
Extract of Lobelia,	,,	,,	,,	,,
Extract of Tobacco,	,,	,,	,,	,,
Extract of Curara,	,,	,,	,,	,,

* F.P. denotes faint putrescence. Where these letters are absent, the mixture remained free of odour.

** B.P denotes British Pharmacopœia.

† The average of the averages in right hand column corresponds with the average of the three averages of the perpendicular columns.

‡ The various blanks in the columns indicate that the substances opposite which they are placed are incapable of preventing putrefaction and the appearance of animalculæ at 1-10.

§ Simple aqueous solutions of the poisonous vegetable extracts soon teem with life.

Note.—The parts of water indicated in the tables represent dropped minims, forty to the dram.

Example.—1-500 signifies one grain, if the chemical substance be a solid, or one dropped minim, if a fluid, in 500 minims of water, forty of which make one dram.

The substances used have been classified into fifteen groups, according to their chemical relations—placing the group that shows the highest average preventive power "first" on the table, and the one that shows the lowest, "last." The various substances have also been tabulated in their respective groups on the same principle.

The metallic salts, from their showing the highest average preventive power, viz., 1-371·18, form Group I. Sulphate of copper here has not only the highest individual average, but its three preventive points, in the three solutions, are very much higher than those of any other substance in the group. Nitrate of silver, on the other hand, exhibits the lowest individual average. This is undoubtedly to be attributed to its peculiar property of being decomposed by the combined action of light and organic matter, and further, in the urinous solution, by the chlorides present. The results of the acetate of lead, in the urinous solution, are also affected by the same cause, viz., the formation of the insoluble chloride. The metallic salts as a body are very uniform in their action.

In Group II., the organic acids, "benzoic" has the highest, and "acetic" the lowest average preventive power. Of the seven members composing this group, carbolic acid occupies the fifth rank. There are also two blanks in the albumen column—the one opposite "acetic" and the other opposite "tartaric" acid, indicating that these are incapable of preventing the appearance of animalculæ in the albuminous solution at 1-10.

Group III., Salts of the alkaline earths. Here chloride of aluminum is "highest" (I do not refer, of course, to the commercial liquid chloralum, which, however, has been included), and a sulphurous smelling fluid, said to be bisulphite of lime, "lowest." Were it not for the extremely high average (1-2000) of the chloride of aluminum in the hay column, this group would occupy a comparatively subordinate position. Sulphate of magnesia is blank in the albumen column.

The average of Group VIII., inorganic alkaline salts, would be

"extremely low" were it not for that of the bichromate of potassium, which has the "very high" average of 1-900. The salts of soda and Condy's fluid are all blank in the urine and albumen columns, except the borate of soda, which, however, has a merely nominal preventive power.

Group X. The aromatic oils are inert in the urinous and albuminous solutions, but show good averages in hay.

Group XIII. Animal substances. Here the extract of cantharides shows three entire blanks, while the tincture of musk is blank in the urine and albumen columns.

Group XV. The poisonous vegetable extracts show blanks in all the columns, which is not surprising, seeing that their aqueous solutions, *per se*, soon teem with life.

These are a few of the more prominent points in the tables to which it has been thought worth calling attention.

On comparing the preventive powers of the different substances, in the various solutions, it is found, without exception, that the figures in the urine and albumen columns, though generally less than, and often equal to, never exceed, those in the hay column. Similar relations exist between the urine and albumen; *e.g.*, the preventive powers of a substance may be equal in both solutions, still that in the "albuminous" never surpasses that in the "urinous," and though occasionally equal, is often less. This principle is rendered easily apparent by contrasting the three average preventive powers of the three solutions of any entire group, when it will be found that a higher per centage, of all the individual substances of the group, is required to prevent the appearance of animalculæ in urine, than in hay, and a still higher in albumen than in urine.

This seems accounted for as follows:—Premising that an infusion of hay is poorer in organic matter than urine, and that the latter fluid is less rich than albumen, it appears, from the experiments, that the germ generating or developing force is strongest where the organic matter is richest, and weakest where the organic matter is

poorest. Hence the "quantity" of the "preventive" substance requires to be proportioned to the "quality" of the "organic."

But this principle applies only to averages. The preventive points of many single substances are equal in the three solutions; seemingly indicating that the potential vital energies of the latter are also equal; this view, however, is obviously unsatisfactory, the cause probably being some peculiarity in the preventive body.

It is to be remembered, also, that the averages given only hold good in the limits stated. It is possible, nay, highly probable, that were the incubatory stage prolonged in those cases where "*no life*" is recorded, "*abundant life*" might be the result. Hence the preventive points would require to be lowered—strengthened, in proportion as this stage was extended.

In those cases, again, where life persists at 1-10, it is quite likely that were the strength of the trial substance increased, so that, where possible, instead of it constituting the "minimum" proportion in the fluid in which it is dissolved, it composed the "maximum," we should have to report, instead of "*life*," "*no life*." In other words, it is not meant that these substances are not preventive at all, but, simply, that they are not preventive at 1-10, in the time, and at the temperature, stated.

It may be assumed, to a very limited extent, that, in several cases, solutions somewhat weaker, than those recorded, might be preventive. Any error, however, in this respect, is on the side of safety; having made sure that the strengths enumerated are reliable.

Although a higher magnifying power might have been employed, still, that made use of indicates, if not the actual, at least the relative preventive tendencies of the various substances, which, practically, is the point of greatest importance. Yet, I think it will be admitted, that the 12th of an inch object-glass, magnifying about 711 diameters, is an instrument of no mean power, considering that germs in active motion, and measuring about $\frac{1}{40.000}$th (·000025) of an inch in diameter, can be seen by it.

The following bodies appear to merit special observation for

reasons which shall be obvious when referring to them—sulphate of copper, bichloride of mercury, sulphate of iron, arsenious, benzoic, hydrocyanic, picric, carbolic, and sulphurous acids, chloride of aluminum, chloride of calcium, bisulphite of lime, alcohol, chloral hydrate, bromal hydrate, caustic potash, hard soap, hydrochlorate of strychnia, acetate of morphia, chlorate, bichromate, and permanganate of potassium, Condy's fluid, salts of sodium, especially the chloride and borate; camphor, musk, cantharides, quassia, and capsicum.

Regarding sulphate of copper, as already stated, its average preventive point is very high, viz., 1-933·3. Why, is not very apparent, as it does not seem to possess any distinctive peculiarities other than being astringent, styptic, and escharotic, and that in a comparatively moderate degree.

The bichloride of mercury stands high, having an average of 1-663, which, however, is about 1-3rd less than that of the cupreous salt. This seems an unexpected result, considering its well-known powerfully corrosive, and acknowledged antiseptic properties, and might at first be attributed to its being rendered inert by combination with the albumen in that solution. But in the two other solutions, its preventive points are also about 1-3rd below those of the sulphate of copper; while that, like most other metallic salts, has also the property of forming an insoluble compound with albumen, and combining also with the nascent sulphuretted hydrogen. Again, its urinous solution, at the preventive point, has a faint putrescent odour; while the same solution of sulphate of copper, though 1-3rd weaker, is quite free from smell.

The ferrous sulphate, or proto-sulphate of iron, stands comparatively low in the albumen column, but has a fair average. This substance, if mixed with organic matter, possesses the power, in addition to the properties of the other metallic salts, of appropriating oxygen that would otherwise combine with that matter, and so induce putrefaction, which it thus delays. This power, however, is proportioned to the quantity of the salt present, and is, of course,

exhausted when its oxidation is completed. The organic matter, being then without any means of defence from its natural foe, oxygen, is now acted on by that element, and putrefaction ensues. The mixture, however, now contains the "ferric sulphate" (Fe_2O_3, $3SO_3$) instead of the "ferrous sulphate."

Arsenious acid—(a small quantity of "hydrochloric" had to be used to render this substance soluble). The violent symptoms which arsenious acid originates when swallowed, and its strong antiseptic action on the viscera of persons poisoned by it, are well known, yet its preventive powers are greatly inferior to those of the ferrous sulphate. Judging from the experiments, the explanation of this seems to be, that, as the latter salt surpasses the arsenic in preventive power, it would also surpass it as an antiseptic under similar circumstances.

Amongst the organic acids the "benzoic" is first in average, though third in the albumen column. This substance, it appears, has not received the attention it merits. No doubt gum benzoin has long been used in the preparation of fumigating pastiles to overcome unpleasant odours, but the general impression is that these are merely masked, not destroyed. The results of the experiments, however, indicate something more than this; because, although the albuminoid preventive point of picric acid is much higher than that of "benzoic," and though that of carbolic acid equals it, still it has an average greater than the former, and double that of the latter. Its mode of action is not clear; probably it is assisted by the small quantity of empyreumatic oil always present in its commercial crystals.

Hydrocyanic acid of the B.P. in its group stands second in the average, and first in the albumen column. As this solution, however, only contains one in fifty of the anhydrous acid, it follows that its real preventive point is 1-25,000, a power nearly double that of the united averages of each individual substance, which is only 1-14613; or, in other words, one minim of the pure acid has a preventive power nearly equal to that of two minims or grains

of each of the various substances combined. This acid seems to be the germicide "par excellence," stifling life amid putrefaction.

Picric acid, in its group, ranks next to hydrocyanic acid in the albumen column. It is easily obtained by mixing nitric and carbolic acids, when evolution of nitrous fumes ensues, with the formation of a tarry-looking residue—crude picric acid. It has an excessively bitter taste, a strong peculiar odour, and a yellow colour so singularly intense that it gives a perceptible tint to a million parts of water. Its *modus operandi* as a preventive agent is not very clear.

We now come to consider a substance which has recently risen to great importance, viz., "carbolic acid." Highly as I esteem my former teacher, Professor Lister, and greatly as I admire his patient, arduous research, and fertility of resource, all of which I had abundant opportunities of witnessing and profiting by while he was developing his theory of the "antiseptic system;" and satisfied as I am of the comparatively "marvellous results" which he and others have obtained, and are obtaining, with this substance, yet, the conclusion is irresistible from the experiments, that, if these results are to be attributed solely to the germ-killing or preventive powers of carbolic acid, then it must yield the palm to many superiors. If, as is alleged, germs are the source of putrefaction, then the strongest preventives *must* be the best antiseptics, and *vice versa*. Now, as seen in the table, carbolic acid occupies a very mediocre place as a preventive, therefore it is legitimate to conclude that it stands no higher as an antiseptic. But if it does not check putrefaction by killing germs, or directly preventing their multiplication, how, then, does it act? Although unable to formulate the change that takes place when it unites, in large proportion, with organic bodies, for which it has a strong affinity, still the result of such change certainly is the formation of a compound capable of resisting the attacks of oxygen, of water, and, consequently, of germs. In other words, a compound is formed which is proof against putrefactive tendencies.

The next substance—sulphurous acid—it was expected would have stood at the top of its group, seeing that a few years ago it was extolled by two Scotch medical practitioners as the "paragon of parasiticides." Here it is lowest as a germicide. It seems to act like the ferrous sulphate by appropriating the oxygen that would otherwise decompose the organic matter. Hence it is converted into sulphuric acid, which, however, stands at the top of the group. The cause of this apparent anomaly is not clear. Probably, from the sulphurous acid being volatile, a portion may escape oxidation, and thus the residual sulphuric acid is proportionately lessened.

Dr. Angus Smith, in his work on Disinfectants, states that "sulphurous acid deodorizes putrid matter (whether by giving or taking oxygen is not explained), but that if used in a liquid or moist substance containing organic matter (it was so in this instance), it is decomposed, and sulphuretted hydrogen given out."

Chloride of aluminum, amongst the salts of the alkaline earths, stands first. Its very high preventive power (1-2000) in the "hay solution," already noticed, seems remarkable, and was verified several times. It also shows a high average in the other solutions.

The commercial liquid known as "chloralum" has been included in this list. It is merely an aqueous solution of chloride of aluminum. As anticipated, its average is low.

Solution (saturated) of chloride of lime has a low preventive point (1-25) in the albumen column. This substance acts as an oxidizer, its chlorine combining with the hydrogen of the water, forming hydrochloric acid, while oxygen is liberated.

Bisulphite of lime.—This body is chiefly used in the preservation of flesh. As a preventive it stands much lower than sulphurous acid, a solution of which it greatly resembles.

The preservative powers of alcohol have long been known. These are the result of its strong attraction for water. As a preventive it stands below mediocrity.

Bromal hydrate and chloral hydrate stand at the top of the alcoholic series, and are above the average of preventives. Doubtless these bodies possess corresponding antiseptic properties.

The very low preventive power of potash in the albumen column (1-10) is worthy of note. Such a mixture is, obviously, highly caustic, and has a powerful softening and disintegrating action on organic tissues. Still, it was necessary to use it of such strength to prevent the appearance of life, as at 1-25 bacteria, &c., were abundant. It seems incredible that creatures so seemingly fragile, so astoundingly minute and numerous, that the more common infusorial animalculæ are Titans in comparison, and could swallow a shoal of them in a mouthful, can exist in a liquid so corrosive as to produce sloughing, if kept for a short time in contact with the skin, and to cause certain death in the human subject, if swallowed in small quantity. Their bodies seem composed of some chitonous, or insoluble substance resembling caoutchouc or gutta percha, all of which are unaffected in solutions of the alkalies.

Anhydrous stearate of soda or hard soap has a very low preventive average (1-25), at which point it is of the consistency of curdled milk. From a sanatory point of view this is a fact of importance, as it proves that though soap, in conjunction with water and friction, be an excellent detergent in virtue of its solvent action on effete organic matter, yet it does not follow that it is even a moderate preventive. It might be worth considering whether a domestic soap could be made at a cheap price, combining good detergent, with the highest preventive, powers.

The hydrochlorate of strychnia, notwithstanding its energetic toxic action on the higher animals, is only a moderate preventive.

The acetate of morphia is powerless as a preventive in the "albuminous solution," and nearly so in the "urinous."

Chlorate of potassium. From the high character borne by this substance as a gargle or wash, in cancrum oris, gangrenous stomatitis, diptheria, &c., where it is supposed by some to act by

destroying certain fungi, or germs, or specific poison, it was expected that it might have a high preventive power, whereas it is blank both in the urine and albumen columns. Moreover, it was observed that it has very remarkable power in hastening decomposition.

The permanganate of potash stands second in its group, but has a comparatively low average; its albuminoid mixture at the strength noted is of the consistence of jelly.

Condy's fluid is blank both in the urine and albumen column.

With the exception of hydrocyanic acid, the bichromate of potassium stands unrivalled as a preventive in the albuminoid solution, and, besides, has a very high average. It is superior, however, to hydrocyanic acid in respect that its urinous and albuminous solutions, at the preventive points, are quite sweet and pure, while those of hydrocyanic acid have a distinct putrid odour. In a sanatory sense, this substance seems to merit more attention than it has hitherto received. Dr. Angus Smith shews, in a table of experiments, in his work on disinfectants, "to determine the amount of each of 14 of the most energetic antiseptic bodies necessary to prevent the evolution of sulphuretted hydrogen in a mixture of equal parts of blood and water," that all of them, in from six to ten days, had evolved more or less of that gas, except the one containing bichromate of potassium. It is strong proof of the decided and valuable antiseptic powers of this salt, that the same result should be gained by two different observers, using two different tests—i.e., the evolution of sulphuretted hydrogen, and the appearance of animalculæ, and the one also being unaware of the results obtained by the other. It seems, also, a proof that antiseptics act on the putrescible matter, and not on the germs, as free access of air was allowed, the bichromate not being volatile.

The hyposulphite, borate, sulphate, and chloride of sodium, are almost useless as preventives, being blank in the urine and albumen columns. These compounds, in the circumstances, decidedly accelerate decomposition, and it is worthy of observation that here *common salt stands lowest in preventive power*. Dr. Gregory, in his "Organic Chemistry" (page 33), states that "the presence of bases greatly promotes the absorption of atmospheric oxygen by organic substances, which is the reason why alkalies assist *eremacausis*."

The extract of capsicum, of cantharides, of quassia, and tincture of musk—preparations which it was thought might prove good preventives—are comparatively worthless.

There are a few other bodies that merit particular attention, but the short time at my disposal precludes me from noticing them.

One substance, however, viz., Chromic acid, I much regret was not included in the list. Having observed, after the tables were arranged and this paper written, the great difference in the preventive powers of bichromate of potassium and of potash, it was surmised that probably the potency of the former body resided in its acid, of which it contains two equivalents. Desirous that this point should be settled, even though too late to embody the issues in the table, Chromic acid was subjected to the same tests as the other substances, and the results fully confirmed these anticipations.

PREVENTIVE POWERS OF CHROMIC ACID.

Hay, 1-4000. Urine, 1-1400. Beef Juice and Egg Albumen, 1-1200.
Average, 1-2200.

With the exception, therefore, of Hydrocyanic acid, Chromic acid surpasses all the substances enumerated as a preventive; and without exception, all, as an antiseptic. Were it placed in its proper position in the table, it would raise its group (the inorganic acids) from the fourth to the first place.

Chromic acid is unquestionably an antiseptic of surpassing power, and must ere long take the foremost place as a sanatory agent. If brought in contact with organic matter CrO_3—Chromic acid, or teroxide of Chromium, is reduced to Cr_2O_3—sesquioxide of Chromium, one and a half equivalents of oxygen being liberated for each molecule of the acid decomposed. It is therefore, both

theoretically, and, as I have ascertained, practically, an excellent deodorant, while it possesses, at least, *double the antiseptic power of carbolic acid.* It is also perhaps a stronger coagulator of albumen than any other chemical substance. As such, it greatly exceeds carbolic acid, nitric acid, &c. Moreover, it is extremely soluble in water, a property of primary importance so far as the disinfecting of clothing and the prevention of putrefaction is concerned. It is also perfectly devoid of odour. It may be objected that it is not volatile; but this might be obviated to a considerable extent, if desired, by using its aqueous solution in the form of spray. Still, in many instances, volatility in reference to antiseptics and disinfectants is a property which, in their practical application, is not always to be regarded as a recommendation; because their maximum sanatory effect cannot be obtained unless they are used much stronger than can be tolerated by the senses, and in enclosed spaces, necessitating, in either case, the removal of any occupants. As commonly employed, there is much reason to believe that, in the majority of cases, they merely disguise, or, at best, only palliate, offensive odours.

Further, in a sense, even their volatility is an objection, inasmuch as they are liable to be dissipated or diluted by atmospheric air to a degree which renders them comparatively inert either as antiseptics or disinfectants.

Although it was not made a point of special attention in the experiments to ascertain whether, and if so, at what rate, any of the substances accelerated or retarded putrefaction, still it was observed, as already partially stated, that (except the bichromate of potassium) the alkaline salts, caustic potash, sulphate of magnesia, and spirits of nitrous ether greatly hastened it.

Again, among many of those bodies whose preventive points show a fair average, there was a difference of from two to four days in the occurrence of decomposition. Of course this was only ascertained at strengths below their preventive points.

In every case where the odour of putrefaction was strong, excepting the caustic potash and albumen solution, large and small animalculæ were abundant. In a few cases, where it was faintly perceptible, these were absent; and in a few, where no odour could be detected, only the most minute germs were present. Commonly, where the mixture kept free from odour, germs were absent. In some of the solutions, more especially the albuminous with chloroform, only the very minutest germs could be perceived previous to the sixth day, and these always in such myriads, that though magnified fully 700 diameters, they gave the field of the microscope the aspect of the finest "stipling" or punctate grain work of the engraver, visible to the naked eye.

The smaller forms seem to possess greater vital energy than the larger, as they are first and last seen in the solutions.

These are the chief replies which were obtained to my frequent and probably peculiar interrogations of nature. Several inferences and questions arise from these, some of which have already been, of necessity, cursorily considered.

First.—It does not appear that germs are the cause of putrefaction, because in several of the urinous solutions, viz., that with chloroform, chloral hydrate, tartaric acid, chloride and sulphate of aluminum, sulphate of zinc, camphor, soap, hydrocyanic acid, hydrochloric acid, tartrate of antimony, and bichloride of mercury, germs were absent, while they had, at the same time, a faint but distinct putrescent odour. It is not likely that this absence of life is attributable to any exclusive property of these bodies, but rather that their solutions were examined at a stage immediately preceding the appearance of germs, a state which all the solutions probably passed through, and which, if looked for at the proper time, might be found in any of them. Curiously enough, however, these conditions existed only in the "urinous," while the very opposite are found in the "albuminous" solutions, viz., the presence of germs, and the absence of putrid odour. Caustic potash and albumen is, however, an exception, the

odour being, as already stated, very putrid and germs absent. Of course, this was ascertained during the experiments to find the various preventive points. The bodies with which it occurred were carbolic, acetic, benzoic, and hydrochloric acids, permanganate of potash, solution of chloride of lime, sulphate of copper, and ferrous sulphate. It is possible that in some of these cases the nasal organ, which is more liable to derangement of function than the eye, may have been deceived, or, that the odour of putrefaction may have been masked by the smell of some, at least, of the extraneous substances. In any view, that the fluid was in a state of incipient decomposition, though not appreciable by its smell, I submit was satisfactorily evinced by the presence of bacteria and vibriones in considerable numbers. The reason why life preceded odour in the albuminous fluid need not, I think, be ascribed to any property of the chemical substances, but rather to the rich quality of the fluid forming a most favourable nidus, and suitable pabulum, for the multiplication of these active creatures. Germs may thus precede odour, but decomposition more or less precedes germs. As observed by Beale, "active bacteria, introduced into a healthy wound, or among the living tissues, will die, although the most minute germs present, which escape death, may remain embedded in the tissues in a perfectly quiescent state." And again, "bacteria germs grow and multiply wherever a change takes place in the solids and fluids of the organism, which develops compounds suitable for the pabulum of these living bodies."—(Disease Germs: their Supposed Nature, p. 59.)

On the other hand, the reason why the odour of putrefaction preceded the appearance of life in the urinous solutions, is probably owing to the chlorides, free acid, &c., in that fluid, preventing the development of germs till decomposition had somewhat advanced; but whatever be the cause, the fact remains patent, viz., that *slight*, and in one case (potash and albumen) *extensive*, putrefaction may exist while none of these bodies are present, proving that germs are not the cause of that process.

But we may go further than this, by stating that it is more than probable that *germs rather prevent putrefaction*. You have doubtless all read, and pondered over, the broad and brilliant philosophical deductions, which seem, to me, to compose the fairiest of all "the fairy tales of science," regarding the uses in nature of infusoria, by that illustrious and veteran master in natural history, Professor Owen.

"Consider," he says, "their incredible numbers, their insatiable voracity, and that it is the particles of decaying vegetable and animal bodies which they are appointed to devour and assimilate. Surely we must in some degree be indebted to those ever-active invisible scavengers for the salubrity of our atmosphere. Nor is this all. They perform a still more important office, in preventing the gradual diminution of the present amount of organic matter upon the earth; for when this matter is dissolved and suspended in water in that state of comminution and decay which immediately precedes its final decomposition into the elementary gases, and its consequent return from the organic to the inorganic world, these wakeful members of nature's invisible police are everywhere ready to arrest the fugitive organized particles and turn them back into the ascending stream of animal life. Having converted the dead and decomposing particles into their own tissues, they themselves become the food of large infusoria, and of numerous other small animals, as the rotifera, which in their turn are devoured by larger animals, as fishes; and thus a pabulum fit for the nourishment of the highest organized beings is brought back by a short route from the extremity of the realms of organic matter."—(On the Invertebrata.)

Here one individual designates germs "ever-active invisible scavengers," whose duties are to polish our sunbeams, purify our atmosphere, and sweeten our waters; "wakeful members of nature's invisible police," who are ceaselessly collaring and gobbling up the organic runaways, so as to prevent them injuring their more sensitive neighbours, as well as themselves. Others, again, and

especially Professor Lister, denounce them as the cause of many, perhaps of most, diseases—the invisible and ruthless scourge of our hospitals, often in a few hours rendering hopelessly abortive the best-directed remedial measures; and, therefore, a war of extermination, or a "stamping out" procedure, ought to be perpetually waged against them. Between two such eminent authorities differing so widely, it is difficult to decide without exact data. These, it is considered, have been indicated, if not furnished, by the experiments, the results of which agree with the opinion of the great majority of those who have studied the subject.

How is it that germs have at all come to be considered as the cause of putrefaction? I presume, from the fact, that along with fungi, they are generally found in decomposing organic solutions. But assuming them to be the scavengers which Professor Owen says they are, then that is just where they ought to be, and just where we should expect to find them.

If meat or vegetables are found fresh some months after being hermetically sealed in tins from which the air has been exhausted, or, after having absorbed some substance with the property of preventing fermentation, or putrefaction, it is concluded that this is owing to the absence of germs: instead of that the absence of germs is owing to the freshness of the substances.

Helmholtz, Pasteur, and others seem to have shewn that germs may be mechanically excluded from organic solutions, with the result that the fluid does not get turbid. But these experiments appear more adapted to illustrate the theory of Biogenisis, than that germs are the cause of putrefaction. It has probably been already proven that the phenomenon of putrefaction originates in some other cause than the presence of germs, and that it necessarily precedes their appearance. Moreover, it seems a just inference that their exclusion will prolong the putrefactive process by allowing entire decomposition to ensue, but will not stop it.

It is generally considered that when a decomposible fluid remains destitute of haze and mouldiness, it is not decomposing. What is this haze and mouldiness? Animalculæ and fungi—nature's invisible sanatory executive—which Helmholtz with his membrane, Dusch and Schröder with their cotton wool, Schwann with his red-hot glass tubes and fuming sulphuric acid, and Pasteur with his crooked-necked flasks, are supposed to intercept or suppress in the course of their sanatory duties. Conversely it is held that the proof of putrefaction in an organic solution is a growing haze and mouldiness in it (*i.e.*, animalculæ and fungi). Very good; but the proof of a thing is not necessarily the cause of it, any more than a municipal sanatory officer is the cause of the municipal filth and fever, he being only a sign of it; or any more than the absence of the proof indicates the absence of the thing itself. But further, the proof may actually be antagonistic to the cause, as animalculæ, though indicating decomposition, are busy preventing it; as the presence of the fire brigade indicates fire, while its members are extinguishing it.

Again, as already stated, commonly where the mixture kept free from *appreciable odour and haze*, germs were absent. Some may say that this absence of putrefaction was owing to the absence of germs; but I am inclined to hold that the absence of germs was rather owing to the absence of putrefaction, and the absence of putrefaction owing to the presence of an antiseptic. No means were taken to exclude germs, but rather the reverse. Why, then, were they absent here? Simply because they were not required. And how did they happen to stay away when not required? Because the conditions favourable to their active existence and increase, and which it is their natural function to prevent, were absent. Conversely, when these conditions prevail, they are present. Thus the oscillations of decomposition and recomposition are seen to be compensatory.

After these observations, it will scarcely excite surprise if it be asked, *Is it necessary to kill germs?* and, considering their

numbers and size, *Is it possible*, even supposing their destruction would be of any conceivable benefit?—and the answer be, It is as unnecessary as it is impossible. Their services may be dispensed with by substituting better scavengers, or by preventing the noxious conditions which nature has appointed them to obviate; but in such cases they will save you the trouble of killing them, by leaving spontaneously in search of a living elsewhere. Again, while it seems possible to supersede them by more energetic artificial means, and to prevent, to a great extent, the conditions that require removal, still the results of the experiments appear to indicate that it is also possible to substitute worse scavengers (*i.e.* substances that accelerate putrefaction), and even to remove the natural ones without replacing them by others, (as in those substances which are putrid at the preventive point.)*

In the foregoing remarks no reference is made to the "fungi" which were frequently present, especially in the vegetable solutions, and were observed generally before, but often simultaneous with, the motile bodies.

The vitality of these plants is more easily overcome than that of the animalculæ, because they always disappeared from the solutions before the latter. "Although some hold that fungi are both the predisposing and exciting causes of certain diseases in man and animals with which they are associated, yet it is generally believed that the positions in which they are found are their natural ones, and that they only grow and multiply on dead organic matter, or on organisms in which the vital powers are deteriorated, and the healthy processes of life consequently imperfectly performed."

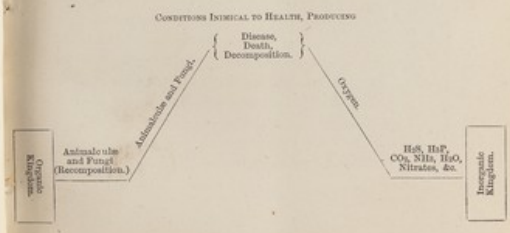
Assuming the latter view as the correct one, then fungi, like animalculæ, are also scavengers. Their myriad spores, which are constantly floating in the air, alighting on organic matter in an incipient state of decay, strike their roots into it, and begin to recompose it into their own structures.

* See also table at end.

Thus, though these plants may be considered, like their congenors, the animalculæ, as a sign of decay, which they undoubtedly are, yet the great majority of facts tend to show that they are not the cause of that process, but the consequence, the symptoms of decomposition, but, at the same time, the tangible proofs of recomposition. Remove, or prevent the morbid states which yield a soil suitable for their germination and multiplication, and they will cease to germinate and multiply. Remove the cause and the symptoms will disappear.

Second—What is the cause of putrefaction? Having in some degree, perhaps, proven that animalculæ and fungi rather prevent than cause putrefaction, it may now be attempted to show, what is already very generally admitted, that putrefaction is also caused by a scavenger, the name of which is Oxygen.

The operations of this body, like those of its animal and vegetable co-workers, are, however, preceded by others, viz., diminished vitality or death of the organism, which may again proceed from a variety of states inimical to health and life. The method of procedure of oxygen in its incessant labours to keep sweet and pure the abodes of organic nature, is to dissolve complex aggregations of matter by, at least, initiating chemical transposition, and sweep the metamorphosed and disunited molecules back into the inorganic world in the form of sulphuretted hydrogen, phosphuretted hydrogen, carbonic acid, ammonia, water, nitrates, &c. Therefore we have—



Third.—In what manner do substances prevent the appearance of animalculæ and putrefaction?

1. Is it by exhaling constantly a destructive atmosphere on their myriads which are perpetually swarming in the neighbourhood of organic matter? It does not appear that the substance mixed in the organic solution at, or even above, its preventive strength, and though volatile, has any effect on germs at, or near, its surface, but not actually in the solution; because if an open phial, containing beef juice only, be suspended in an open flask also containing beef juice, but to which has been added the ascertained preventive quantity of either a volatile or fixed substance, it will be found, that, though neither life nor putrescence appears in the latter, the former will, in about three days, be teeming with life, and quite putrid. This experiment may be varied by using hay or urine, with the same results. Germs and putrefaction being practically synonymous, the preventive point of a substance may, therefore, be taken as a measure of its antiseptic power.

2. Is it by destroying their ova? I think it must be assumed that "the preventive" either permanently destroys the ova, or suppresses their development for an indefinite time, or renders the soil inimical to their growth, whether they be considered as lodged in the putrescible substance previous to the addition of "the preventive," or as being continuously depositing on it subsequently.

3. Is it that the compounds necessary to evolve life "de novo" are chemically changed by the preventive, so that life is incompatible with the altered conditions?

If spontaneous evolution be regarded as possible, this seems the only explanation, and implies alterations in the molecular structure and sensible properties of the organic substance which, though difficult to formulate, may easily be considered, from the known characters of the given preventive, as amply sufficient to account for the non-apppearance of life.

Before concluding, I wish to direct attention to the following

table of experiments, which were made with the view of ascertaining the relative germicidal and deodorizing powers of the various substances at their ascertained preventive points. A deviation was made from one of the original conditions of the previous experiments, which, though it has not interfered with the unity of the table by itself, has partly depreciated its value, inasmuch as its results cannot be justly compared with those of the preceding table. The change referred to is, that equal parts of the preventive and putrid solutions were used, whereas in the former tables only one part of the albuminous mixture was added to seven parts of the preventive fluid. Had the latter instead of the former proportions been tried in these experiments, the result would have been undoubtedly more negative, but therefore more uniform with those previously obtained.

Viewing the table, however, as a group of independent results obtained under identical conditions, it was thought that it might not be uninteresting to append it to this paper, seeing that it is so nearly allied to the main subject, and consists exclusively of actual facts, from which a great many important conclusions may be drawn, but which are submitted without observation.

TABLE

Shewing the results of adding an equal part of a putrid solution of Beef Juice and Egg Albumen, full of Animalcule, to solutions of various substances of strengths known to be preventive.

I. SUBSTANCES THAT DESTROY LIFE BUT NOT SMELL.*

NAME OF SUBSTANCE.	Strength.	Effect on Animalcule in 15 minutes after mixing the Solutions.	Whether the Mixture is deodorised.	Whether Animalcule are present after four days.	Condition after four days as to Odour.
Sulphate of Copper,	1-800	Death.	No.	A few vibriones.	F. P. †
Stearate of Soda (Hard Soap),	1-25	Death.	No.	No.	Putrid.
Sulphate of Berberia,	1-25	Death.	No.	Abundant.	F. P.
Chloride of Zinc,	1-300	Death.	No.	No.	None.
Nitric Acid,	1-200	Death.	No.	No.	None.
Nitro Hydrochloric Acid,	1-100	Death.	No.	No.	None.
Hydrochloric Acid,	1-100	Death.	No.	No.	None.
Hydrochlorate of Arsenic,	1-50	Death.	No.	No.	None.
Benzoic Acid,	1-200	Death.	No.	No.	None.
Acetate of Lead,	1-300	Death.	No.	Abundant.	Very putrid.
Sulphate of Iron and Potassium (Iron Alum)	1-300	Death.	No.	Abundant.	Very putrid.
Sulphuric acid,	1-100	Death.	No.	Very abundant.	Very putrid.
Sulphate of Zinc,	1-300	Death.	No.	No.	Very putrid.
Caustic Potash,	1-10	Death.	No.	No.	Very putrid.
Sulphurous Acid,	1-50	Death.	No.	No.	F. P.
Bisulphite of Lime,	1-25	Death.	No.	No.	F. P.
Oxalic Acid,	1-200	Death.	No.	No.	None.

II. SUBSTANCES THAT DESTROY LIFE AND PARTLY SMELL.

Bichromate of Potassium,	1-900	Death.	Considerably.	A few vibriones.	F. P.
Tincture of Iodine,	1-50	Death.	Partly.	No.	F. P.
Hydrochlorate of Strychnia,	1-300	Death.	Partly.	No.	None.

III. SUBSTANCES THAT DESTROY BOTH LIFE AND SMELL.

Alcohol,	1-20	Death.	Yes.	No.	F. P.
Solution of Chloride of Lime,	1-25	Death.	Yes.	No.	None.
Bichloride of Mercury	1-500	Death.	Yes.	No.	None.
Picric Acid,	1-350	Death.	Yes.	No.	F. P.

IV. SUBSTANCES THAT PARTLY DESTROY SMELL BUT NOT LIFE.

Bromal Hydrate,	1-500	Myriads of active Vibriones.	Partly.	No.	None.
Camphor,	1-50	Myriads of active Vibriones, also large Animalcules.	Partly.	No.	None.
Chloral Hydrate,	1-300	Myriads of active large and minute Animalcules.	Partly.	No.	Putrid.
Carbolic Acid,	1-200	Teeming with Vibriones.	Partly.	No.	F. P.

V. SUBSTANCES THAT DESTROY SMELL BUT NOT LIFE.

Nitrate of Silver,	1-50	Myriads of minute life.	Yes.	Abundant.	F. P.
Permanganate of Potassium,	1-25	Teeming with minute life.	Yes.	No.	F. P.

VI.—SUBSTANCES THAT NEITHER DESTROY LIFE NOR SMELL.

Nitrate of Ammonium,	1-25	Myriad life.	No.	Abundant.	Putrid.
Ferrous Sulphate,	1-100	Abundant minute life.	No.	Abundant.	Very putrid.
Hydrocyanic Acid,	1-500	Abundant life.	No.	No.	F. P.
Perchloride of Formyle (Chloroform),	1-25	Life.	No.	No.	None.
Acetrate of Antimony,	1-360	Abundant life.	No.	Abundant.	Putrid.
Biborate of Soda,	1-25	Abundant life.	No.	Abundant.	Very putrid.

* The headings of the various divisions refer exclusively to the results tabulated in the third and fourth columns.

† F. P. denotes faint putrescence.

Note.—In the above table, with two exceptions—nitrate of silver and permanganate of potassium—life is always accompanied by putrefaction, though the converse is not the case—another proof that germs are not the cause of putrefaction.

15 MAR 73

APPENDIX No. .

THE RELATIVE POWER OF SOME REPUTED ANTISEPTIC AGENTS.

By ~~Staff~~ Surgeon-Major O'Neial

THE following experiments were made at Dublin, Athlone, and Netley, for the purpose of ascertaining the power of certain reputed antiseptic agents.

To effect this purpose with accuracy, it became necessary to select some test fluid, which could be always obtained in the same condition, and containing the same amount of organic matter. In fact, a fixed standard, by which the power of each antiseptic might be tested, and its value, in relation to other antiseptics, accurately ascertained, by reference to a common standard, as the relative value of articles of commerce is ascertained by referring them all to the common standard money.

At first sight, sewage appeared the most obvious thing to use as a test, being itself that to which antiseptics are most commonly applied. But, as the experiments would necessarily be extensive, and would require to be frequently repeated, it would be obviously impossible to have all, or indeed any two of the specimens of sewage, used in exactly the same condition, and any difference whatever would lessen that accuracy, which has been a primary object in these experiments. If, for example, the particular specimen of sewage used for testing carbolic acid, happened to contain a larger proportional amount of organic matter than that used for chloralum, the latter would be allowed a considerable advantage over the former, in estimating their respective antiseptic powers.

A perfectly fresh infusion of beef was selected as a test fluid, easily managed, and which could be obtained, at all times, of the same strength of organic matter.

The method of conducting the experiments was as follows:—

An infusion was made of lean beef, in warm distilled water. This was cooled, and when cold filtered, the object being to get rid of the fat, the presence of which might interfere with the uniformity of the fluid. A few globules of fat more in one vessel than another, giving a larger proportion of organic matter, would of course affect results; of this filtered infusion, a quantity was measured accurately from a burette into a platinum vessel, was evaporated, weighed, incinerated and weighed, and the amount of decomposable organic matter it contained thus precisely ascertained.

It was arranged so that the infusion should be always strong enough to bear dilution, and its amount of organic matter being ascertained, as above detailed, a simple calculation gave the quantity of distilled water necessary to be added to each separate infusion, in order to dilute them all down to the one uniform standard of 0·5 grammes of organic matter to 100 c.c. of water.

A quantity of the agent to be experimented on was then weighed, dissolved in an accurately-measured quantity of distilled water, and passed from a burette, in the necessary quantities, into a series of clean, wide-mouthed glass jars. To each of these were added such quantities of the beef infusion as were necessary to give the proportions detailed in Table A.

Where the agent was not sufficiently soluble in water, the quantities required were separately weighed, made into a paste, with a small quantity of distilled water, and then thoroughly mixed with the beef infusion.

One of the jars contained beef infusion alone, as a standard of comparison.

The whole were then placed in a press in the laboratory, freely exposed to air, and to a moderate light; but not at all to the direct action of the sun.

All the antiseptics experimented upon were thus subjected to exactly the same conditions, with the exception of temperature.

A self-registering thermometer was, however, left in the press, and the results of its readings given in Table A.

The contents of each glass were examined daily by a microscope, with a power of one-eighth the observations noted, and the results condensed in Table A.

A preliminary experiment was made with the following agents, namely:—

1. Thymol.
2. Zylol.
3. Chloride of calcium.
4. Sodium chlorate.
5. Potassium chlorate.
6. Sulphate of zinc.
7. ~~Magnesia~~ chloride.
8. Calcium bi-sulphite.
9. Chloralum.
10. Chloride of aluminium.
11. Chloride of zinc.
12. Permanganate of potassium.
13. Sulphate of copper.
15. Bi-sulphite of sodium.
16. Carbolic acid.
17. Bi-chromate of ~~potash~~.

The result of this experiment was to show that the first eight had little or no power, and they were therefore dismissed from further consideration, after the correctness of this conclusion had been confirmed by repetition of the experiment.

The two first agents, thymol and zylol, were tried because they were at the ~~same~~ time used in Dublin, in the treatment of small-pox. They had the reputation obtained, I believe, from Germany, of being powerful remedial agents in this disease through exercising an antiseptic influence on the blood. Their use was, however, shortly discontinued, as they were found to be of no practical value.

During the course of the observations it became manifest that the progress of decomposition might, for the consideration of the comparative practical value of the several agents, be conveniently divided into four stages, namely:—

1. From the commencement of the experiment, to the observance of the first animalcule in independent motion.
2. From this point to the existence of bacteria, &c., in abundance.
3. From the latter point to the first perception of a putrescent smell.
4. To the point when the specimen became so offensive as to require to be thrown away. It is not pretended that these stages are distinctly marked; on the contrary, the decomposing fluid passes gradually, and more or less slowly, from one to another. They are, however, useful in considering the value of antiseptic agents in the different objects for which they are used,—medical, surgical, and sanitary.

In medicine they are administered internally, with the object of exercising, after absorption, into the circulation, an antiseptic influence on the blood as in the case of thymol and zylol for small-pox, and sulphites by Professor Polli in various diseases. But these experiments show, that even in the proportions of equal parts of the agent to organic matter, they do not altogether prevent the development of animalculæ, and cause one to doubt the possibility of introducing into the system an amount of any antiseptic sufficiently large to bear to the organic matter of the blood, such a proportion as would exercise any material influence on disease germs contained in it.

A different conclusion may be drawn when the agent is used externally.

In the case of carbolic acid, for example, animalculæ were not abundant before the ninth day in the experiment in which it was used in the proportion

of one to twelve. When employed as a lotion to an ulcer or wound, the decomposable matter against which it would have to contend would be the organic matter contained in the secretion on the surface of the ulcer, and however weak the lotion might be, it is probable that the amount of carbolic acid actually applied would be much greater than that of the organic matter.

These experiments, therefore, whilst showing that carbolic acid is not a very useful agent for sanitary purposes, do not at all affect its character as a direct local application in surgical practice.

Intermediate between these is the case in which an agent is administered internally, with the object of producing a direct local action.

It will be seen from Table A, that sulphate of copper, when used in the proportion of one to twelve retards decomposition so that animalculæ do not become abundant before the fifth day, and smell is not perceived before the twenty-first day. Probably the beneficial influence of this agent in the diarrhœa of typhoid fever is not confined merely to its action as an astringent; but is due also to its antiseptic qualities. Acting as a direct preventative of decomposition in the contents of the bowel, it removes, or lessens the amount of septic matter liable to be absorbed by the lower portion of the canal.

It is, however, from a sanitary point of view, that the action of the agents experimented upon are under consideration here.

It appears practically impossible to employ any of the agents for the treatment of sewage in such proportions as would altogether prevent the development of animalculæ, and it becomes necessary to consider their relative value from the points just short of the second and third stages in Table A: that is, the proportions in which they would prevent the development of animalculæ in abundance for a given period, say ten days; and again in which they would prevent putrescent smell for the same period.

In all the experiments, animalculæ having distinct (however feeble) motion were observed to be present on the second day. This was distinctly ascertained, by repeated trials, to be the case even with the most powerful of the agents, bi-chromate of ~~potash~~ Potassium.

Chloralum appears to be an agent of little power, as when employed even in so large a proportion as one part to two of organic matter, animalculæ became abundant on the fifth day, and putrescent smell commenced on the seventh. Its price is low, but this does not appear to compensate for its want of power.

Chloride of Aluminium.—In the experiment with this agent, animalculæ became abundant, before the tenth day, in those containing six parts of organic matter, and putrescent smell in those containing eight parts. In the proportions of one, two, and four, fungi were developed in great abundance, and there was a musty but not offensive odour. The agent is expensive, and of no practical value.

Chloride of Zinc.—With this agent animalculæ became abundant on the sixth day, in the proportion of one to two of organic matter, and putrescent smell was observed on the thirteenth day, in the proportion of one to four. In lower proportions fungi were abundant. In addition to its want of power, this agent is costly.

Permanganate of Potash appears from Table A to have considerable power as a deodorant, but hardly any as a germicide. The experiment lasted a month, and, though no smell was observed in those from fourteen parts of organic matter downwards, they literally teemed with bacteria vibriones, &c., after the eighth day, even in the experiments containing equal parts of the agent and organic matter.

Sulphate of Copper was under observation for six weeks. Animalculæ became abundant on the sixteenth day in the experiment containing four, and smell was noted on the twenty-first day in that containing twelve parts of organic matter. Those containing a lesser amount remained free from smell throughout, but fungi were developed abundantly in all, except that containing equal parts, which remained free from smell and fungi, showing only a very few animalculæ in feeble motion.

Bi-sulphite of Soda is of no value. In the experiment containing four parts of organic matter, animalculæ were abundant on the fourth day, and putrescent smell was observed on the tenth. In an equally short time fungi became

abundant in the lower proportions, and the higher ones ran rapidly into putrescence.

Carbolic Acid (Calvert's No. 2).—In the experiment containing eight parts of organic matter, animalculæ became abundant on the tenth day, and in that containing six parts not till the twenty-third. Putrescent smell was observed on the twelfth day in that containing twelve parts. There was a very slight development of fungus in the one containing six parts, but in none of the others. Those containing four parts and less of organic matter showed, throughout the experiments, animalculæ in distinct and tolerably active motion, but they never became abundant. No fungi were formed, and no putrescent smell was developed.

Bi-chromate of Potash.—In the first series of experiments this agent was tried up to thirty parts of organic matter, and was under observation for thirty-nine days. The agent having resisted up to this point, a second series, in higher proportions, was commenced, and was still going on when this paper was written on the twenty-fourth day. As with all the other agents, animalculæ having distinct independent motion were observed on the second day. In the first series these were few and their motion feeble, requiring occasionally several examinations before being satisfactorily made out; but in the higher proportions, in the second series, they became gradually more numerous, and, in the proportion of one to one hundred and fifty, reached the point of abundance on the eleventh day, and developed slight smell on the twenty-first day. In the experiments containing six parts and less of organic matter there was no fungus. From this proportion upwards it gradually increased, and became abundant at an early date in the higher ones.

This is the most powerful of all the agents tried. It appears quite equal to carbolic acid in preventing the development of fungi, and is much superior to it and all the others in limiting (none of them prevent altogether) the development of animalculæ. It is also by far the most powerful as a deodorant.

TABLE A.

	Agent ..... 1 Organic Matter 1				Agent ..... 1 Organic Matter 2				Agent ..... 1 Organic Matter 4				Agent ..... 1 Organic Matter 6				Agent ..... 1 Organic Matter 8				Agent ..... 1 Organic Matter 10				Agent ..... 1 Organic Matter 12			
	Animalcule.		Smell.		Animalcule.		Smell.		Animalcule.		Smell.		Animalcule.		Smell.		Animalcule.		Smell.		Animalcule.		Smell.		Animalcule.		Smell.	
	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.
Chlorolum	2	...	...	...	2	5	7	20	2	5	5	11	2	3	6	9	2	3	6	9	2	3	5	8	2	3	5	8
Chloride of Aluminium	2	...	...	...	2	31	...	...	2	12	23	...	2	5	13	44	2	4	7	18	2	4	7	16	2	4	7	15
Chloride of Zinc	2	...	...	...	2	6	...	...	2	4	13	...	2	4	7	...	2	3	7	...	2	3	7	20	2	3	7	26
Permanganate of Potash	2	8	...	...	2	8	...	...	2	8	...	...	2	7	...	...	2	7	...	...	2	7	...	...	2	7	...	...
Sulphate of Copper	2	...	...	...	2	...	...	...	2	16	...	...	2	16	...	...	2	8	...	...	2	5	...	...	2	5	12	...
Bi-sulphite of Soda	2	...	...	...	2	9	...	...	2	4	10	...	2	3	6	...	2	3	6	...	2	3	6	...	2	3	5	11
Carbolic Acid (Calvert's No. 1) ...	2	...	...	...	2	...	...	...	2	...	...	...	2	23	...	...	2	10	...	...	2	9	...	...	2	9	12	17
Bi-chromate of Potash	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...

Potassium

	Agent ..... 1 Organic Matter 60				Agent ..... 1 Organic Matter 120				Agent ..... 1 Organic Matter 150			
Potassium												
Bi-chromate of Potash	2	...	...	...	2	...	...	...	2	11	21	...

× To be moved to page 101 and to have a separate heading, (Table A 2nd Series) so as to correspond with para last but one —

TABLE A—*continued.*

	Agent ..... 1 Organic Matter 14				Agent ..... 1 Organic Matter 16				Agent ..... 1 Organic Matter 20				Agent ..... 1 Organic Matter 24				Agent ..... 1 Organic Matter 30				Beef Infusions 0·5 grains to 100 c.c.				Temperature.			
	Animalculæ.		Smell.		Animalculæ.		Smell.		Animalculæ.		Smell.		Animalculæ.		Smell.		Animalculæ.		Smell.		Animalculæ.		Smell.					
	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Presence.	Abundance.	Slight.	Offensive.	Highest Maximum.	Lowest Maximum.	Highest Minimum.	Lowest Minimum.
Chloridum	2	3	5	8	2	3	5	8	2	3	5	8	2	3	5	8	2	3	5	8	2	3	5	8	69	53	58	40
Chloride of Aluminium	2	4	7	15	2	4	6	14	2	4	6	12	2	4	5	12	2	3	5	12	2	3	4	12	61	51	53	44
Chloride of Zinc	2	3	7	26	2	3	6	24	2	3	6	23	2	3	6	20	2	3	6	17	2	3	5	8	69	53	58	40
Permanganate of Potash	2	6	...	...	2	6	17	...	2	5	6	13	2	4	5	14	2	3	4	14	2	3	4	11	61	51	53	44
Sulphate of Copper	2	3	15	...	2	3	9	...	2	3	9	...	2	3	7	21	2	3	6	21	2	3	4	22	69	53	58	40
Bi-sulphite of Soda	2	3	5	11	2	3	5	11	2	3	5	11	2	3	5	11	2	3	5	11	2	3	4	13	61	51	53	44
Carbolic Acid (Calvert's No. 2) ...	2	5	11	16	2	4	11	16	2	4	11	15	2	4	10	15	2	4	10	14	2	3	5	11	60	56	55	46
Bi-chromate of Potash	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...	2	...	...	...	2	3	6	13	60	51	55	46

The word "animalculæ" is used as a convenient term for organisms having independent motion and includes "Microzymes," "Bacteria," "Vibriones," &c.

Potassium

having

52

Dr Parkes
with the Author's
Compliments

ON PUTREFIERS AND ANTISEPTICS.

BY

JOHN DOUGALL, M.D.,

MEDICAL OFFICER OF HEALTH FOR THE BURGH OF KINNING PARK, GLASGOW.

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ON PUTREFIERS AND ANTISEPTICS.

THE following experiments were made to ascertain—I. Whether putrefaction can be accelerated by adding certain chemicals to fresh organic fluids. II. The relative antiseptic powers of different bodies, as evinced by their preventing the appearance of fungi and animalcules in organic fluids with which they are mixed. III. The relative aerial antiseptic powers of different volatile bodies, as evinced by their preventing the appearance of fungi and animalcules in organic fluids exposed to their vapours, and by their action on vaccine lymph.

I.—WHETHER PUTREFACTION CAN BE ACCELERATED BY ADDING CERTAIN CHEMICALS TO FRESH ORGANIC FLUIDS.

During fully two years' assiduous experimentation to determine the antiseptic powers of different bodies, various phenomena of putrefaction—as the differentiation, number, form, habits, &c., of the organisms in putrefying fluids, with several other occult metamorphoses—were of necessity frequently observed. These, however, though of absorbing interest, were not specially noted, because not directly contributing to the object of research. Furthermore, it frequently appeared that several of the chemical bodies tested seemed rather to favour than retard putrefaction, these having either a neutral or alkaline reaction, except spirits of nitric ether and magnesic sulphate. Subsequently, it was

resolved to ascertain, by experiment, whether the substances alluded to, including a few others having the same reactions, were in reality promoters of putrefaction.

I.

Three and a-half drams distilled water, containing one in 500 of one of the following substances, also half a dram of a filtered solution of beef juice, sp. gr. 1·4, were put in glass tubes which had been previously rinsed with fuming nitric acid, and afterwards with distilled water. Four drams of beef juice and water, without any chemical added (simple solution), were also set aside for comparison. Temperature ranging from 58° to 63° Fah.

Soda and Salts.

*Soda, ...	Solution.	Alkaline.
Bi-borate of Sodium, ...	Do.	Neutral.
Sulphate of do., ...	Do.	Do.
Hyposulphite of do., ...	Do.	Do.
Chloride of do., ...	Do.	Do.
Stearate of do. (Hard Soap), ...	Do.	Do.

Potash and Salts.

*Potash, ...	Solution.	Alkaline.
*Nitrate of Potassium, ...	Do.	Neutral.
*Chlorate of Potassium, ...	Do.	Do.
Permanganate of do., ...	Do.	Do.
Condy's Fluid (Permanganate of Potassium), ...	Do.	Do.

Ammonia and Salts.

Ammonia, ...	Solution.	Alkaline.
Bromide of Ammonium, ...	Do.	Neutral.
Chloride of do., ...	Do.	Do.

Various.

Pepsine, ...	Solution.	Neutral.
*Lime, ...	Do.	Alkaline.
Spirits of Nitric Ether, ...	Do.	Faintly acid.
Acetone, ...	Do.	Neutral.
Acetate of Morphia, ...	Do.	Do.
Sugar, ...	Do.	Do.
Sulphate of Magnesium, ...	Do.	Faintly acid.
Charcoal, ...	Do.	Neutral.

In all these mixtures, excepting that with ammonia, permanganate of potassium, and the simple solution, life was observed on the second day. On the third day life was present in the permanganate and in the simple solution, and on the fourth day in the ammonia solution.

II.

Filled same tubes with equal parts of a solution consisting of one part of the fore-mentioned bodies in 500 parts of water and solution of beef juice, sp. gr. 1·4. Filled also one tube with equal parts of beef juice and water (simple solution); temperature, 58° to 64° Fah. Life appeared in all the mixtures in twenty-four hours, except in the permanganate and simple solutions. In the latter it was very sparse in thirty-six hours, but without bad odour, while it did not appear in the former till the lapse of sixty hours. The solutions of the bodies marked with a star were very putrid in thirty-six hours.

III.

Filled same tubes with equal parts of a solution consisting of one part of the fore-mentioned bodies in 500 parts of water, and infusion of hay. Filled also one tube with equal parts of water and hay infusion (simple solution). In twenty hours life was present in the soap solution, and in all the others in thirty hours, except the permanganate, ammonia, simple, and bi-borate of sodium solutions, in which it appeared in forty hours.

IV.

To seven parts of a saturated aqueous solution of sulphuretted hydrogen was added one part of a solution of beef juice; mixture strongly acid. No life till fourth day; mixture neutral on fifth day, and odour extremely putrid. Equal parts of a saturated aqueous solution of sulphuretted hydrogen and solution of beef juice were mixed. Life was present in ten hours, but putrefaction progressed very languidly, judging from the continued faintness of odour.

Hence it appears that an excess of sulphuretted hydrogen producing acidity first antisepts, but, when the reaction becomes neutral, intensifies putrefaction; while conversely a weak solution of the gas hastens the commencement of putrefaction, but subsequently renders that process more protracted.

The results of these experiments show that the substances enumerated above, with the narrow exception of permanganate of potassium, ammonia, and bi-borate of sodium, hasten decomposition when mixed in small portions with fresh putrescent or fermentable fluids. Moreover, it is seen that ammonia is a putrescent in Experiment II., and biborate of sodium in Experiments I. and III., the only permanent exception being permanganate of potassium, which, in the circumstances, is slightly antiseptic.

It is obviously probable that all these bodies have not equal putrefying energy; yet putrefaction ensues so rapidly after they are mixed with the organic fluids that it would be manifestly difficult to determine their relative putrefying powers, assuming the time when microzymes appear in the individual mixtures as a measure of such powers. Though this, however, were correctly ascertained, it would only exhibit the different tendencies of the various bodies in making a fresh substance putrid, without showing how much or how little each had intensified the subsequent phenomena of putrefaction. This could only be accurately known—1st, by noting the time of the disappearance of organisms from the separate mixtures; 2nd, by the cessation of the bad odour; 3rd, by the fluid becoming comparatively clear. It has also been stated that when an albuminous solution has ceased to putrefy, it gives no precipitate with nitric acid and heat. I have found, however, a precipitate still obtainable from such a fluid, after organisms, putrid odour, and cloudiness had apparently disappeared. It must be remembered, nevertheless, that I refer only to fluids whose decomposition has been characterized by the presence of vibrios, bacteria, putrid odour, and cloudiness—true putrefaction. On the other hand, where the fluid has fermented, known by the

presence of torulæ, penicillia, faint mouldy aroma, and transparency, no precipitate whatever can be obtained, and that even when the products of fermentation are abundantly present. Indeed, I have found, by comparison, that fermentation is a much more active and effective process than putrefaction, and that the former process frequently changes into the latter, while the converse seldom takes place, and only partially. In the foregoing experiments these data could not be used on which to base the results, as my object was not to determine relative but actual putrefying power—the analyses were *qualitative* not *quantitative*, and this having been ascertained in the affirmative, clears the way for future investigations into relative putrefying powers. Of course I do not aver, as hitherto unknown, that certain bodies hastened decomposition of organic matter. All admit that oxidizers act thus, *qua* their oxygen, while Dr Gregory states* that Eremacausis (slow oxidation) is greatly assisted by the presence of alkalies, but it will be conceded that far less is known of putrescents than of antiseptics, and that more detailed knowledge regarding this action of such bodies is not only highly desirable, but, in a sanitary point of view, quite indispensable. Although I have not yet experimented to determine the relative putrefying powers of the substances tabulated, still, as those marked * seemed specially vigorous at the outset, it may be reasonably inferred they will continue so throughout, and hence, in the meantime, should be considered as the most active in the group.

The results of the experiments further indicate that the alkalies and alkaline earths, and their alkaline and neutral salts, with a few exceptions, hasten decomposition when present in small proportion, in fluids containing organic matter. Indeed, from observing the reaction of the various substances in the fourth division, it may be safely predicated that the great majority of bodies soluble in, or miscible with, water, and having a neutral, alkaline, or faintly acid reaction, induce putrefaction, *not fermentation*, in favourable circumstances. This point merits the attention of sanitarians,

* Organic Chemistry, page 30, ed. 1852.

and practically means that all domestic soap-suds, spent leys from soap works, bleach works, paper works, &c., liquors accumulated at the base of the waste heaps of chemical works where soda-ash is manufactured, being generally more or less alkaline, hasten putrefaction of organic matter in sewage, if allowed to mix with it. We may get a hint from nature here. Healthy human urine and fæcal matter are acid; but when they putrefy, either in or out of the body, are alkaline.

The *modus operandi* of these putrefiers I have been unable to solve satisfactorily, and an attempted explanation of which leads into the labyrinths of search after final causes, ending in the regions of hypothesis. Certainly they are not oxidizers, Condy's fluid, carbon, and probably chlorate of potassium, excepted, though they seem to favour oxidation, perhaps in the majority, by their softening and disintegrating influence, thus causing a greater surface to be presented to the atmosphere. By the same action they may weaken the cohesive power of the organic molecules, thus facilitating their chemical transformation into simpler compounds, which is held by many to take place without the intervention of oxygen.

It will be observed in the table that saltpetre, common salt, and sugar, substances which preserve meat when used in large quantities, act as putrefiers when used in small quantities. The results of the action of permanganate of potassium are similar; and I believe Dr B. W. Richardson has shown the same of ammonia.

II.—The relative antiseptic powers of different bodies, as evinced by their preventing the appearance of fungi and animalcules in organic fluids with which they are mixed.

At the meeting of the British Association, Edinburgh, 1871, I read a paper "On the Relative Powers of Various Substances in Preventing Animalcules in Solutions of Beef Juice, Infusion of Hay, and in Urine." The experiments there detailed were undertaken for the purpose of determining, as far as practicable, the relative preservative powers

of a number of well-known and alleged antiseptics. The average amount of a substance necessary to prevent the appearance of animalcules in a given time, in the three fluids, being assumed as its antiseptic power. The appearance of animalcules as a proof of putrefaction is universally admitted; as revealed by the microscope, whether in great or small numbers, these organisms are not only indubitable evidence of that process, though the medium in which they are seen be apparently clear and untainted, as when putrefaction is incipient or expended, but also its first, last, surest, and most easily demonstrated manifestation. They are only present in fluids containing organic matter, and, moreover, only when that matter is in a state of putrefaction, hence their presence indicates the presence of putrefying matter, and their absence, either the absence of that matter, or that it is not putrefying. The faintest degree of decomposition seems sufficient, by some obscure correlation with their nature, to endow them with existence—generating or developing them—enduing dead matter with life, or kindling the vital forces lying dormant in their germs. Hence, some consider them the cause of putrefaction, others the result; while some hold their function in nature to be malignant, and others to be benign. Without, however, entering into these questions, it was enough for my purpose that they afforded sure data for classifying antiseptic powers.

In the paper alluded to, no special notice was taken of fungi, which are frequently present in such fluids as those examined, and, as many biologists hold these to be as intimately associated with the decomposition of organic matter as mycrozymes are, I subsequently resolved to repeat the experiments with the more decided antiseptics, and ascertain their preventive power on fungi, as well as on animalcules, adding a few substances not previously included, besides varying the mode of procedure, so as to test the correctness of previous results. The experiments were carried on from the beginning of April till the end of October. Those who have an opportunity of comparing the results in the

following tables with those in the paper referred to, will find them to coincide remarkably in the great majority of instances considering the dissimilarity of the data on which they are founded.

I.—*Beef Juice.*

Three and a half drams distilled water, containing 1 in 500 of one of the following substances, also, half a dram filtered solution of beef juice, sp. gr. 1·4, were put in glass tubes previously rinsed with fuming nitric acid, and afterwards with distilled water. These were kept at a temperature varying from 58° to 65° Fah. for 100 days, during which the mixtures were frequently examined with a magnifying power of 700 diameters. The tubes containing volatile substances were closed, the others were left open. The following table shows the results. The blank spaces indicate that no microzymes or fungi were present in 100 days. *Pen.* denotes Penicillia; *Tor.*, Torulæ; *B. P.*, British Pharmacopœia.

Name of Substance.	Reaction of fluid before Beef Juice was added.	Days when Animalcules were seen.	Reaction when Animalcules were seen.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when fluid colour was lost.	Remarks.
Liquor of the Perchloride of Iron (B. P.)	Acid.	42	Neutral.	4 Pen.	Acid.	10	
Chromic Acid	Do.	78	Do.	38 Pen.	Do.	74	
Boracic Acid	Neutral.	2	Do.	...	...	3	
Citric Acid	Acid.	13	Do.	3 Tor.	Acid.	14	The mixture had a disagreeable smell of sewage on the eighth day.
Baric Chloride	Neutral.	1	Do.	85 Tor.	Neutral.	2	
Equal parts of Quinine and Sulphuric Acid	Acid.	23	Do.	3 Tor.	Acid.	10	
Cupric Sulphate	Do.	85	Acid.	20 Pen.	Do.	89	
Mercuric Chloride	Do.	...	...	...	...	...	No change in 176 days.
Ferrous Sulphate	Do.	14	Neutral.	5 Tor.	Acid.	14	
Arsenious Acid, with a trace of Hydrochloric	Do.	2	Do.	...	...	3	
Potassio-ferric Sulphate (Iron Alum)	Do.	24	Do.	4 Pen.	Acid.	29	

Name of Substance.	Reaction of fluid before Beef Juice was added.	Days when Animalcules were seen.	Reaction when Animalcules were seen.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Plumbic Acetate,	Acid.	28	Neutral.	5 Pen.	Acid.	20	
Tartarized Antimony,	Do.	4	Do.	2 Pen.	Do.	4	
Zinc Sulphate,	Do	30	Do.	4 Pen.	Do.	35	
Argentic Nitrate,	Neutral.	...	...	...	..	...	No change in 176 days.
Benzoic Acid,	Acid.	...	...	...	...	...	No change in 176 days.
Hydrocyanic Acid (B. P.),	Neutral.	22	Neutral.	21 Tor.	Neutral.	24	
Picric Acid,	Acid.	44	Do.	11 Pen.	Acid.	25	
Oxalic Acid,	Do.	28	Do.	4 Pen.	Do.	12	
Carbolic Acid,	Neutral.	12	Do.	50 Tor.	Neutral.	12 sourish	
Aluminic Chloride,	Acid.	19	Acid.	4 Pen.	Acid.	10	
Alum,	Do.	14	Neutral.	3 Pen.	Do.	16	
Chloride of Lime,	Alkaline.	27	Acid.	27 Tor.	Do.	29	
Chloralum	Acid.	10	Neutral.	3 Tor. & Pen.	Do.	10	
Bisulphite of Calcium,	Do.	4	Do.	92 Tor.	Neutral.	3	

Name of Substance.	Reaction of fluid before Beef Juice was added.	Days when Animalcules were seen.	Reaction when Animalcules were seen.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Nitric Acid,	Acid.	18	Neutral.	4 Pen.	Acid.	16	
Sulphuric Acid,	Do.	...	...	5 Pen.	Do.	5 musty	
Hydrochloric Acid,	Do.	28	Neutral.	4 Pen.	Do.	10	
Nitro-Hydrochloric Acid,	Do.	10	Do.	3 Pen.	Do.	10	
Sulphurous Acid,	Do.	24	Do.	4 Pen.	Do.	20	
Alcohol,	Neutral.	4	Do.	4 Tor.	Neutral.	4	
Tincture of Iodine B. P.,	Do.	1	Do.	80 Tor.	Do.	2	
Strychnia, with a trace of Hydrochloric Acid,	Acid.	7	Do.	4 Pen.	Acid.	9	
Sulphate of Beberia,	Neutral.	4	Do.	2 Tor.	Neutral.	4	
Potassic Dichromate,	Acid.	90	Do.	40 Pen.	Acid.	60	
Quinine (pure),	Neutral.	1	Do.	...	...	2	
Zinc Chloride,	Acid.	4	Acid.	...	...	4	
Simple Solution,	Neutral.	1	Neutral.	74 Tor.	Neutral.	2	

II. *Egg-Albumen.*—Three and a half drams of distilled water, containing 1 in 500 of one of the following substances, were put in glass tubes previously rinsed with fuming nitric acid, and afterwards with distilled water. To each tube was then added half a dram of a solution consisting of one part of white of egg to four parts of water. They were kept at a temperature varying from 55° to 66° Fahrenheit for 100 days, during which their contents were frequently examined with a magnifying power of 700 diameters. The following are the results. The blank spaces indicate that no microzymes or fungi were observed in 100 days:—

Name of Substance.	Reaction of fluid before Albumen was added.	Effect on Albumen when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Liquor of the Perchloride of Iron (B.P.),	Acid.	Slight coagulation.	7	Neutral.	...	...	7	
Chromic Acid,	Do.	Dense coagulum.	...	...	...	...	...	No change in 182 days.
Boracic Acid,	Neutral.	Hazy.	10	Neutral.	...	...	10	
Citric Acid,	Acid.	Nil.	10	Acid.	4 Tor.	Acid.	10	
Picric Acid,	Do.	Dense coagulum.	...	...	44 Pen.	Neutral.	53	
Carbolic Acid,	Neutral.	Hazy.	38	Neutral.	36 Tor.	Do.	38 Sour.	
Nitric Acid,	Acid.	Nil.	15	Do.	5 Pen.	Acid.	11	
Hydrochloric Acid,	Do.	Faint haze.	9	Do.	...	...	9	
Nitro-Hydrochloric Acid,	Do.	Nil.	41	Do.	14 Tor. & Pen.	Do.	14	
Baric Chloride	Neutral.	Do.	30	Do.	15 Tor.	Neutral.	10	
Equal parts of Quinine and Sulphuric Acid,	Acid.	Faint haze.	8	Do.	...	...	10	

Name of Substance.	Reaction of fluid before Albumen was added.	Effect on Albumen when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Cupric Sulphate,	Acid.	Dense coagula.	...	...	...	...	...	No change in 182 days.
Mercuric Chloride,	Do.	Do.	...	...	...	...	...	No change in 182 days.
Ferrous Sulphate,	Do.	Red haze.	35	Neutral.	40 Tor.	Neutral.	35	Red haze caused by the peroxidation of the Iron.
Arsenious Acid, with a trace of Hydrochloric,	Acid.	Nil.	33	Do.	5 Pen.	Acid.	13	
Potassio-Ferric Sulphate (Iron Alum),	Do.	Dense red coagulum.	46	Do.	...	...	50	
Plumbic Acetate,	Do.	Considerable coagula.	90	Do.	75 Pen.	Neutral.	78	
Tartarized Antimony,	Do.	Hazy.	43	Do.	9 Tor	Acid.	13	
Zinc Chloride,	Do.	Dense coagulum.	18	Acid.	...	...	16	
Zinc Sulphate,	Do.	Do.	90	Do.	70 Pen.	Do.	90	
Argentic Nitrate,	Do.	Do.	...	...	...	...	...	No change in 182 days.
Benzoic Acid,	Do.	Nil.	...	...	...	...	...	No change in 182 days.
Hydrocyanic Acid, (B.P.)	Neutral.	Do.	15	Neutral.	...	...	15	
Oxalic Acid,	Acid.	Do.	40	Do.	5 Pen.	Acid.	12	
Aluminic Chloride,	Do.	Do.	...	...	8 Pen.	Do.	13	Very exuberant development of fungi.
Alum,	Do.	Slight coagula.	38	Neutral.	15 Tor.	Do.	36	

Name of Substance.	Reaction of fluid before Albumen was added.	Effect on Albumen when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Chloride of Lime,	Alkaline.	Transparent elastic coagulum.	40	Neutral.	...	...	40	
Chloralum,	Acid.	Dense coagulum.	9	Do.	...	...	9	Very fœtid.
Bisulphite of Calcium,	Do.	Nil.	9	Do.	...	...	10	
Sulphuric Acid,	Do.	Do.	30	Do.	10 Tor.	Acid.	10	
Sulphurous Acid,	Do.	Slight haze	8	Do.	...	...	10	
Alcohol, *	Neutral.	Nil.	10	Do.	...	...	12	
Tincture of Iodine,	Do.	Do.	15	Do.	...	...	17	
Strychnia, with a trace of Hydrochloric Acid,	Acid	Hazy.	15	Do.	...	...	30	
Sulphate of Beberia,	Neutral.	Dense coagula.	8	Do.	...	...	10	
Potassic dichromate,	Acid.	Slight coagula.	...	...	...	...	...	No change in 182 days.
Quinine (pure),	Neutral.	Nil.	4	Do.	...	...	16	In 60 days mixture clear, life gone, taste bitter, gives precipitate with HNO_3.
Simple Solution,	Do.	Do.	3	Do.	..	...	6	Still very putrid in 100 days.

* The protoplasmic membrane at top, and a deposit collected at the bottom of the tubes with alcohol and iodine were of a rich aniline hue. This has probably some connection with the spirit in these mixtures. Erdman (Jour. für Prakt. Chem., XCIX., p. 385), quoted by Mosler, has discovered that vibrios have the power of producing aniline colouring matter from protein substances. Parke's Prac. Hygiene, p. 239. Third Edition.

III. *Egg Albumen, Urine, and Beef Juice.*—Three and a half drams of distilled water, containing 1 in 500 of one of the following substances, were put in glass tubes, previously rinsed with fuming nitric acid, and afterwards with distilled water. To each tube was then added half a dram of a solution, consisting of equal portions of white of egg, fresh human urine, and solution of beef juice. They were kept at a temperature varying from 57° to 68° Fah. for 100 days, during which their contents were frequently examined with a magnifying power of 700 diameters. The following are the results. The blank spaces indicate that no microzymes or fungi were observed in 100 days :—

Name of Substance.	Reaction of fluid before Solution was added.	Effect on Solution when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Liquor of the Perchloride of Iron (B. P.),	Acid.	Nil.	10	Neutral.	2 Pen.	Acid.	10	
Chromic Acid,	Do.	Dense coagulum.	...	...	...	...	...	
Boracic Acid,	Neutral.	Nil.	6	Neutral.	2 Pen.	Neutral.	6	
Citric Acid,	Acid.	Do.	11	Acid.	2 Pen.	Acid.	17	Exuberant development of fungus tufts on 2nd day.
Picric Acid,	Do.	Dense coagulum.	53	Neutral.	4 Pen.	Acid.	53	
Carbolic Acid,	Neutral.	Slight haze.	30 myriads of organisms, motions languid.	Do.	...	...	...	On 30th day fluid smells strongly of carbolic acid. On 50th day minute life very sparse. Several large animalcules in active motion. Odour of the acid distinct. 70th day those conditions still exist. 100th day no change.

Name of Substance.	Reaction of fluid before Solution was added.	Effect on Solution when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Nitric Acid,	Acid.	Nil.	11	Neutral.	2 Pen.	Acid.	8	
Hydrochloric Acid,	Do.	Do.	4	Do.	2 Pen.	Do.	4	Very fœtid.
Nitro-Hydrochloric Acid,	Do.	Do.	43	Do.	2 Pen.	Do.	6	
Baric Chloride,	Neu l.	Hazy.	2	Do.	...	...	...	
Equal parts of Quinine and Sulphuric Acid,	Acid.	Nil.	50	Do.	2 Pen.	Do.	50	
Cupric Sulphate,	Do.	Dense coagula.	...	...	...	...	...	
Mercuric Chloride,	Do.	Do.	...	...	...	...	...	
Ferrous Sulphate,	Do.	Small dense coagula.	4	Acid.	...	...	4	
Arsenious Acid, with a trace of Hydrochloric,	Do.	Nil.	...	...	3 Pen.	Acid.	6	
Potassio-Ferric Sulphate (Iron Alum),	Do.	Hazy.	31	Alkaline	3 Pen.	Do.	16	About 20 isolated fungus tufts on 3rd day clinging to sides of tubes.
Plumbic Acetate,	Do.	Dense coagula.	60	Neutral.	11 Pen.	Do.	60	
Tartarized Antimony,	Do.	Hazy.	5	Faintly acid.	82 Tor. sparse.	Neutral	5	

18

Name of Substance.	Reaction of fluid before Solution was added.	Effect on Solution when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks.
Zinc Chloride,	Acid.	Small dense coagula.	10 sparse	Acid.	5 Pen.	Acid.	12	
Zinc Sulphate,	Do.	Do.	5	Faintly acid.	...	...	5	
Argentic Nitrate,	Neutral.	Dense opalescence.	8	Neutral.	Not determined.	Not determined.	8	At some period subsequent to the 8th day, this tube went amissing, hence the remark in the fungus column.
Benzoic Acid,	Acid.	Nil.	...	...	...	...	...	
Hydrocyanic Acid,	Neutral.	Slight coagula.	4	Neutral.	...	...	4	
Oxalic Acid,	Acid.	Nil.	40	Alkaline	2 Pen.	Acid.	16	
Aluminic Chloride,	Do.	Do.	80	Neutral.	2 Pen.	Do.	20	
Alum,	Do.	Do.	51	Do.	2 Pen.	Do.	55	
Chloride of Lime,	Alkaline.	Hazy.	35	Do.	24 Pen.	Neutral.	40	
Chloralum,	Acid.	Nil.	11	Do.	2 Pen.	Acid.	14	
Bisulphite of Calcium,	Do.	Hazy.	4	Do.	80 Tor. sparse.	Neutral.	4	
Sulphuric Acid,	Do.	Nil.	84	Do.	2 Pen.	Acid.	6	

19

Name of Substance.	Reaction of fluid before Solutions were added.	Effect on Solution when mixed with fluid.	Days when Animalcules were seen.	Reaction of mixture when life appeared.	Days when Fungi were seen.	Reaction when Fungi were seen.	Days when bad odour was felt.	Remarks
Sulphurous Acid,	Acid.	Slight haze.	4	Neutral.	...	...	4	
Alcohol,	Neutral.	Nil.	1	Do.	78 Fen. sparse.	Neutral.	1	
Tincture of Iodine,	Do.	Do.	1	Do.	...	...	1	
Strychnia, with a trace of Hydrochloric Acid,	Acid.	Do.	8	Do.	4 Pen.	Acid.	6	
Sulphate of Beberia,	Neutral.	Slight coagula.	7	Do.	5 Tor. sparse.	Neutral.	8	
Potassic Dichromate,	Acid.	Hazy.	...	...	63 Tor. sparse.	...	...	
Quinine (pure),	Neutral.	Nil.	4	Neutral.	42 Tor.	Alkaline	6	Animalcules in considerable numbers still present on 42nd day.
Simple Solution,	Do.	Do.	1	Do.	...	...	1	

The exact average relative antifermentive and antiputrefactive powers of the various bodies and groups cannot be correctly ascertained in consequence of the blank spaces in the tables, given on the next four pages, but assuming these to be filled in with 100 (days), the following approximative averages are obtained from the groups :—

Name of Group.	Antiputrefactive Power.	Antifermentive Power.	Total Antiseptic Power.
I. Metallic Salts,	46·6	64·1	110·7
II. Organic Acids,	42·1	43·6	85·7
III. Inorganic Acids,	35·6	37·7	73·3
IV. Salts of the Alkaline Earths,	29·6	41	70·6
V. Organic Salts,	12·7	44·4	57·1
VI. Tincture of Iodine and Alcohol,	5·3	76·5	81·8
Totals,	171·9	307·3	

Abstract of the preceding Tables.

Name of Substance.	Reaction of Simple Solution.	Days and reaction when Animalcules appeared in			Average Antipuirefactive power.	Days and reaction when Fungi appeared in			Average Antifermentive power.
		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.	
I. *Metallic Salts.*									
Mercuric Dichloride,	Acid.	...	...	...		...	...	...	
Argentic Nitrate,	Do.	...	...	8 Neutral.		...	...	Not determined	
Cupric Sulphate,	Do.	86 Acid.	...	...		20 Pen. Acid.	...	...	
Liquor of the Perchloride of Iron (B. P.),	Do.	42 Neutral.	7 Neutral.	10 Neutral.	19·6	4 Pen. do.	...	2 Pen. Acid.	
Plumbic Acetate,	Do.	28 do.	90 do.	60 do.	59·3	5 Pen. do.	75 Pen. Neutral.	11 Pen. do.	30·3
Zinc Chloride,	Do.	4 Acid.	18 Acid.	10 Acid.	10·6	...	...	5 Pen. do.	
Zinc Sulphate,	Do.	30 Neutral.	90 do.	5 do.	41·6	4 Pen. Acid.	70 Pen. Acid.	...	
Ferrous Sulphate,	Do.	14 do.	35 Neutral.	4 do.	17·6	5 Tor. do.	40 Tor. Neutral	...	
II. *Organic Acids.*									
Benzoic,	Do.	...	...	...		...	...	...	
Picric,	Do.	44 Neutral.	...	53 Neutral.		11 Pen. Acid.	44 Pen. Neutral.	4 Pen. Acid.	19·6

Name of Substance.	Reaction of Simple Solution.	Days and reaction when Animalcules appeared in			Average Antipuirefactive power.	Days and reaction when Fungi appeared in			Average Antifermentive power.
		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.	
Carbolic,	Neutral.	12 Neutral.	38 Neutral.	30 Neutral.	26·6	50 Tor. Neutral	96 Tor. do.	...	
Oxalic,	Acid.	28 do.	40 do.	40 Alkaline	36	4 Pen. Acid.	5 Pen. Acid.	2 Pen. Acid.	3·6
Hydrocyanic (B. P.),	Neutral.	22 do.	15 do.	4 Neutral.	13·6	21 Tor. Neutral	...	...	
Citric,	Acid.	15 do.	10 Acid.	11 Acid.	11·3	3 Tor. Acid.	4 Tor. Acid.	2 Pen. Acid.	3
III. *Inorganic Acids.*									
Chromic,	Do.	78 do.	...	...		38 Pen. Acid.	...	...	
Sulphuric,	Do.	...	30 Neutral.	84 Neutral.		5 Pen. do.	10 Tor. Acid.	2 Pen. Acid.	5·6
Hydrochloric,	Do.	28 Neutral.	9 do.	4 do.	13·6	4 Pen. do.	...	2 Pen. do.	
Nitro-Hydrochloric,	Do.	10 do.	41 do.	43 do.	30·3	3 Pen. do.	14 Tor. and Pen. Acid.	2 Pen. do.	6·3
Arsenious, with a trace of Hydrochloric,	Do.	2 do.	33 do.	...		...	5 Pen. Acid.	3 Pen. do.	
Sulphurous,	Do.	24 do.	8 do.	4 Neutral.	12	4 Pen. Acid.	...	...	
Boracic,	Neutral.	2 do.	10 do.	6 do.	6	...	...	2 Pen. Neut.	
Nitric,	Acid.	18 do.	15 do.	11 do.	14·6	4 Pen. Acid.	5 Pen. Acid.	2 Pen. Acid.	3·6

Name of Substance.	Reaction of Simple Solution.	Days and reaction when Animalcules appeared in			Average Antiputrefactive power.	Days and reaction when Fungi appeared in			Average Antifermentive power.
		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.	
IV. *Salts of the Alkaline Earths.*									
Aluminum Chloride,....	Acid.	19 Acid.	...	80 Neutral.		4 Pen. Acid.	8 Pen. Acid.	2 Pen. Acid.	4·6
Potassio-Ferric Sulphate (Iron Alum),	Do.	24 Neutral.	46 Neutral.	31 Alkaline	33.6	4 Pen. Acid.	...	3 Pen. Acid.	
Chloride of Lime,.....	Alkaline	27 Acid.	40 do.	35 Neutral.	34	27 Tor. Acid.	...	24 Pen. Neutral.	
Sulphate of Aluminum and Potassium (Alum), ...	Acid.	14 Neutral.	38 do.	51 do.	34·3	3 Pen. Acid.	15 Tor. Acid.	2 Pen. Acid.	6·6
Bisulphite of Lime, ...	Do.	4 do.	9 do.	4 do.	5·6	92 Tor. Neutral.	...	80 Tor. Neutral.	
Barium Chloride, ...	Neutral.	1 do.	30 do.	2 do.	11	85 Tor. Neutral.	15 Tor. Neutral.	...	
Chloralum, ...	Acid.	10 do.	9 do.	11 do.	10	3 Tor. and Pen. Acid.	...	2 Pen. Acid.	
V. *Organic Salts.*									
Equal parts of Quinine and Sulphuric Acid,	Do.	23 do.	8 do.	50 do.	27	3 Tor. Acid.	...	2 Pen. do.	
Quinine (pure), ...	Neutral.	1 do.	4 do.	4 do.	3	...	...	42 Tor. Alkaline.	

Name of Substance.	Reaction of Simple Solution.	Days and reaction when Animalcules appeared in			Average Antiputrefactive power.	Days and reaction when Fungi appeared in			Average Antifermentive power.
		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen		Beef Juice.	Egg Albumen.	Beef Juice, Urine, and Egg Albumen.	
Tartarized Antimony,...	Acid.	4 Neutral.	43 Neutral.	5 faintly ac.	17·3	2 Pen Acid.	9 Tor. Acid.	82 Tor. Neutral.	31
Strychnia, with a trace of Hydrochloric Acid, ...	Do.	7 do.	15 do.	8 Neutral.	10	4 Pen. do.	...	4 Pen. Acid.	
Beberia Sulphate, ...	Neutral.	4 do.	8 do.	7 do.	6·3	2 Tor. Neutral.	...	5 Tor. Neutral.	
VI. *Iodine and Alcohol.*									
Iodine, ...	Do.	1 do.	15 do.	1 do.	5·6	80 Tor. do.	...	...	
Alcohol,.....	Do.	4 do.	10 do.	1 do.	5	4 Tor. do.	...	78 Pen. Neutral.	
VII. *Salts of the Alkalies.*									
Potassic Dichromate,...	Acid.	90 do.	...	...		40 Pen. Acid.	...	63 Tor. do.	
Simple Solution, ...	Neutral.	1 do.	3 Neutral.	1 Neutral.	1·6	74 Tor. Neutral.	...	...	

N.B.—The blanks in the columns indicate that no putrefaction or fermentation occurred in 100 days.
The averages are calculated only where figures appear in the three columns.

These averages (*vide* table p. 21) are obtained by dividing the sum total of the individual averages (counting 100 days to each blank space) by the number of substances in the group. The salts of the alkalies are excluded, as only one of these—potassic dichromate—was experimented with. The groups are arranged in conformity with their antiputrefactive power—the strongest first and the weakest last. It is worthy of observation that though the figures in the antifermentive column are placed arbitrarily with reference to their group, but promiscuously as regards their power, yet when added to the corresponding figures in the antiputrefactive column, the result, *i.e.*, the total antiseptic powers, with the exception of the last figures, agrees with the method of arrangement in the first column. This is a decided indication that antiputrefactive, and not antifermentive power is the safest standard for estimating antiseptic power.

The results of the experiments prove—

1. *That Putrefaction and Fermentation are not identical processes, though apparently producing similar results.*—Putrefaction *per se* in a fluid is characterized, as already stated (*vide* putrefiers), by the presence of myriads of microzymes, putrid odour, haziness, neutral, alkaline, or faint acid reaction and slowness of change,* as compared with that produced by fermentation. Fermentation *per se* in a fluid consists in the presence of torulæ, tufts of mycelia, leptothrix filaments and other cryptogams, mouldy aroma, transparency of fluid, distinct acid reaction† and rapidity of change, as compared with that produced by putrefaction. These processes, however, though obviously different, apparently yield similar results. When consummated, the organic molecules, according to all observers, are broken down into simpler compound units: the microzymes, fetor, haziness, fungi, and mouldy aroma disappear, while the liquids hitherto responding to the tests for albumen cease to do so. Though these results, however, resemble

* I possess a quantity of beef juice prepared nine months ago, which is still quite putrid.

† Dr Dundas Thomson found that the air of three cholera wards had an acid reaction, and contained fungi without distinct evidence of the presence of vibriones; while air from a sewer was alkaline and contained vibriones.—"Air and Rain," by Dr R. A. Smith, pp. 482.

each other, perhaps were the residual fluids submitted to fractional distillation they would yield products characteristic of the change the solutions had undergone from which they were derived.

2. *Simple Solutions of Organic Matter, when they Decompose, Putrefy in the first instance.* *Vide* Simple Solutions and 5. This is so well known that to mention it may seem superfluous, were it not that it also expresses its antithesis.—Simple solutions of organic matter, when they decompose, do not ferment in the first instance. The proximate cause of this is the neutral reaction of the solution.

3. *Different Organic Solutions vary in the Time of their Decomposition.*—This is well illustrated in the following table, where the number of days and of blanks occurring in the animalculæ and fungi columns in the separate solutions are exhibited.

Beef Juice.				Beef Juice, Urine, and Egg Albumen.				Egg Albumen.			
Animalcules.		Fungi.		Animalcules.		Fungi.		Animalcules.		Fungi.	
Days.	Blanks	Days.	Blanks	Days.	Blanks	Days.	Blanks	Days.	Blanks	Days.	Blanks
721	4	612	6	678	6	428	12	767	8	355	23
1121		1212		1278		1628		1567		2655	

Assuming each blank to represent one hundred days, the above totals are obtained, showing that while the three solutions vary in the time in which they decompose, there is yet a remarkable gradation and harmony of results. For example, the total animalculæ days in beef juice is 1121; in beef juice, urine, and egg 1278; and in egg albumen 1567. In the same order we have the total fungi days: thus, 1212, 1628, 2655; or, if the totals be read as they occur in the table, it will be seen there is almost a consecutive increment from first to last. These figures prove that beef juice is the most, and egg albumen the least, prone to change of the three solutions.

4. *Putrefaction is more difficult to prevent than Fermentation.*—This is seen in the table of approximate averages where

putrefaction is found to ensue in 172, and fermentation in 307 days. Again, in the table appended to 3, in the antiputrefactive columns there are only 18 blanks, and in the antifermentive columns 41 blanks.

5. *The majority of Neutral and faintly Acid Solutions of Organic Matter, when they Decompose, Putrefy in the first instance.*—Numerous examples in the tables prove this. In the few exceptions fermentation has immediately or soon merged into putrefaction.

6. *Neutral Mixtures remain Neutral after Putrefying.*—This statement has only one exception—pure quinine in the third fungi column is alkaline.

7. *Fermentation and Putrefaction may proceed for a short time together, but almost never originate simultaneously.*—The only exception here is alcohol, where, in the beef juice columns, it will be seen animalcules and torulæ appeared on the same day (fourth).

8. *Fermentation very frequently subsides into Putrefaction, but Putrefaction is seldom intensified into Fermentation.*—Out of 65 instances in which fermentation and putrefaction occurred in the same solution, fermentation changed into putrefaction 55 times, and putrefaction into fermentation only 10 times. In the former the change took place early, generally in from two to ten days. In the latter, late, in from 50 to 90 days. In the ten instances torulæ were present only, and sparse, except in the alcohol solution, in which were penicillia only. Hence, when fungi appear in a fluid which has putrefied they are almost always torules.

9. *When Penicillia are found in a Solution of Organic Matter it is almost always Acid.*—In the 47 solutions in which penicillia occur, 42 are acid and only 5 neutral. Those in the acid solutions were greatly more exuberant than the others. This peculiarity characterizes the whole fungus tribe, its habitat being a soil of pure vegetable mould, such as decayed wood, which, according to Mulder, contains geic, humic, and ulmic acids. The juices also of fungi are acid. The boleti yielding oxalic, or, according to some, boletic acid, while other species, as the agarici, &c., contain fungic acid, some say also hydrocyanic acid. For some time past I have tested the reaction of a great many fungi, mucors included, by bruising them on litmus paper, and found it always reddened.

10. *Moderately Acid Solutions of Fresh Organic Matter generally first quickly Ferment and soon Putrefy.*—This is so in 50 of the 81 acid mixtures recorded. The exceptions have neither fermented nor putrefied, as mercuric dichloride, benzoic acid, &c., or putrefied only, as sulphurous and hydrochloric acids, or fermented after putrefying, as tartrate of antimony and bisulphite of lime, or fermented without putrefying, as sulphuric acid, picric acid, and potassic dichromate. Hence fermentation or putrefaction may be produced at will.

11. *Most Fresh Organic Fluids rendered Acid, becoming Putrid, become Neutral.*—This law has very few exceptions—eight out of 81 instances. Zinc chloride is a prominent one, being putrid and acid at once in its three solutions.

12. *The same Acid substance in different proportions may determine Putrefaction or Fermentation.*—Though I have strong reason to believe this applies to most of the acid substances, yet only one example can be given. In the egg albumen column, Group IV., under chloralum (aqueous solution of aluminic chloride) animalcules appeared on the ninth day, and under aluminic chloride, fungi on the eighth day; the weaker solution determining putrefaction and the stronger fermentation.

13. *The Odours of Fermentation and of Putrefaction are distinct.*—Organic matter in a state of fermentation has usually an earthy or mouldy aroma, frequently not unpleasant, sometimes, indeed, slightly odoriferous. I have observed milk, after being exposed to the vapours of various volatile media, strewed with woolly and fawn coloured tufts of mucedinæ, and having a slight odour of decaying rose leaves. Moreover, as already noticed, the citric acid and beef juice solution, during its fermenting stage, gave out a faint orange perfume. The effluvia of putrefaction vary greatly, but their mildest forms are repulsive, and usually characteristic of their source. A putrescent taint is *sui generis*, and easily discriminated from the musty rotten-straw odour of fermentation.

The peculiar nauseating fetor of pus from necrosed bone is a well known instance, and is, doubtless, owing to the abundant presence of lime salts, chiefly the tribasic phosphate. When caustic lime (see putrefiers) and chloride of lime were used, the odour of their putrid mixtures at once recalled that of osseous suppuration.

14. *Almost all Antiputrefactives are Acid.*—A slight glance at the tables will show the truth of this axiom. Indeed, it may be safely asserted that marked putridity and acidity conjoined is a chemical impossibility. Much more so is "strongly acid and weakly putrid." The converse, however, "strongly putrid and weakly acid," is possible but transient, the mixture soon getting neutral, and, in fact, was the condition of those solutions in the tables forming the exceptions.

15. *The best Antiputrefactives are Acid.—Vide* mercuric dichloride, chromic acid, potassic dichromate, cupric sulphate, benzoic acid, argentic nitrate.

16. *Good Antifermentives are commonly bad Antiputrefactives, and are Neutral.—Vide* baric chloride, quinine (pure), beberia sulphate, iodine, alcohol.

17. *A good Antifermentive may be a fair Antiputrefactive.—Vide* zinc chloride, sulphurous acid, hydrocyanic acid, quinine, iodine.

18. *Bad Antifermentives are commonly good Antiputrefactives, and are Acid.—Vide* aluminic chloride, sulphuric, oxalic, nitro-hydrochloric, and arsenious acids, plumbic acetate, &c. To illustrate more fully the truth of this axiom, compare the figures of aluminic chloride, when it will be seen that this body has manifestly caused fermentation, as, in its absence, all its mixtures being then mere simple organic solutions, would undoubtedly have putrefied. *Vide* 2. Hence, if fermentation be an evil in a pathological sense equal to putrefaction (some think it worse), the use of this body and others of a like nature as antiseptics is either obviously useless or positively injurious.

19. *A good Antiputrefactive may be a fair Antifermentive.—Vide* potassic dichromate, iron alum, zinc sulphate, cupric sulphate, plumbic acetate, picric acid, &c.

20. *Fermentation may begin and end per se in an Organic Solution.—Vide* all mixtures figured in the fungi columns and blank in the corresponding microzyme columns.

21. *Putrefaction may begin and end per se in an Organic Solution.—Vide* all mixtures figured in the microzyme columns and blank in the corresponding fungi columns.

22. *The best Antiseptics are Acid.—Vide* mercuric dichloride, benzoic acid, chromic acid, cupric sulphate, argentic nitrate, potassic dichromate.

An antiseptic is a substance which prevents synchronously fermentation and putrefaction of organic matter. If it but do the first, it is merely an antifermentive, if only the second, it is simply an antiputrefactive. A body may seem to have a high antiseptic power in regard to preventing fungi, because it has a low antiputrefactive power in regard to preventing microzymes, as where putrefaction is strong fungi seldom appear, or only sparsely and at long intervals. The converse also obtains, though not to such an extent. A body may seem to have a high antiseptic power in regard to preventing microzymes because it has a low antifermentive power in regard to preventing fungi, as where fermentation is strong microzymes may not appear, though they generally do in abundance in comparatively short intervals.

The foregoing conclusions, be it remembered, are founded solely on the results of experiments based on clearly defined and uniform data, and while these results may be fairly relied on as regards their data, still, variations in the latter, as an increase or diminution in the quantity or quality of the organic matter, in the proportions of the chemicals used, in the temperature of the medium, or in the time allowed for the experiments would, of course, involve corresponding diversity of results—fermentation might be made to give place to putrefaction in the first instance, and *vice versa;* antiseption might be rendered strong where it is weak, weak where it is strong, or be powerfully produced by a body which in the proportions used in the tables is almost void of such action. But the conclusions, though of neces-

sity different from those of these experiments, would only differ in degree, and not in quality.

The many possible and probable sources of error attending such investigations can only be properly appreciated by those who have engaged in them. The errors in this instance cannot be many, and must have been trivial, as the observations recorded are practically repetitions of one experiment, independent of the diverse composition of the contents of the tubes. One source of error—"variation of temperature"—may, however, have somewhat vitiated the results, although all the solutions must have been uniformly affected by it. At the beginning of the experiments an attempt was made to obviate this by recording the diurnal and nocturnal temperatures of the medium in which the mixtures were kept, with a self-registering thermometer, but it had to be given up, as the results for many reasons were found practically useless. It will be seen that the maximum variation of temperature was about 11° Fah. Doubtlessly such an elevation or depression has a marked effect in accelerating or retarding decomposition of organic matter, and in this lies the erroneous element, as some of the mixtures may have chanced to be exposed for a longer or shorter time to the minimum, and some in like manner to the maximum temperature.

The sanitary worth of antiseptics may be viewed in two aspects—first, their monetary value; second, their antiseptic value. It so happens that those having the highest antiseptic power are also highest priced, while some having a moderately high power are comparatively low priced. Hence it is often cheaper and even more effective to use an excess of moderate, than limited quantities of strong antiseptics, the high price of which must frequently cause them to be employed too sparingly to be effective.*

* Since the foregoing experiments were made, an abstract of a paper in Comptes Rendus, by A. Rabuteau and F. Papillion, entitled "Researches on the Antiferment Properties, &c., of Silicate of Soda," has appeared in the *Chemical News*, 11th October, 1872. The authors record a series of experiments to ascertain the effect of silicate of soda upon the alcoholic, lactic, amygdalic, and ureaic fermentation. They found that it acts in the same manner as borax, but more energetically, *i.e.*, as an antiferment, and that it is also a strong disinfectant.

Having procured some silicate of soda, and submitted it to the same tests as the substances enumerated in the tables, the result was as anticipated, viz., no fermentation, and putrefaction not perceptibly delayed, but probably accelerated. It seems also absolutely destitute of disinfectant action. In the above respects silicate of soda therefore coincides with other alkaline salts. The authors are nevertheless correct in ascribing antiferment properties to this body, providing a distinction is implied between fermentation and putrefaction. *Vide* axioms 1 and 16. The same remarks apply to borax; *vide* putrefiers.

III. The relative Aerial Antiseptic powers of different Volatile Bodies as evinced by their preventing the Appearance of Fungi and Animalcules in Organic Fluids exposed to their vapours, and by their Action on Vaccine Lymph.

Much diversity of opinion exists regarding the efficiency of the volatile antiseptics or disinfectants in preventing or arresting decomposition of, or destroying organic matter by their vapours, and more particularly in purifying an atmosphere contaminated with zymotic virus or with emanations from open sores, &c. Their antiseptic value is usually rated by the amount of their consumption, which may generally be pretty accurately measured by the extent to which they are advertised.

Few attempts have hitherto been made to ascertain relative aerial antiseptic powers. Dr R. Angus Smith has recorded a series of interesting experiments* made to test the influence of volatile substances and strong gases in preventing putrefaction. These consisted in suspending pieces of lean meat in bottles containing small quantities of the volatile substances, or filled with the stronger gases. The bottles with the volatile substances were kept under observation for fifty-one days, and those with the stronger gases for twenty-eight days—any changes in the meat apparent to the naked eye, or as regards smell occurring in these intervals, being noted.

Dr A. E. Sansom has also made some experiments,† founded on rather indefinite *data*, to compare the restrictive power of various volatile media over the appearance of

* Disinfectants and Disinfection, pp. 101-108.

† The Antiseptic System, p. 105.

fungoid organisms in flour paste and maceration of galls. With these exceptions, I am not aware of any similar investigations on record, and, as may be seen in the sequel, those about to be detailed differ in many respects from the foregoing, and are perhaps as practically exhaustive as laboratory experiments could well be.

Four drams of each of the putrescible substances (*see tables*) were put in glazed earthenware saucers about two inches in diameter and half an inch deep. These were set on a small stand six inches high, at the bottom of which was placed a breakfast tea saucer filled with the antiseptic, except in those cases specified below. The whole was then covered with a bell jar of one cubic foot capacity, its roof being six inches above the saucers containing the putrescents, and one foot above the saucer containing the antiseptic. In each experiment the chemical was placed beneath the jar twelve hours before introducing the putrescible bodies, so that purely antiseptic results might be obtained.

The nitrous acid was generated by adding six grains copper foil to nitric acid every twenty-four hours. Hydrochloric acid gas was liberated by adding sulphuric acid to the liquid acid; the temperature of the mixture rises and the gas is evolved. Carbolic acid crystals, liquified by a minimum quantity of water, were used. The sulphurous acid was generated by the ignition of four grains of sulphur under the jar every twenty-four hours. Chloride of lime, iodine, and camphor were used in their natural state. Two drams of chloroform, also of ether, were used every twenty-four hours. Antiseption was continued for six days, during which any substance found to contain microzymes or fungi was removed, and after which any substance found without either of these was exposed to ordinary air and kept under observation for twelve days, and the results noted. Strength of egg albumen, one to four of water.

Antiseptic.	Putrescents.	Reaction.	Days when Anthrobia were seen.	Reaction.	Days when Fungi were seen.	Reaction.	Days when bad odour was felt.	Remarks.
Exposed to ordinary air, ...	Milk.	Neutral.	4	Acid.	2 Pen.	Acid.	4 sour pungent.	Coagulated on 2nd day.
	Beef juice.	Acid.	1	Do.	1 Tor.	Do.	1	
	Urine.	Do.	2	Do.	...	...	2	
	Egg albumen.	Alkaline.	3	Neutral.	...	...	6	
	Hay solution.	Neutral.	12 hours.	Do.	3 Tor.	Neutral.	..	
	Mixture of equal parts of all above.	Faintly acid.	12 hours.	Acid.	...	...	12 hours.	
Carbolic Acid, ...	Milk.	Neutral.	...	...	...	...	...	Coagulated on 8th day.
	Beef juice.	Acid.	...	...	...	...	...	
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	2 sparse.	Neutral.	3 Tor.	Acid.	...	All the substances acid on 6th day.
	Mixture of all above.	Faintly acid.	2	Acid.	...	...	4	In 12 hours abundance of vertical bacteria. In 48 hours abundance of free life, but movements languid.

Antiseptics.	Putrescents.	Reaction.	Days when Animalcules were seen.	Reaction.	Days when Fungi were seen.	Reaction.	Days when bad odour was felt.	Remarks.
Chloroform,	Milk.	Neutral.	5	Acid.	7 Pen.	Acid.	...	
	Beef juice.	Acid.	3	Alkaline	...	...	3	
	Urine.	Do.	5	Acid.	9 Tor.	Acid.	...	
	Egg albumen.	Alkaline.	9	Alkaline	...	...	12	
	Hay solution.	Neutral.	5	Neutral.	11 Tor.	Neutral.	...	
	Mixture of all above.	Faintly acid.	3	Do.	...	...	3	
Camphor,	Milk.	Neutral.	...	...	4 Pen.	Acid.	...	
	Beef juice.	Acid.	12 hours.	Acid.	...	...	1	
	Urine.	Do.	2	Neutral.	7 Tor.	Neutral.	4	
	Egg albumen.	Alkaline.	12 hours.	Alkaline	...	...	2	
	Hay solution.	Neutral.	12 hours.	Neutral.	7 Tor.	Neutral.	...	
	Mixture of all above.	Faintly acid.	12 hours.	Do.	...	...	1	
Sulphuric Ether,	Milk.	Neutral.	...	...	3 Pen.	Acid.	...	
	Beef juice.	Acid.	12 hours.	Neutral.	...	...	1	

Antiseptics.	Putrescents.	Reaction.	Days when Animalcules were seen.	Reaction.	Days when Fungi were seen.	Reaction.	Days when bad odour was felt.	Remarks.
Sulphuric Ether,	Urine.	Acid.	1	Neutral.	...	...	4	
	Egg albumen.	Alkaline.	12 hours.	Alkaline	...	...	1	
	Hay solution.	Neutral.	12 hours.	Neutral.	7 Tor.	Neutral.	...	
	Mixture of all above.	Faintly acid.	12 hours.	Do.	...	...	1	
Iodine,	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	...	...	...	...	...	
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	Gelatinized on 5th day.
	Hay solution.	Neutral.	...	...	...	...	...	
	Mixture of all above.	Faintly acid.	...	...	...	...	...	All the substances acid on 2nd day.
Chloride of Lime,	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	15	Alkaline	11 Pen. & Tor.	Acid.	15	On 15th day has the odour of putrid urine.

Antiseptics.	Putrescents.	Reaction.	Days when Animalcules were seen.	Reaction.	Days when Fungi were seen.	Reaction.	Days when bad odour was felt.	Remarks.
Chloride of Lime,...	Urine.	Acid.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	...	...	...	...	...	
	Mixture of all above.	Faintly acid.	...	...	...	...	...	All the substances acid on 3rd day, and all dried up soon in consequence of the absorption of their water by the chloride of lime. They were subsequently liquified with water, and set aside for the specified time.
Sulphurous Acid,...	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	...	...	15 Pen.	Acid.	...	Gelatinized on 4th day.
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	...	...	15 Pen.	Acid.	...	All the substances acid in 24 hours.

Antiseptics.	Putrescents.	Reaction.	Days when Animalcules were seen.	Reaction.	Days when Fungi were seen.	Reaction.	Days when bad odour was felt.	Remarks.
Sulphurous Acid,...	Mixture of all above.	Faintly acid.	...	...	15. A few fungus spots on surface.	Neutral.	...	
Nitrous Acid, ...	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	...	...	...	...	...	
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	15 sparse.	Acid.	...	...	...	
	Mixture of all above.	Faintly acid.	...	...	...	...	...	All the substances acid in 12 hours.
Glacial Acetic Acid,	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	...	...	...	...	...	
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	...	...	...	...	...	
	Mixture of all above.	Faintly acid.	...	...	...	...	...	All the substances highly acid in 12 hours.

Antiseptics.	Putrescents.	Reaction.	Days when Acid-mixtures were made.	Reaction.	Days when Pieces were dried.	Reaction.	Days when last Tube was felic.	Remarks.
Hydrochloric Acid,	Milk.	Neutral.	...	...	...	...	...	
	Beef juice.	Acid.	...	...	...	...	...	
	Urine.	Do.	...	...	...	...	...	
	Egg albumen.	Alkaline.	...	...	...	...	...	
	Hay solution.	Neutral.	...	...	...	...	...	
	Mixture of all above.	Faintly acid.	...	...	...	...	...	All the substances acid in 12 hours.

The Effects of the various Volatile Media on the Physiological Properties of Vaccine Lymph.—To the separate vapours separate minims of vaccine lymph were exposed for twenty-four hours along with the putrescents. The dried spot remaining was then moistened with glycerine and water, its reaction taken, and the mixture sealed in capillary tubes till an opportunity occurred for vaccinating a child with it. All the mixture was used in one insertion, so as to make sure of obtaining its full effect.

The lymph in every instance was at first alkaline and the glycerine neutral. The following are the results:—

Carbolic Acid.

By a mistake the lymph was allowed to remain thirty-six instead of twenty-four hours under the jar. Mixture of lymph and glycerine neutral. Vaccination produced a perfect vesicle, from which I filled twelve tubes.

The lymph from two of these tubes was mixed with one minim of a 1 in 50 aqueous solution (1-50th grain) of carbolic acid. Reaction neutral. In twelve hours the mixture, now dried into a transparent film, was carefully laid past.

Chloroform.

Two of the twelve tubes used. *Vide* carbolic acid. Mixture of lymph and glycerine alkaline. Vaccination successful. Vaccination also successful with the mixture of lymph and carbolic acid, now ten days prepared. Filled ten tubes from vesicle of latter.

Camphor.

Lymph from first vesicle, *vide* chloroform, mixture of lymph and glycerine alkaline. Vaccination successful. Vaccination also successful with one of the ten tubes, *vide* mixture of carbolic acid and lymph. Filled six tubes from vesicle of latter.

Sulphuric Ether.

Two of the six tubes used, *vide* camphor, mixture of lymph and glycerine alkaline. Vaccination successful. Vaccination also successful with one of the twelve tubes, *vide* carbolic acid.

Iodine.

Two of the ten tubes used, *vide* mixture of carbolic acid and lymph; mixture of lymph and glycerine neutral. Vaccination successful. Filled eight tubes. Vaccination also successful with one of the six tubes, *vide* camphor.

Chloride of Lime.

Two of the ten tubes used, *vide* mixture of carbolic acid and lymph; mixture of lymph and glycerine acid. Vaccination unsuccessful. Vaccination successful with one of the ten tubes, *vide* mixture of carbolic acid and lymph.

Sulphurous Acid.

Two of the twelve tubes used, *vide* carbolic acid, mixture of lymph and glycerine acid. Vaccination unsuccessful. Vaccination successful with one of the eight tubes, *vide* iodine.

Nitrous Acid.

One of the twelve tubes and one of the ten tubes used, *vide* carbolic acid, also mixture of carbolic acid and lymph; mixture of lymph and glycerine acid. Vaccination unsuccessful. Vaccination successful with one of the six tubes, *vide* camphor.

Glacial Acetic Acid.

Two of the eight tubes used, *vide* iodine; mixture of lymph and glycerine acid. Vaccination unsuccessful. Vaccination successful with one of the eight tubes.

Hydrochloric Acid.

Two of the ten tubes used, *vide* mixture of carbolic acid and lymph; mixture of lymph and glycerine acid. Vaccination unsuccessful. Vaccination successful with one of the six tubes, *vide* camphor.

In consequence of the inequality in the volatile properties of the chemical substances, the results, with regard to the putrescents, though experimentally, are not entirely correct, it being obviously impossible, or, at least, extremely difficult, to obtain uniform volumes of their vapours, and also to maintain these at a fixed point of concentration for a period of

days. It was, however, attempted to approximate a standard of unity by compensating weak volatility with increase of quantity, and strong volatility with diminution of quantity, and that independent of the recognised antiseptic, disinfectant or destructive status of each substance. For example, carbolic acid, though held by some to be the *ne plus ultra* of antiseptics, was used in large quantity, because only moderately volatile. So also were chloride of lime and iodine; glacial acetic acid I feel sure was used in excess, as from its extreme volatility and pungency a smaller portion would probably have produced the same result. Sulphurous, nitrous, and hydrochloric acids being very volatile, were employed in comparatively minute portions. The results of the experiments show that camphor and sulphuric ether in the circumstances favour putrefaction, and that the preservative power of chloroform is merely nominal. The remaining seven bodies have, however, produced effects distinctly proving them ærial antiseptics. Of these bodies, glacial acetic acid and hydrochloric acid are strongest and equal in strength. Nitrous acid ranks next, in respect that microzymes were present only, and that in one instance, were sparse, and late in appearing. Chloride of lime comes next, as with it only one substance showed life, also at a late period. Next we have sulphurous acid, in which fungi appeared in three of the substances, all late. Last and weakest is carbolic acid, inasmuch as microzymes were present in two instances so early as the second day, accompanied by torulæ in the hay solution on the third day. As nearly equal quantities of chloride of lime and carbolic acid were used, these were more fairly matched than any of the others. The result is manifestly in favour of the former. As the conditions of the experiments infinitely surpassed in severity any produced by their practical application; and, seeing that withal, putrefaction and fermentation were not entirely prevented, hence aerial antiseption or disinfection in the great majority of cases is fallacious.

The effects of the various volatile media on the physiological properties of vaccine lymph are strikingly conclusive,

as seen in the details of the experiments and in the following

SUMMARY OF RESULTS OF VACCINATIONS.

Successful.	Reaction of Mixture of Lymph and Glycerine.	Unsuccessful.	Reaction of Mixture of Lymph and Glycerine.
Carbolic Acid (vapour), ...	Neutral.	Chloride of Lime,.....	Acid.
Carbolic Acid and Lymph,	Do.	Sulphurous Acid, ...	Do.
Chloroform, ...	Alkaline.	Nitrous Acid,.....	Do.
Camphor, ...	Do.	Glacial Acetic Acid,.....	Do.
Sulphuric Ether, ...	Do.	Hydrochloric Acid, ...	Do.
Iodine, ...	Neutral.		

These results coincide remarkably with those obtained by the action of the various vapours on the putrescents, iodine forming an exception, as it prevented the appearance of fungi and microzymes, but failed simultaneously to affect the lymph; so also, but conversely, chloride of lime, sulphurous acid, and nitrous acid, which partially failing in the first respect, succeeded entirely in the second. Comparing the action on lymph of the bodies on the right hand column with their action on the putrescents, and the time of their exposure, it is seen that the contagious property of the lymph is more easily destroyed by them than the growth of microzymes and fungi is prevented. These results *per se* are singularly and suggestively explicit. They show that the mixture of lymph and glycerine of the successful vaccinations was either neutral or alkaline; while that of the unsuccessful was, without exception, acid. Hence volatile acids, or a volatile body causing acidity by chemical affinity, as the chlorine from chloride of lime, which produces hydrochloric acid and free oxygen, are the best destructives of the active properties of vaccine lymph, and therefore *a priori* of variolous matter and the other zymotica.

It would be unjust to those substances in the right hand column were no allusion made to what I consider the ordinary, though some may think the extraordinary, behaviour of carbolic acid in these experiments. As already mentioned, it was allowed to act 12 hours longer on its lymph than were the other bodies, thus giving it a decided advantage over them. The experiment also was made in August, the higher temperature of which would increase its volatility; most of the other experiments were made subsequently, and at lower temperatures, yet the effect on the lymph was *nil*. Again, in order to confirm or confute this result, pure carbolic acid was, as stated, actually mechanically incorporated with lymph. This mixture may either be looked on as composed of pure lymph and carbolic acid—1 in 50, or of equal parts of lymph and water, with carbolic acid—1 in 100, —a considerable strength, it will be admitted, yet the effect was *nil*. But lest the genuineness of the vesicles produced by the two kinds of carbolized lymph should be questioned, it will be seen that their lymphs were repeatedly vaccinated "in and in" with unvarying success. So the grand card castle where resided the volafile talismanic virtues of carbolic acid has collapsed, at the conjuring touch of a mere pellicle of lymph. If a minim of vaccine matter is unaffected after being buried for 36 hours in the heart of a cubic foot of concentrated carbolic vapour, or after being mixed with the acid *per se* in the proportion of 1 in 50 for 10 days, must not the highly attenuated carbolic vapour, alone attainable in practical aerial disinfection, of necessity be powerless to cope with an atmosphere saturated and smelling with the contagium of small-pox. Moreover, it is manifest that the comparatively small quantity of the acid commonly used must in many cases fail to affect the variolous poison, though incorporated with it by the sanitarian brush in the usual manner.

These results suggest the strong probability of carbolic acid rather preserving than destroying the contagious property of vaccine lymph, and if so, I am afraid it will rather preserve than destroy the active principle of vari-

olous matter. This view, I presume, will at once be acquiesced in by those who believe in its unequalled antiseptic powers. But unless it can be shown that the acid forms a permanent harmless compound with variolous lymph or pus, which is extremely unlikely, judging from its action on vaccine lymph, I doubt if the subtle and malignant powers of small-pox poison are even temporarily suspended by it as at present used; but, if so, it is highly probable that they are released at some subsequent period, when the acid has become volatilized, as active and as virulent as at first.

In conclusion, there are several other points to which attention might be directed, but which the reader may glean from the tables. It is worth observing and comparing the large quantity of carbolic acid, and the small quantities of sulphurous and nitrous acids used, and their effects.

N.B. Since the above was in type, I have received from Dr Charles Cameron, Dublin, his Report on Public Health, 1872,* to which is appended a very able article "On the application of gases as a means of destroying contagion." In the latter some very interesting experiments are recorded. The following, with vaccine lymph and chloride of lime, confirm my own:—"Four ivory points, charged with vaccine lymph, were subjected during 24 hours to the influence of the gases evolved from one oz. bleaching powder decomposed by acid. With these points I successfully re-vaccinated two persons. The other (two) points failed. Six charged points were next exposed to the gases evolved from two oz. chloride of lime per 16½ cubic feet, but attempts to vaccinate with these points proved unsuccessful." "If ordinary gaseous disinfection sometimes fails to destroy the vitality of vaccine, and has no effect on microzymes, we cannot rely on it as a means of destroying the contagiums of zymotic diseases, which certainly are near akin, if not to bacteria, at least to the virus of vaccine."

* By Charles Cameron, M.D., Prof. of Hygiene, Royal College of Surgeons, Analyst to the City of Dublin.

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On Protoplasmic Life,

AND THE ACTION

OF

HEAT AND ANTISEPTICS UPON IT.

BY

Dr. F. CRACE-CALVERT, F.R.S., F.C.S.,

Corresponding Member of the Royal Academy of Turin, of the Medical Society of Brussels, the Industrial Society of Mulhouse, Honorary Member of the Pharmaceutical Societies of London and Paris, &c., &c.

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the following apparatus, which gave very satisfactory results, and enabled me to obtain water which remained free from life for several months.

It consists of two flasks, A and B (A rather larger than B), fitted with perforated caoutchouc stoppers.* These flasks are connected by the tube D. Into the stopper of A is fitted a tube C, to which is joined a piece of caoutchouc tubing, which may be closed by the clip E. Through the stopper of B is a syphon, F, the long limb of which is cut and joined with caoutchouc tubing, which can be closed by the clip G. Through this stopper is a third tube, H, connected by caoutchouc with the tube I; this can be closed by the clip K. The tube I is about 3 feet long, and goes into the vessel L, which is partly filled with water.

The water to be distilled is mixed with solution of potash and permanganate of potash, and placed in the flask A.† Before distillation is commenced, a rapid current of pure hydrogen or some other gas must be passed through the apparatus by the tube C to displace the air and carry off all the germs the air may have contained. The clip G is first left open, then this closed and the clip K opened, which allows the gas to pass through the water in the vessel L.

The gas should be passed through for about fifteen minutes. The clip E is then closed, and the distillation carried on. When the operation is complete, the gas must be again passed through the apparatus, and the connection with the tube I broken by closing the clip K. The water is drawn off through the syphon F. The long tube acts as a safety-tube, and is made so long that the absorption is noticed in ample time to close the clip before any air can enter through that tube.

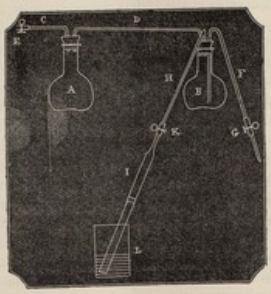
The water has to be re-distilled three or four times before it is obtained free from germs, and must be kept in the apparatus in which it is distilled until wanted, to prevent any contact with air.

Some water which had been distilled on the 20th of September, 1870, and was still free from life on the 7th of December, was introduced by the syphon H into twelve small tubes, and left exposed to the atmosphere for fifty hours, when the tubes were

* The stoppers and caoutchouc tubing used for the various joints must be new, and must be well boiled in water before use.

† The reasons why I employed permanganate of potash (in large excess) were that, under the influence of heat, its oxidising powers were much increased, and that it gave off no gas that could interfere with the purity of the water; further, a solution of the salt not even yielding oxygen when boiled.

closed. Every eight days some of the tubes were opened, and their contents examined. Therefore, on the eighth and fifteenth



days examinations were made, when no life was observed; on the twenty-first, two or three other tubes were examined, and again no life was detected; whilst in the series opened on the 2nd of January, 1871 (that is to say, twenty-four days from the time the tubes were closed), two or three black vibrios were found in each field.

Being impressed with the idea that this slow and limited development of protoplasmic life might be attributed to the small amount of life existing in the atmosphere at this period of the year,* a second series of experiments was commenced on the 4th of January. The distilled water in the flask being still free from life, a certain quantity of it was put into twelve small tubes, which were placed near putrid meat at a temperature of 70° to 80° F. for twelve hours, and then sealed. On the 10th of the same month the contents of some of the tubes were examined, when two or three small black vibrios were observed under each field. This result shows that the fluid having been placed near a source of protoplasmic life, germs had introduced themselves in twelve hours in sufficient quantity for life to become visible in six days instead of twenty-four. Other tubes of this series were opened on the 17th of January, when a slight increase of life was noticed;

* During the intense cold weather of last December, I found it took an exposure to the atmosphere of two days, in a room having a temperature of 12° C., before life appeared in a solution of white of egg made with pure distilled water, whilst as the weather got warmer the time required became less.

but no further development appeared to take place after this date, as some examined on the 10th of March did not contain more life than those of the 17th of January.

This very limited amount of life which had been observed suggested the idea that it might be due to the employment of perfectly pure water, and that the vibrios did not increase, owing to want of the elements necessary for sustaining their life. I therefore commenced a third series of experiments. Before proceeding to describe this series, I must call attention to the fact that the water in the flask had remained perfectly free from life up to this time, a period of nearly sixteen weeks.

On the 9th of February 100 fluid grains of albumen from a new-laid egg were introduced, as quickly as possible and with the greatest care, into 10 ounces of pure distilled water, free from life, and contained in the flask in which it had been condensed, and an atmosphere of hydrogen kept over it. On the 16th some of the fluid was taken out by means of the syphon F, and examined, and no life being present, twelve tubes were filled with the fluid, exposed to the air for eight hours, and closed. On the 21st the contents of some of the tubes were examined, when a few vibrios and microerphymes were distinctly seen in each field. On the 27th other tubes were examined, and showed a marked increase in the amount of life. In this series life appeared in five days, and an increase in ten, instead of requiring twenty-four days, as was the case when pure water only was employed.

Albumen, therefore, facilitated the development of life. Of course the contents of the flask were examined at the same time, but in no instance was life detected. I believe that these three series of experiments tend to prove the fallacy of the theory of spontaneous generation; for if it were possible, why should not life have appeared in the pure distilled water, or in the albuminous solution, which were kept successively in the flask B, as well as in the fluids which were contained in the tubes, and had been exposed to the atmosphere or near animal matter in a state of decay, and had thus become impregnated with the germs of protoplasmic life? What gives still further interest to these experiments is that, having operated during the severe weather of last winter, when little or no life existed in the atmosphere, I was able to impregnate the fluids with germs without introducing developed life, as was ascertained by examining some of the fluids in the small tubes before closing them.

The quantity of life produced in the above-recited experiments

being comparatively small, I was led to infer that this might be due to the influence of the atmosphere of hydrogen employed to displace the air in the apparatus used for obtaining the water. I, therefore, on the 2nd of March, prepared a solution of albumen similar to that before employed, but expelled the air out of the apparatus by pure oxygen; and as the contents of the flask B were free from life on the 8th of March, a series of small tubes were filled and exposed for twenty-six hours to the atmosphere near putrid matter, and then sealed. Several of these tubes were opened on the 11th, and immediately examined, when only a few cells were observed in each field. A second lot was opened on the 14th, and they showed considerable increase of life, there being two or three vibrios under each field. A third quantity was opened on the 25th, when no increase had taken place. This latter result tends to show that although oxygen appears to favour the development of germs, still it does not appear to favour their reproduction.

As the weather had become much warmer, and a marked increase of life in the atmosphere had taken place, some of the same albumen solution employed in the above experiments was left exposed in similar tubes to its influence, when a large quantity of life was rapidly developed and continued to increase. This result shows that the increase of life is not due to reproduction merely, but to the introduction of fresh germs; for, excepting this fresh supply, there appears to be no reason why life should increase more rapidly in the open than in the closed tubes.

On Putrefaction.

This paper is intimately connected with that on Protoplasmic Life, and in which I have endeavoured to show that putrefaction is probably due to it.

I have already shown* that when albumen from a new-laid egg is carefully introduced into pure distilled water, and the solution is excluded from the atmosphere, protoplasmic life does not appear; but, if the same solution be exposed to the air for from fifteen to forty-five minutes, then minute globular bodies appear, having an independent motion, which I shall call microphymes. The time required for the life to appear varies according to the period of the year, the amount of moisture present in the atmosphere, and the temperature.

Although M. Pasteur has noticed the meteorological conditions which influence that life, still he did not observe the extraordinary rapidity with which fluids are impregnated, and that the impregnation is proportional to the surface exposed. And that such is the case is proved by the following experiment:—On the 18th of May, 1871, two portions of an albumen solution, each measuring four hundred grains, were placed, the one in a test-tube having a diameter of three quarters of an inch, the other in a test-glass, which had a diameter of two inches at the surface of the liquid. In the tube vibrios were not developed until after twelve days, while in the glass only five days were required for their appearance.

Another illustration of the abundance of this life may be cited. If, in place of pure distilled water, the water supplied by the Manchester Corporation be mixed with albumen and placed in a test-tube, instead of the time required for development of vibrio life being twelve days, only twenty-four hours are necessary.

In the hope of throwing some light on the question of the necessity of the presence of oxygen for the maintenance and reproduction of this life, the following experiments were made:

* Proc. Roy. Soc., Vol. xix., p. 471.

Into five glass bulbs equal volumes of a solution of albumen in Manchester water were introduced. One of these bulbs was left in contact with the atmosphere for twenty-four hours, after which the ends of the tube were hermetically sealed at about two inches on each side of the bulb. After passing oxygen, hydrogen, nitrogen, and carbonic acid over the other four solutions respectively, the tubes were also hermetically sealed. These bulbs were kept closed for twenty-seven days, during which it was observed that the albumen in the bulb containing oxygen speedily became turbid, and, later, the one containing air, while the other three remained clear. At the expiration of this period the tubes were broken and the contents examined. A large quantity of vibrio life was found in those containing oxygen and common air, whilst those containing nitrogen, carbonic acid, and hydrogen showed very small quantities of vibrios, hydrogen the least, thus proving oxygen to be essential to the development of putrefactive vibrios. During these experiments a striking instance occurred of the efficacy of oxygen in the development of vibrio life. A small crack was observed in one of the hydrogen bulbs, and it was found that by diffusion enough air penetrated to develope an active growth of animalcule life.

In further support of the view that oxygen is necessary to the existence and development of this life, I may state that under certain conditions these animalcules produce such an amount of carbonic acid and other gases as to exclude oxygen to such an extent that their own development and life is impaired. If an albumen solution full of animalcules, but not emitting any putrid odour, be placed in test-tubes, and some of the tubes be closed while others are left open, it will be found after a few weeks that in those left in the air life has much increased, and that they emit a very putrid odour—whilst the life in the closed tubes not only has not increased, but appears to be in a dormant condition; for, on the latter fluids being again brought in contact with the oxygen of the atmosphere, their activity returns. The albumen in the closed tubes does not emit any putrid odour.

M. Pasteur has made the remarkable observation that oxygen destroys peculiar classes of vibrios or Bactœria, which exist in butyric fermentation.

Having already stated that liquids exposed to the atmosphere become impregnated with microphymes, I will proceed to describe their gradual development into vibrios, and their ultimate transformation into what I call microerphymes. Figure 1 represents it

a few hours after a liquor becomes impregnated; microphymes, which have a diameter of about the one hundred and twenty-eight thousandth part of an inch, appear to agglomerate and form masses, as shown in figure 2. Some of the microphymes then become elongated into vibrios, which, though attached to the mass, have an independent motion, and as the force exerted by the vibrios predominates towards one or another direction, so is the mass moved over the field of the microscope. Their appearance is represented in figure 3. As the development proceeds the mass is broken up, and at last each vibrio has an independent existence, and may be seen swimming or rolling about the fluid. The changes may be seen in figures 4 and 5. Their size is about the twenty thousandth part of an inch. These I call ordinary vibrios. They gradually grow into long vibrios, which attain a length of the six thousandth of an inch. These are represented by figure 6.*

These long vibrios gradually become changed into cells, which I have called microerphymes. The first process in the transformation is the division of the vibrio into two independent bodies or parts. At first an extremely faint line appears across the centre of the animalcule, and gradually increases in distinctness until the vibrio appears like two smaller vibrios joined together. The separation takes place, and each part acquires an independent existence. These parts again divide, and the process of subdivision is carried on until they appear to be nothing more than cells, having a swimming power which enables them to pass over the field of the microscope with great rapidity. These I call microerphymes. The changes I have endeavoured to represent in figure 7.

After twelve or eighteen months all the vibrios disappear, and are replaced by microerphymes, either in motion or at rest. If they are placed in a solution of fresh albumen, vibrios are again abundantly developed. The probable explanation of this fact is, that in the fresh albumen they have all the circumstances favourable to their growth and reproduction, whilst the putrid albumen has become so completely modified as to be incapable of affording them the requisite conditions of reproduction. Thus, when albumen has undergone complete putrefaction, through the influence of microscopic life, its properties are quite altered; a deposit has taken place which, under the microscope, appears to consist of shoals of small particles of matter which have

* See plate at end of pamphlet.

no life, whilst the supernatant solution has become perfectly clear, possesses considerable refractive power, and has lost the property of becoming coagulated by heat.

Further, no albumen solution emits a putrid odour until the above-mentioned deposit has made its appearance, and its degree of putrescence is always in direct ratio to the number of vibrios present.

During these investigations other animalcules were observed.

Where vibrios are found there often appear bodies which have a resemblance to vibrios, but which are not identical with them, for they are straight, motionless, and comparatively opaque. Whether they are a variety of vibrios, or animals of a distinct class altogether, or Bacteria, I have as yet been unable to determine.

I will now give a description of some forms of life which I have noticed in special cases.

When the outside of the shell of a fresh egg is washed with distilled water, and the washing mixed with a solution of albumen, after about nine days, not only a large quantity of vibrios exist, but peculiar animalcules will be found having the appearance of *lozenge-shaped* discs, although they have in reality a spheroidal form. This animalcule has a *cilium* several times the length of its body, which it moves with great rapidity as it passes over the field of the microscope. When fully developed the body has a length of the three thousandth of an inch. It is shown in figure 8.

This animalcule is also observed in other circumstances, viz., in the flocculent precipitate which takes place in an albumen solution in which a large quantity of vibrios have existed. At first there appears a large quantity of motionless cells, which gradually acquire an independent motion. After some time, they separate from the amorphous deposit, and develope into a peculiar animalcule. The drawing, figure 9, will give an idea of it.

When these animalcules are developed in large numbers the vibrios lose their power of motion and appear to be quite dead, as they oscillate in the current produced in the fluid by the motion of these peculiar animalcules. At the same ime a much larger quantity of offensive gases are given off than is the case when vibrionic putrefaction only takes place. It is probable, therefore, that they have a greater power of absorbing oxygen than vibrios, and thus, either by the want of oxygen in the liquid or by the presence of noxious gases, the vibrios die.

In the same fluid may often be observed animalcules having a similar appearance to the one just described, but having two cilia instead of one, as will be seen by reference to figure 10.

These two classes of animalcules may also be observed in sewage, and in water which has been in contact with leaves in a state of decay.

A very active agent of putrefaction are transparent cells, which I call "putrid cells." These cells may be of various sizes from a mere point to comparatively large cells, as represented in figure 11. Often inside a large cell a number of small ones are observed, which gradually increase in size until they appear to burst the parent cell and have an independent existence.

When these putrid cells develope in an albuminous solution, and they render it milky and turbid, it emits a bad putrid odour.

I shall now call attention to the decomposition of animal matter by means of a fungus.

If an albumen solution be left exposed to the air, there soon appears in it a fibrous mass, which gradually increases until the filaments become interwoven, and when viewed under the microscope has the appearance shown in figure 12.

The spores of this fungus must be abundant in the atmosphere, as the fungus, which I believe to be *mycilium*, generally appears in an albuminous solution when exposed to the atmosphere. This fungus, like the above-mentioned animalcule life, absorbs oxygen and emits carbonic acid, which explains why it cannot develope in a liquid containing a large quantity of vibrionic life, and conversely why, in a liquid containing a large quantity of this fungus, vibrios cannot increase.

This fungus is always found under the surface of the albuminous liquid, whilst grape-like bodies are observed to float on the surface; they are not, I believe, the spores of the mycilium, but another variety of fungus, when in large quantities always emits a mouldy odour, particularly so when it dries up and assumes the appearance represented in figure 14. It is so light that it is dispersed by blowing.

Lastly, there is another fungus which I have found in several fluids, and always growing among the filaments of mycilium. It has a blood-red colour, which it gradually imparts to the fluid in which it grows. It is easily affected by external causes, losing its bright colour and becoming dark. It has no distinct structure, and has the appearance represented in figure 15.

4.—*Putrid Albumen in Oxygen.*

A series of experiments was instituted, with the view of ascertaining the exact action which vibrios exert on the decomposition of a solution of putrid albumen when placed in an atmosphere of oxygen. To effect this, a small quantity of putrid albumen was mixed with some fresh albumen, and four ounces of such fluid were introduced into a corked bottle, which was then filled with an atmosphere of pure oxygen.

The gaseous contents were ultimately submitted to analysis, and gave the following results:—

Oxygen	37·609	.....	36·857
Carbonic Acid	57·550	.....	57·760
Nitrogen	2·990	.....	3·350
Hydrogen	1·851	.....	2·033
Hydrosulphuric Acid	trace	.....	trace
	100·000		100·000

It is interesting to observe that hydrogen and nitrogen are given off in the putrefaction of albumen, as their presence proves that this decomposition is not due to a purely chemical or molecular change, for then we could not have these gases in a free state; but that they are generated by microscopic animal life, which unfolds albumen into other substances, which present properties different to those of albumen, as stated in one of my previous papers. Thus, one part of the original albumen has become insoluble; another remains in solution, and has quite different properties to albumen; and lastly, a third portion of the albumen has been consumed by the vibrios. Whether the gaseous products given off during these changes, as above stated, are the refuse products of the life of vibrios, or secondary products of the unfolding of albumen by vibrios, in the same way as sugar is unfolded under the vital action of the torula into carbonic acid and alcohol, is yet a question which remains to be solved.

The putrid fluid was next submitted to careful microscopic examination, and it was observed that nearly the whole of the vibrios were lifeless, for they floated about in accordance with the movement imparted to the fluid; and that only a few could be detected which had distinct self-motion, and were capable of swimming across the field.

The above experiments confirm my previous statement that the vitality of vibrios is destroyed by the gases which they generate. (See pages 38 and 50.)

Action of Heat on Protoplasmic Life.

Those investigators of germ-life who favour the theory of spontaneous generation have assumed that a temperature of 212° F., which is the boiling-point of the fluid which they have generally experimented upon, was sufficient to destroy all microscopic animal life, and that the life they subsequently observed in these fluids was developed from non-living matter.

I therefore made several series of experiments, in the hope that they might throw some light on the subject.

The first series was made with a sugar solution, the second with an infusion of hay, the third with solution of gelatine, and the fourth with water that had been in contact with putrid meat. The hay and putrid-meat solutions were taken because they had often been used by other investigators; sugar was employed, being a well-defined organic compound free from nitrogen, and which can easily be obtained in a state of purity; and gelatine was selected as a nitrogenised body which can be obtained pure and is not coagulated by heat.

To carry out the experiments, I prepared a series of small tubes made of very thick and well-annealed glass, each tube being about four centimetres in length, and having a bore of five millimetres. The fluid to be operated upon was introduced into them, and left exposed to the atmosphere for a sufficient length of time for germ-life to be largely developed. Each tube was then hermetically sealed and wrapped in wire gauze, to prevent any accident to the operator in case of the bursting of any of the tubes. They were then placed in an oil-bath, and gradually heated at various fixed temperatures, at which they were maintained for half an hour.

Sugar Solution.—A solution of sugar was prepared by dissolving 1 part of sugar in 10 parts of water. This solution was made with common water, and exposed all night to the atmosphere, so that life might impregnate it. The fluid was prepared on the 1st of November, 1870, introduced into tubes on the 2nd, and allowed to remain five days. On the 7th of November twelve tubes were kept without being heated, twelve were heated to 200° F., twelve to 300°, and twelve to 400° F.

The contents of the tubes were microscopically examined on the 1st of December, twenty-four days after heating. (See Table, page 18.)

The black vibrios referred to in these tables are far more opaque than the other varieties of vibrios, and are the most important of all, as I have found them to resist not only very high temperature, but all chemical solutions. I shall, in my paper on putrefaction and the action of antiseptics, describe the various vibrios and give drawings of them.

Hay Infusion.—An infusion of hay was made by macerating it in common water for one hour, then filtering the liquor, and leaving it exposed to the atmosphere all night, when it was sealed in the small tubes, twelve of which were used for each experiment. The infusion was made on the 4th of November, sealed in tubes on the 5th, and heated on the 7th.

The results were examined on the 1st of December, 1870, twenty-four days after being heated. (See Table, page 18.)

Gelatine Solution.—A solution of gelatine, prepared of such strength that it remained liquid on cooling, was exposed for twenty-four hours to the atmosphere. It was then introduced into the small tubes, and sealed. The solution was made on the 4th of November, the tubes sealed on the 5th, and subjected to the different temperatures on the 7th.

The fluids were examined on the 1st of December, 1870, twenty-four days after being heated. (See Table, page 19.)

Putrid-Meat Fluid.—Water was placed in an open vessel, and a piece of meat suspended in it until it became putrid and contaminated with myriads of animalcules; it was then dipped into distilled water, and this fluid was placed into the small tubes, sealed on the 7th of November, and heated on the same day.

The contents of the tubes were subjected to examination on the 1st of December, or twenty-four days after having been heated. (See Table, page 19.)

The general conclusion to be drawn from the above four Tables is that protoplasmic life is but slightly affected by a temperature of 212° F., and that even at a temperature of 300° F., it is not entirely destroyed, excepting in the case of gelatine, and that a temperature of 400° F. is necessary to completely destroy life in all the other fluids. These experiments, therefore, clearly show that the animal life found by previous experimenters in fluids which have been submitted to heat was not necessarily due to heterogenesis, but might be owing to the life which had remained in the

fluids after having been treated, and I am justified in the conclusions, as I have seen no experiment reported where the temperature to which the fluids were exposed exceeded 300° F.*

I am the more justified in making this statement, as I have repeatedly examined the contents of tubes which had been submitted to a temperature of 400° F., both immediately after cooling and at various intervals up to thirty days, and was unable to detect the slightest trace of life.

This result corroborates those recorded in my previous paper, and proves that the spontaneous-generation theory cannot be maintained.

It further occurred to me that it might be interesting to examine if the putrid-meat fluids that had been heated still possessed the property of propagating life. To enable me to throw light on this point a solution was prepared by mixing the albumen of a new-laid egg with pure distilled water free from life (prepared as described in my previous paper), and equal volumes of this solution were placed in six small test tubes, (which had been cleansed with hot vitriol and well washed with pure water). To one tube were added two drops of the putrid-meat solution, which had been heated to 100° F., to a second two drops of that heated to 212° F., to a third two drops of the liquid heated to 300° F., to a fourth an equal bulk of fluid heated to 400° F., and to a fifth the same quantity heated to 500° F. In the sixth the original albuminous solution was kept for comparison.

The tubes were then sealed, and kept from the 1st of February to the 9th.

Results of Examination.

Albumen solution.	Albumen solution, with putrid-meat liquor, heated to 100° F.	Albumen solution, with putrid-meat liquor, heated to 212° F.	Albumen solution, with putrid-meat liquor, heated to 300° F.	Albumen solution, with putrid-meat liquor, heated to 400° F.	Albumen solution, with putrid-meat liquor, heated to 500° F.
No. life.	Abundance of life.	Abundance of life.	Much less life than in the two previous fluids.	In each drop only 2 or 3 small black vibrios, moving to and fro.	In each drop only 2 or 3 small black vibrios, moving to and fro.

* It is with pleasure that I find these experiments to confirm the suggestion of Dr. Beale, in his work entitled "Disease Germs, their Supposed Origin," page 50 (which I read a few weeks ago), that "living forms might live, though exposed, under certain conditions, to a temperature of 350° F."

These results clearly show that, at the temperature of 100°, 212°, and 300° F., life and its germs had not been destroyed, whilst at 400° F. they had; for the results of the examination were in this case exactly identical with those of the albumen solution itself; therefore I conclude that the life found was doubtless introduced during the preparation of the solution, and was not due to any life having remained in the fluids that had been heated, as it was in these cases far more abundant.

Although perfectly aware of the interesting researches of Professor Melsens, proving that the most intense cold does not destroy the active power of vaccine lymph, still I thought it desirable to ascertain the effect of a temperature of 15° F. on well-developed germ-life, similar to that which had been subjected to the action of heat; therefore some putrid-meat liquor, containing a large quantity of microzyma and vibrios, was subjected for twenty hours to the influence of a temperature ranging between the freezing-point of water and 17° below that point, when the ice was melted, the liquor examined, and it was found that the animalcules had retained their vitality, but appeared very languid, and their power of locomotion had greatly decreased.

Two hours after melting the ice the liquor was again examined, when the animalcules appeared to be as energetic as before.

Table No. 1.

Sugar Solution, not heated.	Heated for half an hour at 212° F.	Heated for half an hour at 300° F.	Heated for half an hour at 400° F.	Heated for half an hour at 500° F.
There were about 30 animalcules under each field of the microscope, principally *small black vibrios*, 3 or 4 *ordinary swimming vibrios*, a few Bacteria, and a few microzymes.	A great portion of the life had disappeared, no animalcules were swimming; still this temperature had not completely destroyed life. 4 to 5 *small black vibrios* were observed moving energetically to and fro; 2 or 3 *ordinary vibrios* were also observed moving, but not swimming about.	The sugar was slightly charred, but the life was not entirely destroyed, as 1 or 2 *ordinary vibrios* and 1 or 2 *small black vibrios* were observed in motion under the field of the microscope.	The sugar was almost entirely decomposed; no trace of life was observed.	No life observed.

Table No. 2.

Hay infusion not heated.	Heated for half an hour at 212° F.	Heated for half an hour at 300° F.	Heated for half an hour at 400° F.	Heated for half an hour at 500° F.
Fungus matter was observed growing on the surface of the fluids in two of the tubes. On subjecting the contents of some of the tubes to examination, from 20 to 25 animalcules were observed under each field of the microscope, principally microzyma and one or two ordinary vibrios.	No fungus matter was noticed on the surface in any of the tubes. A few small black vibrios were present as in the original solution.	No fungus matter present, but some of the small black vibrios were still present, although in less numbers.	No fungus matter observed. The fluid was filled with irregular masses of coagulated matter, but life had disappeared.	No life present.

Table No. 3.

Gelatine solution not heated.	Gelatine solution heated for half an hour at 100° F.	Heated for half an hour at 212° F.	Heated for half an hour at 300° F.	Heated for half an hour at 400° F.
There were seven or eight animalcules under each field, five or six of which were quite different to any thing observed in the other fluids. They had long thin bodies, and were swimming with a peristaltic motion. One or two ordinary vibrios were also present; but the black vibrios were absent.	Life seemed to have only slightly decreased, and none of the animalcules were swimming. The peculiar animalcules mentioned in the first column appeared to retain still its peristaltic motion, but not sufficient power to move across the field, a few ordinary vibrios were observed moving to and fro.	A very decided diminution of life was recorded.	No life present.	No life present.

Table No. 4.

Putrid meat solution not heated.	Heated for half an hour at 100° F.	Heated for half an hour at 212° F.	Heated for half an hour at 300° F.	Heated for half an hour at 400° F.	Heated for half an hour at 500° F.
A large quantity of life was present—namely, microzyma and several distinct species of vibrios, among which were a number of the small black ones frequently mentioned.	This temperature had but slightly affected the life present; the animalcules being as numerous as in the liquid not heated. However, some long vibrios appeared considerably affected, as they were much more languid in their movements.	This liquid differed from those previously examined, being turbid and coagulated. Life was still present; and although heat had deprived the animalcules of the power of locomotion, still they retained a sufficient amount of vital force to place it beyond a doubt that life was not destroyed.	The liquid was quite clear, the albumen (which is coagulated at 200°) appeared to be re-dissolved. A large quantity of the life in the fluid was destroyed, but some vibrios still remained, the small black ones being the most numerous.	All life had disappeared.	All life had disappeared.

On the Effects of Heat on Protoplasmic Life, Previously Dried.

The following series of experiments was instituted as a sequel to those already published, and with the view of ascertaining whether animalcules which had been previously dried would resist a higher temperature without being destroyed than animalcules when heated *with* the fluid in which they were living.

In hope of throwing some light on this point, small thin slips of glass, three centimetres in length and five m.m. in breadth, were taken, and one side of each slip was smeared with a thin coating of putrid albumen containing a considerable quantity of microscopic life.

Fifty-six of these slips of glass were prepared on the 5th of July, 1871, and allowed to dry in the air for twenty-four hours, when twenty-eight of them were taken and sealed in tubes of the same size as those used in the experiments already cited, whilst the remaining twenty-eight were placed in an air-bath, and kept at a temperature ranging between 212° and 220° F. for fourteen hours, and then introduced in tubes and sealed.

Therefore there are two series of experiments—one in which the putrid fluid was merely dried on the slips at natural temperature; the other in which the fluid, after being dried in the air, was heated to 212° F. for 14 hours.

Four tubes from each series were set apart; the remaining tubes of both of the series were enveloped with wire gauze, and placed in an oil-bath, which was gradually heated and kept for half an hour at the following temperatures:—100° 200°, 300°, 400°, 500°, and 600° F. Four tubes from each series were taken out at each temperature, and those that remained were kept in the bath with its temperature at another fixed point for half an hour. The results of these experiments are given on pages 15 and 16, and are similar to those already recorded on pages 22 and 23.

Experiments were also made with the view of ascertaining if the germs of microscopic animal life were destroyed at the same temperatures as the animalcules themselves, and the results are given on pages 24 and 25. The facts observed prove that the germs in an aqueous solution do not appear to have a greater vitality than the animalcules themselves.

A further series was made substituting a solution of albumen free from life for the water used in the last experiments, the purpose being to facilitate the development of life by the employment of a fluid favourable to the maintenance of such life.* The tables given on pages 26 and 27 shew that the results are the same as with water.

* It is an interesting fact, which I believe is not generally known, that albumen, although coagulated in a solution below 212° F., redissolves again when heated under pressure at a temperature between 300° and 400°.

Series I.—Slips (*dried in air*) and heated at the various temperatures. Moistened with water on the slide of the microscope, and examined after fifteen minutes.

Not heated.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. Six or seven living animalcules were present under each field. All were moving to and fro. The majority are ordinary vibrios; a few of them, however, are long vibrios.	2. Life is present to about the same extent, and with the same vitality, as exists in 1.	3. Same class of vibrios exist here as in 1 and 2, and to about the same extent and with the same amount of vitality. So that up to this point there appears to be little or no difference in the amount of life.	4. There is a very decided decrease in the amount of life here. All the long and nearly all the ordinary vibrios have been destroyed. Two or three small vibrios are still present, moving to and fro under each field.	5. The presence of life was doubtful.	6. No life present. The albumen on the glass-slip has been charred, and pieces of the charred mass appear under the microscope to resemble shellac.	7. No life present. The albumen on the slip is charred to a greater extent than 6, while small carbonised pieces were mixed with those resembling shellac.

Series II.—Slips (*dried in bath for fourteen hours between* 212° *and* 220° *F.*) were then heated at the various temperatures. Moistened with water on the slide of the microscope, and examined immediately afterwards.

Dried in bath, but not heated after.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. There are seven or eight animalculæ under each field. They are principally ordinary vibrios, a few long and very long vibrios are present.	2. Exactly the same class of life is present as in 1. It exists to the same extent, and with the same amount of vitality.	3. There is no difference in the life present to that existing in 1 or 2.	4. There is a very decided decrease of life to that observed in 1, 2, or 3. All the ordinary, long, and very long vibrios have been destroyed, and only a few small black ones are left, which are sufficiently distinct in their appearance and motion to show that they are still alive.	5. The presence of life was doubtful.	6. No life present.	7. No life present.

Series III.—Slips *(dried in the air)*, and afterwards having been heated to the different temperatures, were introduced into small tubes containing pure distilled water on the 11th of July, hermetically sealed, and on the 15th of July the water in each of the tubes was examined.

Water alone for comparison.	Not heated at all.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. No animalculæ life present.	2. Four or five ordinary vibrios are present in the whole drop, two or three of which are swimming.	3. Forty *bacteria* and vibrios are sprinkled about under each field and 10 or 12 long and ordinary vibrios are swimming.	4. One or two large or ordinary vibrios are observed under most of the field moving to and fro.	5. A few energetic microcerphymes and small vibrios are present. No *bacteria*.	6. No trace of animalculæ life is present, but a number of pieces of matter are oscillating to and fro.	7. No animalculæ life present.	8. No animalculæ life present.

24

Series IV.—The Slips (which had been *dried in bath at* 212° *and* 220° *F.*), were afterwards heated to the different temperatures, and the slips then taken from their original tubes were introduced on the 11th of July into tubes containing pure distilled water, and examined on the 15th.

Merely dried in bath, not heated.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. About one ordinary vibrio was observed moving to and fro under each field.	2. The appearance of this fluid is exactly the same as No. 1.	3. About seven or eight *bacteria* are present under each field, and one ordinary swimming vibrio is present under most of the fields.	4. One or two ordinary swimming vibrios were observed in the whole drop of liquid employed.	5. No animalculæ life present.	6. No animalculæ life present.	7. No animalculæ life present.

25

Series V.—The Slips (which had been *dried at natural temperature*), were afterwards heated to the different temperatures, and then introduced into tubes containing equal quantities of an albumen solution. They were then hermetically sealed on the 11th of July, and examined on the 15th.

Albumen alone for comparison.	Not heated at all.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. No distinct vibrio life is present.	2. Four or five vibrios were observed moving to and fro throughout. No *bacteria* were observed.	3. Four or five *bacteria* and vibrios were present, and one or two long vibrios were swimming.	4. One or two ordinary vibrios moving to and fro, and one or two *bacteria* were observed, but no swimming life was present.	5. A few *bacteria* were present throughout the field, and a very few were moving to and fro.	6. No life.	7. No life.	8. No life.

Series VI.—Examination of the water solutions into which had been dipped the slips of glass (*dried in bath between the temperature of* 100° *and* 120° *F.*) previously to being heated to the different temperatures. The slips were then introduced into tubes containing an albumen solution, were sealed on the 11th of July, and examined on the 15th.

Albumen alone for comparison.	Not heated at all.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. No distinct vibrio life present.	2. About ten or twelve ordinary *bacteria* and vibrios are present and twelve or fourteen ordinary vibrios are swimming. There are also some fungi, which give the fluid a turbid appearance when shaken up.	3. There is nearly the same amount of life, and the fluid has much the same appearance as in No. 2.	4. Life is present, but not to the extent as it exists in Nos. 2 or 3.	5. A few *bacteria* and vibrios are present and one or two ordinary vibrios and microcerphymes are swimming.	6. No life.	7. No life.	8. No life.

On the Action of Heat on Protoplasmic Life Dried on Cotton Fabrics.

The papers which I have previously published on the action of heat on animalculæ life having been almost entirely of a purely scientific character, I propose in this to give a series of experiments which have a direct bearing on the question of the disinfection of fabrics and wearing apparel by the use of heated stoves, with the object of destroying animalculæ life, and, possibly, contagion.

To carry out these views, a piece of ordinary grey calico was treated chemically, and washed until free from all sizing material, and dried; this prepared cloth was then steeped in a solution of putrid albumen containing abundance of animalculæ life, wrung out, and dried at the natural temperature. It was cut into small pieces, 5 centimetres square, and each of the pieces was folded in two, rolled up, and introduced into a small strong glass tube of 6 centimetres in length and 9 millimetres in diameter; each tube was then hermetically sealed at the blowpipe.

These specimens of calico were prepared on the 8th of August, and on the 9th thirty-five small pieces were sealed in tubes. On the 10th, five of the tubes were set aside, and the remaining thirty were placed in an oil-bath and gradually heated up to 100° F., and kept at that temperature for half-an-hour. Five of the tubes were then taken out, and the temperature raised successively to 200°, 300°, 400°, 500°, and 600° F., kept at each temperature half-an-hour—five tubes being taken out at the end of each half-hour.

On the following day, one of the tubes which had been heated at each determined temperature, was opened, the cloth placed in a small quantity of water, and a quarter of an hour afterwards some of the solution so obtained was examined under the microscope, and the facts observed are specified on Table I. (page 31.)

On the 14th of August, one of the small tubes from each series was broken, and the heated cloth introduced into seven

tubes, each containing 400 grains of pure distilled water free from life, and left for three days to ascertain if the life which had resisted 200° and 300° F. was still capable of reproduction, and if the germs, as well as the animalculæ, had been destroyed by a temperature of 400°, 500°, and 600°. See Table II. for results, page 32.

A second series was made by introducing similar pieces of cloth (which had been also heated to the different temperatures already stated) into tubes containing an albumen solution, made by mixing one part of white of egg with four of absolutely pure distilled water, free from any trace of life, and these set aside for the same length of time to corroborate the above results, and the different fluids of this series were microscopically examined on the 17th of August, and Table III. shows the results, page 33.

From an examination of table I., it will be seen that life was *not* destroyed in the fabric at a temperature of 300° F., whilst *it was* destroyed at 400°, and that these results coincide with those obtained when microscopic life was submitted to heat in the fluids in which it had been generated, but differ from those observed in the experiments described in my last paper, where a fluid full of microscopic life was placed on slips of glass dried carefully, either at natural temperatures or in an air-bath at 212°, previously to being heated in the oil-bath at higher temperatures, when animalculæ life was only destroyed after 400°; these results are no doubt due to the albuminous nature of the protoplasmic life existing under these circumstances. These facts are in accordance with results obtained by my learned master, M. Chevreul, forty years since—namely, that it requires a much higher temperature to coagulate albumen when once deprived of its water than when it is in its natural state, as in an egg, and all chemists know that the coagulating-point of an albuminous solution bears a certain ratio to its degree of concentration, namely, the more it is diluted the higher the temperature required. The only explanation I can offer for the destruction of life at 300° in dried calico, is that the fabric having been, as will be seen further on, softened at 300°, has undergone a certain amount of decomposition, a part of its elements have united together to produce water, which, becoming high-pressure steam at the temperature at which I operated, has coagulated the albumen, which enters into the composition of vibrios, etc.

It will also be remarked that the amount of life in the cloth heated to 300° is similar to that on the plates of glass—viz., that

in both instances it is much more abundant than when such animalcules were heated in the fluids in which they had been generated, which illustrates the fact that the larger the proportion of fluid which exists in the animal itself, the lower is the temperature at which life is destroyed.

As it was interesting to know what changes the calico had undergone at the various temperatures above cited, one of each of the small tubes which had been heated to the different temperatures was broken, and its contents carefully examined. The piece of fabric which had not been heated at all and those which had been heated to 100° and 200° F. were quite sound; whilst that heated to 300° was of a slightly brown colour, much injured, and for all practical purposes completely spoiled. At 400° F. the cloth was very much charred, almost black, and crumbled easily into powder when rubbed between the fingers; and the tubes contained a large volume of gases, which escaped when it was open, owing to the destruction of the cloth. At 500° F. the cloth had been completely carbonised, and the tube contained also much gas. At 600° F. the cloth was completely converted into charcoal, and various hydrocarbon gases generated.

The above results satisfactorily demonstrate that germ life existing in a fabric is not completely destroyed at 300° F., whilst it is at 400°. These results not only confirm those previously obtained, but establish the fact that *a temperature which will not destroy germ life is quite sufficient to materially injure cotton fabric.* From these carefully conducted experiments, I am led to the conclusion that no beneficial results can be obtained by the employment of public stoves as a means of destroying germ life and probably contagion.

TABLE I.—Results of the examination of the water solution into which the fabrics had been dipped immediately after they had been subjected to the various temperatures.

Water used for experiment.	Cloth not heated.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1. No vibrios nor bacteria.	2. A considerable quantity of vibrios are present, and moving to and fro, but no swimming vibrios.	3. About the same quantity of life as in 2, and about the same amount of vitality.	4. There are fewer vibrios present than in 2 or 3, but a large quantity still exists, and they possess about the same vitality as in columns 2 or 3.	5. Considerably less life than in 4, but several distinct vibrios are present in each field moving to and fro.	6. No life.	7. No life.	8. No life.

TABLE II.—*Water Series.*—Examinations of the fluids into which some of the heated fabrics had been introduced on the 14th August, 1871; examined microscopically on August 17th.

Water alone for comparison.	Cloth. Not heated.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1.	2	3.	4.	5	6	7.	8.
No vibrios nor *bacteria*.	The whole of each of the fields of the microscope is covered with long, ordinary, and small-sized *bacteria*. Three or four ordinary, long, and small vibrios are swimming in each field.	Thirty to forty long *bacteria*, and three or four long swimming vibrios are present in each field.	Forty or fifty long, ordinary, and small *bacteria* are present under some of the fields, and very large shoals of *bacteria* under others. One or two long vibrios are swimming about.	The lower part of each field is covered with ordinary, long, and small *bacteria*. And a considerable number of long vibrios are present.	No *bacteria* and no vibrios present.	No *bacteria* and no vibrios present.	No *bacteria* and no vibrios present.

TABLE III.—*Albumen Series.*—Examination of the albuminous solutions into which had been introduced the heated fabrics on the 14th August, 1871, and examined on August 17th.

Albumen solution alone for comparison.	Cloth. Not heated.	Heated to 100° F.	Heated to 200° F.	Heated to 300° F.	Heated to 400° F.	Heated to 500° F.	Heated to 600° F.
1.	2.	3.	4.	5.	6.	7.	8.
No vibrios present.	Twenty to thirty ordinary vibrios and microzyms, and a small number of long vibrios are swimming about in all directions.	Nine or ten ordinary swimming vibrios, and four or five long vibrios are moving to and fro in each field.	About the same amount of life is present here as was observed in No. 3.	Four or five long swimming vibrios, and some vibrios joined together float in each field.	No vibrios present.	No vibrios present.	No vibrios present.

On the Relative Power of Thirty-four Substances to Prevent the Development of Protoplasmic and Fungus Life.

To carry out this series of experiments, test-tubes having a length of about fifteen centimetres and a diameter of one and a half centimetres, were rinsed with hot vitriol, washed thoroughly with water, and heated to dull redness.

Into each of the tubes was introduced ·026 grammes of one of the substances, the action of which I desired to study. To it was added 26 grammes of a solution of albumen, containing one part of white of egg in four parts of *pure distilled water*, prepared as described in my paper on protoplasmic life.

The reasons why I employed one part of substance in a thousand parts of fluid are two-fold. First, the employment of larger proportions of substance would, in some instances, have coagulated the albumen. In the second place, it would have increased the difficulty of observing the relative powers of the most efficacious antiseptics in preventing the development of the germs of putrefaction or decay.

In order the more thoroughly to mix the substances in the tubes, a small glass stirring rod was heated to redness and suspended by a copper wire at the open end of each tube, so as to allow the end of the rod to remain constantly in the liquid.

After having stirred the mixture together in each of the tubes, a drop was taken and examined under a microscope having a magnifying power of eight hundred diameters, and this operation was repeated with the contents of each tube for thirty-nine consecutive days, during which time the tubes were kept in a room, the temperature of which did not vary more than three degrees, namely, from 12.5° to 15.5° C.

I shall now proceed to describe the antiseptic power of the various substances which I have employed, and which I have divided into classes, so as to shew in a more marked manner the relative action of substances having a similar chemical composition. Thus I have grouped together the acids into one class, the alkalies into another, and so on, with the chlorine compounds, the sulphates and sulphites, etc., etc.

In order the better to illustrate the influence of the antiseptics used, I examined two solutions of albumen, one of which was

kept in the laboratory, the other in the open air. These solutions may be considered as standards of comparison.

Standard Solutions.

Substances taken.	Number of days required for development of life.	Time after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days after which mouldy odour could be detected.
Albumen solution kept in laboratory.	12	50	18	None
Albumen solution outside laboratory	5	19	None	None

The results shew that the albumen solution which was exposed outside the laboratory became impregnated with animal life in less than half the time required by that kept in the laboratory. This, I believe, is due to the more frequent change of the mass of air inside the tube, thereby introducing a larger number of germs.

It was also observed that the vibrios developed themselves with greater rapidity in the tube outside than in the one inside the laboratory, so much so that there were as many vibrios in six days in the former as in thirty days in the latter.

Many of my observations already tend to shew that in a fluid mass where there is a great amount of vibrio life, fungi do not make their appearance, and this is probably the reason why, no fungus life appeared in the albumen solution exposed outside the laboratory.

Class 1.—*Acids.*

This class is represented by:—1. Sulphurous. 2. Sulphuric. 3. Nitric. 4. Arsenious. 5. Acetic. 6. Prussic Acids.

Substances taken.	Number of days required for development of vibrio life.	Number of days after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days required before mouldy odour could be detected.
1. Sulphurous acid.	11		21	
2. Sulphuric acid.	9		9	11
3. Nitric acid.	10	50	10	23
4. Arsenious acid.	22		18	50
5. Acetic acid	30		9	50
6. Prussic acid	9	23	No fungus life appeared.	

Sulphuric, nitric, and acetic acids produced slight precipitates with the albumen solution, whilst the other three acids did not.

The mouldy odour perceptible in sulphuric acid after 11 days, had entirely disappeared after 50 days.

Sulphurous acid was the only one of this series which gave off no odour during the whole time the experiments were conducted.

It is interesting to observe that acids do not prevent the appearance of either protoplasmic or fungus life; in fact, they rather promote the growth of fungi, and in two instances, those of sulphuric and acetic acids, this growth was so abundant that after three or four weeks the entire surface of the fluid was covered with a thick mass, which in the case of sulphuric obtained the height of above a centimetre.

I have already mentioned that when fungus life becomes abundant, and covers the surface of a fluid, vibrio life gradually disappears, and at last becomes extinct.

Acids also appear to prevent the phenomena of putrefaction, but the results obtained cannot be considered decisive, the temperature at which the experiments were conducted not being that most favourable to the promotion of putrefaction. But I have made a special series of experiments on this point which I have noticed further on.

Class 2.—Alkalies and Alkaline Earths.

1. Caustic Soda. 2. Ammonia. 3. Potash. 4. Hydrate of Lime.

Substance taken.	Number of days required for development of vibrio life.	Time after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days required before mouldy odour could bedetected
1. Soda.	23	31	18	29
2. Potash.	16			
3. Ammonia.	24	50	20	None
4. Lime.	13	19	None	None

The alkalies, and especially lime, promote the development of vibrio life, and greatly facilitate the phenomena of putrefaction, and with the exception of lime, do not prevent the production of fungi.

Class 3.—Chlorine and its Compounds.

1. Solution of chlorine. 2. Chloride of Sodium. 3. Chloride of Calcium. 4. Chloride of Aluminium. 5. Chloride of Zinc.

6. Bichloride of Mercury. 7. Bleaching Powder. 8. Chlorate of Potash.

Substances employed.	Number of days required for development of life.	Time after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days when mouldy odour could be detected.
1. Solution of chlorine.	7	21	21	
2. Chloride of sodium.	14	50	50	
3. Chloride of calcium.	7	None	18	50
4. Chloride of aluminium.	10	None	21	50
5. Chloride of zinc.	None	None	50	None
6. Bichloride of mercury.	None	None	81	None
7. Bleaching powder.	7	18	16	
8. Chlorate of potash.	17	None	19	None

The results obtained with the above eight compounds are most interesting and instructive, and in several instances are quite different to those which might have been anticipated.

Thus it will be observed that only two of the chlorides prevent the appearance of protoplasmic life, and that none, not even bichloride of mercury, entirely prevent fungus life, although in the latter case it required eighty-one days for its development.

Again, when chlorine or bleaching powder are employed in the small proportion above stated, they do not prevent the production of vibrio life. It might be objected that the quantity employed is small, but that objection does not stand good, because other substances used in the same proportion exercise a far greater action than these two supposed powerful antiseptics. To ascertain its action in larger quantities, the following experiments were made:—

First, a pure solution of albumen was taken.

Secondly, the same solution, to which was added one per cent. of chloride of lime.

Thirdly, to this solution five per cent. of chloride of lime was added.

The experiments were commenced on the 12th March, 1872, and kept at a constant temperature of 80° Fahr. for several days.

No. 1. Pure solution of albumen. *After 4 days* two or three

vibrios were present, moving to and fro under each field of the microscope.

After 14 *days* the fluid had become as clear as pure water, and microscopic life was rather abundant.

After 17 *days* fungi had made their appearance on the surface, and only a small quantity of microscopic life was observed.

After 22 *days* there was a thick growth of "pennicellium glaucum" on the face of the fluid; it emitted a mouldy odour, and a few vibrios were moving to and fro under each field of the microscope.

No. 3. Five per cent hypochlorite of lime.

After 4 *days* a distinct smell of chlorine was still noticed, although the albumen had undergone decomposition, gases being given off.

After 14 *days* only one or two vibrios were observed throughout each drop; it emitted an odour of a peculiar chlorine compound. The fluid had a brownish yellow colour, and a dense precipitate at the bottom.

After 17 *days* no fungi were present; it emitted a faint odour (not putrid), and contained little or no vibrio life.

After 22 *days* it emitted still the peculiar odour before noticed, mixed with a very faint putrid one; a number of microzymes and three or four vibrios were observed moving under each field.

I have further ascertained by a long series of experiments that when 5 per cent. or larger quantities of bleaching powder are put in contact with any animal substance such as albumen, fibrin, wool, &c., a part of their carbon is converted into carbonic acid, whilst a certain portion of the nitrogen is liberated in a free state.

I may further add that when large quantities of bleaching powder have been used, but not in excess, all protein substances will still after a time enter into putrefaction.

Extraordinary as this statement may appear, I am happy to be able to confirm it by the observation of a savant who has had great experience in its use, namely, M. Devergie, General Inspector of the Cemeteries of France, and Director of the Morgue at Paris. He says that corpses decay more rapidly if placed in contact with bleaching powder, or a solution of it, than if water alone is employed.

The above facts shew that bleaching powder is of no value as a preventive of decay or putrefaction, and that the

theory that it has such an affinity for the hydrogen of germs as to destroy them, is erroneous; as hypochlorite of lime does not act on the hydrogen of animalcules and organic substances as above stated, but on their carbon and nitrogen.

Class 4.—*Sulphur Compounds.*

1. Sulphate of Lime. 2. Sulphate of Protoxide of Iron. 3. Bisulphite of Lime. 4. Hyposulphite of Soda.

Substances taken.	Number of days required for development of vibrio life.	Time after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days after which mouldy odour could be detected
1. Sulphate of Lime.	9	24	19	None
2. Sulphate of Protoxide of Iron.	7	None	15	
3. Bisulphite of Lime.	11	21	14	None
4. Hyposulphite of Soda.	11	50	18	None

The results obtained with this series are very instructive and useful, especially with reference to sulphate of protoxide of iron and bisulphite of lime, which have been employed as antiseptics for many years.

We here see that they do not prevent the presence of either vibrio or fungus life, and if no putrid odour was perceived with sulphate of iron it was doubtless due to the sulphate of iron fixing the sulphuret of ammonium by the formation of sulphuret of iron and sulphate of ammonia.

It is probable, however, that if used in much larger quantities, these two compounds might have preserved the albumen for a longer period of time.

Class 5.—*Compounds of Phosphoric Acid.*

Phosphate of Soda and Phosphate of Lime, were taken.

Substances taken.	Number of days required for development of vibrio life.	Time after which putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
1. Phosphate of Soda.	13	None	17	23
2. Phosphate of Lime.	7	26	22	None

Neither of these salts prevent the formation of vibrios or fungus life, but they present this peculiarity, that, while phosphate of lime does not prevent putrefaction, phosphate of soda does, and the reverse is the case as regards the development of the mouldy odour.

Class 6.—*Compound acting by Oxidation only.*

Permanganate of Potash.

Substance taken.	Number of days required for development of vibrio life.	Number of days before putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
Permanganate of potash.	9	50	22	None

This salt does not prevent the formation of vibrios; in fact, life increased rapidly for several days, and then appeared to be comparatively stagnant. The most interesting facts concerning this salt are that at no time could a mouldy odour be perceived, and that fifty days elapsed before any putrid smell could be detected.

Class 7.

Carbolic and Cresylic Acids, and Turpentine.

Substances taken.	Number of days required for development of vibrio life.	Number of days before putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
1. Carbolic acid.	None	None	None	None
2. Cresylic acid.	None	None	None	None
3. Turpentine	14	None	42	None

The first two products are the only ones of the thirty-four substances employed which entirely prevented the appearance of vibrio and fungus life, for eighty-one days, during which these experiments were conducted.

The employment of essential oils having been often recommended as a means of preventing putrefaction and decay, turpentine was taken as a representative of this class of substances.

It will be noticed that vibrios and fungi appeared after fourteen and forty-two days respectively, but in neither case did any disagreeable odour arise.

Class 8.—*Sulphocarbolates.*

Potash, Soda, and Zinc.

Substances taken.	Number of days required for development of vibrio life.	Number of days before putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
1. Sulphocarbolate of potash.	16	33	17	21
2. Sulphocarbolate of soda.	18	50	19	25
3. Sulphocarbolate of zinc.	None	None	27	50

This class of salts having been much recommended of late as therapeutic agents, I deemed it advisable to compare them with other well known antiseptics.

From the table given above it will be observed that the salts of potash and soda present no very remarkable features, whilst that of zinc is a most powerful agent, and yields in none of its properties to chloride of zinc, except that fungus life appears after twenty-seven days instead of after fifty as in the case of the chloride.

Class 9.

Organic compounds used in medicine. 1. Sulphate of Quinine. 2. Picric Acid. 3. Pepper.

Substances taken.	Number of days required for development of vibrio life.	Number of days before putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
1. Sulphate of quinine.	26	None	None	None
2. Picric acid.	17	None	19	65
3. Pepper.	8	None	None	None

The results given in this table are most interesting and instructive, for although none of these substances prevent the presence of animal life, still they (inert as they appear) prevent mouldiness and the odours of putrefaction.

This peculiar power of sulphate of quinine to arrest the production of fungus life deserves notice, especially when considered in connection with the remarkable efficacy which quinine exerts in cases of intermittent fever, and leads to the inference that this class of disease may be due to the introduction into the human

system of fungus germs, which would locate themselves in the spleen and create fever, and this is rendered more probable if we consider that these fevers are prevalent only in low marshy situations where vegetable decay abounds, and never appear to any extent in dry climates, even in the midst of dense populations where ventilation is bad and putrefaction is rife.

The only substance now remaining to be noticed is charcoal. The results obtained were:

Substance taken.	Number of days required for development of vibrio life.	Number of days before putrid odour could be detected.	Number of days required for development of fungus life.	Number of days before mouldy odour could be detected.
Charcoal.	6	50	21	None

I was induced to employ this substance owing to its having been much used as a purifier.

The results above obtained clearly indicate that it has no antiseptic properties, and that, as shewn by the researches of Dr. Stenhouse and myself, it prevents the emanations of putrid odours and noxious gases by its extraordinary condensing power, due to its porosity, and thus brings them in contact with the oxygen of the atmosphere, which oxidizes them and converts their elements in inodorous compounds.

In comparing carefully the above general results, I am led to class the specific action of the thirty-four substances under four distinct heads, namely, those which prevent the development of protoplasmic and fungus life; those which arrest the production of vibrio life, but do not that of fungus life: those which permit the production of vibrio life, but prevent the appearance of fungus life; and those which allow the development of both protoplasmic or fungus life.

The first class is represented by only two substances, carbolic and cresylic acids.

In the second class there are also two substances, chloride of zinc and bichloride of mercury.

The third is composed of five substances, lime, sulphate of quinine, pepper, turpentine, and prussic acid.

The fourth class is represented by the remaining twenty-five substances.

It may also be interesting to note the state of the fluids after fifty days.

The albuminous solutions containing sulphurous acid, chlorate of potash, picric acid, pepper, and turpentine, allowed the growth of vibrio and fungus life, but did not emit any unpleasant odour.

The solutions which only emitted a mouldy smell, are those of arsenious and acetic acids, the chlorides of calcium and aluminium, sulphate of protoxide of iron, and phosphate of soda.

Those which gave off only a putrid odour are nitric and prussic acids, lime, soda, ammonia, bleaching powder, chlorine solution, chloride of sodium, sulphate of lime, bisulphite of lime, hyposulphite of soda, phosphate of lime, permanganate of potash, the sulphocarbolates of potash and soda, and charcoal.

Confidence may be placed in these results, as they corroborate a previous series of experiments commenced on March 17, and continued until the beginning of July.

Many of the facts cited in the foregoing pages were so unexpected that a series of experiments was made substituting gelatine for albumen. It was begun on the 17th of May, and lasted forty-seven days.

Without entering into minute details, an outline will be here given of some of the most marked differences observed between the gelatine and albumen solutions, thus—Vibrio life appeared in two days in gelatine solution, and bacteria after four or five; during the whole time which lasted these experiments, life was far more abundant in these solutions than in the albumen ones. A distinctly putrid smell was given off after twenty-six days.

The action of bleaching powder differed in this case from its action on albumen. It took twenty-six days for life to appear as compared with seven in the case of albumen, and at no period during the twenty-seven days following was life abundant. No putrid odour was at any time emitted, but a mouldy one could be detected after thirty days.

With chlorine solution vibrio life was only observed after forty days. No putrid or mouldy smell was given off at any time.

Protosulphate of iron gave results with this solution quite different from those with albumen, which it will be remembered allowed vibrios to appear after seven days, and fungi after fifteen. With gelatine, however, neither protoplasmic nor fungus life appeared during the forty-seven days.

Arsenious acid also presented a marked difference in its action on the two solutions. With albumen twenty-two days elapsed before vibrios were present and eighteen before fungi appeared, but in the gelatine animal life appeared after two days, while fungi could not at any time be found.

The action of the other substances on the two solutions were so nearly identical that it is unnecessary to describe them here.

I may state that Mr. Robert Hamilton, F.R.C.S., published in the *Lancet*, of November 11th and 18th last, a paper having some similarity with mine, but as I confined my attention to certain particular points, while he had other objects in view, the mode of conducting the experiments differed considerably. I therefore deem it unnecessary to notice any apparent differences that may exist between our results, leaving to other experimenters to repeat and decide.

On the Relative Power of Thirty-four Substances in Arresting Putrefaction, and the Development of Protoplasmic and Fungus Life.

These experiments were undertaken as complementary to those described in my previous paper, and consisted in adding to a solution of albumen, swarming with microscopic life, one thousandth part of each of the substances already enumerated, and examining the solutions immediately upon their addition, and also after one and sixteen days.

The solutions to be tested were placed in tubes similar to those described in my last paper. The experiments were begun on the 20th September, 1871, the temperature being from 15° to 18° C. during the time they were conducted.

Table 1.

Standard Albumen Solution.

Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Life very abundant.	Little apparent difference.	Odour more intense and disagreeable, and great increase of life.	Still further increase in quantity of life, and odour very bad.

It will be observed that the amount of life and putrescence increased during the whole of the sixteen days that the experiments lasted.

Table 2. *Class* 1.—*Acids.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Sulphuric Acid.	Locomotive power of animalcules destroyed.	Only a faint putrid odour, and a small amount of life.	Mycilium fungus appeared in rather considerable quantity. Fluid had faint putrid odour. Small amount of life.	Fluid clear as water. Mycilium present on surface of fluid. Mouldy but not putrid odour. Larger quantity of life.
Nitric Acid.	Locomotive power of animalcules much impaired.	Faint putrid odour. Small amount of life.	Large amount of life, but not so great as in standard.	Odour, turbidity of fluid, and amount of life greater than standard.
Arsenious Acid.	No change.	No difference.	Odour and amount of life greater than in standard.	Odour, turbidity, and amount of life equal to standard.
Solution of Sulphurous Acid.	No change in quantity of life, but power of locomotion injured.	Odour similar to standard. Power of locomotion regained.	Odour same as on first day, but life greater than in standard.	Equal to standard in all respects.
Acetic Acid.	Decrease of life.	Faint putrid odour, but quantity of life same as previous column.	Fluid clear. Still faint putrid smell. Mycilium fungus present, and small amount of animalcule life.	The growth of fungi has stopped. Turbidity greater than in standard. Odour about the same, and quantity of life greater.
Prussic Acid.	Animalcules deprived of power of motion.	Faint putrid odour. Animalcules lively.	Fluid clearer than standard, but odour not so bad, and quantity of life less.	Same as standard in all respects.

It will be noticed in reading the contents of the above table, that four of the Acids, Sulphuric, Nitric, Acetic, and Prussic, appear to have paralysed the locomotive power of microscopic life, whilst Arsenious and Sulphurous have produced no influence. After twenty-four hours life had reproduced itself in the four first-mentioned Acids. On the sixteenth day the difference is very marked and interesting, animal life having greatly increased in Nitric, Arsenious, Prussic, and Sulphurous Acids, but had made but small progress in Sulphuric and Acetic Acids, fungus life having appeared in these two latter Acids, and increased rapidly during the remaining ten days, and thereby, as previously stated, prevents the development of animal life. (See page 13.)

Table 3. *Class* 2.—*Alkalies.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Soda.	Large proportion of animalcules injured.	Fluid more turbid and putrid than standard. Animalcules lively again. Quantity of life same as at first.	Odour and quantity of life greater than standard.	Odour, turbidity and quantity of life much greater than standard.
Potash.	The same as soda.	The same as above.	The same as above.	The same as above.
Ammonia.	No effect produced.	Fluid emitted a more offensive odour, and was more turbid than standard, and the animalcules had increased.	Fluid more turbid and offensive, life more abundant than in standard.	Exactly similar to standard.
Lime.	No difference.	Similar to standard.	Putrid odour more intense, and animalcule life greater than in standard.	Worse in every respect than standard.

Soda and potash at first impaired the power of motion of the animal life, still the alkalies, as well as lime, greatly facilitate the development of life. Ammonia had no action or influence whatever.

Table 4. *Class 3.—Chlorine Compounds.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Solution of Chlorine.	No change	All odour of chlorine has disappeared and putrid odour and life same as standard.	Odour more offensive and life greater than in standard.	Odour very bad and turbidity and life greater than in standard.
Chloride of Sodium. } Chloride of Calcium. }	.....	The same as above.	Same as above.	Same as above.
Chloride of Zinc.	Dense white precipitate produced. Life seriously affected.	White precipitate had subsided and carried with it most of the animalcule life. Fluid still possessed a faint putrid smell.	No putrid odour. Very small amount of life.	No putrid odour until stirred when a faint odour could be perceived. Very small amount of life.
Bichloride of Mercury.	White precipitate produced Animalcule life much injured.	The white precipitate had carried down with it much life, but some still remained in the liquor, and it had a faint putrid odour.	Very faint putrid odour. Large quantity of life present.	Putrid odour very faint. Turbidity equal to standard.
Bleaching Powder.	Less life than in standard.	Little or no putrid odour.	Distinct putrid odour. Fluid not so turbid as standard, nor life quite so abundant.	Same as standard.
Chloride of Aluminium.	Same as standard	Putrid odour & amount of life not quite equal to standard.	Putrid odour not so bad as standard.	Equal to standard.
Chlorate of Potash.	Same as standard	Same as standard.	Odour not so bad, and life considerably less than standard.	Same as standard.

The above results show that immediately after the addition of the above solutions to putrid albumen, the chlorine solution had no action, bleaching powder but a slight one, and only chloride of zinc, and bichloride of mercury, especially the latter, affected germ life. After twenty-four hours all odour of chlorine had disappeared from the solution, but that the putrid odour and amount of life had not decreased. With bleaching powder little or no putrid odour remained.

With chlorides of zinc and mercury the greater part of life had disappeared, only a slight putrid odour remained, and this continued for the whole of the sixteen days (this fact distinguishing them from the rest of the class), whilst the chlorides of sodium, calcium and aluminium, and chlorate of potash gave off an odour as intense as the standard. On the sixteenth day the chlorine and bleaching powder solutions had become putrid, and the amount of life was equal to the standard. I consider this latter result to be as interesting and remarkable as those to which I called attention in my first paper, as they confirm the fact that the powerful antiseptic and disinfecting powers which have been ascribed to them, do not exist, and I have come to the conclusion that they are merely deodorisers, and that only when employed in large proportion, as compared with the amount of organic matter to be acted upon.

Table 5. *Class 4.—Sulphur Compounds.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Sulphate of Lime.	No action.	No change.	No change.	
Sulphate of Iron.	Life very slightly decreased.	Odour and life are less. The presence of sulphuret of iron was distinctly visible.	Did not appear to have changed during the six days.	Putrid odour same as in standard, quantity of life rather greater. A large amount of sulphuret of iron had been produced.
Hyposulphite of Soda.	Equal to standard.	Equal to standard.	Odour more repulsive, and quantity of life greater than standard.	Putrid odour, turbidity and life equal to standard. Trace of sulphuretted hydrogen liberated.
Bisulphite of Lime.	Equal to standard.	Similar in all respects to standard.		

This class is interesting, two of the substances having been largely employed by medical men, namely, sulphate of protoxide of iron and bisulphite of lime, as the above results clearly show that this iron salt, like chlorine and bleaching powder, is a mere deodoriser and not an antiseptic, while bisulphite of lime employed in the above proportion possesses neither antiseptic nor deodorising properties; but probably, if employed in larger proportions, the compounds would doubtless be found to possess a certain antiseptic power.

Table 6. *Class 5.—Phosphate Compounds.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Phosphate of Soda.	Same as standard.	Same as standard.	Odour and life more abundant than in standard.	Odour and turbidity nearly equal to standard, but quantity of life greater.
Phosphate of Lime.	Similar to standard.	Life apparently equal to standard, but odour more disagreeable.	Similar to standard.	Similar to standard.

This class calls for no remark.

Table 7. *Class 6.—Permanganate of Potash.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Permanganate of Potash.	Part of permanganate decomposed. Amount of life equal to standard.	No putrid odour. Life equal to standard.	Slightly putrid odour. Life rather more abundant than in standard. All permanganate decomposed.	Equal to standard. All permanganate decomposed.

On the admixture of the highly-coloured solution of salt with putrid albumen it was partly discoloured, but the amount of life did not appear decreased. After twenty-four hours all odour had disappeared, but after sixteen days the fluid was as full of life and had an odour as strongly putrid as the standard. These facts tend to prove that this salt is a deodoriser by oxidation, but not an antiseptic.

Table 8. *Class 7.—Tar Series.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Carbolic Acid.	Animalcule life very much injured.	Faint putrid odour. Amount of life very small.	No putrid odour. Amount of life still very small.	Same as six days.
Cresylic Acid.	Life very much injured.	No putrid odour. Life almost destroyed.	No putrid odour. Life almost destroyed.	Same as six days.
Turpentine.	Same as standard.	Same as standard.	Identical to standard.	

Although carbolic and cresylic acids prevent the production of vibrio life for any length of time, still they do not completely destroy it when once it has existed. The only class of life, however, which remains, is a small black or opaque vibrio, the same and the only one which can resist a temperature of above 300° C.; no change occurred during the sixteen days. Cresylic acid appears to be slightly more antiseptic than carbolic acid, as all putrid odour disappeared from the liquid. On comparing these results with those recorded in the previous paper, there can be no doubt but that these two bodies possess antiseptic properties of a high degree.

Although turpentine prevents, as we have already shewn, the development of life for a certain period, still it does not appear to affect that life when once formed.

Table 9. *Class* 8.—*Sulphocarbolates.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Sulphocarbolate of Soda.	Animalcule life appeared to be injured.	Putrid odour and turbidity greater than in standard, and animalcules had recovered their vitality	Similar to standard.	Similar to standard.
Sulphocarbolate of Potash.	Animalcule are not so much injured as with soda salt.	Similar to above.	Similar to standard.	Similar to standard.
Sulphocarbolate of Zinc.	Animalcules nearly all destroyed.	Only faint putrid odour. In other respects equal to previous column.	Very faint putrid odour. Very small amount of life.	Very faint putrid odour. Very small amount of life.

The only salt which appears to act actively on microscopic life is that of zinc, and also is the only one of the series which prevents the development of protoplasmic life and putrefaction. As this compound is inodorous its employment deserves, I believe, the serious attention of medical practitioners.

Table 10. *Class* 9.

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days
Sulphate of Quinine.	Power of locomotion of animalcules almost destroyed.	Slight putrid odour. Power of locomotion not entirely recovered	Faint putrid odour. Large quantity of life.	Faint putrid odour. Large quantity of life.
Picric Acid.	The same as above.	Slight putrid odour. Locomotive power partially regained.	Equal to standard	Equal to standard.
Pepper.	Same as standard.	Similar to standard.	Same as standard.	Identical to standard.

It is very instructive to compare the results obtained with these substances with those obtained with the same class of compounds in our previous paper. Sulphate of quinine and picric acid on being added to the putrid albumen solution, appear to deprive in a marked manner the vibrios of their power of locomotion, but after twenty-four hours they partially recovered their vitality, and after sixteen days, little difference exists between the standard solution and the above, thus showing that these substances, as well as pepper, have little or no action on germ life once developed.

Table 11.

Class 10.—*Charcoal.*

Name of Substance.	Examination immediately after mixing.	Examination after one day.	Examination after six days.	Examination after sixteen days.
Charcoal.	Equal to standard.	Less odour than in standard, but more animalcule life.	Less odour than in standard, but more animalcule life.	Less odour than in standard, but more animalcule life.

It will be observed, in looking over this table, that the results correspond with those described in the previous paper, that carbon does not act on vibrio life, and that its properties are merely those of a deodoriser.

Some of the results recorded in this paper were so contrary to the generally received opinion as to the power of the substances to arrest decay, that I repeated them. I therefore made second trials with the following substances, viz.: chlorine solution, bleaching powder, sulphurous acid, bisulphite of lime, and protosulphate of iron, but the results were in every case in accordance with those herein described.

It is a remarkable fact that it is easier to prevent life from appearing in a fluid highly favourable to its production, than to destroy it entirely when once developed. I have in no instance succeeded in destroying it in the latter case, whilst in several instances it was prevented in the former.

On the Relative Powers of Various Substances to Prevent Putrefaction.

The two series of experiments above described suggested to me a third, namely, the time which would elapse before a solution of albumen when kept at a temperature of between 70° and 80° F. would acquire a distinct putrid smell, and also the time necessary for sulphuretted hydrogen and ammonia to be given off.

The following table gives a *resumé* of the results obtained during the eighty days which the solutions were examined.

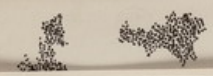
Time required for Development of Putrid Odours in Albumen Solutions kept at 80° F.

1. Standard Solution.		5. Sulphur Compounds.	
Albumen kept in laboratory for comparison.	16 days.	Sulphate of lime.	14 days.
2. Acids.		Protosulphate of iron	16 ,,
Sulphurous acid.	45 ,,	Bisulphite of lime.	16 ,,
Sulphuric acid.	16 ,,	Hyposulphite of soda	11 ,,
Nitric acid.	16 ,,	6. Phosphates.	
Arsenious acid.	None	Phosphate of soda.	16 ,,
Acetic acid.	None	Phosphate of lime.	16 ,,
Prussic acid.	35 ,,	7. Permanganate of potash	11 ,,
3. Alkalies.		8. Tar Series.	
Caustic soda.	72 ,,	Carbolic acid.	None
Caustic ammonia.	26 ,,	Cresylic acid.	None
Caustic lime.	14 ,,	9. Sulphocarbolates.	
4. Chlorine Compounds.		Sulphocarbolate of potash.	35 ,,
Solution of chlorine.	16 ,,	Sulphocarbolate of soda.	26 ,,
Chloride of sodium.	16 ,,	Sulphocarbolate of zinc.	None
Chloride of calcium.	11 ,,	10. Sulphate of quinine.	None
Chloride of aluminium	16 ,,	Picric acid.	26 ,,
Chloride of zinc.	38 ,,	Pepper.	16 ,,
Bichloride of mercury	None	Turpentine	35 ,,
Chloride of lime	9 ,,	11. Charcoal.	
Chlorate of Potash	38 ,,		

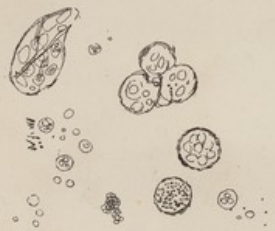
These results show that out of the thirty-four substances which I employed only six prevented putrefaction. From the previous experiments it is not surprising that carbolic and cresylic acids, bichloride of mercury, and the salts of zinc should prevent putrefaction, but it is certainly very remarkable that sulphate of quinine should possess that property to such a high degree.

The results here described confirm in a general way those given in the first of this series of papers. (See pages 34 to 44.)

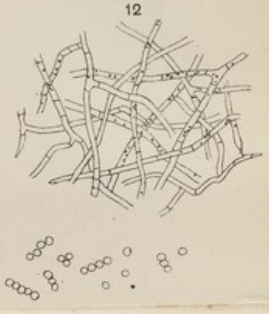
I cannot conclude these papers without expressing my sincere thanks to my assistant, Mr. William Thomson, for his valuable and intelligent help in these long and tedious investigations.

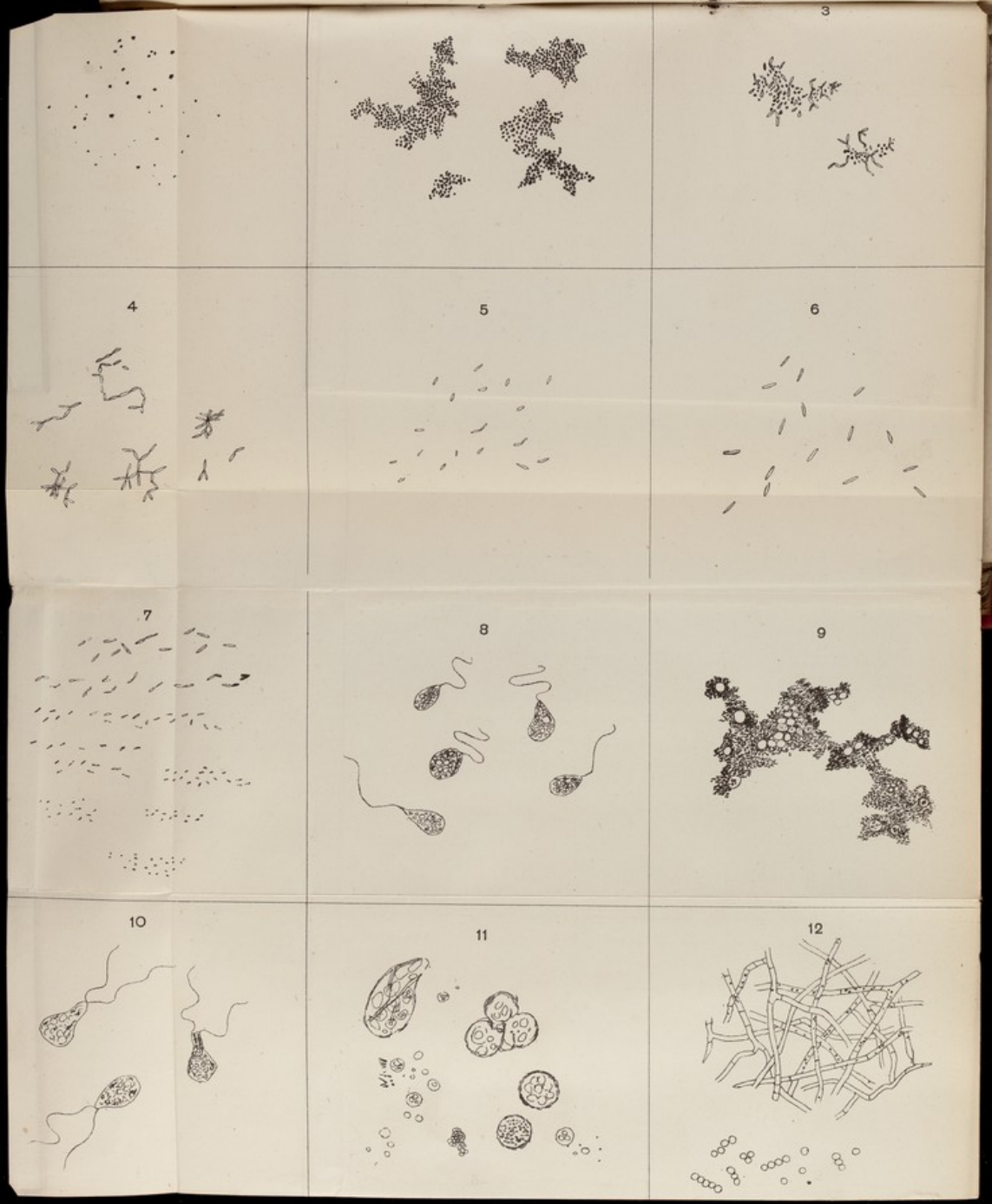


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THE EVOLUTION OF LIFE:

REVIEW OF DR. BASTIAN'S BOOK ENTITLED "THE BEGINNINGS OF LIFE" (a).

THESE volumes constitute a very able and elaborate defence of the doctrine of the evolution of life, hitherto very generally known as Heterogeny, but now styled by Dr. Bastian "Archebiosis," and a complete array of experimental and other evidence in its favour. The case of the heterogenists in the present state of public opinion on philosophical and religious subjects, has to be conducted in the face of almost overwhelming prejudices, which will long prevent the evolutional

(a) "The Beginnings of Life." By H. Charlton Bastian, M.D., &c.; 2 vols. Macmillan. 1872.

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Of the sanitary authorities constituting the union, only seven

theory of life from being generally accepted. But such advocacy as that of Dr. Bastian must have great influence in reconciling to that theory the rising generation of students and thinkers. Were those prejudices, however, to be set aside, there would remain little serious opposition of a scientific character to the doctrine in question, except that of the French *savant*, Pasteur, who may be considered the apostle of the contrary doctrine of panspermism. Almost all other observers with whose writings we are acquainted concur in supporting the doctrine of heterogeny. Pouchet, Joly, Musset, Mantegazza, Wyman, Schaffhausen, Bennett, Meunier, Child, Pennetier, Cantoni, Frémy, Sanderson, Le Gros, Onimus, and Garner, as well as Bastian, have all been constrained by the nature of the results obtained by them from their experimental researches, to declare more or less decidedly in favour of evolution. But Pasteur, owing to his manipulative accuracy and ability in stating his case, is in himself a host. The trustworthiness of his experiments has never been seriously doubted; and if the decision of the question were made to rest on his results as interpreted by himself, he would undoubtedly be pronounced to have made good his position (*a*). Other observers, however, have not been content merely to repeat Pasteur's experiments; they have devised new ones, the results of which seem to entitle them to give an interpretation to those of Pasteur very different from that put forward by him. Thus, the celebrated Alpine experiments, contrived for the purpose of showing that aerial germs, which, according to the panspermists, abound in

(*a*) "Mémoire sur les Corpuscles Organisées dans l'Atmosphère: Examen de la Doctrine des Générations Spontanées," par L. Pasteur; *Annal. Scien. Nat.*, partie zool., t. xvi., pp. 6-98; Paris, 1861. "Expériences et Vues Nouvelles sur la Nature des Fermentations," par L. Pasteur; *Comp. Rend.*, t. lii., pp. 1260-64; 1861.

population and wealth, variety of soil, agricultural industry and climate, must naturally hold a very important position.

Of the sanitary authorities constituting the union, only seven

the atmosphere at low levels, are absent at great altitudes, and wherein boiled and hermetically-sealed flasks, containing organic infusions, exhibited no living organisms after being made to communicate with the external air, when repeated with more concentrated infusions, by MM. Joly and Musset, acting in concert with M. Pouchet, manifested an abundance of infusorial life (*b*). The latter observer, who is entitled to be considered the leader of the modern heterogenists, had previously demonstrated that the production of organisms, and the peculiar character of those produced were to a great extent determined by the nature of the substances entering into the composition of the infusions or organic solutions, and not by any germs conveyed into them from the air to which

(*b*) "Expériences sur l'Hétérogénie Exécutées dans l'Intérieur des Glaciers de la Maladetta (Pyrénées d'Espagne)," par MM. Joly et Musset; *Comp. Rend.*, t. lvii., p. 538-61, 1863.

they were exposed after the boiling process (*c*). He found, for instance, that when three vessels severally charged with urine, cider, and a mixture of both, were exposed side by side, three distinct kinds of organisms were obtained, but that which occurred in the mixed fluid was a hybrid between the two others, deriving its mongrel qualities, clearly not from the air, but the liquid menstruum. Again, Professor Wyman, of Cambridge U.S.A., by working with small quantities of infusion in proportion to the size of his flasks, and consequently with relatively greater volumes of calcined air, hardly ever failed to develop organisms in experiments which, in the hands of Pasteur, had produced none; but found that when the boiling, which Pasteur was content to continue for ten minutes, was prolonged for five or six hours, no

(*c*) "Hétérogénie," par F. A. Pouchet; Paris, 1859. "Nouvelles Expériences sur la Génération Spontanée," par F. A. Pouchet; Paris, 1864.

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organisms were ever produced, even under the above apparently favourable conditions (*d*.) The conclusion arrived at by Dr. Hughes Bennett was that his experiments were totally adverse to the atmospheric germ theory, and indicated that the production or non-production of infusorial organisms, so far as the air was concerned, depended rather on the temperature, density, and other physical qualities of that medium than on the presence therein of infusoria or their germs (*a*). Dr.

(*d*) "Experiments on the Formation of Infusoria in Boiled Solutions of Organic Matter enclosed in Hermetically Sealed Vessels and supplied with Pure Air," by Jeffries Wyman, M.D.; *Silliman's Amer. Journ. Scien. and Arts*, vol. xxxiv., pp. 79-87; New Haven, 1862. "Observations and Experiments on Living Organisms in Heated Water," by Jeffries Wyman, M.D.; *Sill. Amer. Journ. Scien. and Arts*, vol. xliv., pp. 152-69; New Haven, 1867.

(*a*) "The Atmospheric Germ Theory," by John Hughes Bennett, M.D.; *Edin. Med. Journ.*, vol. xiii., pp. 810-33; March, 1868.

Burdon Sanderson, by an extensive and accurate series of experiments, has more recently thrown considerable additional light on the generation of the more constantly found of the organisms under consideration, namely, bacteria (wherewith vibrios are generally associated), to which he gives the name of microzyms. According to him, "liquids which contain no particle distinguishable under the highest powers of the microscope can often be proved to possess the property of evolving microzyms [after being boiled in sealed flasks] without contact with external media," but "no amount of exposure [of the liquids] has any effect in determining the evolution of microzyms, the air being entirely free from such organisms" (*b*). By his elaborate

(*b*) "The Origin and Distribution of Microzyms (bacteria) in Water and the circumstances which Determine their Existence in the Liquids and Tissues of the Living Body;" *Thirteenth Report of Med. Off. of Priv. Coun.* for 1870, pp. 40, 59; London, 1871.

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Of the sanitary authorities constituting the union, only seven

researches, Dr. Bastian has confirmed the above observations, and has moreover adduced numerous proofs that when the boiled infusions are sufficiently strong and of a highly fermentable order, infusorial life is freely evolved, even *in vacuo*, the absence of pressure being, according to him, a condition peculiarly favourable to such evolution. Of his many experiments perhaps none is more conclusive than that distinguished as No. 6, wherein "active protamœbæ and ciliated monads were taken from a hermetically sealed flask, which eight weeks previously had been exposed to a temperature of 270°—275° F.; and these very organisms were killed afterwards by the temperature of 140° F." (*c*).

It would seem, therefore, that Pasteur had not succeeded in grasping the whole truth, although what he did lay hold of

(*c*) "Beginnings of Life," vol. i., p. 475.

was true, so far as it went. Thus, that most telling of all his experiments, wherein certain infusions contained in flasks having long drawn-out and bent but unsealed necks, after boiling, could be kept for an indefinite period without producing organisms, so long as the necks remained intact, but gave rise to them after standing a day or two when the necks were broken off short, so as to put the contents in free communication with the atmosphere, only proved that the particular infusion used was such as would not produce infusorial life when shut out from the external air after being boiled. But this negative result cannot be accepted as proof that different organic infusions will behave in the same manner, in the face of the experiments of other observers which have given positive results with other infusions, or by working under different conditions.

In addition to the weighty direct evidence which can be brought forward against pan-

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spermism, in the shape of experiments by independent observers, wherein organisms were actually obtained under the observance of precautions against the intrusion of "germs" even greater than those insisted upon by Pasteur, there rise up on all sides against that doctrine, arguments based on the difficulty of admitting the atmosphere to be constantly loaded with the seed of all the various organisms to which pansperm-ists attribute the different kinds of fermentation and putrefaction. According to Pasteur, each fermentive process has its own peculiar air-borne ferment, in the absence of which it cannot arise. There is one for the vinous (alcholic), another for the vinous (acetic), another for the cerevisic, another for the malic, another for the tartaric (calcic tartrate , another for the lactic, another for the butyric, another for the agrostic (hay infusion), another for the urinous (ammoniacal), another for the albuminous (ordinary putrefaction), and so

on; *(a)* and one and all, or their supposititious germs, are constantly present in the atmosphere to such an extent as to be ever ready severally to produce their peculiar transforming actions. It might be just possible to believe this in the case of liquids freely exposed to the air, whence might be derived all the various germs supposed to be floating therein, and among them perchance the particular kind suited to produce the fermentation peculiar to the recipient fluid. But, in such a case, for instance, as the occurrence of ammoniacal urine in the bladder from the partial

(a) "Etudes sur les Mycodermes: rôle de ces plantes dans la fermentation acétique," par L. Pasteur; *Comp. Rend.*, t. liv., pp. 265-70; 1862. "Nouvel exemple de fermentation déterminée par des animalcules infusoires pouvant vivre sans gaz oxygène libre, et en dehors de tout contact avec l'air atmosphérique," par L. Pasteur; *Comp. Rend.*, t. lvi., p. 416; 1863. "Recherches sur la Putréfaction," par L. Pasteur; *Comp. Rend.*, t. lvi., p. 1189; 1863. Pasteur en réponse à Frémy; *Comp. Rend.*, t. lxxiv., pp. 404-9; 1872.

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retention caused by an enlarged prostate, how is it possible to admit the invasion of atmospheric ferment germs as its cause? Is the urethra in this instance, occluded as it is by the pressure of the prostate gland, not much more impervious to the air than the drawn-out and bent necks of Pasteur's flasks? Nevertheless we are required, by that authority and his followers, to believe that the evolution of ammonia from urine cannot take place except by the agency of air-borne ferment germs, and those, too, of one particular kind out of the many which the atmosphere is imagined always to have in stock!

But for what is called the "germ theory of disease," which attributes all the communicable distempers to parasitic fungi, and such like, the question of spontaneous generation would have little direct interest for the Medical Profession. It is true that in the evolution of the lowest forms of life there is something remotely like the

evolution of the infectious diseases. There is no trustworthy evidence, however, in favour of the notion that those diseases are due to the presence of independent living organisms of any kind, nor even to the setting up in the body of a process which can, except by a figure of speech, be compared to fermentation and called "zymosis." Few but unqualified observers or shallow pathologists could continue long to entertain the idea that the spreading diseases are of parasitic origin. This remarkable minority, nevertheless, exists (*b*). But it is worthy of remark that many of the

(*b*) "Le typhus, le choléra, la fièvre jaune, la dyssentérie, les fièvres intermittentes, et la pourriture de l'hôpital; sont-ils dûs aux infusoires qui jouent le rôle de ferments?" par J. Lemaire; *Comp. Rend.*, t. lxvii., pp. 653-56; 1868. "Recherches sur le rôle des infusoires, pour servir à l'histoire de la pathologie animée," par J. Lemaire; *Comp. Rend.*, t. lxvii., pp. 739-42; 42; 1868. "The Correlation of Putrefaction, Fermentation, and Morbid Infection," by A. E. Sansom, M.D.; *Brit. Med. Jour.*, Nov. 19, 1870.

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writers who believe in the parasitism of infection, and regard panspermism with favour, have been previously committed in favour of carbolic acid as a sanitary or medicinal agent. Thus, a very prominent authority on disinfection, who has warmly eulogised Pasteur, was patentee of a carbolic disinfectant (*c*). The chief apostle of the germ theory of disease in France is Lemaire, who is the author of a bulky volume in praise of carbolic acid (*d*), as well as a fervid advocate of panspermism. And in this country the principal supporters of the germ pathology are an eminent surgeon who has played a pro-

(*c*) "Improvements in deodorizing and disinfecting sewage and other offensive matter, also applicable to deodorizing and disinfecting in general," by R. Angus Smith, Doctor of Philosophy; and Alexander McDougall, manufacturing chemist; *Specifications of Patents*, No. 142, A.D., 1854.

(*d*) "De l'acide phénique, et de son action sur les végétaux, les animaux, etc.," par Jules Lemaire; Paris, 1863.

minent part in popularising antiseptic surgery by means of carbolic acid; a London physician, who claims to have made a valuable discovery in recognising the therapeutic properties of the sulpho-carbolates (*a*); and a provincial Professor, who is best known as a manufacturer of carbolic acid.

Not content with the part which he has successfully performed in the manufacturing world as the chief producer of carbolic acid, the latter of these authorities has taken upon himself to play the additional part of experimental exponent of the philosophy of putrefaction and infusorial life in connection with the germicidal virtues of that product. In opposition to what has been observed by all other experimenters on the subject, he imagines himself to have proved that there is found in putrefying liquids "a small black vibrio"

(*a*) "The sulpho-carbolates in Medicine," by Arthur E. Sansom, M.D.; *The Practitioner*, July, 1869.

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which retains its vitality after exposure to the temperature of 300° F.; and in virtue of the charmingly simple notion that disease germs are of the nature of vibrios, jumps to the conclusion that disinfection by heat cannot be relied on for the destruction of the contagia which hang about infected clothing, whereas carbolic acid can *(b)*. And this in the face of the celebrated experiments on actual contagia by the late Dr. Henry, of Manchester, whereby he established that the activity of vaccine matter was completely destroyed by a temperature of 140° F., and that the virus of scarlet fever was rendered inert by a temperature of 204° F. *(c)*. Although

(*b*) "On the action of heat on germ life," by F. Crace Calvert, F.R.S.; *Report British Assoc. Prom. Science*, for 1871, pp. 122-23.

(*c*) "Experiments on the Disinfecting Powers of Increased Temperatures," by William Henry, M.D., F.R.S.; *Phil. Mag. and Ann. Science*, vol. x., pp. 363-69; Lond., 1831. "Further Experiments on the Disinfecting Powers of increased Temperatures," by

the value of disinfection by heat could not thereby be rendered doubtful, were the said "small black vibrio" even proved to be a veritable salamander, it is satisfactory to know that Dr. Burdon Sanderson (*d*) has shown that microzyms are killed by dessication alone at so low a temperature as 108° F., and that Dr. Bastian (*e*) has confirmed his conclusions on this point; and that, moreover, according to Pasteur (*f*) himself, vibrios die immediately they come in contact with air (*g*).

William Henry, M.D., F.R.S.; *Op. cit.*, vol. xi., pp. 22-31; 1832.

(*d*) Sanderson: *Loc. cit.*, pp. 49, 50.

(*e*) Bastian: "Beginnings of Life," vol. ii., p. 5.

(*f*) Pasteur: "Recherches sur la Putréfaction;" *Comp. Rend.*, t. lvi., p. 1190; 1863.

(*g*) In respect to this, Calvert reports results the very opposite of Pasteur's. Having experimented with atmospheres composed of various gases, he found that "oxygen is an essential element in the production of putrefactive vibrios." ("On Putrefaction," by F. Crace Calvert, Ph. D., F.R.S.; *Proc. Roy. Soc.*, vol. xx.,

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The question of the origin and growth

p. 186; 1872.) But Pasteur has stated that *all* vibrios are killed by contact with oxygen. His words are:—"J'ai reconnu que *tous* les vibrios peuvent vivre sans gaz oxygène libre, et qu'ils périssent au contact de ce gaz." ("Recherches sur la putréfaction;" *Comp. Rend.*, t. lvi., p. 1190; 1863.) May it not be surmised that the main result arrived at by Calvert from his experiments was but a foregone conclusion in accordance with the maxim, "nothing like leather." Pasteur, anyhow, cannot be suspected of such bias. His statement as to the destructive effects of oxygen on vibrios, which can hardly be disputed by those who swear by his other statements, would, if there were truth in the fanciful notion that disease germs are a kind of vibrio, afford a very conclusive argument in favour of the superior efficiency of the direct oxidizing disinfectants, such as Ozone, Condy's Fluid, &c. Not that oxygen stands in need of support of this circuitous kind; for, it is evident that so long as the vivifying and purifying action of the air continues to be performed by that all-potent element, we may have every confidence in it as the most efficacious of disinfecting agents. It will be time enough to give up oxygen and turn to carbolic acid, when the atmosphere shall be found to possess the latter substance as one of its components.

of the lowest infusorial organisms is still in a somewhat hazy state. The conditions of the experiments that have been made for the purpose of determining it, have not been sufficiently alike to render the results strictly comparable. More precision is necessary. All the modifying conditions ought to be noted. We would suggest that future observers should take exact account of the following conditions of their experiments:

Description of the substances of which the infusions are composed.
Proportions of such substances relatively to the quantity of water.
Specific gravity of the infusions.
Kind and size of vessels.
Relative proportions of air and infusion in the vessels.
Temperature at which the infusions are boiled before sealing the vessels.
Length of time the boiling is continued.
Temperature at which the vessels are kept after the boiling.
Length of time they are kept.

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Whether kept in the dark or in the light.
Whether kept motionless or occasionally moved or shaken.
Power of microscope used in the examination of the infusions.
Appearance of the corpuscles seen (if any).
Amount of Brownian movement seen in them.
Amount and nature of so-called "vital" movements.

And when all this is done, there will remain for consideration the question—Are such infusorial organisms as bacteria independent living beings, in any other sense than that in which blood corpuscles are so considered?

Strongly impressed with the value of Dr. Bastian's writings, and with his competence as an investigator of the intricacies of protoplasmic organisation, we take leave of his work with the following quotation, which summarises some of his principal conclusions: — "The evidence we have gathered together tends to show, that the differences which exist between various kinds of matter depend, in the main, upon

differences in molecular structure or mode of aggregation. This conclusion is forced upon us by the phenomena of allotropism and isomorphism, by the consideration that thousands of wholly different substances are compounded merely of carbon, hydrogen, and oxygen, in similar or different proportions, and by multitudes of other facts of a like nature. Some of these aggregates are stable, whilst others are highly unstable. Slight external influences suffice to alter the crystalline form of certain bodies, some of which, such as mercuric iodide, undergo the most remarkable changes. Such alterations are all passages from one mode of statical aggregation to another mode of statical aggregation. And yet crystalline matter is often capable of undergoing a very different kind of rearrangement by which it is converted into a colloid (*a*). The colloid is distinguished by

(a) The inverse rearrangement by means of which blood corpuscles are transformed into "blood crystals,"

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its extreme mutability; its existence is a continual metastasis. It is, in fact, a dynamical state of matter. Further aggregations and rearrangements may take place amongst its molecules, and give rise to other forms of matter possessing the mutability which distinguishes colloids in a more eminent degree, and to such an extent as to enable them to carry on a *continuous* series of molecular changes, in response to the incidence of mere ordinary physical forces. This is, however, but a further degree of complexity in a direction already indicated. All intermediate degrees of molecular mobility may be traced (amongst various crystalline and colloidal states of matter) between the distantly successive

in blood when kept shut up in a flask, is very significant of a close relationship between crystallisation and initial organisation. *Vide* Pasteur: "Examen du rôle attribué au gaz oxygène atmosphérique dans la destruction des matières animales et végétales;" *Comp. Rend.*, t. lvi., p. 739; 1863.

changes from one to another mode of polar equilibrium which is alone possible with the majority of crystals, and the continuous changes of living matter. The lapse from one mode of statical equilibrium to another, if it take place with sufficient rapidity and be associated with a concurrent process of growth, will give rise to that continuous series of molecular changes which characterise what we know as 'living' matter. And yet the molecular aggregate which displays this responsive mobility and power of self-division—because it has been called a living thing, and because theoretical notions have been formed concerning 'life'—has been supposed to be separated from other closely-related kinds of matter by an impassable gulf" (*b*).

(*b*) "Beginnings of Life," vol. ii., pp. 117-19.

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First (General) Report

ON THE

SANITARY CONDITION

OF

WEST KENT SANITARY DISTRICT.

A report based only on the few concluding months of a year, the duties of which partook more of initiation and organization than routine, must necessarily be deficient in those varied and essential facts hereafter to be relied upon as marking the sanitary condition, progress, or deterioration of a district. Some practical view, however, of the work to be done, obtained by actual investigation, may usefully supplement this paucity or supply their absence.

Among the larger areas recently formed into sanitary districts under the Public Health Act of 1872, that of West Kent, from its size and importance, its proximity to the metropolis, its population and wealth, variety of soil, agricultural industry and climate, must naturally hold a very important position.

Of the sanitary authorities constituting the union, only seven

had completed their arrangements for joining on the appointment of the medical officer of health on the 25th of August last. Three have since entered, and also an entirely new member, cordially admitted from considerations of mutual advantage. One more, taking part in the original scheme, remains yet outside; but it may well be doubted if any further increase can, for the present at least, be either advantageous or desirable.

The district consists of the following enumerated unions and townships, to which the acreage, number of houses, and population, at the census of 1871, are subjoined, viz.:—

West Kent Sanitary District.	Acres.	Houses.	Population.
Bromley Union	40,338	5,924	32,184
„ Town			
Sevenoaks Union	67,293	4,772	24,262
„ Town			
Tunbridge Union*	40,909	4,163	21,790
„ Town			
Cranbrook Union	41,436	2,781	14,005
Tenterden Union	47,325	2,241	10,871
„ Town			
Maidstone Union†	34,670	3,204	15,821
Malling Union‡	47,410	3,896	20,182
Total	319,381	26,987	139,115

The entire area is equivalent to about 500 square miles.

Geologically, the district thus constituted lies chiefly on the Upper and Lower Cretaceous formations, the series of which descend southward by belts passing in an east and west direction. Commencing with Bromley as the northern boundary, to

* Exclusive of Southborough and Tunbridge Wells.
† Exclusive of Maidstone Borough.
‡ Exclusive of Wrotham Local Board District.

as far south as a line extending by Wickham-Breaux, Keston, Farnborough, and Orpington, the strata consist of tertiary deposits, viz.:—the Clay, Woolwich and Reading beds, and Thanet Sand of the London basin. Thence southwards are cretaceous deposits so far as the chalk hills ranging through Betsom's Hill, Otford, Kensing, and Wrotham, to Hoborough on the Medway. Southward of this range there occur in succession the upper Greensand, Gault clay, Folkestone, and Hythe beds, and Atherfield clay, forming a widening belt, which passes right thorough Sevenoaks, Malling, and Maidstone Unions. Further southwards comes the more uniform broad belt of the Wealden, which is bordered on its southern margin by Tunbridge Wells (or Hastings) sand, varied with beds of Wadhurst and Ashhurst clay.

The Medway and its watershed form a central and very striking feature of the district. This watershed commences at the west of the county, extending in breadth between Cockham Hill of the Sevenoaks range on the north, and Marsh-green on the Tunbridge Wells range to the south, giving a width of from four to five miles. This part of the valley is drained by its Eden tributary, which the Medway proper joins at Penshurst. Still proceeding east, it becomes connected with the watersheds of the Teiss, lying south, and that of the Beult, lying south-east. Their waters join the Medway near Yalding; and the combined streams, making a bend to pass through the Maidstone hills, find their way in a northerly direction out of the district by Wouldham, close upon Rochester.

Those parts of the district north and south of the ranges of hills forming the Medway or Kent valley, are generally of a high, and always of good, elevation, the southern portion of the union of Tenterden excepted, which, declining towards Romney

Marsh, forms part of the watershed of the Rother, which falls into the sea at Rye.

On the north, the watershed of the Darent is formed by the uplands on either side of Westerham, Bearstead, and Sundridge, continued between Riverhead and Chevening Hill, Otford and Sepham Hill; then, becoming contracted at Shoreham, it passes out of the district at Shoreham Castle to the Thames at Dartford Creek. Two other small watersheds, the Cray and the Ravensbourne—also terminating beyond the district at the Thames, the former along with the Darent—complete a rough sketch of the hydrography of the district.

The brooks, streams, and rivers are too frequently, if not universally, used for drainage; not only by single and clustered houses on their banks, but practically by the numerous villages and hamlets scattered over the district. In this respect Tunbridge has initiated a most important and commendable reform, by withdrawing its sewage from the Medway and utilizing it upon the neighbouring fields.

The climate of West Kent, amongst English varieties, is very warm and genial, though with some occasional sharpness in winter. It has comparatively a very moderate rainfall. It enjoys the advantage of being near the sea, while protected by chalk hill ranges from the severity of its gales. These hills lessen also the keenness of the easterly class of winds. The variety of northern upland, central plain, with southern hill and dale, give a certain range of climate, from the freshness of plateau and hill to the warm shelter of nook and valley.

The chief industry is agriculture; but, besides ordinary farming operations, fruit and hops are extensively grown. The latter culture is the cause of the motley immigration of hop-pickers into the county during summer and autumn, who are said to

have numbered 20,000 in 1873. Outside of towns, these find varied lodging in huts of every kind; in tents, barns, and other out-buildings, under circumstances often objectionable in a sanitary point of view, entailing sickness in bad seasons, and rendering advisable some special sanitary supervision. Trade specialties are represented by breweries, paper mills, lime and cement works in certain localities; but the northern and western parts of the district are largely residential, both for people engaged in London commerce, and others from all parts of the country, who enjoy independent means. The district is also temporarily much resorted to for health, change, or amusement.

With so much done by nature to foster the health of the population, it were well to find that science also had been equally watchful and provident in the sanitary arrangements and constructions; so that, among other requirements, the dwellings, especially of the cottage class, should be dry within and paved without, furnished with proper drains to remove slops and other refuse water from the premises, with efficient privy and ashpit erections to conserve the animal and vegetable refuse without nuisance, and with convenient access to a wholesome and sufficient water supply.

An examination of the greater part of the district has demonstrated in this, as in all others examined throughout the country, an almost universal defectiveness in these requisites of a decent and healthy dwelling, owing probably as much to ignorance by owners and builders of what was necessary, and how it was to be accomplished, as to other causes, or to any unchristian indifference to the health of the cottagers. Here and there, from local facilities, or within the jurisdiction of some local board, part of these evils may be more or less remedied, but

substantially the faults and the work of improvement to be undertaken are very generally the same.

Confining the present review to general faults of chiefest importance, the first in order is undoubtedly the great privy nuisance, not only because it sickens the enforced occupant within, but because it also fouls the air without, the ground below, and very often the water supply of the house, or of the neighbouring, well.

With the exception of some few earth and hopper closets, the usual construction of the cottage privy throughout the rural and even some of the town localities is as follows—a brick or wooden casing for privacy, with a cesspit either wholly beneath, or, more frequently, extended out beyond it to rear or side, where it remains open, or partially covered with planks or brambles, or it is sometimes sealed with earth or flag. The latter certainly modifies the amount of smell outside; but in all cases the inevitable odours within are most sickening and injurious, and frequently penetrate to the very doors and windows of the dwellings. The cesspit, too, often overflows the ground, or its fluid contents run off into a ditch or other cesspool. Probably no mode of universal nuisance could be contrived equal to this one of the cottage privy; for here is the filthiest matter stored up quite undiluted with anything to disinfect it, of a moisture provocative of extremest corruption, often impregnating the wells and the earth with foulness, and ever charging the air outside, but especially inside, with noxious and offensive gases. Could any contrivance be more effective for continuous mischief? Could science be more worthily occupied than upon its improved construction, or in making known its latent consequences?

The privy is in truth only an extra room of the dwelling; but unlike other rooms, which may remain unvisited, must be used

daily by every member of the family, and much oftener in times of illness, when the system is most sensitive and susceptible to insanitary influences.

A privy ought to be nearly altogether free from smell, and thus capable of being placed within the five or six yards required for the convenient use of the household. Its arrangements ought to include the anti-putrefactive principles of dryness; of exclusion of sun-light and heat; of a paved foundation; of the regular use of a small, and therefore practicable, quantity of dry earth for disinfection; and supplemented by an adjacent dustbin for ashes and refuse. Finally, it should be isolated from other erections, built wholly on (not below) the ground level, and provided with efficient means of ventilation.

Moule's earth closets, if properly cared for and attended to, may fulfil the requirements; but they involve a dependence on mechanism, and more of attention, labour, and dry earth than can be practically ensured among cottagers. For successful use of them the sanitary authority itself should undertake the whole management. The medical officer of health has, therefore, thought it one of his earliest duties, from the magnitude and cumulative effects of this great evil, to print and circulate a suitable plan for the proper construction of privy and dustbin satisfying the above requirements, and adapted as much as possible to the prevailing structures and to the habits of the population. The plan and specification will be annexed to the report.

Drainage is the next essential. For this a trapped gully-hole on the outside premises, several feet away from any door or window, should be made for the reception of all the slops, waste, and surface water, to be conveyed by a glazed earthenware drain pipe into a main or public sewer, terminating in a series of two properly constructed cesspools in the middle of a

sufficiently distant field, where the clear contents of the second, overflowing from the first, and filtered if necessary, may be profitably utilized. Such a system is nearly always available; but the details must be varied by the engineer, according to local circumstances and conditions. The drains should be freely ventilated, and easy of access for repair; they should never enter the dwelling, but receive the drainage through the gully trap only.

The present mode is to drain into imperfect cesspools, within the same or neighbouring premises, all being more or less near the dwellings; into ditches, or into ponds, creating nuisances; and where near enough, into streams and rivers, polluting their waters. One cesspool may serve three or more houses, and nearly all are in dangerous proximity to wells. In most of the villages some drains used in common, leading into brooks, ditches, or cesspools, are found; but, no supervision being maintained, their effect is rather to create a public nuisance in affording relief to a private one. Often no drainage exists, and then the slops, &c., being thrown down, form a nuisance by festering on the surface, or in some ground hole at the rear. Among the towns, Tunbridge is just completing a promising system of sewerage and utilization, which will doubtless be followed by other communities able to undertake the necessary works, and these may be much smaller than is commonly supposed.

The consideration of water supply comes to be considered here, not because it is inferior in importance to the other attributes of a healthy dwelling, but because its chief sources, the wells and the brooks, are so constantly polluted by imperfect privies, and the ordinary vicious system of drainage by cesspool and stream. If these evils were wholly rectified, the well

water, at least, might generally be freed from its most noxious element; and, if not altogether satisfactory in other respects, might still be sufficiently wholesome to obviate the necessity of providing public supply where great obstacles or expense would be involved. In such a case a third great sanitary difficulty would often be most economically solved by a complete removal of the first and second. A similar improvement would also take place as to brooks and rivers, if the privy and drainage reform were fully carried out, coupled with the general removal of minor and manufacturing nuisances.

In addition to the brooks, rivers, and wells so generally polluted, a water supply is resorted to from springs, the rainfall catchment of the roof, and not unfrequently from ponds and even ditches. The two latter of these are always impure; but good spring flows, properly protected, are invaluable, as also roof catchments of rain when received into proper tanks.

Practically, neither of the latter are much availed of in the district, though both sources might be extensively utilized. Springs, wherever flowing at a higher source, might readily and with immense advantage be converted into village fountains at a small expense, either by the sanitary authority or by the generosity of a local philanthropist. It is marvellous, with so much wealth, that the bestowal of such a great boon should not be a very general and attractive form of benevolence and good report.

In reviewing these salient defects in the sanitary arrangements of the district, their generality, frequent inter-dependence, and latent powers of mischief are very apparent. Also how much has to be done in these points alone, as well as the desirability of their speedy accomplishment.

Of the remedies pointed out, the main drainage of villages,

&c., can be best and most completely achieved, because the sanitary authority is empowered itself to do the work, and to borrow the requisite money on very easy terms of interest and repayment. This latter advantage can be made available also for the construction of private or house drains, with the privilege to owners of repaying the sanitary authority for their share of the work in moderate periodical payments, as "private improvement expenses." It were much to be wished the same facilities could be availed of for the required alterations of privies and the necessary construction of dustbins or ashpits; such an aid would much facilitate and quicken the improvement.

The imperfection of sanitary constructions undoubtedly deteriorates the habits and senses of the population as to the sweetness and cleanliness of the homestead, which again re-acts in producing further insanitary results. Familiarity with a constant privy nuisance dwarfs all the minor ones; the want of dustbins renders cottagers too tolerant of the consequent dirty and unsightly refuse heaps about the premises; while the use of privies by several families subverts all notions of decency. With such surroundings, the difficulties of raising up a clean and virtuous population are proportionately aggravated. Cottagers are helpless with these constructions. Owners and sanitary authorities can alone effectuate the required improvements, than which nothing would more tend to banish sickness and promote good habits and contentment.

The largeness of the district naturally requires some time for a first, even cursory, inspection, conjoined as it must be with current duties. This, however, has been nearly accomplished within the areas of the original combined authorities, and those of the remainder will be gone through without delay. In these visits, the medical officer of health has been accompanied by the several

sanitary inspectors, who have afforded him the advantage of their intimate local knowledge, and received the benefit of his information and advice as to their office and action, as well as in the difficulties they must often encounter. So far, he has reason to speak well of their qualifications and zeal, and has also had many opportunities of witnessing improvements made from their suggestions, and has verified numerous abatements of nuisances following their legitimate interference. Nearly all have large areas for their duties, and devote their whole time and attention to the work, with manifest and undoubted advantage. The comparative progress made in each district will, at no distant period, however, furnish the truest test of their relative success.

The necessary steps have been taken to ensure during 1874 the weekly returns usually supplied to medical officers of health, the necessity for which is too apparent to require explanation here. He has also been kindly furnished with those for the fourth quarter of 1873, which enables him to construct a few partial tables of the mortality for future reference and comparison. It must be noticed, however, that the death-rate of 1873 was a favourable one, especially as to zymotic diseases. These tables will be subjoined, as well as one compiled from the quarterly reports of the Registrar-General, showing the comparative death-rates of the whole year for all the unions.

Efforts have been made to procure similar returns monthly of sickness, but with very partial success, owing, it would seem, to the remuneration not yet being specified. These returns would give most valuable aid to sanitary work, and are in fact required by the Local Government Board. They should be made with care, and on the responsibility alone of the medical men themselves recording and attending the cases of parochial sickness.

In conclusion, the medical officer of health ventures to express a hope and conviction that the district will realize great benefit from the new sanitary organization. It has, indeed, often to point out great faults to many by whom the evil effects are little understood, and to encounter a not unnatural prejudice as an inconvenient innovator on old and inveterate customs, as well as by having to suggest unpalatable expense. It will, however, meet these difficulties by full and courteous explanation and soundness of advice. On his own behalf, the health officer is grateful for the very kind reception extended to him by all the authorities, and the considerate attention they have hitherto given to his suggestions.

C. O. BAYLIS, M.D., M.R.C.S., &c.,
Medical Officer of Health for West Kent Sanitary District.

February 28, 1874.

No. 11. Beulah Road,
Tunbridge Wells.

TABLE I.

DEATHS Registered in the BROMLEY UNION during the Fourth Quarter of 1873.

BROMLEY. PARISHES.	DEATHS.			ZYMOTIC DEATHS FROM						
	Total.	Under One Year.	Sixty and upwards.	Small-Pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhœa.
Bromley (P. & L. B. Dt.) ...	40	9	12	...	1	...	...	...	...	...
Beckenham	24	9	4	...	1	...	...	1	...	1
Hayes	1	1	...	...	...	...	...	...	...	...
W. Wickham ...	2	1	1	...	...	...	...	...	...	...
Keston	4	...	2	...	...	...	...	...	...	...
Locksbottom ...	1	...	...	...	...	...	...	...	...	...
Cudham	5	...	3	...	...	...	...	...	...	...
Knockholt	...	...	...	...	...	...	...	...	...	...
Farnborough ...	4	2	1	...	...	...	...	...	...	...
Chelsfield	5	4	4	...	...	...	...	...	...	...
Orpington	9	4	...	...	...	...	...	1	...	...
St. Mary Cray ...	14	5	2	...	...	...	...	2	...	1
St. Paul's Cray ...	3	2	...	...	...	...	...	...	...	...
Chislehurst	12	4	1	...	...	...	...	...	...	...
Foots Cray and North Cray	9	2	4	...	...	...	...	1	...	...
Workhouse	10	...	7	...	...	...	...	...	...	...
Total	143	43	39	...	2	...	...	5	...	2

TABLE II.

DEATHS registered in the TUNBRIDGE UNION during the Fourth Quarter of 1873.
(Exclusive of Tunbridge Wells and Southborough.)

TUNBRIDGE UNION. PARISHES.	DEATHS.			ZYMOTIC DEATHS FROM						
	Total.	Under One Year.	Sixty and upwards.	Small Pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhœa.
Bedborough	...	...	...	...	...	...	...	...	...	...
Ashurst	...	...	...	...	...	...	...	...	...	...
Speldhurst	8	3	2	...	...	...	...	...	...	...
Tunbridge (Ph. & L. Bd.)	8	1	2	...	...	...	...	...	...	...
Hadlow	8	3	3	...	...	...	...	...	...	2
Capel	5	2	1	...	...	...	...	...	...	...
Tudely	6	3	1	...	...	...	...	...	...	1
Pembury	5	...	2	...	...	...	...	...	...	...
Brenchley	16	4	3	...	...	...	..	2	...	...
Horsemonden	4	1	2	...	...	...	...	...	...	1
Union Workhouse	13	...	6	...	...	...	...	...	...	...
TOTAL	73	17	22					2		4

16

TABLE III.

DEATHS registered in the CRANBROOK UNION during the Fourth Quarter of 1873.

CRANBROOK UNION. PARISHES.	DEATHS.			ZYMOTIC DEATHS FROM						
	Total.	Under One Year.	Sixty and upwards.	Small-pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhœa.
Frittenden	...	...	...	...	...	...	...	...	...	...
Cranbrook	10	...	7	...	...	...	...	...	...	...
Bennenden	10	2	4	...	...	...	...	1	...	...
Goudhurst	9	1	5	...	...	...	...	1	...	...
Sandhurst	3	0	2	...	...	...	...	...	1	...
Hawkhurst	11	4	4	...	...	...	...	...	...	...
Union Workhouse	6	1	3	...	...	...	...	...	...	...
	49	8	25		...	...	...	2	...	...

17

TABLE IV.

DEATHS registered in the TENTERDEN UNION during the Fourth Quarter of 1873.
(Inclusive of the Borough.)

TENTERDEN UNION. PARISHES.	DEATHS.			ZYMOTIC DEATHS FROM						
	Total.	Under One Year.	Sixty and upwards.	Small-pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	* Diarrhœa.
Newenden	1	...	...	...	...	...	...	...	...	...
Rolvenden	4	...	3	...	...	...	...	...	...	...
Biddenden	9	4	1	...	...	...	...	...	1	...
Wittersham	5	2	2	...	...	...	...	...	...	...
Stone...	1	...	1	...	...	...	...	...	...	...
Ebony	1	1	...	...	...	...	...	...	...	...
Tenterden (Ph. & L. Bd.) ...	6	2	1	...	...	...	...	1	...	...
Appledore	1	...	...	...	...	...	...	...	...	...
Kenardington	...	...	...	...	...	...	...	...	...	...
Woodchurch	6	1	1	...	...	...	1	...	...	...
High Halden	5	1	3	...	...	...	...	...	...	...
Union Workhouse	2	...	1	...	...	...	...	...	...	...
	41	11	13	...	...	...	1	1	1	...

18

TABLE V.

DEATHS registered in the MAIDSTONE UNION during the Fourth Quarter of 1873.
(Exclusive of the Borough.)

MAIDSTONE UNION. PARISHES.	DEATHS.			ZYMOTIC DEATHS FROM						
	Total.	Under One Year.	Sixty and upwards.	Small-pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhœa.
Yalding	12	5	1	...	...	...	...	2	...	...
Nettlestead	2	...	1	...	...	...	...	...	...	...
Teston	...	...	...	...	...	...	...	...	...	...
W. Farleigh	...	...	...	...	...	...	...	...	...	...
Hunton	2	1	1	...	...	...	...	...	...	...
Marden	11	1	6	...	...	...	...	...	2	...
Staplehurst	7	2	3	...	...	...	...	...	...	...
Linton	2	...	1	...	...	...	...	...	1	...
Loddington	...	...	...	...	...	...	...	...	...	...
Boughton Monchelsea ...	5	...	3	...	...	...	...	...	...	...
Otham	2	1	1	...	...	...	...	...	...	...
Bearstead	7	2	4	...	...	...	...	...	...	...
Loose	8	3	3	...	...	...	...	...	...	...
E. Farleigh	6	3	...	...	...	...	...	...	...	...
E. and W. Barming ...	3	1	1	...	...	...	...	...	...	...
Union Workhouse	16	2	5	...	...	...	...	...	...	...
	83	21	30	...	...	...	...	2	3	...

19

TABLE VI.

DEATHS registered in the three Towns of BROMLEY, TUNBRIDGE, and TENTERDEN, during the Fourth Quarter of 1873.

CAUSES OF DEATH.	BROMLEY.				TUNBRIDGE.				TENTERDEN.			
	Under One Year.	One Year and under Five.	Five Years and under Sixty.	Sixty Years and upwards.	Under One Year.	One Year and under Five.	Five Years and under Sixty.	Sixty Years and upwards.	Under One Year.	One Year and under Five.	Five Years and under Sixty.	Sixty Years and upwards.
ALL CAUSES	9	5	15	11	6	6	12	9	2	1	3	2
CLASS I.—Measles	...	1	...	...	...	...	...	...	...	...	...	...
Diphtheria	...	...	...	...	...	1	...	...	...	...	...	...
Whooping Cough ...	...	...	...	...	...	...	...	...	1	...	...	...
Enteric Fever	...	...	...	...	...	1	2	...	...	...	...	...
Diarrhœa	...	...	...	...	2	...	...	...	...	...	...	...
Puerperal Fever ...	...	...	...	...	...	...	1	...	...	...	...	...
„ II.—Cancer	...	1	1	...	...	...	...	...	...	...	...	...
„ III.—Scrofula	...	...	...	...	1	...	...	...	...	...	...	...
Hydrocephalus	...	1	...	...	...	...	...	...	...	...	...	...
Phthisis	...	...	3	...	...	...	2	1	...	...	...	...
Tabes Mesenterica ...	...	...	...	...	...	...	...	...	...	...	1	...
„ IV.—Apoplexy	...	...	1	1	...	...	1	...	...	...	...	1
Paralysis	...	...	1	...	...	...	...	...	...	...	1	...
Convulsions	1	...	1	...	1	...	...	...	...	...	...	...
„ V.—Disease of Heart ...	...	...	1	3	...	...	3	2	...	...	...	...
Angina Pectoris ...	...	...	...	...	...	...	...	1	...	...	...	...
„ VI.—Bronchitis	1	2	1	3	...	2	1	1	...	...	...	...
Pneumonia	...	...	2	...	...	1	...	1	1	...	...	1
„ VII.—Enteritis	...	...	1	...	...	...	...	...	...	...	...	...
Peritonitis	...	...	...	1	...	...	...	...	...	...	...	...
Disease of Liver ...	...	...	...	...	...	...	1	...	...	...	1	...
„ VIII.—Albumenuria ...	...	...	...	...	...	...	1	1	...	...	...	...
„ IX.—Disease of Uterus ...	...	...	...	...	...	...	...	...	...	...	...	...
„ X.— „ Bones ...	...	...	...	...	...	...	...	...	...	...	...	...
„ „ Joints, &c.												
„ XI.— „ Skin	...	...	...	...	...	...	...	...	...	...	...	...
„ „ Muscles, &c.												
Abscess	...	...	...	...	...	...	...	...	...	1	...	...
„ XII.—Premature Birth, &c. ...	3	...	...	...	1	...	...	...	...	...	...	...
„ XIII.—Childbirth	...	...	2	...	...	...	...	...	...	...	...	...
„ XIV.—Old Age	...	...	...	3	...	...	...	2	...	...	...	...
„ XV.—Atrophy and Debility ...	4	...	...	...	...	...	...	...	...	...	...	...
„ XVI.—Accidental Violence ...	...	...	1	...	...	...	...	...	...	...	...	...
Not specified ...	...	...	...	...	1	1	...	...	...	...	...	...
Males	4	4	7	3	3	2	7	3	2	1	2	1
Females	5	1	8	8	3	4	5	6	0	0	1	1

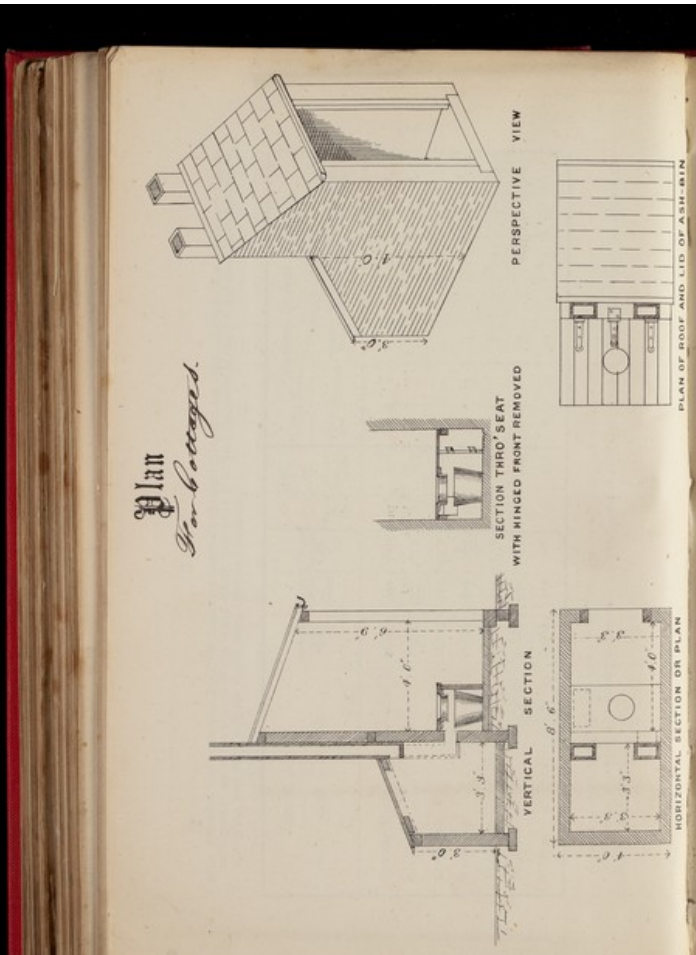
TABLE VII.

TABLE compiled and computed from the Registrar-General's Quarterly Reports for 1874. Showing the number of Deaths registered in the Seven Unions of the WEST KENT SANITARY DISTRICT (inclusive of every Local Board and Borough*), during the Year 1873, with the Death Rates computed therefrom.

UNIONS OF THE WEST KENT S. DISTRICT.	Estimated Population. †	Total Number of Deaths.	Death Rate per 1,000.	DEATHS.		ZYMOTIC DEATHS FROM						
				Under One Year.	Sixty and upwards.	Small-pox.	Measles.	Scarlatina.	Diphtheria.	Whooping Cough.	Fever.	Diarrhœa.
Bromley Union	34,546	554	16·0	151	171	...	4	...	2	21	4	40
Sevenoaks	26,706	327	13·2	78	125	1	...	...	2	3	3	6
Cranbrook	14,123	215	15·2	35	92	...	...	...	...	2	5	6
Tunbridge	46,746	809	17·3	176	245	...	3	4	9	21	12	28
Tenterden	10,857	194	17·8	45	68	...	...	1	7	1	2	4
Maidstone	42,685	930	21·7	198	284	...	...	3	1	20	18	28
Malling	23,769	395	16·6	90	118	2	...	10	...	3	12	12

* Some of these, though outside of the West Kent Sanitary District, could not be eliminated.

† These are arrived at by assuming a continuance of mean rates of difference, equal to what took place in the decade preceding the last Census.



REPORT OF DEPUTATION

APPOINTED BY

THE BOARD OF POLICE

"TO PROCEED TO LONDON,
AND OTHER CITIES IN THE KINGDOM, WHERE
USEFUL INFORMATION REGARDING THE

CONSTRUCTION OF SMALL-POX HOSPITALS

IS LIKELY TO BE OBTAINED."

GLASGOW:
PRINTED BY ROBERT ANDERSON, 22 ANN STREET.
1874.

EXTRACT *from* MINUTES *of* MEETING *of the* BOARD OF POLICE OF GLASGOW, *held on* 17*th Nov.*, 1873.

BAILIE WALLS, IN THE CHAIR.

"The Committee of Health reported the following Extract from their Minutes:—'The Committee having fully considered the subject of erecting an Hospital at Belvidere for Small-pox Patients, on the site recommended in the Report by Drs. Gairdner and Russell, and the Master of Works, submitted to the Board, on 9th September, 1872, and having examined the Plans of the proposed Hospital which had been prepared by Mr. Carrick, and keeping in view the complaints made against continuing the Hospital at Parliamentary Road for the treatment of Small-pox Patients, and relative Reports by the Medical Officer, the Committee agreed to recommend that the erection of a Small-pox Hospital at Belvidere should not be further delayed, and that the Plans thereof submitted by Mr. Carrick be approved of, and the Clerk authorised to advertise for offers for the erection of the Buildings.' The Board approved of the foregoing Minute, but in respect of the magnitude and importance of the interests involved in the subject appointed the following gentlemen, viz., Messrs. Ure and M'Lellan, and the Medical Officer and Master of Works, to proceed to London and other Cities in the Kingdom where useful information regarding the construction of Small-pox Hospitals is likely to be obtained, and to report to the Board the result of the investigations, and delay proceedings further in the matter until said Report is given in."

JOHN LANG, *Clerk.*

REPORT OF DEPUTATION.

THE Deputation visited and inspected with more or less minuteness four of the Hospitals erected by the Metropolitan Asylums Board (viz., the small-pox hospital and fever hospital at Stockwell, the small-pox hospital and fever hospital at Homerton); the London Fever Hospital, Liverpool Road; also, St. Thomas' Hospital, as being the newest and grandest General Hospital in the kingdom, and the Herbert Hospital at Woolwich, as reputed to be the finest specimen of a pavilion hospital in existence. From London we proceeded to Bradford, where we saw a new pavilion hospital for infectious diseases.

During our tour we took advantage of such opportunities as occurred of gaining information on other departments of Sanitary work than that expressly contained in our Commission. When in London, accompanied by Bailie Bain, we visited the Phosphate Sewage Company's Works, at Barking. We also visited the poorer parts of Whitechapel, inspecting many of the houses, and examining the sanitary arrangements of the courts, under the guidance of Inspectors furnished by Dr. Liddle, Medical Officer of Health. At Bradford we visited the extensive Defecation Works, erected in the suburbs for the treatment of the Town Sewage, according to the system of the Peat Engineering and Sewage Filtration Company; and in company of Dr. Butterfield, Medical Officer of Health, made a tour of the lower parts of the town. From Bradford we went to Rochdale in order to make ourselves acquainted with the famous Rochdale system of disposing of town refuse. In view of the proposal to erect public Mortuaries in Glasgow, which is before the Board, we also visited Liverpool, where they have been in use for three years. Dr. Russell remained a day longer there than the other members of the deputation, and with an inspector provided by Dr. Trench, examined the house accommodation and sanitary arrangements of the worst parts of Scotland and Vauxhall Wards. He also visited Netherfield Road Hospital for the treatment of private cases of infectious disease.

Before proceeding further, we beg to record our best thanks to J. A. S. Stewart, Esq., and J. Charrington, Esq., Chairmen of the Stockwell and Homerton Hospital Committees, and to the officials in charge of the various hospitals, for the facilities afforded us to make our inspections, and their attention during our visits. Through the kindness of T. H. Wyatt, Esq., architect, we obtained the loan of the complete working plans of Stockwell Small-pox Hospital, and we obtained the same favour in reference to both Fever and Small-pox Hospitals at Homerton. We despatched these plans to Glasgow, and tracings of them were taken in the Master of Works' Office. We must also say how much obliged we feel to Dr. Huntley Gordon, Surgeon-General, Herbert Hospital, Woolwich, for the time and pains expended in showing us all the well-considered details of that splendid Hospital.

The most important part of the Report we have to present as the result of our inspections of the hospitals enumerated above, is in reality the amended plans of the proposed Small-pox Hospital, which are herewith submitted. Since our return, we have had repeated meetings and consultations over those plans. It is scarcely possible to embody, in writing, all the details noted and utilised; but we shall endeavour to state the principal results of our observations in regard to Epidemic Hospital construction.

The pavilion system seems now universally accepted as the only one suited for the treatment of contagious diseases.

The pavilions of the London Hospitals were all two storeys in height. This is undoubtedly a deviation from the original idea of a pavilion, caused by the value of ground, and we recommend that in our case we should adhere to the open-roofed ward—raised from the ground level, so as to allow a free space between the floor and the ground—which has been adopted in our existing hospitals. It is indeed doubtful whether, taking into account the additional space between two-storeyed pavilions as compared with one-storeyed, and the utilized cubic contents of the open roof in the latter, the saving of expense effected by the adoption of the loftier pavilion is of material importance.

Another important feature in all the hospitals inspected, was the union of the pavilions by covered and enclosed passages or "corridors." In our present hospitals no such connection exists, and we report decidedly in favour of our own plans. The essential idea of the pavilion is the isolation of

the hospital unit or ward by open air. To provide a continuous communication between the wards or blocks by closed corridors is, therefore, a sacrifice of the essential idea to administrative convenience. Although it was obvious that attempts, more or less thorough, had been made by the architects of the several hospitals to neutralize by abundant ventilation the radical defects of a corridor, still the success of those attempts was only partial, and by at least two of the Medical Superintendents the inconsistency of the corridor was acknowledged. The corridors of the Herbert Hospital, Woolwich (a general hospital), are the most unobjectionable that could be devised, but they are all the more expensive. If any provision is to be made for communication, we can approve only of a covered way, with sides entirely open, such as unites the pavilions of the Herbert Hospital with the administrative block.

Coming to the hospital unit or ward, the dimensions of the wards of the small-pox hospitals erected by the Metropolitan Asylums' Board at Stockwell and Homerton are—

Length, 70 feet; breadth, 26 feet; height, 14 feet; number of beds, 12.

This gives to each patient a cubic space of 2123 feet. In our present hospitals we have only 1500 cubic feet, and have always been satisfied both with the purity of their atmosphere and the comparative curative results. Still we feel disposed to yield to the universal practice in all the epidemic hospitals recently erected, and to adopt 2000 cubic feet.

The disposition of the cubic space in reference to the bed is a matter of equal importance with the amount per bed. In a ward each bed may be regarded as having a block of air to itself. It is quite clear that the same cubic space may be obtained with the most variable dimensions of this block. If the ward is too broad, then the beds will be too close laterally, and the ceiling may be low; if it is too narrow, then there will not be enough room to pass up and down the ward freely. All the wards we examined are much broader than in our hospitals. The breadth given above is 26 feet, while the Fever Hospital at Homerton is 28 feet broad. Our pavilions are only 20 feet broad. We think that this is nearer the proper width; but in order to preserve symmetry in the allotment of the increased cubic space, we propose for the new wards a width of 22 feet.

Here the question of accommodation for convalescent patients arises. In the hospitals inspected, "day-rooms"

were provided in which convalescents sat during the day, but returned to the ward to sleep. Our practice has always been to provide a distinct "convalescent ward," in which convalescents both slept and sat. This plan has the advantage of separating those who have recovered from those who are still ill and possibly dying, both by day and by night. Any disadvantage from sitting and sleeping in the same room is imaginary as contrasted with that of sleeping in the main ward, especially as convalescents, in suitable weather, spend most of their time out of doors. While, therefore, proposing to retain the "convalescent ward," some improvements are suggested: (1) As the patients will be going about and much out of doors, the increased cubic space is unnecessary; (2) To afford more room for those who are moving about, the "convalescent ward" should be two feet broader than the main ward; (3) As a larger proportion of small-pox patients than of fever patients are convalescent at one time, the distribution of the beds may be altered from 11 and 5 to 10 and 6 for acute and convalescent wards.

We, therefore, recommend that the dimensions of the acute wards should be—

Length, 56 feet 6 inches; breadth, 22 feet; height (average), 16¼ feet; number of beds, 10.

Each bed will, in this way, stand in an area of floor space 11 feet by 11 feet 3⅜ inches, with an average height of 16½ feet.

For the convalescent ward, we recommend—

Length, 21½ feet; breadth, 24 feet; height, 16½ feet; number of beds, 6.

This gives a cubic space of 1419 feet to each bed.

Equal in importance to the dimensions and cubic space of air allotted to each bed is the means provided for its regular and rapid renewal, *i.e.*, the ventilation. We found arrangements more complicated, but none more effective than those with which we are familiar in our present hospitals.

Closely associated with the purity of the atmosphere of the ward is the nature of the exposed surfaces—the walls, roof, and floor. It is very important (1) that they should be as impervious as possible, and (2) that they should be smooth, and that no horizontal surfaces should afford a lodgment for dust, and those organic particles which abound in the atmosphere of a hospital. This latter condition is easily fulfilled, and as a considerable aid in that direction we recommend that in the construction of the open roof the principal

rafters should be of light T iron, and the ties of thin rod-iron.

The making of the walls, &c., impervious is a more difficult question. All the hospitals visited in London have their walls, &c., coated with Parian cement, excepting at Homerton, where they are faced with best pressed brick and painted with silicate. In all cases we found the Parian discoloured and somewhat blotchy, especially in St. Thomas', where an attempt to secure uniformity by introducing distemper into the cement had signally failed. In order to aid the Board to a decision, we have procured estimates of the comparative cost of Parian cement, Keene's cement, white enamelled brick, and best pressed brick painted with silicate.* The wood-work of the roof, we think, should be varnished.

To make the floor impervious is of still greater importance, but it is also more difficult. That part of the floor running along beneath the beds, especially in an Epidemic Hospital, is certain to be bathed in foul fluids from time to time, and, should the seams be wide and the wood porous, it is impossible by any washing to prevent the impregnation of the timber, and the giving off of noxious effluvia. In any case, the washing of hospital floors made of deal, and the subsequent drying in the unavoidable presence of the patients, is not pleasant and cannot but be injurious. The ward floors of the Herbert Hospital and St. Thomas' Hospital are oak (Dantzic oak, as described in the specification of the Herbert Hospital), and in the words of Galton's Report on the former hospital, "have been carefully oiled and bees' waxed so as to be maintained clean by rubbing, without the necessity of frequent washing which renders the wards damp." At St. Thomas' the waxing had been abandoned; but even without this process a floor of oak closely laid presents in comparison with an ordinary deal floor an impervious surface. We recommend this matter to the consideration of the Board.

Another matter in relation to the pavilions may be noted. We recommend the providing of hot water for baths, &c., not from a large central boiler, but from a small portable boiler, with an open fireplace, in the bath-room of each ward. In our present hospitals we have experience of both systems. The former is not suited for Pavilion Hospitals, especially such as ours. There is a great loss of heat and consequent expenditure of fuel in sending hot water over such a wide area. The more each ward of a Pavilion Hospital can be made self-

*See Appendix.

contained the better. At Belvidere all the wards are supplied with boilers, and each nurse can manage her own firing and hot water supply. If the best form of boiler is selected, and it is raised so as to allow the hot water to gravitate to the points where it is needed, we are satisfied the supply would be more uniform, and the saving of fuel considerable.

It seems unnecessary to enter into any written description of the minor points of ward construction, or of the arrangements of the administrative blocks, for which it may suffice to refer to the plans.

As to the scale on which the administration should be planned, we advise, that it should be in excess of the immediate requirements of the wards presently to be erected. These will contain 160 beds, but it might be necessary under epidemic pressure to run up additional temporary pavilions, for which the site affords ample room. In that case the administration would have extra demands made upon it. This is the part of a hospital which is least capable of hurried and temporary extension, and it is therefore wise to be liberal in the primary provisions made. The kitchen, provision, and clothes store, and especially the nurses' sleeping accommodation should be of ample size and extent.

In reference to the nurses' dormitories, we think it essential to the improvement of the status of our nurses, which is so difficult to maintain, that they should each be provided with a sleeping room. There ought also to be a commodious sitting-room, which might be used as a dining-room. These apartments should be entirely away from the wards. For discipline, it is recommended that they should communicate with the Matron's house. All these points have been effected in the plans.

In the administrative blocks the buildings need not be restricted as to height. By the addition of a storey to the kitchen and store range, the scrubbers may be accommodated, and additional room for storage of bedding and napery obtained.

There is one matter to which we must allude before concluding—the disposal of the excreta from the Hospital. We regret that we saw nothing in the course of our inspections to inform or instruct us in this respect. The excreta from every one of the hospitals enumerated, infected and uninfected, passed directly into the local sewer-system without even systematic attempts at disinfection. It seems to us inconsistent with the primary object of a hospital for

infectious diseases, viz., *prevention*, that while we wash and disinfect his clothes, and retain his body even per force, we should allow a patient's excreta to pass from day to day into the public sewers. There is no doubt that by absolutely destroying all the excreta and effluvia given off from a person ill of an infectious disease, we diminish by so much the specific infecting matter in existence, and make a step towards exterminating that particular disease. We would, therefore, in this respect, advise the Board not to follow the example of the hospitals we visited. We have had in practice for many months, at the present Small-pox Hospital, a method by which no excreta reach the sewers, and we think it is worthy of consideration whether, with such modifications of details as experience has shown to be necessary, the charcoal system should not be adopted in both hospitals at Belvidere. However it may be accomplished, we ought to resolve that no excreta shall leave the hospital in a state in which they can spread specific poison.

As to the general sanitary matters, such as disposal of refuse, house construction, &c., &c., to which, as we have said, we gave some attention during our tour, we shall reserve the results of our observations for another occasion.

JOHN URE.
A. H. M'LELLAN.
JAS. B. RUSSELL, M.D.

APPENDIX.

	Sqrs. Yds.		Cost per Pavilion.
White Enamelled Brick, ...	455 @ 13s. 6d.	...	£307 2 6
Cream Coloured ,, ...	455 @ 8s. 6d.	...	193 7 6
Best Pressed Red ,, ...	455 @ 5s.	...	113 15 0
1st Quality Keene's Cement, ...	455 @ 6s. 9d.	...	153 11 3
2nd ,, ,, ...	455 @ 5s. 3d.	...	119 8 9
Parian Cement, ...	455 @ 7s. 6d.	...	170 12 6
3 Coats Plaster, ...	455 @ 7d.	...	13 5 5
3 ,, Silicate, ...	455 @ 5¼d.	...	10 8 6¼
2 ,, ,, ...	455 @ 4¼d.	...	8 1 1½

Dr Parkes
with the Authors kind regards

THE PATHOLOGY

OF

CHRONIC BRIGHT'S DISEASE

WITH

CONTRACTED KIDNEY,

WITH SPECIAL REFERENCE TO THE

THEORY OF "ARTERIO-CAPILLARY FIBROSIS."

BY

GEORGE JOHNSON, M.D., F.R.S.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE IN KING'S COLLEGE, LONDON, AND PHYSICIAN TO KING'S COLLEGE HOSPITAL.

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1873.

THE PATHOLOGY

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In the fifty-first volume of the 'Medico-Chirurgical Transactions' I published a paper in which I described and figured what I look upon as a genuine hypertrophy of the muscular walls of the minute arteries from various tissues in cases of chronic Bright's disease. I there stated that I had observed a constant relation between hypertrophy of the arterial walls and hypertrophy of the left ventricle of the heart. And I gave what I believe to be the true physiological interpretation of the phenomena. I have looked upon this discovery of hypertrophy of the arterial walls as the most interesting result of a long series of observations extending over a period of rather more than a quarter of a century, and I supposed that my doctrine relating to this particular subject had met with very general assent. In this belief, however, I now find that

I was mistaken. The recently issued volume of the 'Transactions' contains a very elaborate paper by Sir Wm. Gull and Dr. Sutton in which the authors, while giving me some credit for having directed attention to thickening of the arteries, dissent entirely from my interpretation of the phenomena, and propound doctrines of great novelty and interest relating to the pathology of Bright's disease. In the present communication, I propose to inquire whether these novel doctrines are consistent with known pathological facts and established physiological principles.

The authors give a brief summary of the conclusions at which they have arrived at pp. 295-6 of their paper. The chief novelty to which they lay claim as a discovery is the doctrine that a "hyalin-fibroid" change in the minute arteries and capillaries in various tissues is the primary and essential phenomenon in cases of chronic Bright's disease with contracted kidney. This degeneration of the blood-vessels, they say, commonly begins in the kidney but it may begin in other organs; so that hypertrophy of the heart with degeneration of the blood-vessels may be found associated with healthy kidneys, the contraction and atrophy of the kidney when present being but "part and parcel of the general morbid change."

The morbid state commonly called chronic Bright's disease with contracted kidney is believed by Sir Wm. Gull, and Dr. Sutton to be essentially an "arterio-capillary fibrosis," the morbid changes being allied to senile changes, but "probably due to distinct causes not yet ascertained." Before proceeding to examine their description of the hyalin-fibroid change in the blood-vessels and in the kidneys, I desire to direct attention to some difficulties which have been suggested to me by a perusal of their paper.

In order to prove that the cardiac and vascular changes are not a result of the kidney disease they refer, at p. 286 (vol. lv), to three cases in which the kidneys were "little contracted" but the minute arteries and capillaries were much thickened by "hyalin-fibroid" substance. On turning to these cases I find that case 7, at p. 300, was that of a man,

forty-two years of age who had very emphysematous lungs, with capillary bronchitis; case 10, at p. 303, was sixty-nine years of age and died of senile gangrene, and case 20, at p. 309, was that of a man, æt. 51, who had vegetations on the aortic valves, with dilated heart's cavities and hypertrophied walls.

The cardiac changes in these three cases are supposed by the authors to be a result of hyalin-fibroid degeneration of the minute arteries and capillaries. I would suggest, however, that these changes might with more reason be referred in the first case to the emphysematous lungs with obstruction, primarily of the pulmonary, and secondarily of the systemic circulation; in the second case, to degeneration of the walls of the larger arteries resulting in senile gangrene; and in the third case to incompetence of the aortic valves, which is usually deemed sufficient to explain dilatation and hypertrophy of the heart.

In further proof of their doctrine that the arterio-capillary change is the primary and essential condition they refer again, at p. 286, to cases in which the kidneys were healthy while the heart was much hypertrophied.

Three cases are given in illustration of the co-existence of cardiac hypertrophy with healthy kidneys. Case 2, at p. 297, was that of a woman sixty-three years of age, who, therefore, may probably have had senile degeneration of her large arteries. The heart was heavy, but, death having resulted from rupture of the left ventricle, its muscular tissue must have undergone other changes than hypertrophy; and kidneys weighing fifteen ounces could scarcely have been healthy, even though their surface was "almost smooth." Case 3, at p. 297, was a man, seventy-seven years of age, whose hypertrophied and dilated left ventricle may probably be referred to senile changes in the larger arteries. And, lastly, case 19, at p. 308, was that of a man, aged sixty-two, who died suddenly comatose. His cerebral arteries were very atheromatous, and his lungs were emphysematous. In the condition of the larger arteries and lungs, we have, I think, a sufficient explanation of the dilatation and hyper-

trophy of the left ventricle, but, in addition, we find that the kidneys weighed only eight ounces; yet we are told that "with the exception of the capsule be.a adherent they were normal." We find then that one case in which the kidneys weighed fifteen ounces and another in which their weight was only eight ounces are set forth as examples of cardio-vascular changes with healthy kidneys. This implies that the kidneys of adults may differ in weight in the proportion of nearly two to one and yet be quite sound in structure.

While the authors refer to cases such as these to prove that hypertrophy of the left ventricle occurs unconnected with renal disease, they maintain that there is a constant association between the cardiac hypertrophy and the hyalin-fibroid change in the minute arteries and capillaries, and they state that there is a direct relation between the cardiac and the vascular change (see pp. 289 and 290). Yet it would seem that this pathological law is not without exceptions, for we find it stated at p. 292, par. 2, that "in a few cases the vessels of the pia mater were thickened by this 'hyalin-fibroid' change whilst the kidneys and heart were healthy."

Their explanation of the manner in which the vascular change causes the cardiac hypertrophy is the following (p. 290, par. 3):—"The hyalin-fibroid material in the walls of the arterioles must be an impediment to elasticity, and it can be experimentally shown that greater force is required to propel a fluid continuously through a non-elastic than through an elastic tube. The left ventricle, therefore, owing to this diminished elasticity of the arterial walls has, of necessity, to contract with greater force to carry on the circulation."

Now, upon this statement I beg to remark, that the explanation of the hypertrophy of the left ventricle is clearly a physiological one; the hypertrophy is said to be a result of the necessity for more forcible contraction to carry on the circulation. Yet the authors, at p. 285, referring to my explanation of the changes in the heart and arterioles as a physiological result, express their belief that "the cardio-vascular changes are throughout a morbid one." It appears,

however, that the authors of this criticism agree with me that the hypertrophy of the left ventricle is a physiological result of excessive muscular contraction called forth by some impediment in the course of the circulation; we differ only as to the cause of the impediment; and I submit that the explanation which I just now quoted from their paper is inconsistent with the facts of anatomy and with the doctrines of physiology.

In that explanation they appear to confound the elasticity of the larger arteries with the muscularity of the smaller vessels. The elastic resiliency of the larger arteries assists in propelling the blood onwards, and converts the intermitting jet from the heart into a continuous stream in the minute arteries and capillaries. The loss of this elasticity, therefore, by degeneration of the arterial walls, involves a loss of propulsive power, and thus imposes extra work on the heart. Hence, the common occurrence of hypertrophy of the left ventricle as a result of senile degeneration of the large arteries. On the other hand, the muscular contractility of the minutest arteries is a force which antagonises the heart and the larger elastic arteries; so that atrophy and degeneration of the muscular walls of the minute arteries tends to impair not the propelling but the retarding forces concerned in the circulation; and such degenerative changes would involve not an increase but a decrease of resistance to the blood-stream; unless the minute arteries and capillaries had their walls converted into rigid tubes with constricted canals, of which changes in the minute vessels I shall presently show that we have no proof.

The authors state, at p. 295, vol. lv, that the "hyalin-fibroid" change in the arterioles is attended with "atrophy of the adjacent textures in whatever organ it occurs." But I find it difficult to reconcile this statement with another statement which appears at, p. 287, par. 2, to the effect that "the minute arteries *in the walls of the heart* have been found much thickened by the formation of the 'hyalin-fibroid' substance;" and yet the walls of the heart are not *atrophied* but *hypertrophied*. Again the doctrine that

atrophy of adjacent tissues is an invariable result of thickening of the arterial walls is inconsistent with the fact that this thickening occurs in the minute renal arteries, not only in the small granular kidney, but also in the large smooth kidney of chronic Bright's disease. Obviously, then, thickening of the arterial walls is not, as the authors assert, invariably associated with atrophy of the adjacent tissues.

I now proceed to discuss the nature of those changes in the walls of the blood-vessels to which the authors of the paper give the name of "arterio-capillary fibrosis." When their paper was read before the Society, after hearing the description of the appearances in the walls of the vessels and examining the drawings, but before seeing their specimens, I ventured to suggest that the so-called "hyalin-fibroid" change was simply a result of the external fibrous coat of the artery being distended and rendered translucent by the fluid in which the specimens were preserved, that fluid being, as stated by the authors, a mixture of glycerine and camphor water (p. 280).

An examination of their specimens confirmed my first impression, and my subsequent inquiries have thoroughly convinced me that the hyalin-fibroid appearance is not an indication of a pathological change occurring during life, but a post-mortem physical result of the imbition of fluid by the coats of the vessels. I am indebted to the courtesy of Dr. Sutton for the opportunity of examining a number of his specimens, and I learnt from him that they were all mounted in the glycerine fluid by an assistant before they were examined microscopically by any one.

The authors, therefore, up to the time of reading their paper, had had no opportunity of comparing the appearance of the vessels when recently removed from the body with that which they assume after immersion in glycerine. This comparison I have made in a number of instances, and the following is the result.

1. I have never yet seen the hyaline thickening of the external fibrous tunic in vessels examined immediately after removal from the body.

2. The appearance is very common, but not constant, after the vessels have been immersed in a mixture of glycerine and camphor water. I have different specimens of arteries from the same subject. Some mounted in the glycerine fluid are conspicuously hyaline, while others preserved in weak spirit and water or in a solution of common salt of sp. gr. 1030 present a perfectly normal appearance. The solution of salt, however, is not sufficiently antiseptic to preserve the specimens for more than a few days.

3. The glycerine fluid produces the hyaline thickening of the minute arteries of the pia mater after death from various diseases, having no relation to Bright's disease, and not being associated with hypertrophy of the left ventricle. Thus, for example, I have specimens of pia mater arteries from a woman who died of diabetes at the age of forty, and whose heart weighed only 6¼ oz. (see figs. 1 and 2). From another

FIG. 1.



Normal artery from the pia mater, mounted in dilute spirit. × 200.

FIG. 2.



Artery from the same pia mater as No. 1; but the fibrous tissue has been distended and rendered hyaline by the glycerine and camphor water in which it is mounted. × 200.

woman, æt. 40, who died of cancer of the ovary, whose heart weighed 8 oz. From an infant, aged eleven months, who, having had a cough for a few days, died suddenly, and probably from spasm of the larynx. The kidneys, heart, and all the organs were quite healthy, but the arteries of the pia mater, preserved in the glycerine fluid, present the "hyalin-fibroid" change very conspicuously.

I have also specimens of pia mater arteries from a boy, æt. 15, who had been in good health until he was attacked by typhoid fever, during the course of which he died from perforation of the bowel. There was no *post-mortem* evidence of disease except such as resulted from the fever. In particular the heart and kidneys were healthy. Portions of pia mater have been preserved in three different fluids; 1st, in dilute spirit; 2nd, in salt and water, sp. gr. 1030; and, 3rd, in equal parts of glycerine and camphor water. In Nos. 1 and 2 the vessels appear quite normal; in No. 3 the fibrous tunic is much distended and extremely hyaline. These specimens are on the table for inspection this evening.

With regard to the physical conditions which favour the imbibition of fluid by the fibrous tunic of the arteries it is certain that while the process is favoured and quickened by acidity of the fluid it is prevented by its alkalinity.

The following experiment affords a good illustration of this principle:—Immerse a small piece of pia mater in water, or in glycerine, slightly acidulated by acetic acid; the arteries in a few seconds assume the hyaline appearance; then neutralize the acid by liquid ammonia, and immediately it will be found that the fibrous coat has shrunk, losing its hyaline character and presenting the fibrous wavy outline which it had before it was distended by the acid fluid. I have specimens of normal arteries from the same subject, one set preserved in the acid fluid, having the fibrous tunic distended and hyaline; the others, after being distended by the acid, have been made to shrink and again assume the fibrous character by the addition of ammonia; and in that condition they are preserved (see figs. 3 and 4).

FIG. 3.



Normal artery from the pia mater, the fibrous tunic distended and rendered hyaline by dilute acetic acid. × 200.

FIG. 4.



Artery which, after immersion in dilute acetic acid, and thus having been rendered hyaline like No. 3, has had its fibrous coat again contracted by the addition of ammonia. × 200.

This experiment shows that the appearance of the minute arteries may be rapidly and greatly changed by chemical conditions, which would be influenced by various circumstances, including, amongst others, probably the mode of death and the time after death at which the vessels are examined.

I look upon it as certain that the "hyalin" appearance so often referred to is always the result of post-mortem changes in the arteries. I believe that the appearance in question has never been seen in vessels recently removed from the body, and not subjected to artificial agencies; though it is possible that the change might occur, in the vessels of the pia mater, for instance, as a result of post-mortem maceration in the subarachnoid fluid.

It is incumbent on those who believe that the appearances described are the result of ante-mortem pathological processes to demonstrate them in vessels recently removed from the body and unchanged by artificial agents.

The glycerine fluid while distending the fibrous tunic often presses the muscular coat more or less irregularly inwards, so as to narrow the canal of the artery; frequently, too, it appears, as it were, to fuse together the muscular elements, rendering them indistinct and giving the thickened wall of the artery a homogeneous or a granular appearance. In other specimens the distension of the arterial wall appears to separate the muscular elements to a greater distance from each other (see Plate VI, fig. 3, in vol. lv). These changes are supposed by the authors of the papers to indicate atrophy of the muscular coat of the artery. I believe them to be simply due to post-mortem physical changes.

I should not now select as good examples of hypertrophy such specimens as are represented in figs. 2 and 3, Plate I, appended to my paper in the fifty-first volume of the 'Transactions.' In these specimens the muscular tissue has been rendered indistinct by the imbibition of glycerine. The appearances here described are quite distinct from the fatty degeneration of the walls of the vessels resulting from senile degeneration, and quite different from the waxy or lardaceous degeneration of the arterial walls.

In some of the arteries the fluid, passing between the middle and inner coat of the vessel, separates them from each other and gives that appearance which the authors describe and figure as a "thickening of the tunica intima." This appearance is shown by the authors in figs. 3 and 4, Plate V, vol. lv.

Again, they affirm that the thickening extends to the walls of the *capillaries;* it is an "arterio-capillary fibrosis." Here, again, I am compelled to dissent. I have seen no capillaries thickened, except the Malpighian capillaries of the kidney; and that, in cases of Bright's disease, is probably a result of the continued transudation of albuminous and fibrinous materials through their walls. I have seen no thickening of

the capillaries in the pia mater. Fig. 2 in Plate VI, which is described as a thickened capillary, I take to be an artery thickened and transformed by glycerine. Fig. 1, in the same plate, with transparent oval bodies in the walls, is unlike any vessel that I have seen, and extremely unlike any capillary of the pia mater.

I do not think it necessary or desirable to criticise at length the author's account of the minute anatomy of the contracted kidney. The sum and substance of their description is that "hyalin-fibroid" material is found here, there, and everywhere in the kidney.

In the year 1847 two papers on renal pathology were read before this Society and published in the thirtieth volume of the 'Transactions.' Mr. Simon was the author of one of these papers and I contributed the other. Both papers were read the same evening; in both papers the same appearances were described and figured, but Mr. Simon and I differed essentially in our interpretation of the phenomenon. The appearances which Mr. Simon described as microscopic cells and cysts I interpreted as tubes denuded and rendered transparent by the destruction and removal of their gland-cells. I have since seen no reason to doubt that my explanation of the appearances in question was the true one, or that the description then given of the minute anatomy of the contracted kidney was, in the main, correct. The appearances which then formed the subject of controversy and which may readily be demonstrated in the contracted Bright's kidney are completely ignored or lightly passed over by those pathologists who make unsatisfactory attempts to explain the structural changes by hyperplasia of connective tissue, or hyalin-fibroid deposit between the tubes.

The description which Sir William Gull and Dr. Sutton give of the appearance presented by the renal arteries in the contracted kidney is so remarkable that it demands especial comment. Fig. 7, in Plate VI, represents a transverse section of a thickened renal artery, and in the description of the plate the artery is said to be "greatly

thickened by hyalin-fibroid changes in the outer layer of the vessels."

The surface of the section shows two distinct layers of fibres, an inner longitudinal and an outer circular, both of which are *muscular*, as may be clearly seen in many specimens in my possession. Sir William Gull and Dr. Sutton, however, assume that the inner longitudinal layer alone is muscular, and at p. 274 (par. 6) they say that "external to the muscular nuclei there was a quantity of hyalin-fibroid substance, and the layer formed by this material was much thicker than the muscular layer" (Plate VI, fig. 7, in vol. lv). Again, they say (at p. 278, par. 4), "Where the kidney disease was far advanced hyalin-fibroid changes were seen in the minute renal arteries precisely similar to those observed in the arterioles of the pia mater and of other parts of the body." It is evident, therefore, that they believe the outer layer in the renal artery to be the counterpart of the so-called "hyalin-fibroid" layer in the arteries of the pia mater. The fact being that this layer in the pia mater arteries is external to the circular muscular fibres, and is composed of the distended fibrous tunic; while in the renal artery the fibrous tunic is inconspicuous and that which they mistake for it is the circular layer of muscular fibres.

In my first published account of the hypertrophied renal arteries in cases of chronic Bright's disease ('Med. Chir. Trans.,' vol. xxxiii) I described and figured the inner longitudinal and the outer circular layer of muscular fibres. The appearances described are quite constant; they were well seen in some of the author's specimens exhibited at the conversazione in June, and were clearly represented by their artist; yet they have been misinterpreted in the manner described. The existence of hypertrophy of the muscular walls is denied and the hyalin-fibroid change is supposed to explain the phenomena.

At the foot of page 277 we find, in a note, the following statement:—

"Recent observations on the minute anatomy of the

kidney raise a doubt whether casts formed in the convoluted tubules* of the cortex can escape."

It would be interesting to know what observations have led to this curious piece of scepticism. No one who has studied the microscopic diagnosis of renal diseases can, for a moment, doubt that casts from the convoluted tubes of the cortex continually escape and are found in the urine. If this were not so the various forms of tube-casts would throw no light upon the state of the cortical portion of the kidneys, which is acknowledged to be the seat of Bright's disease. We find, however, that these casts from the cortex sometimes retain their convoluted form even after passing through the straight tubes. They often contain epithelial cells which, from their characteristic appearance, are known with absolute certainty to have come from the cortex and not from the tubes of the cones. And, lastly, in some cases of hæmaturia, blood which has been poured into the very extremities of the convoluted tubes from ruptured Malpighian capillaries, escapes and appears in the form of blood casts in the urine.

To doubt the possibility of the blood passing from the right to the left side of the heart through the lungs would be scarcely more unreasonable than to doubt the escape of casts from the convoluted tubes of the kidney in most cases of Bright's disease.

The authors object to my doctrine that the hypertrophy of the left ventricle, in cases of chronic Bright's disease, is a consequence of the increased resistance to the circulation occasioned by excessive contraction and resulting hypertrophy of the minute muscular arteries. They say truly that hypertrophy of the heart, although usually present in the advanced stages of the contracted kidney, is absent in a large proportion of cases of large white kidney.

My belief is that when, in cases of chronic Bright's disease with either large or small kidneys, the left ventricle is hypertrophical without disease of the valves or of the

* In the note referred to, "convoluted *tubercles*" is evidently a misprint for "convoluted *tubules*."

large arteries to explain it; in all such cases the minute arteries throughout the body, but especially in the skin and mucous membranes, will be found to have their muscular walls hypertrophied. I have as yet met with no exception to this rule. In explanation of the common occurrence of hypertrophy of the left ventricle in connection with the contracted kidney and its frequent absence in cases of large white kidney, I beg to suggest the following considerations:

1st. With the contracted kidney there is, as a rule, absence of dropsy or but little dropsy, a free secretion of water by the kidney, with retention of urinary solids in the blood; and this uræmic condition is, in various ways, the cause of death. On the other hand, in cases of large white kidney, there is a more scanty secretion of water, and, in consequence, a state of hydræmia and dropsy. In the first class of cases the urinary solids accumulating in the blood probably excite the contraction of the systemic arterioles, and thus directly cause their hypertrophy and indirectly that of the left ventricle. In the second class of cases—those associated with large white kidneys—while there is some retention of urinary solids there is a disproportionate accumulation of water, by which the contaminated blood is diluted and thus rendered less irritating to the minute arteries.

This is probably, in part at least, the explanation of the fact that the arterial resistance is less and the cardiac hypertrophy less constant and pronounced in cases of large white kidney with consequent hydræmia than in cases of contracted kidney with resulting uræmia.

2nd. But there is yet another class of facts to be taken into account. In connection with the lardaceous form of disease in the kidney and in other organs there is usually a widely spread tendency to degeneration of the small arteries. The degenerative changes affecting the muscular walls impair their contractile power, and the result is not an increase but a decrease of resistance in the terminal arteries. The peripheral resistance which calls forth increased contraction with resulting hypertrophy of the left ventricle is wanting, and in these circumstances there is absence of cardiac

hypertrophy. It appears to me that these apparently exceptional cases prove the rule; namely, that there is, in the absence of valvular lesions and senile degeneration of the walls of the large arteries, a direct and constant relation between hypertrophy of the left ventricle of the heart and hypertrophy of the muscular walls of the minute arteries throughout the body. The occasional, though rare, occurrence of fatal cases of contracted kidney without cardiac hypertrophy is not inconsistent with the theory that when they coexist they stand to each other in the relation of cause and effect. Cases of large white kidney sometimes run their course without the occurrence of dropsy; but we do not, therefore, deny or doubt that the dropsy which is frequently associated with this form of disease is a result of the blood changes induced by the renal disease.

It is stated at p. 289 of the author's paper "that in eighteen cases the left ventricle was found hypertrophied without valvular disease or pericardial adhesion to explain it, while the kidneys were healthy excepting that they were slightly granular, or, in other words, 'coarse' and contained some cysts." It is therefore inferred that in these eighteen cases the hypertrophy was "the older and preceding condition." Upon this statement I would suggest—1st. That a minute examination of the kidneys might have shown more advanced degeneration than appeared upon the surface; and, 2nd, it is not unlikely that in some of these cases the cardiac hypertrophy was, in part at least, explained by degeneration of the walls of the larger arteries, as in some of their cases occurring in old people to which I have before referred.

The atheromatous degeneration of the large arteries, which is often associated with chronic Bright's disease, may be in part, perhaps, explained by the noxious influence of contaminated blood, and in part by the undue strain to which the arteries are subjected between the two opposed forces, that of the hypertrophied left ventricle behind and that of the resisting muscular arterioles in front.

I need not here repeat my objection to the author's

doctrine that hypertrophy of the left ventricle is always associated with and explained by the hyalin-fibroid change in the minute arteries; this change being, as I believe, not a pathological change at all but the result of artificial influences operating after death.

The statement that the morbid changes included under the head of chronic Bright's disease with contracted kidney are the result of "causes not yet ascertained" is not in accordance with the results of my own observation.

There are few cases of this form of disease which are not traceable to some probable cause. Amongst the most common causes is the excessive consumption of food and of alcoholic stimulants either with or without the association of decided gouty symptoms; but I have seen many cases in which, as I believe, this form of renal disease has resulted from chronic dyspepsia in persons of strictly temperate habits. The one condition which is common to all these cases is that the degeneration of the kidney is directly caused by the excretion of abnormal and irritating materials, the products of faulty digestion; more commonly associated with habitual excess of food and stimulants, but not rarely without such excess.

I have always maintained that Bright's disease, in all its forms, may be traced back to some anterior morbid change in other tissues and organs, the proximate cause of the renal disease in every case being the contamination of the circulating blood by abnormal products, which the kidney, in the discharge of its excretory function, is called upon to eliminate.

The doctrine that the area of the morbid state under discussion "may be said hypothetically to correspond to the area vasculosa" is too transcendental for everyday use, and it is not in accordance with the fact that the morbid changes are, as a rule, limited to definite portions of the arterial system, while the veins and capillaries, excepting only the Malpighian capillaries, are not implicated. The term "arterio-capillary fibrosis" appears to be a misnomer, for the capillaries generally are unchanged, and if my interpretation of the phenomena is correct there is no morbid "fibrosis" of the arteries.

In conclusion, I beg to express my thanks to Sir William Gull and Dr. Sutton for having so forcibly directed attention to the cardio-vascular changes in chronic Bright's disease. Differing as we do entirely in our interpretation of the phenomena we agree in the opinion that the subject is one of great interest and importance.

THE PATENT

UNIVERSAL DRY CLOSET.

MR. LOUIS MOSER,

12, Anglesea Place, Southampton,

Sole Proprietor.

TRADE MARK:—

"UNIVERSAL DRY CLOSET."

The disposal of excrement by arrangements less offensive and deleterious than the midden or cesspool system—(so general in the Towns and Villages of this Country)—is a question of the most urgent necessity. It is generally conceded that improvement in the public health is largely promoted by the abolition of cesspools, but the alternative of these unsanitary arrangements is a question of considerable controversy. The advocates of the Water-carriage system insist upon the general adoption of Water Closets. But in the present state of the Sewage question Municipal Authorities regard with little favour any plan that would increase the volume of Sewage water, and in many cases they are prohibited from constructing any new connections

to the Sewers. Even were the ultimate mode of disposal of Sewage settled, and every town had decided upon its scheme of Sewage utilization, the erection of works or the preparation of farms, together with the requirements of increased water supply (a subject of no small difficulty in manufacturing districts),new Sewers or enlargement of old ones, laying drains, &c., must necessarily postpone the abolition of Cesspools to an indefinite period, whereas our Sanitary necessities are *immediate*.

Under these circumstances, considerable attention has been directed to Dry Conservancy as a means of escape from a portion of this Sanitary difficulty. Various schemes of Dry Closets have been proposed, and some have been extensively tested, but hitherto none of these have been found to meet *both the Sanitary and economical requirements of Municipal Authorities.*

Where such plans have proved efficient Sanitarily, the economic conditions have been an insuperable obstacle to their extension in towns.

Earth Closets have obtained admittedly a high character for efficiency, but this efficiency is secured by means at variance with sound economic principles. To provide, prepare, and cart *into* a town the enormus bulk of dry earth required to properly deodorize the excreta of the inhabitants, and then cart the increased bulk and weight *back* to the country, is a task no Sanitary Authority has ventured to undertake.

Other systems of (*so-called*) Dry Closets, whilst economically satisfactory, are sanitarily most objectionable. Some are but one remove from the offensive Cesspool, the excreta being collected without any attempt to deodorize them. The inventors of the "Universal Dry Closet" claim for their system *Sanitary efficiency with economy, both in supervision and cost of working.* Under the "Universal" system, *all* the dry refuse of towns, that under other conditions *must be carted away, is retained and made available for Sanitary purposes.* Fine ashes, the sweepings of flues, dry street sweepings, sweepings of hay and straw stores, spent dye woods, cotton, wool, and flax manufacturers' waste, &c., are by the simple

mechanical details of the "Universal" utilized so as to produce the best Sanitary result, with the lowest cost in working, and increased value of the product.

DESCRIPTION OF THE APPLICATION OF THE PATENT "UNIVERSAL DRY CLOSET" TO EXISTING CONVENIENCES.

The Cesspool being first cleansed out, is well dressed with fresh lime, filled up with dry ashes, brick rubbish, or other material, and flagged six inches below the Closet floor. It is then ready for the adoption of the Patent "Universal" arrangement, which combines the two essential requirements of a PERFECT DRY CLOSET.

1st.—The entire and immediate absorption of the urine, effected in the front compartment by refuse matter, in itself utterly worthless, and requiring no special preparation.

2nd.—Efficient deodorization and covering of the solid excreta with a layer of fine ashes or other deodorizing material, mixed, if desired, with disinfectants. This deodorization being effected by automatic arrangements, the only alteration necessary is to charge the receptacle and fill the Hopper once a week or fortnight, according to the number using the Closet.

Thus it combines the maximum of sanitary efficiency with the minimum of expense in application and supervision in working, and is the only Dry Closet that meets the various requirements of Municipal and other Local Sanitary Authorities.

THE SPECIAL ADVANTAGES OF THIS CLOSET ARE:—

1ST.—SANITARY EFFICIENCY.

The chief difficulty of municipal authorities, particularly in manufacturing districts, is the rapid and economical removal of excreta from towns. This Closet provides a thoroughly efficient

and practical method, which, whilst economical in application, fully answers all sanitary requirements. It is perfectly workable in large towns, requires no special attention or care on the part of the users, and the minimum of supervision on the part of the local authorities.

These most important results are attained by the distinct and separate arrangements for deodorizing the solids and absorbing the liquids, which are not possessed by any other Dry Closet in existence, and by the simplicity and perfection of the distributor. These arrangements, by enlarging the range of defecating material, not only render the system workable in towns, but provide a Closet *second to none in sanitary efficiency*, as the direct and most powerful absorbents can be used for the urine, whilst the most suitable deodorizers can be applied to the fæces.

Under all Earth Closet systems the range of available material is limited to earth, or earth and ashes, while in this Closet any dry refuse can be used.

The advantages gained over the ordinary forms of Dry Closets, in which weaker absorbents are used and the excreta left uncovered or imperfectly hidden, is too obvious to need any further comment. In Closets in daily use by forty or fifty men the deodorization is perfect and complete. No odour is perceptible, even when the receptacles are allowed to stand full for days, and even weeks, in a confined place, and one receptacle used thus has been kept for four months without any offence. It is impossible to have a stronger proof of the efficiency of this Closet.

2ND.—CONVENIENCE IN USE AND IN MUNICIPAL ARRANGEMENTS.

No special instructions are requisite for the general use of this Closet. The hopper only requires re-filling when the receptacle is changed. The absorbents used are always readily available, and must, if not thus utilized, be carted away. Each town thus furnishes its own supply, without having to resort to any external source. The newly-charged receptacle itself, when

carted to the house, contains the material for re-charging the hopper.

The weight of the absorbents and deodorizers used is *very small* compared with other systems. While ordinary Earth Closets require fully 1¾lb. of dry earth to defecate the evacuation of each person, this Closet requires only half that weight. A corresponding saving is therefore effected in the preparation and cartage of material, and in the removal of manure, the latter being also increased in value. *No special arrangements are required for the conveyance of the receptacles through the streets, whether full or empty, as is the case in some systems.*

3RD.—ECONOMY IN APPLICATION.

This arrangement is applicable to all existing privies, at a small cost.

The alterations effected are such as will facilitate the ultimate adoption of Water Closets where required, thus providing an immediate remedy for the existing difficulty at a small cost.

The expense of cartage, when systematically carried out, will not much exceed the existing disgusting system.

4TH.—VALUE OF THE MANURE.

On this point it is needless to say more than that the value of human excreta is well known, and is doubtless the source from whence the national supplies of fertilizers will be principally drawn in the future. It is obvious, therefore, that this must bring in a large revenue when systematically collected.

AGENTS FOR THE SUPPLY OF THE "UNIVERSAL DRY CLOSET."

Leeds—Mr. THORP, 20, Bowman Lane, Leeds.
Stockport—Mr. WILLIAMSON, 25, Warren Street, Stockport.
Southampton—Messrs. BULL & SONS, Builders, Belvidere, Southampton.

LIST OF PRICES.

	£	s.	d.
No. 1.—Complete *self-acting* Apparatus, consisting of hopper (holding about 50 charges), levers, &c., ready for adaptation to any existing closet or privy	1	2	6
No. 2.—Ditto, with extra large hopper, holding about 80 charges...	1	5	0
No. 3.—Complete pull-up apparatus, consisting of hoppers, levers, &c., with japanned pull handle	1	7	6
No. 4.—Ditto, with extra large hopper	1	10	0
No. 5.—Ditto, with ordinary sized hopper, lever, &c., brass and porcelain pull handle, enamelled rim, &c., ready for fixing	2	0	0
No. 6.—Ditto, extra size hopper	2	2	6

The above prices do not include receptacles.

COMMODES.

	£	s.	d.
Strong *self-acting* Commode for Nursery House, in plain deal, varnished, with light portable pail complete, ready for use	3	3	0
Ditto, ditto, grained to correspond with bedroom furniture	3	7	6
Ditto, ditto, in mahogany, French polished	5	10	0

These Commodes are only 22 inches long, 24 deep, and 32 inches high; sound and well-made articles. Larger sizes manufactured to order.—Unless otherwise ordered, a bag of deodorising material is sent out with every Commode.

RECEPTACLES.

	£	s.	d.
No. 1.—Strong Galvanised Iron Receptacles, size 20-in. diameter, 16-in. deep, with handles and patent divider	0	10	0
No. 2.—Ditto, ditto, 23-in. deep	0	11	6
No. 3.—Extra strong non-absorbent Wood Tubs or Receptacles, with strong iron handles and patent dividers, round, 20-in. diameter, 16-in. deep, to hold about 50 charges	0	7	6
No. 4.—Round ditto, 20-in. diameter, 21-in. deep, to hold about 80 charges	0	10	0
No. 5.—Oval ditto, 24 + 20-in., 16-in. deep	0	9	0
Extra Pails for Commodes (Galvanized Iron)	0	8	0

SUNDRIES.

	£	s.	d.
Framed Seat, ready for fixing in ordinary privies, consisting of bearers, seat, fall, riser, hopper box, with No. 1 complete self-acting apparatus; any size from 2-ft. 6-in. to 3 ft. 6-in. in width	2	5	0
Ditto, ditto, with No. 2 apparatus	2	7	6
Strong Wood Lids for receptacles, iron-hooped, and painted with non-absorbent paint, Nos. 1 to 4, per dozen	2	0	0
No. 5.—Oval ditto, per dozen	2	7	0
Strong Hessian Bags, to hold one charge of deodorizing material for filling hopper, per dozen	0	8	0
Ditto, ditto, containing samples of absorbent and deodorizing powder, each	0	1	0

Packages and Carriage in all cases charged extra.

The attention of Local Boards and Sanitary Authorities is directed to the circumstance that the Seats are specially designed to facilitate the adoption of the "Universal Dry" system of conservancy: being sent out complete in every respect, they can be fixed by any joiner in a couple of hours after the cesspools are filled up and properly flagged.

THE "UNIVERSAL CLOSET LATCH."

	£	s.	d.
Twelve variations of Keys, numbered 1 to 12, per dozen...	0	17	0
Keys for ditto, per dozen	0	5	0
Master Keys, to pass all the latches, per dozen ...	0	9	0

The Latches are designed to facilitate systematic removal under this system. There are twelve variations of keys, but the whole are passed by a master key, giving the Corporation or other servants access to the closets and ashplaces without annoyance to the owners. These Latches are simple, strong, and cannot be picked except by expert locksmiths.

In order to meet the wishes of Corporation and Sanitary Committees introducing this system, the Proprietor will send out competent persons to submit plans and advise generally on the alteration of existing Privies, &c., the organization of the plans for the collection of material, and the removal and manufacture of the manure.

TESTIMONIALS.

(From J. H. ALDRIDGE, Esq., M.D., M.R.C.S., &c., &c., Southampton.)

SOUTHAMPTON, 25*th September*, 1871.

DEAR SIR,

I have great pleasure in bearing my testimony to the value in a sanitary sense of your "Dry Closet." I have seen others, but consider your patent far superior to any other extant. I trust that you will experience that success that your presevering industry in this department deserves.

Yours very truly,

JOHN H. ALDRIDGE, M.D., Edinburgh, M.R.C.S., London, L.A.C., &c., &c.

L. MOSER, Esq.

(From Messrs. JOSEPH BULL & SONS, Contractors, Belvidere, Southampton.)

SOUTHAMPTON, 25*th September*, 1871.

DEAR SIR,

We have had your Closet fixed in our works for nearly two months, and given it a most severe test, inasmuch that it is fixed in the Joiners' Shop, and has been in constant use daily by forty or fifty men. We consider it a valuable invention, and is highly satisfactory.

Yours truly,

JOSEPH BULL & SONS.

Mr. MOSER.

(From JAMES LEMON, Esq., C.E., &c., Borough Surveyor of Southampton.)

PUBLIC HEALTH OFFICES, SOUTHAMPTON, *September 25th*, 1871.

SIR,

I have much pleasure in stating that your "Dry Closet," which you erected at my request at Messrs. Bull & Sons' Works, so as to give it a severe test, has answered exceedingly well.

It has this great merit, that any *dry refuse* can be used in its working, thus removing the difficulty in connection with other closets, which necessitate a constant supply of *dry earth*.

JAMES LEMON, C.E., M.R.I.B.A.,
Mr. Louis Moser, Southampton. Borough Surveyor, Southampton.

(From Dr. SYSON, Medical Officer of Health for the Borough of Salford.)

Town Hall, Salford, General Health Department, *Nov. 15th*, 1871.

Sir,

I visited the Leeds Exhibition of dry closets, and carefully examined the various models there exhibited. I feel no hesitation in stating that your distributor appeared to me the best of all to meet the requirements of a dry system of conservancy, and I consequently have brought it under the notice of the General Health Committee of this Borough, who have ordered a number to be erected in Salford, for which you will receive the order in due course.

I am very glad to find that the suggestions which I made to you some two years ago are now practically carried out. On the evils consequent on the old open ash-pit system I need not enlarge, for they now exist only on sufferance. The principle of your closet appears so excellent, that I feel by the use of your distributor the tub closets in Salford might be rendered sightly, inodorous, and economical.

I am, Sir, yours sincerely,

E. J. SYSON,
Medical Officer of Health for Salford.

(From Dr. DUNCAN, M.B., London, F.R.S.)

To L. Moser, Esq.

I have great pleasure in bearing my testimony to the excellence of your Universal Dry Closet, with whose details I am familiar. I shall not hesitate to advise those who require such articles to obtain yours.

T. MARTIN DUNCAN, M.B., London, F.R.S.

Lee, Surrey, *October 2nd*, 1871.

(From M. J. MAC CORMACK, Esq., M.B., S.C.D., Medical Officer of Health, Lambeth.)

I have carefully and on more than one occasion examined the system of Dry Closets, introduced and patented by Mr. Louis Moser, of Southampton. I have great pleasure in bearing my testimony to its value as a sanitary measure.

It is especially adapted for towns and villages where no arterial drainage exists, also for private dwellings, manufactories, and other public institutions where water closets are not applicable.

The mechanism is simple, not so easily put out of order as water closets, and the products are available for fertilisation, which is what Professor Liebig so strongly urges upon all communities as the proper use of sewage, instead of its being allowed to run to waste as it now is.

M. J. MAC CORMACK, M. B. (S.C.D.),
Medical Officer of Health, Lambeth.

(From M. K. ROBINSON, Esq., M.D., &c., Medical Officer for Leeds.)

Officer of Health's Department, Leeds, *29th January*, 1872.

Dear Sir,

In reply to your enquiry respecting the value of your experimental "Dry Closets" put in at your own expense with the sanction of the Sanitary Committee at Leeds, I have great pleasure in informing you that in a sanitary point of view they are a great success. The simplicity of the self-acting mechanism employed, by which every deposit is at once covered and disinfected, is in my opinion a great recommendation, and in this particular surpasses any of the dry closets yet introduced into practical operation in Leeds. A Chancery injunction restrains the Corporation at present from making any fresh connexion of water closets with the existing system of sewage, and there is also on the part of owners of property an objection to water carriage as a means of dealing with excreta. You have therefore introduced your patent at an opportune time, which, as a substitute for the old fasioned midden, is an immense improvement, and one well worthy of adoption where insuperable objections to water carriage exist.

I am,

Yours, &c., &c.,

(*Signed*) M. K. ROBINSON, M.D., &c.,
Medical Officer of Health for the Borough of Leeds.

L. Moser, Esq.

Sanitary Department, Leeds, *29th January*, 1872.

Sir,

I, and the undersigned proprietor James Wood, have great pleasure in bearing our testimony to the excellence, in a practical sense, of your "Universal Dry Closet," which you erected in the Fox and Grapes Yard,

Kirkgate. I have seen various ideas carried out, but consider your patent far superior to any of them.

We are,

Yours faithfully,

(*Signed*) ROBT. LANKESTER.
JAMES WOOD.

L. Moser, Esq.

(From W. WRAY, Esq., Chairman of the Sanitary Committee, Leeds.)

Officer of Health's Department, Leeds, 31*st January*, 1872.

Dear Sir,

In reply to your letter, I beg to say that I have been pleased with the result of your experimental Closets, in which you yourself had sufficient confidence to induce you to introduce at your own cost in Leeds. Allow me to thank you for showing the practicability of improving the sanitary condition of towns by your ingenious invention. If you will make a proposition in writing, saying what you will undertake to cleanse one district of the Borough for, introducing your system after owners of property have filled up their sumps, I will lay such a proposition before the Sanitary Committee at an early meeting.

I remain,

Dear Sir,

Yours faithfully,

(*Signed*) WM. WRAY,
Chairman of the Sanitary Committee, Leeds.

L. Moser, Esq.

Nuisance Inspector's Office, Corn Exchange, Leeds.

31*st January*, 1872.

Dear Sir,

I have great pleasure in bearing testimony to the efficiency of the Dry Closets invented by you, which I have seen tested in this town. They are far superior to the old privy, and with proper supervision may be made applicable to all large towns.

Dear Sir,

Yours truly,

(*Signed*) T. NEWHOUSE,
Inspector of Nuisances, Leeds.

L. Moser, Esq.

25, Black Swan Yard, North Street, Leeds, 4*th March*, 1872.

Dear Sir,

I have great pleasure in certifying to the success of your patent Dry Closet, and the reception room for ashes, &c., now in operation in the Black Swan Yard.

* * * * * * *

Yours respectfully,

L. Moser, Esq. JOSH. SIMPSON.

(From Mr. E. MUSPRATT, General Manager of Works at Eaton Hall, Seat of the Marquis of Westminster.)

Eaton Hall Works, Chester, 15*th April*, 1872.

Dear Sir,

After some two months' use of one of your Dry Closets, I feel it both a pleasure and a *duty* to testify to the very efficient working of the same, thereby producing the most entire sanitary results.

* * * * * *

Yours truly,

(*Signed*) E. MUSPRATT,
General Manager of Works.

Mr. L. Moser.

(From C. W. HANKIN, Esq., B.A., Head Master King Edward VI. Grammar School.)

The Polygon, Southampton, 26*th April*, 1872.

Dear Sir,

I have much pleasure in reporting to you the continued efficiency of the Earth Closet, constructed after your patent, which has now been in constant use for the last three months. Nothing can be simpler or less troublesome than the management it requires, and, unlike the old water closets, any trifling derangement which may occur from the lack of familiarity with removing tub, charging the hopper, &c., on the part of servants, is easily and speedily remedied without the expense and nuisance which are incurred in manipulating the latter. It would give me much satisfaction in case the Trustees of this School should be induced to introduce your closets in the alterations about to be made in the building of this School, the present water closet being a constant source of trouble, expense, and annoyance, being constantly liable to be out of order and never free from ill odour.

Yours truly,

C. W. HANKIN, B.A.,
Head Master of King Edward VI. Grammar School, Southampton.

Louis Moser, Esq.

(From J. R. STEBBING, Esq., J.P.)

MANOR HOUSE, MILLBROOK, NEAR SOUTHAMPTON, 26*th April*, 1872.

I have much pleasure in bearing the strongest testimony in favour of the "Universal Dry Closet." I had some doubts, before seeing it practically used, of its success; but having done so where any defects would be most readily developed, I can testify to its simplicity, freedom from damage, and cleanliness. I am about to fix one in my house.

(Signed) J. R. STEBBING, J.P.

LOUIS MOSER, Esq.

(From J. PEPPER, Architect, Bradford.)

LEEDS, 9*th May*, 1872.

SIR,

I have inspected one of your Earth Closets to-day, and am much pleased with same. Are free from smell, and work in every way satisfactory. I shall be glad to recommend the same in our district.

I remain, Sir, yours respectfully,

LOUIS MOSER, Esq. J. PEPPER, M.A.

(From J. H. THORP, Builder, Leeds.)

LEEDS, *May* 13*th*, 1872.

DEAR SIR,

I have the highest opinion of your Patent Universal Dry Closet, and having used a considerable number by other makers, I think yours combines superior qualities to any others at present in the market, which will lead to their being very extensively used.

I am, dear Sir, yours faithfully,

LOUIS MOSER, Esq. JOHN HALL THORP.

WINCHESTER, *May* 23*rd*, 1872.

DEAR SIR,

The Closet you sent to me has been tried, and answers perfectly, and is the most cleanly apparatus ever introduced into workshops or dwellings that has come under my notice. There is not the slightest offence, although in constant use.

I remain, dear Sir, yours truly,

LOUIS MOSER, Esq. T. S. MORRIS.

(Extract from a Report to the Grimsby Town Council, by JOSEPH MAUGHAN, C.E., Borough Surveyor.)

"Assuming then that the Water Closet system is not likely to obtain general prevalence in Grimsby, I have to recommend the adoption throughout the town, wherever new privies are contemplated or old ones require to be remodelled or amended, of "The Patent Universal Dry Closet," specimens of which have been by your direction erected within the precincts of the Town-hall, and one of which I put up on my own premises at the same time, and which without any other supervision than that of a female domestic has acted perfectly, largely economising the consumption of coal by the enforced use of the dry ash screen, and in other respects giving unqualified satisfaction."

SURVEYOR'S OFFICE, TOWN-HALL, GRIMSBY, 2*nd July*, 1873.

DEAR SIR,

I have the pleasure to inform you that the Closets erected here for the convenience of the police and parties frequenting the Magistrates' and County Courts have answered perfectly, and that from a knowledge of this circumstance, and the satisfactory action of the one put up at my dwelling-house, the Town Clerk and Borough Treasurer have had them fixed within their premises in substitution of the cesspools previously in use, and that they are both highly pleased with the result.

This example is being followed at hotels and private houses, and I have no doubt whatever that in a very short time the Dry Closet will really become "universal" in Grimsby.

The Closets now being fixed at the Docks here of the Manchester, Sheffield, and Lincolnshire Railway Company are about completed.

I remain, dear Sir, yours truly,

JOSEPH MAUGHAN,

L. MOSER, Esq. Borough Surveyor.

DEAR SIR,

I had the pleasure of visiting the Leeds Exhibition of 1871, and was glad to find such deep interest taken in Dry Closets. After carefully examining several models which were there for inspection, I have no hesitation in saying, that on account of the simplicity of the self-acting mechanism employed, the one exhibited by Mr. Louis Moser, of Southampton, was the best, being both simple and strong, and in every way adapted for the purpose for

which they are designed. I at once recommended the Health Committee to allow two of the Closets to be fixed on the Corporation premises—one for the use of the public and one for the police force—which they did, and I have great pleasure in stating that they have in every way given satisfaction. During the year 1871 we had an outbreak of small-pox in the borough, and it was found necessary that hospital accommodation should be provided for the patients. In the erection of a building for that purpose, two of the Dry Closets were fixed and have worked well.

In addition to the above, several private gentlemen have had them fixed, and I am hoping to see them universally used, as I am perfectly satisfied it is one of the best systems in operation.

I am, yours very respectfully,

GEO. SIMINSON,

Mr. L. Moser. Sanitary Inspector, Grimsby, Lincolnshire.

Cavalry Barracks, Colchester,

30th July, 1872.

Sir,

In reply to your note of the 25th instant, requesting me to let you know the result of the trial made of your "Universal Dry Commode" in the Hospital of this Regiment, and my opinion of its suitableness for use in Egypt, I have to state that it has been found highly satisfactory.

The Commode has now been continuously in use in one of the wards for about two months and a half, the first part of the time with the deodorizing material which you yourself provided, but latterly with only dry clean sweepings off the road, ashes, &c., and during the whole of that time—excepting on two occasions—there was not the least offensive odour perceptible from it, although the excreta were left unremoved for upwards of three weeks, and notwithstanding the prevalence of unusually hot sultry weather. Its failure of perfect success on the two occasions alluded to were found to arise in one instance from the omission on the part of the attendant to place any material in the front compartment, as directed, for absorbing the urine, and in the other from the contents of the hopper being so pressed into it that they could not be discharged when the Commode was used.

If Egypt has not greatly changed for the better in its sanitary provisions in this respect since my tour of duty there in 1857-58, such a Closet as yours should prove of inestimable value, and I consider that your system is especially adapted for general use in that country, on account of the difficulty of obtaining and applying water for such purposes.

I am, Sir,

Your obedient Servant,

THOS. FRASER, M.D.,

Surgeon Major,

L. Moser, Esq., &c., &c. 10th Hussars.

Camp, Colchester,

3rd September, 1872.

One of Mr. Louis Moser's "Universal Dry Closets" has been in use at this station for the last three months under my observation, and I have found it to answer its purpose very well.

The deodorizing material supplied for filling the "Hopper" seems exceedingly good and effective.

A Closet of this kind is likely to prove very useful in countries where the rain-fall is scanty, as in Egypt, Scinde, &c., also in country houses and other places at home, where the removal of sewage by water is not possible.

If properly understood and the directions attended to no nuisance is experienced from this Closet.

J. SINCLAIR,

Surgeon-Major,

33rd Regiment.

Eton Union, Slough,

24th December, 1872.

Sir—

I believe your Closets to be the best of any invention that I have seen. They have been under my superintendence for about two months, and we never had the slightest smell from them in the casual wards as we used to have with the water closets, and I therefore believe them to be the most genuine piece of furniture ever made.

Yours truly,

GEO. DICKMAN,

Porter,

Eton Union, Slough.

(From the Master of Eton Union, Slough.)

Master's Office,

Eton Union Workhouse,

Slough, Bucks,

5th February, 1873.

Dear Sir—

In answer to your letter received this morning, in which you asked for my opinion respecting your Dry Earth Closets supplied to this Workhouse some few months ago, I have much pleasure in stating that there are

thirty-three of them in use in the Vagrant Cells of this Union, and that they answer the purpose for which they are intended remarkably well in every way; not once during the four months they have been in use have I observed the least disagreeable smell arising therefrom.

Having had experience in different Workhouses for a period of nearly twenty years, I have during that time had many chances of seeing different plans adopted for getting rid of night soil, &c., &c., and I have no hesitation in saying I consider the dry earth system the best, and that the Universal Dry Closet supplied by you is far superior to any I have yet seen (by other makers).

Your Earth Closet being self-acting, which causes a charge of earth to be thrown at once into the receptacle every time the Closet is used, entirely prevents the chances of bad smells arising from neglect of pulling the handle, as in other Closets, and which is so often omitted by many persons, especially among the class of workhouse inmates. Nothing, in my opinion, could be better for any large institution than your Universal Dry Earth Closet, and I shall be pleased to show anyone who may have a wish to see them in use.

I am, dear Sir,

Yours very truly,

J. T. SIMKINS,

Master.

To L. MOSER, Esq.,
Southampton.

(From Messrs. JOSEPH BULL and SONS, Builders.)

SOUTHAMPTON, *6th February*, 1873.

DEAR SIR,

In reply to your inquiry relative to your Universal Dry Closet, we beg to say that it has been in daily use in our Establishment by at least fifty or sixty men for eighteen months, and has given us every satisfaction, and continues to do.

The many scientific men who have seen it here in use expressed themselves much pleased with it.

Yours truly,

JOSEPH BULL & SONS.

L. MOSER, Esq.

FOX AND GRAPES YARD, Kirkgate, Leeds.

12th February, 1873.

Louis Moser, Esq.

DEAR SIR—

I have great pleasure in bearing testimony to the efficiency of your "Universal Dry Closets" of which I have six under my notice continually in use, having been so now for eighteen months, and performed the duties for

which they are intended satisfactorily to myself and others who resort to them.

There being no disagreeable smell perceptible, they are far more preferable than the old system of Closet, or even Water Closets (which are so liable to sudden derangements), being simple in construction, self-working, only requiring the hopper attached to be properly charged with dry ash, sawdust, or any other refuse of a drying nature, a quantity of which, by the self-acting apparatus, is discharged on every occasion the Closet is used.

The same will be shown to any Sanitary Committee Gentleman desirous of seeing the working.

I remain, yours respectfully,

JAS. WOOD

(Wood turner).

Southampton.

Southampton Times Steam Printing Works, Above Bar.

SOME OBSERVATIONS

ON

FIRE-PLACES, STOVES,

AND

VENTILATION,

BY

MESSRS. FREDERICK EDWARDS & SON,

Stove and Range Manufacturers, Hot Water Engineers,

No. 49, GREAT MARLBOROUGH STREET,

REGENT STREET, LONDON.

LONDON:

LONGMANS, GREEN & CO., PATERNOSTER ROW.

PRICE SIXPENCE.

CONTENTS.

	Page
Some Observations on Fire-places	3
Fire-brick grates constructed with fire-brick exterior and improved regulator, &c.	5
Ventilating Grates	6
Smokeless Grates	8
Stephen's and King's Patent Grates	9
On Stoves	9
Pedestal Hot-Air Stoves	9
The Gill-Air-Warmer	10
Dr. Arnott's Slow-Combustion Stove	11
Goldsworthy Gurney's Stove	11
Pierce's Pyro-Pneumatic Stove	11
Heating by Hot Water Circulation	12
On Kitcheners and Kitchen Ranges	12
Smoky Chimneys	13
Ventilation	14
The Smoke of Towns	14
The Utilization of Waste Heat	14
On Heating a Number of Houses from a Single Source	15

SOME OBSERVATIONS

ON FIRE-PLACES, &c.

In a well warmed room there is no perceptible draught, and a person may occupy any part of it without a sense of discomfort. In a badly warmed room a person feels compelled to place himself in proximity to the fire and becomes heated on one side while he remains chilly on the other. In a well constructed grate the fire burns with a steady glow, it surprises by its tenacity of life and leaves little but incombustible matter as refuse. In a badly constructed grate a great portion of the coal appears to turn to cinder, the fire becomes extinguished if not constantly attended to, and, instead of promoting equanimity, it tries the temper. It is essential in a good grate that the whole interior should be of fire-brick. This material retains heat, radiates it powerfully, and assists greatly in the perfect combustion of the coal. Within the last fifty years, the value of fire-brick has become largely appreciated, but unfortunately a very large number of grates, put in by builders to modern houses, have the interior entirely of iron.

Fig. 1 shows the simplest form of fire lump grate, in which only the bars and bottom grate are of metal. This grate is generally fixed within a fire-place as shown in fig. 2, with fire-lumps contained above at *a a*, with ornamental tiles or slabs of slate or iron at the sides *b b*, to fill the opening, and with a register door fixed above the grate to check the escape of heated air up the chimney. The advantage of the register door is considerable, for by checking the unnecessary escape of warm air from the room, it reduces the quantity of cold air that can find access to the room through doors and windows. Useful grates of this kind, even when fitted up with tiles, cost very little more than the cheap and execrable ones commonly put in by builders. The price of fig. 1 varies from 11s. to 22s., according to size, when made of dimensions suitable for ordinary domestic use. When fitted with tiles complete, as shown in fig. 2, the price is generally from 30s. to 40s., supposing the tiles to be of an inexpensive character. The same grate is made for large public rooms with the fire bars as wide as 24, 27, or even 30 inches. The price of the grate alone, as fig. 1, is then from 33s. to 45s.

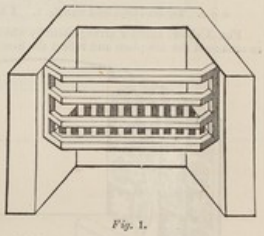


Fig. 1.

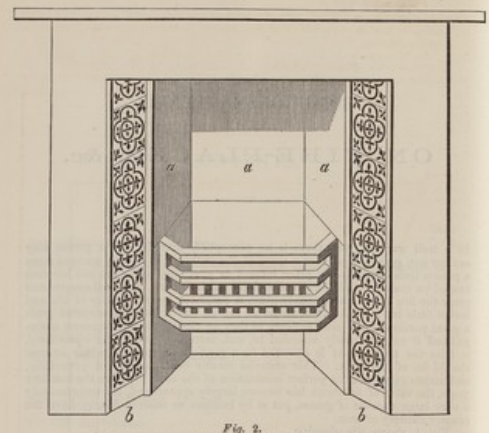


Fig. 2.

a a a. Top fire-lumps used with fig. 1. *b b.* Tiles at sides to fill opening.

Fig. 3 shows another arrangement by which tiles are still further introduced to ornament the fire-place and reflect the heat of the fire.

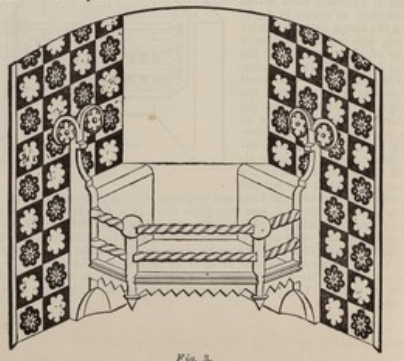


Fig. 3.

Fire-lump grate with brick back and sides, encaustic tile covings

Fire-brick grates constructed with fire-brick interior, fire-brick bottom, regulating ash-pan, and an improved regulating register to control the escape of warm air up the chimney.

These are the grates particularly recommended by Mr. Edwards, in his publication On Domestic Fireplaces.* As in Fig. 2, the interior of the grate

Fig. 4. Fire-lump grate, with brick interior, brick bottom, ash-pan below with regulator and improved regulator to chimney, with handle in front of the grate.

Fig. 5. Section, showing Fig. 4 as fixed in the fire-place, with fire-brick interior and improved regulator in chimney.

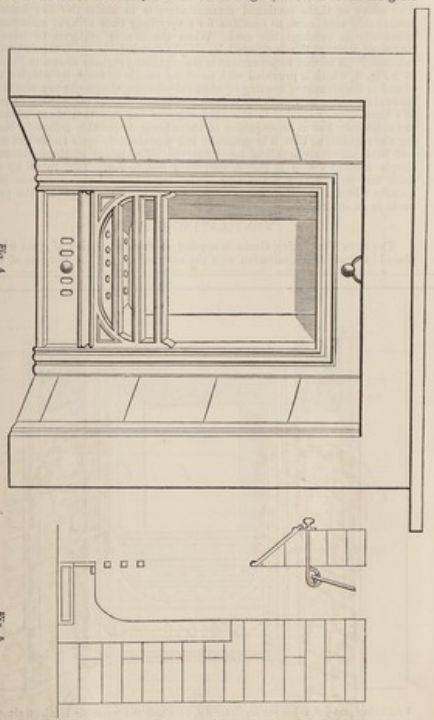


Fig. 4.

Fig. 5.

* Our Domestic Fire-places, new edition, re-written and enlarged, with 149 Illustrations and a Portrait of Count Rumford. London: Longmans and Co. 1870.

from the hearth to the chimney is formed of fire-brick, but instead of an iron grate for the coal to burn upon, a fire-lump bottom is introduced, fitting within a fire-brick chamber. A few holes which pierce the fire-brick bottom allow air to enter the fire from below, and dust to fall through from above. Below the brick bottom a small ash-pan is provided, shown at Figs. 4 and 5, which has a small regulator in front to allow of air being excluded or admitted at pleasure. The effect of these combined arrangements is to make the fire burn with remarkable steadiness, to continue for a very long time without attention, and effectually to consume the coal. When the fire is required to burn with increased activity, this is effected by simply opening the small regulator of the ash-pan.† A further improvement is the regulating register shown in Fig. 5 and at *a* Fig. 6, which is provided with an ebony handle in front, by which it can be closed or the amount of opening regulated at pleasure, the proper practice being to leave the register only sufficiently open to allow the smoke completely to escape. We have already alluded to the advantages of the register; these advantages are commonly lost in consequence of there being no suitable means for regulating the register, so that it is generally left permanently open to its full extent, and becomes of no practical use whatever. The prices of these grates, of a simple form as shown in Fig. 4, vary from 50s. to 70s. Tiles to fill up at the sides may be had at a small cost. The same general principles can be frequently adapted to other forms of grate, at a certain advance on the price if made in an ordinary way.

VENTILATING GRATES.

The term Ventilating Grate is applied to any description of grate which is placed in direct communication with the external air, for the purpose of ensur-

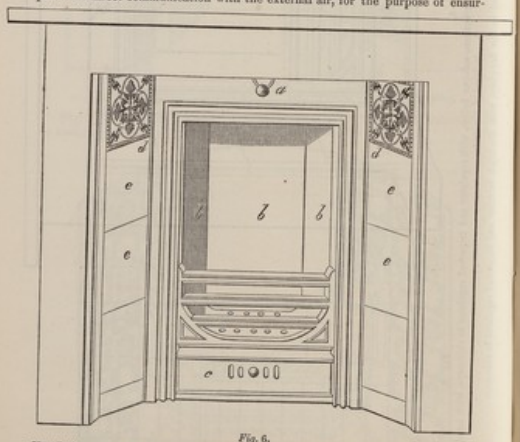


Fig. 6.

Ventilating grate, showing the regulator *d d*, by which the warm air is admitted; *a*, regulator to chimney; *b b b*, fire-brick interior; *c*, ash-pan; *e e e*, tiles at sides of grate.

† When coal is used which produces much dust, an iron grate should be used instead of the brick bottom.

ing a supply of fresh air, which is usually warmed before its admittance. The general plan is so to fix the grate as to leave a hollow chamber behind the grate. This chamber is placed by means of an air channel in communication with an external wall, and by means of certain openings or gratings, which form a part of the grate, it is placed in communication with the room. After a fire has been lighted some little time, a current of air from without passes along the air channel, becomes warmed by contact with the heated exterior portions of the grate, and passes through the openings provided for it into the room. A regulator is usually introduced for the purpose of excluding air or regulating its admission. By this system a considerable amount of heat which is usually wasted becomes utilized, the room is more equably warmed, the ample supply of air avoids both the possibility of a smoky chimney and any sensation of closeness, while draughts from doors and windows are most effectually checked. The grates shown in Fig. 6 can be readily constructed in this way, and so can most others. The principle entails an additional cost on each grate, varying from 20s. to 50s. in addition to the cost of constructing the air channel. All persons who are about to build should seriously consider whether or not they would adopt this system, as the trouble and cost are comparatively small if incurred when the house is in course of construction.

Many of the advantages mentioned are obtained by simply constructing an

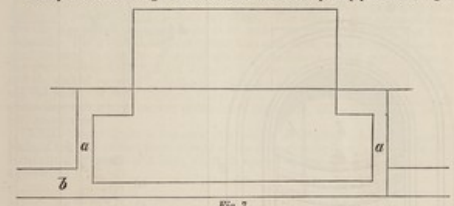


Fig. 7.

Plan of hearth, showing air channel, *a a a*, for admitting fresh air.
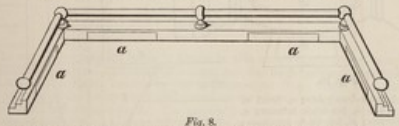


Fig. 8.

Fender curb, showing openings in the curb at *a a a a* for admitting fresh air.

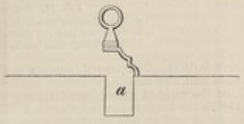


Fig. 9.

Section of fender curb and air channel.

air channel below the floor, from an external wall to the hearth stone. A grating provided with a regulator is put into the hearth to admit the air, or a fender

curb is so constructed as to cover the air channel and admit the air. This last method is shown in Figs. 7, 8, and 9. As the air enters the room in proximity to the fire, it becomes warmed; part passes to the chimney, the remainder circulates in the room. The heated exterior portions of the grate are not utilized in this case, but the other advantages are identically the same. It is most essential that the air channels should be of sufficient capacity, small ones being almost inoperative.

SMOKELESS GRATES.

Much has been done to relieve London from the smoke of furnaces, but in country towns and districts the manufacturers and hotel keepers are still allowed to waste their coal and pollute the atmosphere. Few are aware that nearly all domestic fires may be rendered nearly smokeless. The plan which is generally adopted was brought forward by Mr. Cutler more than fifty years ago, and

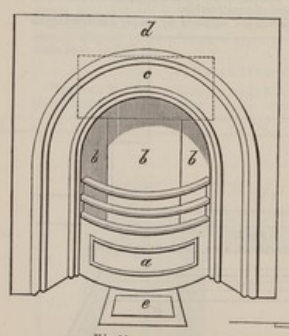


Fig. 10.

Improved smokeless grate; *a*, front of coal-box; *b b b*, fire-brick interior; *c*, sliding blower; *d*, handle to regulator; *e*, opening to ash-pit.

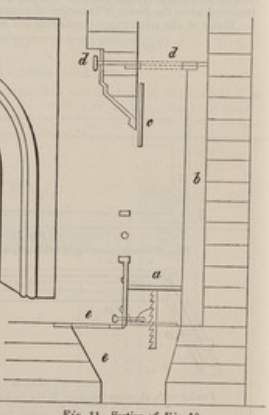


Fig. 11, *Section of Fig.* 10.

a, Coal Chamber with lifting bottom for raising coal; *b*, Fire-lump back; *c*, sliding blower; *d d*, regulator to chimney with handle in front; *e e*, ash pit.

revived by Dr. Arnott in 1854. The principle consists in simply making a fire at the top of a body of coal, and allowing it to burn gradually downwards like a candle, a torch, or the wick of a lamp, until the whole is consumed. A chamber is formed below the fire bars in which there is a movable bottom which is made to lift the body of coal as it is required for consumption. Every one can readily observe that nearly all the smoke of common fires arises from freshly burning fuel. Now, by the smokeless system no fresh fuel is thrown on the top of the fire, an incandescent mass of coal is preserved perpetually at top, the

layers of coal below become prepared for active combustion, and burn as they are raised to the air with little or no smoke. A fire lighted in this way has extraordinary vitality. Many have used the grate in sleeping rooms, and have found it very easy to rouse the fire into a state of activity after it has had several hours of slumber. It appears extraordinary that with all its advantages the grate has not come into more general use; for besides the prevention of the smoke, and the great vitality of the fire, chimney sweeping is almost entirely avoided by it, and it has been used for many years with great approval by a considerable number of persons. Objections to the grate have been to the effect that it is heavy in appearance, and that there is less flame than from a common fire. However applicable these objections may possibly be considered in the case of drawing rooms, they seem to have but little weight in the great majority of cases where simple utility is the primary object of consideration. The prices of these grates usually vary from 65s. to £7. and they cost from 30s. to 40s. for fixing.

STEPHEN'S AND KING'S PATENT GRATES.

Stephen's Patent Grate is very well known, as it has been more largely used during the last twenty years than any other. The effect of the recessed form and the contracted opening behind is to make the fire burn briskly and look cheerful. The polished surfaces reflect heat and light, while glittering bars of steel are used to hide the ashes and add to the brilliancy. The grate, however, has some drawbacks. A rapid draught is incompatible with economy in fuel, and the arched recessed form has the tendency to concentrate instead of diffusing the heat. We are disposed, therefore, to recommend in preference those grates in which a large surface of fire-brick is exposed, and where the fire burns with steadiness.

King's Patent Grate is the same in principle as Stephen's, but it is usually made of the preferable square instead of the semi-circular form, and the draught is regulated by means of a door, which is moved up and down by a touch with a poker.

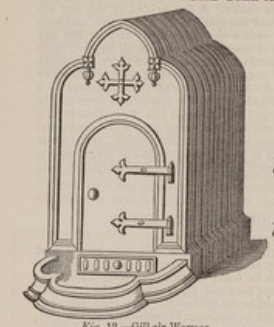
ON STOVES.

PEDESTAL HOT AIR STOVES.

The use of stoves is not much in favour in this country, except for warming the entrance halls and staircases of houses, and for large public rooms which could not be adequately heated by the ordinary open fire. Many have advocated the use of the bulky brick and glazed earthenware stoves much in use on the continent but without avail. The greater portion of economy attributed to these stoves is due to an absence of ventilation and to the care with which cold is excluded by thick walls and double windows. In our temperate and most variable climate such means of warming are not so applicable, and there appears to be no probability, therefore, of our abandoning the open fire.

Of the stoves we are in the habit of using, the ordinary pedestal stove is simply an iron case, which should in all cases be lined with fire-brick. There is an open fire in front and a pipe behind to convey the smoke to the chimney. This stove gives rather more heat than an ordinary fire-place, in consequence of its projection in the room, but otherwise it scarcely differs from it. A superior description is known as the pedestal hot air stove. In this stove there is an inner case to contain the fire and an outer case to radiate heat, the space between the two forming a warm air chamber. Air from the room enters the chamber at the bottom of the stove, becomes heated and escapes into the room through the open top of the stove. Both descriptions of open fire-stove are made very ornamental, and are frequently covered with tiles.

THE GILL-AIR-WARMER.



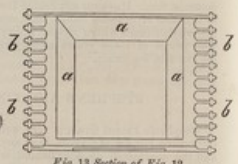


Fig. 13 *Section of Fig.* 12.

a a a, fire-lungs; *b b b b*, gill plates.

Fig. 12.—Gill-air-Warmer.

This is a very valuable stove, as, when properly constructed, it can never become over heated, and if the fire-place be not too contracted in size, a fire may easily be kept burning night and day. The series of plates which enclose the fire are called *Gills*. These receive and radiate the heat of the fire, currents of air passing up between the gills and circulating thence in the open space required to be warmed. The fire door may be thrown open in the day time, but it is closed at night and whenever the fire is required to burn for a long time without attention. The small door below the fire door is then used to admit a small quantity of air to the fire. Used in this manner the stove acts on the slow-combustion principle.

DR. ARNOTT'S SLOW-COMBUSTION STOVE.



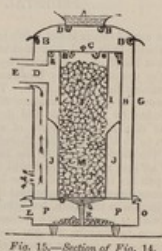


Fig. 14.—Arnott Stove.

Fig. 15.—*Section of Fig.* 14.

a, Water-Vase for evaporation; *b b b b b b b*, sand-grooves; *c*, cap of fire-box; *d*, smaller flue of fire-box; *e*, main smoke-flue of stove; *f*, fuel to supply the fire-pot; *g*, outer case, or body of stove; *h*, case of fire-box; *i*, cylindrical box for fuel; *j*, Stourbridge-clay fire-lungs; *k*, regulator to admit air; *l*, flanch on which the fire-regulating valve is fixed; *m*, fuel in combustion; *n*, sand to surround inner case; *o*, door of ash-pit; *p*, base of the stove; *q*, falling bottom grate; *r*, ashes.

This is a most valuable stove, but it is only suitable for places where a fire is required to be kept perpetually burning during the cold weather. There is no appearance of a fire whatever, the fuel being supplied from the top twice only during twenty four hours. Anthracite coal or coke is burned. A regulator attached to the side of the fire regulates the amount of air given to the fire, and thereby the amount of heat given by the stove. The advantages of the stove consist in the exceedingly small amount of attention it requires, and in the moderate and genial heat which it distributes when kept perpetually burning.

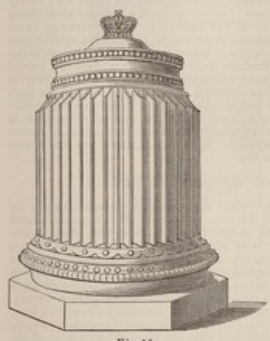


Fig. 16.

Goldsworthy Gurney's Stove.

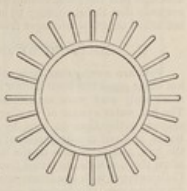


Fig. 17.

Section of Fig. 16.

GOLDSWORTHY GURNEY'S STOVE.

This is a modification of the two preceding. It has a series of iron plates to modify and distribute the heat like the Gill-Air-Warmer, but the plates radiate from a circular fire pot instead of being placed parallel to each other. There is no open fire and the stove, like Dr. Arnott's, is used on the slow combustion principle. The stove stands in a large iron dish in which water is placed. The water carries off much of the heat of the stove, and distributes it in the form of vapour over considerable areas. For churches and other places of large area the stove has been highly valued.

PIERCE'S PYRO-PNEUMATIC STOVE.

This is an improvement on the pedestal hot air stove already mentioned. The open fire is preserved, and the stove is, therefore, not suitable for burning night and day. The inner portion is formed of fire-lump, and within the thickness of the fire-lump there are hollow spaces which form warm air chambers, a channel communicating with an external wall being generally provided for the purpose of supplying fresh air to these chambers, which issues thence through the top of the stove into the apartment.

WHICH IS THE BEST STOVE?

It is frequently a difficult thing to decide which is the best stove to adopt. We can only give a recommendation in general terms, and would say, therefore, that wherever an open fire is considered essential, Peirce's pyro-pneumatic stove or the open fire hot air stove should be selected. Whenever a stove is required to burn with a very small amount of attention and occasionally through the night without attention, the Gill air-warmer is preferable, and wherever a stove is required to burn night and day for weeks or months together, the Arnott stove is the proper one to be used. For churches we recommend either the Gurney stove or the Gill air-warmer. There is often considerable difficulty about fixing a stove when there is no chimney in the hall which can be devoted to it. Sometimes the chimney of an adjoining room is used, but this can only be successfully done under careful precautions. Sometimes a cast-iron flue from the stove is carried up through the landings and through the roof of the house; sometimes a descending flue is used, which after passing through the floor is taken horizontally into a convenient chimney. The Arnott stove and the Gurney stove are not suitable to use with descending flues. The other stoves described may be constructed to be so used, but in all cases of difficulty it is desirable that the place should be inspected by an experienced person, to ensure not only that the stove may be so erected as to act effectually but that it may not afford occasion to danger from fire.

HEATING BY HOT WATER CIRCULATION.

Few are aware that it is only within about seventy years that heating by hot water pipes has been known in this country, and that previous to this period those only who were accustomed to travel were familiar with close stoves. The hot water system of heating is an admirable one, but it entails a greater original outlay than to heat by stoves. By the principle generally adopted a boiler is fixed in the basement and stacks of pipes called *coils* are erected in the hall, and sometimes on the landings and in the passages. Circulating pipes allow the hot water to travel from the boiler and to return to it to be re-heated. Coils covered with handsome ornamental cases are frequently placed in a large drawing-room, dining-room, library, or conservatory, but sometimes, particularly in conservatories, coils are dispensed with, and pipes either pass round the walls just above the floor, or in channels constructed below the floor, the channels being covered by iron gratings. Where the heating requirements are not large the boiler of the kitchen cooking apparatus is often used instead of a separate boiler. The circulation of the hot water through the coils or pipes is permitted or prevented by simply turning a tap.

ON KITCHENERS AND KITCHEN RANGES.

The old-fashioned kitchen range is going gradually out of use; the modern kitchener is slowly superseding it. A great difference of opinion exists as to the real or supposed superiority of the kitchener. Some, while acknowledging its superiority with respect to its cooking capacity, cleanliness and general convenience, believe it to be more extravagant than the old open range. Others complain of the want of ventilation, and that the roasting arrangements in the ovens are not so good as when the open fire is solely used. There can be no doubt that when properly made, properly erected, and properly used, the kitchener is very much superior to the open fire kitchen range. There is no real difficulty about ventilation if the kitchener be properly constructed and properly set, and as for the roasting, that can be most perfectly performed in the ovens if they are properly ventilated and if the roasting water dish is used. Fig. 18 shows one of a good construction, in which tiles are used to the oven doors to confine the heat within the ovens, and a lifting bottom is provided by which a deep or a shallow fire can be used at pleasure, the deep fire being only required when roasting is done in front. There is room for great economy to be practised in our cooking arrangements. No one ever did so much as Count Rumford to develop the scientific use of fuel in cooking. Unfortunately, it is

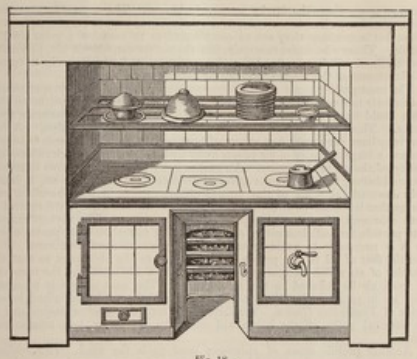


Fig. 18.

scarcely possible to effect a great reform until we have altered our national habits to some extent. The open fire will have to be abandoned, and servants must learn to adapt themselves to new modes. Still, much may be done by making the best of circumstances as they exist, and those who would care to enquire into the sources of improvement will find the subject discussed in the third volume of Count Rumford's Philosophical Essays, and in Mr. Edwards's Essay on the Extravagant Use of Fuel in Cooking Operations.*

Many are unaware how readily the boiler of a kitchener can be used for the purpose of supplying a quantity of hot water for a warm bath and other household purposes. A hot water cistern is fixed in an upper floor, and is connected with the boiler by means of circulating pipes. Branch pipes suffice to convey the hot water to the bath or elsewhere. Sometimes, as has been pointed out, the hot water is made to circulate through a series of pipes for the purpose of warming a hall, or the up-stairs landings or a conservatory, the circulation to those parts being discontinued during the warm season. The great advantage of the system is that a bath can be had at any moment when the kitchen fire is going, and that the heating arrangements are maintained without having recourse to a separate fire.

SMOKY CHIMNEYS.

It is natural for smoke to ascend, the warm current of air which passes from a fire being hotter than the surrounding atmosphere, passes away by the channel of egress and effectually carries off the smoke and other products of combustion. In most chimneys there is an ascending current even when no fire is burning, because the air in the chimney in winter is very commonly warmer than the external air, and therefore lighter. This tendency of air to ascend in the chimney is increased as soon as a fire is lighted, by reason of the expansive force given to the ascending current of air by the heat of the fire. Hence the vast majority of chimneys occasion no inconvenience from smoke. Some fires merely require a little coaxing by opening a window or a door, or by temporarily contracting

* London, Longmans, 1869.

the open space above the fire by means of a blower. The counteracting causes which occasion serious inconvenience are referable to very few and simple principles, but sometimes they are so complicated as to occasion a great deal of trouble. Those who desire to enquire into the matter can consult Mr. Edwards's book on Smoky Chimneys.* A few observations may, however, assist to spread enlightenment on the subject, and not be therefore out of place.

The most prolific cause of chimneys smoking is the want of a proper supply of fresh air to dwelling-houses. Every house ought to be so constructed that a fire could be used in every room without the slightest inconvenience from smoke. There is, unfortunately, scarcely a house in this position. A few fires may burn very well, but others will not burn at all. The reason is simply because a large quantity of air passes away by the chimneys, which reduces the density of the air in the rooms. Air rushes in by doors and windows to restore the equilibrium, but if it cannot enter in sufficient quantity to do so, it inevitably descends at some of the unused chimneys. It is not, therefore, surprising that it should be exceedingly difficult to establish an upward current in these chimneys, when there is a perpetual down current. The obvious remedy is to provide for sufficient air to enter each room to replace what passes away. As it is necessary to avoid draughts, this has to be done with care. It is essential first of all to use proper register doors to the chimneys, so that the quantity of air which escapes may be reduced to a minimum, and the necessity for fresh air be reduced to the same degree. The next course is to admit fresh air in proximity to the fire-place by one of the modes described in our notice of *Ventilating Grates*. Ventilators put into the window sashes and in the external walls of the house will effect the object by less troublesome means.

Another prolific cause of chimneys smoking is the wind. This seldom, however, occasions serious trouble unless the chimneys are very low and lower than an adjoining roof, or surrounding buildings. It is always of advantage to heighten chimneys, and frequently a good wind guard is of great benefit. As a rule the simpler it is the better, as it is indispensable that it should be easily cleaned.

VENTILATION.

Except among the poorer classes, we do not probably suffer a great deal from want of ventilation. It is not everybody who would care to entertain a crowd of visitors in hot weather in suffocating rooms. Every fire-place is a powerful ventilator, which draws off the air of the apartment, and allows it to be replaced by air from outside. In fact, we sometimes suffer from too much ventilation; hence the difficulty of warming our rooms. What often occasions great oppression is the sensation of closeness which is occasioned by a room becoming partially exhausted, and air not being able to enter with sufficient freedom to replace what has passed away. This we have shown to be also a prolific cause of chimneys smoking. We have already pointed out the remedy, viz., first of all to reduce as much as possible the quantity of air which passes away by the chimneys; next to admit as much air as is absolutely necessary, but without draught. These arrangements cause the atmosphere of a room to be altogether more genial.

Where gas is much used, and where a number of persons assemble, special arrangements become indispensable. It is of considerable importance that the products of gas combustion should be removed, if possible, without mixing with the air of the apartment. No kitchener should be used without effectual means of ventilation, so that the smell of cooking operations may not pervade the house.

THE SMOKE OF TOWNS.

The crudest schemes are occasionally put forward even by presumed scientific men for dealing with the smoke of towns. One project is to carry the smoke into the sewers. How the smoke is first of all to be got there has never been clearly

* London, Longmans, 1871. Sixth Edition.

explained. Possibly those who have advanced the proposition have not clearly considered the indispensable details for carrying it out. These details would require that all our chimney arrangements should be reconstructed; that for every few houses there should be a lofty chimney provided with a powerful furnace, which would be supplied with air from the sewer, such air to be replaced by descending currents from the houses sufficiently strong to carry off the smoke from all the fire-places. The smoke would thus be discharged at one lofty tower instead of at a number of chimney pots. This scheme has been fully discussed by Mr. Edwards in his essay on Ventilation and Heat.* We need simply state our conviction that the plan is wholly impracticable, and that the smoke of towns will in the course of time become dealt with in the following manner:—Firstly, by the extension of the Smoke Consuming Act, so as to include all furnaces throughout the country; secondly, by more attention being paid to economy in cooking, which will lead to the abandonment of the open fire and the use of smokeless fuel; thirdly, by the gradual introduction of smokeless grates in which there is scarcely any perceptible smoke except at the lighting of the fire. The question may be further assisted by the introduction of schemes for supplying heat to a number of dwellings from one source, a subject that will be shortly touched upon.

THE UTILIZATION OF WASTE HEAT.

We will only allude to one source of waste under this head. This is the enormous loss we sustain by the heated products of a fire and the warm air of a room being turned to no account whatever after their escape from the fireplace. To show how these products can be utilized in new buildings for contributing warmth to upper rooms, requires greater length than can be given in this pamphlet. Mr. Edwards, Jun., has discussed the matter with the assistance of diagrams in his book on ventilation,† and to this we must beg leave to refer. He has there endeavoured to show how two main chimneys can be used for an ordinary house instead of a chimney for every room.

ON HEATING A NUMBER OF HOUSES FROM A SINGLE SOURCE.

Some persons have asked the pertinent question, Why should we not have heat supplied to our houses as we have gas and water? In one form we can have this already, and that is simply by using gas for heating purposes, but the cost of this method of warming is so great as to be of little avail. One eminent scientific man has hazarded the conjecture that a whole town might be heated from a single source. It will be probably time enough to consider the practicability of accomplishing this after we have succeeded in heating a number of houses, say twenty or fifty. That much may be done even in the present state of knowledge is unquestionable. It is perfectly practicable to erect furnaces that would heat such a number of houses as twenty, either by hot water or hot air, and to supply those houses with an abundant and constant supply of hot water for all household purposes. Open fires would then only be required in exceptionally cold weather, and for boiling and cooking. The wages of a stoker would be of trifling importance when compared with the convenience and the economy of fuel which would be the result. We must content ourselves with this cursory allusion to a matter which will doubtless some day excite much public attention.

* London, Longmans, 1870.

† The Ventilation of Dwelling Houses and the Utilization of Waste Heat from open Fireplaces. London, Longmans, 1870.

Dryden Press: J. Davy & Sons, 137, Long Acre, London.

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LONDON: LONGMANS, GREEN & CO., PATERNOSTER ROW.

REPORT

ON AN

OUTBREAK OF ENTERIC FEVER

CONNECTED WITH MILK-SUPPLY.

BY

JAMES B. RUSSELL, M.D.,

MEDICAL OFFICER FOR THE CITY OF GLASGOW.

REPRINTED FROM "THE GLASGOW MEDICAL JOURNAL," AUGUST, 1873.

I HAVE received so many applications for copies of this Report, and it is so evidently necessary to preserve for future use all the facts concerning those outbreaks of enteric fever connected with milk-supply, that, even at this late date, I venture to publish it in this Journal. To Glasgow readers the story must be stale; but, as it appeared nowhere in full but in the Glasgow newspapers, to others the facts cannot be so familiar. The relation of Zymotic disease to milk is one of the most important questions in etiology of the present day; and Dr Dougall's paper, in last number of this Journal, is the first attempt to explain on general principles the action of milk as a distributing medium. The report is given here exactly as originally presented, with the addition of a foot-note here and there. J. B. R.

Report concerning a Remarkable Local Prevalence of Enteric Fever in Parkhead, Glasgow, in January, 1873.

In my fortnightly report, dated 1st February, 1873, I stated that "a remarkable local prevalence of enteric fever in the Eastern District is at present under my observation, and may probably furnish us with some interesting facts for a future report."

The suburb of Parkhead, to the extreme east of the city boundary, retains the features of a country village, and neither in the character of its inhabitants nor of its buildings is it suggestive of the city. That portion of it to which this report specially refers lies to the south of the Great Eastern Road, comprising Dalmarnock Street, Burgher Street, M'Ewan Street, and Helenvale Street. These streets run out to the open fields, and are made up of cottages and tenements not more than two stories high, tenanted by weavers or their descendants,—comfortable, cleanly, well-living people.

There are no water closets in their houses: the combined ash-pit and privy are universal. The drainage is mostly surface, leading to the fields; at anyrate, there is no communication with a sewer system. The water supply is abundant, from Loch Katrine.

The occurrence of contagious fever of any kind in an epidemic form in the district described has never been observed by the department. During the entire year 1872, only five cases of fever were known to have occurred in the streets named. It was, therefore, somewhat startling to find about the middle of January many families suddenly attacked with enteric fever, and to discover, on house to house visitation, evidences of a severe and distinctly localised outbreak of that fever, limited in duration to that month, and especially to the fortnight between the 8th and 21st.

The facts were suggestive of some common cause acting for a limited time on this limited area. The discovery of this cause was, of course, very desirable. I shall not trouble you with a history of the inquiry, which has been tedious, but merely state the result.

In the first place, I believe we have got a perfectly complete list of every case of fever in Parkhead and Westmuir since 1st November, 1872, made up by inquiry at every house in the district. I have seen a large proportion of the cases and inspected the houses myself. In November there were 8 cases—4 of typhus and 4 of enteric fever. In December there were 3 cases *at least* of enteric fever in two families, one of these was the family of a dairyman, and I say at least three

cases, because, while one child in the family of this dairyman is admitted to have had "*gastric*" fever, it is almost certain that more than one had the disease, though in a mild form. This was the state of matters up to the end of 1872. In January, 1873, enteric fever broke out in 39 families, in which it attacked 53 individuals. In February we know of only two new cases, and those in families previously attacked. The explosive nature of this epidemic is evident from these figures.

In the course of my inquiry, I found that not only had enteric fever been in the family of this dairyman in December, but among those attacked in January were a daughter-in-law who lived next door, and her sister who lived at the other end of the same street. Following this clue, I found that every family in this street where the fever was got milk from the same dairyman. There were 10 families ill of enteric fever, and without exception they were customers of his.

Further, having ascertained the milk supply of all the families attacked in January, I found of the total 39 that 32 were supplied by the dairyman, and yielded 46 cases of fever, while only 7 families were supplied elsewhere, and these yielded only 7 cases.

It certainly was a remarkable fact, and one hardly to be explained away as a mere accident, that having ascertained all the cases of fever without exception in a district, we should find that while one dairyman, whom we shall call X., was associated with 46 cases, the name of only one other dairyman was mentioned twice, and that the remaining five cases were supplied from five distinct sources.

It might, however, be that X. supplied such a proportion of the inhabitants as compared with other dairymen, that if fever broke out in the district at all, it was certain to seize his customers in much greater numbers.

On obtaining a list of the dairymen and milk-sellers in the whole of Parkhead and Westmuir, it was evident that the chances were very much in favour of other dairymen appearing in the position of Mr X. There were 12 sources of milk supply, and taking only those where cows were kept

and no other milk sold, I found that while Mr X. sold the milk of 10 cows, his neighbouring dairymen, 8 in number, sold the milk of 64 cows. It is scarcely conceivable that any mere chance could bring Mr X. into such prominent relationship with enteric fever.

A still more exact test of the relation between Mr X. and the distribution of enteric fever was to find the source from which every family in the district obtained their milk, and so ascertain the proportion of each dairyman's customers attacked with fever. It seemed to me equally sound as a test, and not so laborious, to take the streets where the majority of the cases occurred, and ascertain how many customers each dairyman had in those streets. Burgher Street, Dalmarnock Street, Gray's Lane, M'Ewan Street, and Ravel Row, contained 24 out of the 39 families, and were dissociated in space and yet so short as to be subjected throughout to the action of similar local circumstances, so that every one residing therein would have an equal chance of being caught by an epidemic cause of local origin. The following is the result:—

In Burgher Street 28 families were supplied by Mr X., and out of these 10 were seized with enteric fever in January, and four had suspicious cases of sickness, while 43 families were supplied by other persons and not one of them had fever or a single suspicious case of disease.

In Dalmarnock Street 28 families were supplied by Mr X., and four of these had cases of fever, while 20 were supplied elsewhere, and only one of them had fever.

In Gray's Lane six families were supplied by Mr X., and four had cases of fever, while 20 were supplied elsewhere, and only one of them had fever.

In M'Ewan Street nine families were supplied by Mr X., and two had cases of fever, while nine were supplied elsewhere and had no fever.

In Ravel Row there are just 11 families, of whom only two were supplied by Mr X., and these were the only two families who had fever.

The summation of this milk-census is this, that in five streets where the milk supply of every family was ascertained, out of 73 families supplied by Mr X., 22 had fever, and out of 146 supplied elsewhere only two had fever.

Having thus stated the chief points in proof of the case against Mr X., one or two striking corroborative features of this outbreak may be remarked.

1st. Enteric fever in families supplied by Mr X. frequently attacked several members of the family, so that 32 families yielded 46 cases. In families supplied otherwise it occurred only in isolated cases as we usually see enteric fever in Glasgow.

2nd. The fever was much more fatal in the families supplied by Mr X. Out of these 46 cases there were 6 deaths, while there were no deaths in families supplied elsewhere.

3rd. In two families supplied by Mr X. we were told that the two individuals seized were the only members of the family who used that milk. In both cases it was used with porridge, while the others took sour-milk in one instance and syrup in the other.

4th. The families in Great Eastern Road, Chapel Terrace, and New Road, having cases of fever and supplied by Mr X. where no milk census was taken, are scattered about quite capriciously over a considerable area, and yet they number 10 against 5 supplied elsewhere.

5th. We ascertained the date of sickness in all these cases, and of the 46 supplied by Mr X. 6 sickened in the first week of January, 17 in the second week, 14 in the third week, 9 in the fourth week, while of the 7 cases supplied elsewhere there is no such clustering together.

The period of incubation of enteric fever, *i.e.*, the interval which may elapse between the introduction of the poison into the system and its activity, may extend to three weeks. This carries us back to the end of December, when Mr X.'s family had enteric fever, when the germs of the disease on one or more occasions got into his milk, and were distributed to those unfortunate persons who were, one by one, attacked during the month of January.*

* I have been asked how the germs of the disease got into the milk. The only additional fact which I can give is that the person who habitually milked the cows was the person who nursed the children. As stated already, Loch Katrine water alone is used, drawn from a tap in yard.

I regard this as an extreme illustration of what must frequently happen where the sale of articles of food is conducted in close connection with families with all their attendant ailments. Milk is, from its composition, a peculiarly favourable medium for the propagation of the germs of disease, and particularly of enteric fever. It is seldom that this fever is diffused by milk in circumstances which permit us to trace the disease home to the milk so clearly as in the Parkhead case; and it is very likely that many apparently inexplicable outbreaks of enteric fever in families are caused by milk, or even solid food, contaminated in the retail shops, especially among the poor.* It is a very common practice in all parts of the city for parties to live and rear families in rooms behind shops, through which often the sole access lies, and in which groceries, milk, provisions of all kinds, sweetmeats, fruit, &c., &c., are sold. These shops are "served" by one or both parents, or some grown-up child, and when infectious disease enters such a family it cannot fail to be the source of quite peculiar risk to the public. I have been so much impressed with this by a series of cases in point that I submitted the following three illustrations to Mr Lang, Procurator Fiscal, to ascertain what legal powers existed to deal with them:—

1st. "Provision shop served by mother of family, consisting of eight individuals living in back room, a lodger slept on a shake-down *in the shop*. Two of the children had enteric fever in the beginning of January, and the death of one of them drew our attention to the case. I put the alternative to the parties, either to shut the shop or to allow me to remove the family to the Reception House and Hospital, and after much hesitation the latter course was adopted."

2nd. "Shop for sale of groceries, including bread, butter, ham, 'potted head,' &c., similar construction to previous case, only back room much smaller and no back-door, occupied by man and wife and three children. The man had been ill of enteric fever for 25 days before we discovered the case.

* I have a strong suspicion that an outbreak of enteric fever in Drygate, last autumn, was promoted by milk-supply; at anyrate, it prevailed chiefly among the customers of a dairyman who had that fever in his own family.

During all that time the excreta must have been carried through the shop in order to reach the midden by the close, and the shop was 'served' by the man's only nurse and attendant—his wife. I put the same alternative before this woman, instantly to shut the shop or to send her husband to hospital, and she adopted the former course."

3rd. "I am aware of a baker's shop doing a flourishing business, the proprietors live in a house of four apartments in direct communication with the shop. Two members of this family had small-pox, one dying after an illness of 15 days."

Mr Lang writes his opinion "that persons situated as described in the various instances given in your letter, have not 'proper lodging or accommodation.'" It will therefore be possible by this and other provisions of the "Public Health Act," to deal with such cases so as to save the poorer classes from the obvious dangers of contagious sickness in such circumstances.* I have therefore issued, through Mr Macleod, to the Sanitary Inspectors an instruction "that systematic attention be paid to the health of all families living in the circumstances described, by a more routine visitation than from the character of the people and the locality might be thought necessary. Any case of infectious disease discovered must be specially and immediately reported to the medical officer. He wishes the greatest care to be taken not to injure the interests of the parties referred to by unnecessary publicity in the discharge of this duty; but at the same time there is a very obvious danger to the public from their private sickness, arising from their mode of living, which quite warrants the interference of the department."

(Signed) JAS. B. RUSSELL.

3rd March, 1873.

* I am informed by the Board of Supervision, in acknowledging receipt of the above Report, that "They are of opinion that under Section 42 of the 'Public Health Act' the Local Authority are entitled to take action in such cases." Considerable misunderstanding exists as to the powers conferred by this Section, but it is evident that if the words "proper lodging or accommodation" embrace a consideration in the fullest sense of what is "proper" in relation to public safety, then the Local Authority have the most ample power to remove under warrant any person suffering from infectious disease, and situated in relation to the public as the family of a dairyman are.

DUNN AND WRIGHT, PRINTERS, GLASGOW.

ST. MARYLEBONE.

REPORT

BY THE

MEDICAL OFFICER OF HEALTH

ON THE RECENT

EPIDEMIC OF TYPHOID OR ENTERIC FEVER.

TO THE VESTRY OF ST. MARYLEBONE.

GENTLEMEN,

The Sanitary Committee having at their meeting on the 20th ultimo requested that I would lay before the Vestry a report on the late outbreak of Typhoid Fever in this parish, I have much pleasure in complying with their request; the more so as I know that everything which concerns the health and welfare of the inhabitants of the parish are to you matters of deep and sincere interest.

The subject having been already fully discussed in the Daily and Medical Journals, it will be unnecessary for me to do more than to state, very briefly, (1), the steps that were taken to trace out, and which ultimately led to a discovery of, the cause of the outbreak; (2), the extent to which the epidemic had spread in this and neighbouring parishes as far as could be ascertained by the enquiries made; and, (3), the sanitary precautions which under the emergency I thought necessary to carry out.

On the morning of the 4th of August Dr. Murchison called upon me to inform me that four of his children were then suffering from Enteric Fever, and that the disease had also broken out in several families in the immediate neighbourhood, the names of which he mentioned. The cause of it he suspected to be milk supplied by the Dairy Reform Company from their dairy in Orchard street, and from the circumstances which he detailed relative to the attacks in his own family, together with the fact that all the other families in which the disease existed were, with but one exception,

supplied with milk from the same diary, I could but feel that his suspicions were entitled to the most serious consideration. My first obvious duty, therefore, was to call at the dairy in Orchard street and make a sanitary inspection of the premises; this I did in the afternoon of the same day. The result of my inspection and of a lengthened conversation with Mr. Maconochie, the manager of the dairy, satisfied me that there was nothing in the condition of the premises to account for the outbreak, nor did it appear possible that the milk could be contaminated either by the milk carriers or any other persons employed by the company at the dairy. Thus far my enquiries proved that the company were in no way responsible for the supposed contamination of their milk, and that the causes must be looked for at the farms from whence it was supplied; in the meantime, feeling I should not have been justified in ignoring altogether the possibility of the fever being due to other causes, I extended my enquiries into the state of the sewers and drains of those parts of the parish in which the disease was prevalent, and into the quality of the water supplied. By the kind assistance of Mr. Hallett, the assistant surveyor of the parish, I ascertained that the sewers were in a satisfactory condition, having been all well flushed and cleansed a short time before, and as regards the drains, I could have no doubt of their being in a good structural condition, especially in those houses where cases of fever existed, inasmuch as the occupants of them were medical men of eminence, and other persons who were fully alive to the importance of living under the best and most perfect sanitary conditions that it was possible to obtain. The water supply I found equally free from suspicion, my weekly analyses during the whole year had shown that it was of more than average purity and freedom from organic contamination. On returning home from my visit to the dairy I received intelligence of fresh outbreaks of the fever, all occurring in families who drank the suspected milk. I therefore resolved, without further delay, to write to the manager of the company suggesting the necessity of his suspending the sale of milk from the then sources of supply until the true cause of the fever could be ascertained; my letter to that effect was delivered at the dairy either that same evening or early in the following morning. On the morning of the 5th of August I had an interview with the directors of the company when I laid before them the facts which had come to my knowledge respecting the outbreak, together with the additional and most important fact that Dr. Murchison, an eminent authority on fever, was fully impressed with the belief that to their milk alone the epidemic then prevailing in this and neighbouring parishes was due.

I received from the directors a very courteous reception, and the fullest explanation of the mode in which they conducted business and provided for the purity of their milk; all of which I must admit was very satisfactory as far as it went, but it altogether failed to prove that the milk had not been contaminated before it came into their possession, and that they were not unconsciously supplying poisonous milk to the public.

On the same day I addressed a letter to the three principal medical journals, asking of their readers information of any cases of fever in this and the neighbouring parishes which came under their care or cognizance. I also, with the same object, drew up a circular which was afterwards sent to every medical practitioner in the parish.

From this time until the morning of the 7th of August information of fresh cases of fever was continually brought to my notice by Dr. Murchison and other medical gentlemen, and as on that day the Vestry met, I resolved to inform them of the outbreak. I might have hesitated taking this step from the fear of causing a panic in the district; the responsibility of my position, however, left me no alternative but to do what the law required. I also felt myself called upon to communicate personally with the Medical Officer of the Privy Council, Mr. Simon, in order that in the prosecution of further enquiries I might have the assistance of some gentleman from his department. This I did not seek because I felt personally incompetent to the task, but because it was apparent that the origin of the outbreak was to be sought for in places far away from the Parish of St. Marylebone, beyond which I could exercise no power or authority. Had there been good and sufficient reasons for believing that the cause was to be found in the Parish, I should not have applied for any such aid. Mr. Simon, I must state, hesitated at first to afford me the assistance I asked for, believing, as he kindly said, I was fully competent to the duty without help, but ultimately he yielded to my wish, and appointed Mr. Netten Radcliffe to assist in the investigation.

On the afternoon of this day (August 7th) I wrote to the Directors of the Company (my letter of the 4th, addressed to their Manager, having failed in its object), calling upon them, in terms as strong as I felt justified in using, to suspend for a time the further sale of their milk, and warning them that, by neglecting to do so, they would be alone responsible for the ill consequences that might thereby ensue. To that letter I received the extraordinary reply which has already been made public, which proposed to the Vestry that the Company would suspend the sale of their milk on condition that the Board would undertake to compensate them for "the loss and injury they may sustain in loss of profit and reputation."

On Friday, the 8th of August, Mr. Netten Radcliffe entered upon the scene, and, after calling upon myself and Dr. Murchison, and obtaining from us a general outline of the facts as they then existed, I accompanied him to a meeting of the Directors of the Company. At that meeting we met Dr. Corfield and Mr. Chalmers Morton, who, we were informed, had been appointed by the Directors to assist us in the investigation. It was then arranged that all the farms which supplied milk to the Orchard-street Dairy—eight in number—should be visited, and we were assured that the occupiers of those farms had received instructions to afford us every assistance and information in their power. It was also arranged that we should meet at Oxford, on Monday, the 11th of August, whence we were to start on our enquiries.

It will not be necessary that I should enter into any lengthened detail of the circumstances connected with our inspection of these farms; suffice it to say, that our enquiries were generally directed to the following points:—

1. The system of drainage carried out at the different farms and nature of the soil.
2. The quality and quantity of the water supply.

3. The general condition of the cows and nature of their food.

4. The health of the occupants and labourers on the farms and of the surrounding parishes. This involved enquiries from the District Registrars of certain parishes respecting deaths which were reported to us as having occurred from typhoid fever.

5. The mode by which the washing and cooling of the milk cans was carried out at each farm, the quality of the water used for the purpose, and arrangements generally adopted for preserving the milk free from pollution of any kind.

Subsidiary to these enquiries were others having reference to the extent of the farms, the proportion of arable to pasture land, the number of cows kept, with some others which did not very materially concern the objects we had view.

In seven of these farms our investigations were unsuccessful in discovering the cause of the outbreak. It is true that in some of them the water supply was deficient in quality, and, in my opinion, unfit for drinking purposes, and the well in dangerous proximity to the privy; it is also true that the drainage was unsatisfactory, but beyond this there was nothing to show that the milk from any of these farms was infected with typhoid poison. At the eighth farm, however, the condition of things which then existed, coupled with some antecedent facts which had come to our knowledge on the evening before we made our inspection, demonstrated beyond the possibility of any reasonable doubt that the fountain and origin of the epidemic had been at last discovered. This farm, known as Chilton Grove Farm, is situated some few miles from Thame, in Oxfordshire. It had been in the occupation of a Mr. Jessop, who died on the 8th of last June, since which time it has been carried on by his widow. This lady has three children, of ages varying from 10 to 20 years, who reside with her at the farm. From information furnished to us by Mr. Humphreys, surgeon, of Thame, the medical attendant of Mrs. Jessop and her family, we learnt that Mr. Jessop some short time before his death was attacked with typhoid fever, and although the return made to the District Registrar described the cause of death to "heart disease," yet, from the circumstance of Dr. Giles, of Oxford, whom he consulted in the early stage of his illness, having pronounced it to be typhoid, as well as from the additional fact that some two or three days before he died copious discharges of blood from his bowels,—characteristic of the disease,—had taken place, it is very clear that this gentleman had suffered from the fever in an aggravated form, and that a certificate ascribing his death to "enteric, or typhoid fever" would have been the more correct.

The farm-house and buildings are placed on a slope; at the highest point the privy, which is a mere open pit, is situated, and at the lowest the well, this latter is bricked but not cemented, and receives its water supply from a brook in a field some 300 or 400 yards distant. The distance between the privy and the well is about 18 and a-half yards, and between these, but nearer to the former, is the dairy, where there is a pump which draws its water from the well through a pipe which we were informed was laid in a sort of trench dug to receive it. The altitude of the privy above the well is

about 8 feet, it is therefore obvious that any soakage from it must flow in the direction of the well, and it is also clear that as soon as such soakage reaches the trench its further progress to the well meets with but comparatively little obstruction. At the time of our visit the water in this well was extremely bad, and we were informed by Mrs. Jessop that it had not been used for drinking for nearly two years, but simply for cooking, washing, and for washing out and cooling the milk cans. It was obvious, both to myself and the other gentlemen present, that the water was impregnated with the leakage of the privy, and also from the pig-styes and manure heap which were even in much closer proximity to it and also upon a higher level.

The theory therefore by which I explain the infection of the milk is as follows:—at some time, probably for many days during the illness of Mr. Jessop, he used the privy, his excreta, intensely impregnated with Typhoid poison and mixed with other liquid and decomposing animal matter, was conveyed into the well by percolation; the water of this well, thus poisoned and rendered still more dangerous by drainage from the pig-styes and manure heap, was daily used for washing out the cans, and it is very probable that, after such washing, some small quantities were left accidentally at the bottom of them. On pouring milk into these cans the whole of it became at once infective.

Before closing this part of my subject, I must not omit to state that at the time of our visit to the farm a son of Mr. Jessop, a lad about 14 years old, was ill in bed, suffering, as we were informed by his medical attendant, with a mild attack of Enteric Fever, from which, however, I understand he has quite recovered.

It is hardly necessary I should repeat what has already been made public, viz., that on the instant the Dairy Company was informed of the discovery made at Chilton Grove Farm, their supply of milk from this Farm was stopped.

I now proceed to show the extent to which the epidemic has spread in this and neighbouring parishes, and although in this part of my enquiry I have had the valuable assistance of Dr. Murchison and other medical gentlemen, and have also availed myself of every other possible source of information, I nevertheless feel that my knowledge on this subject is at present very imperfect, and that I can only arrive at an approximate estimate of the number of persons who have been attacked by the disease. I come to this conclusion from the circumstance that up to the present time I have been continually hearing of fresh cases, all of which, with scarcely a single exception, had drunk the milk from the Dairy Reform Company. I can have no doubt that other cases will yet be brought to my knowledge, and that very many besides have occurred which I shall never hear of.

To the letter which I addressed to my professional brethren through the medium of the medical journals, and to the circular which I also sent to every medical man in the parish, I have received upwards of 200 answers; of these 57 gave information of the disease existing in families resident in St. Marylebone and the neighbouring parishes of St. George's, Hanover Square, and Paddington; to this must be added very many other cases

communicated to me by Dr. Murchison, which bring the estimated number of families in which the disease has occurred to about 90, and the number of persons attacked to about 320. These figures exclude all doubtful cases, and are, I am satisfied, considerably less than the actual number.

The largest number of these cases occurred in this parish and the smallest in the parish of Paddington.

The incidents which have been detailed to me as connected with many of these cases are of the most interesting kind and tend to prove incontestibly that they owed their origin to the infected milk. The per centage of deaths that has resulted, I have not been able to arrive at, from the circumstance that many of the patients suffering from the disease were removed to hospitals out of the parish, and others had gone into the country in the interval between drinking the milk and the first symptoms of the attack; of most of these latter cases there is great difficulty in ascertaining the termination; at present, however, I know of about 20 deaths and of this number not more than five or six have occurred in this parish. One I have ascertained occurred in Wales, a second in France, a third in Kent, and others in various parts of the country. The first serious outbreak of the disease appears to have taken place on or about the third week in July, and continued, as can, I think, be satisfactorily shewn, until the 9th of August, or within four days of the suspension of the sale of the infected milk.

It must be understood that this report takes cognizance only of those cases of fever which are supposed to have been produced by the milk supplied by the Dairy Reform Company; others of course have occurred which had their origin in other causes, but these it does not concern me at the present time to notice.

The sanitary precautions which I thought it desirable to carry out, with a view to check the spread of the epidemic, and provide for the health and safety of the inhabitants generally, were as follows:—

1. A frequent flushing and disinfection of the sewers in all those parts of the parish where cases of fever were known to exist. The disinfectants used for this purpose were sulphate of iron and carbolic acid. The former had the effect of oxidizing, or, in other words, burning up the sewage, the latter, being a powerful antiseptic, arrested the progress of animal and vegetable decomposition.
2. The daily watering of the streets with a weak solution of carbolic acid. This was carried out in those streets where the fever prevailed, as well as in all the crowded and densely populated streets and courts in the parish.
3. The daily sprinkling of the gullies and air shafts in the parish with the carbolic powder.
4. The distribution of small packets of carbolic powder to poor families, the sanitary state of whose houses required the use of a powerful antiseptic.

5. The circulation throughout the parish of a large number of posters and hand bills, containing plain directions of the measures necessary to preserve health and guard against an attack of the epidemic.

In concluding my report, I have much pleasure in stating that this epidemic of fever, which was productive of so much anxiety in the early part of last month, and which through the medium of foul drains, impure water receptacles, and other well known disseminators of the infection of typhoid, threatened to assume alarming proportions, has now almost entirely subsided, and if I can be considered as having even in the smallest degree contributed towards effecting this satisfactory result, I shall have reaped a rich reward for my trouble and anxiety.

I remain, Gentlemen,

Your obedient servant,

J. WHITMORE, M.D.,

Medical Officer of Health, &c.

Court House, 23rd September, 1873.

ST. MARYLEBONE.

At the Vestry, *25th September*, 1873. } Present— Mr. Churchwarden Sedgwick, in the Chair.

Ordered,—

That the foregoing Report of the Medical Officer of Health be printed, and taken into consideration at the next meeting of the Vestry.

W. E. GREENWELL,

Vestry Clerk.

Dr Parkes

ON with kind regards

THE CHANNELS from

THROUGH WHICH

CHOLERA IS COMMUNICABLE.

BY

JOHN MURRAY, M.D.,

INSPECTOR-GENERAL OF HOSPITALS.

Read before the Public Medicine Section at the Annual Meeting of the British Medical Association in London, August 1873; and reprinted from the British Medical Journal, August 23rd.

LONDON:
T. RICHARDS, 37, GREAT QUEEN STREET, W.C.

1873.

ON THE CHANNELS THROUGH WHICH CHOLERA IS COMMUNICABLE.

Among the greatest obstacles to the study of cholera are the dissimilarity of the symptoms in the various stages of the disease, and the difficulty of connecting the mild symptoms at its commencement, with those that are so excruciating and virulent in its later stages. The recognition of this connection is of recent date, and until it was established our knowledge of the nature of the poison progressed slowly. The acknowledgment of this fact has materially assisted our investigations, as an early diagnosis of the presence of cholera is of the utmost importance. When this connection is allowed, it gives valuable aid in effecting measures for restraining the dissemination of the disease. In its milder stages, treatment can be employed which will prevent its advance to those later stages in which medicine is often powerless, and sometimes injurious; and in thus checking a simple case, the spread of the cholera poison is most effectually opposed. I consider cholera the most important medical question of the day, and it is to promote its elucidation, and in the hope of benefiting by the experience of the many celebrated and justly honoured members of our profession now assembled, that I submit my opinion on one branch of the inquiry, viz., the channels through which the disease can be communicated. I have selected this point as one of the greatest practical importance, leaving others for future investigation and more able observers.

The history of the views of the medical profession on the contagious nature of cholera is very interesting. I find in the accounts of the disease in India previously to 1817, that it was supposed to arise from a vitiated state of the humours and from putrid bile which had to be purged off. When the epidemic in 1817 commenced, it was considered an aggravated attack of an ordinary seasonable disease, which, like ague, prevailed during the rainy season in Bengal. The progress of the disease in the Upper Provinces, the severe attack in Lord Hastings's camp in Bundelcund in November, and its subsequent history when it attacked the troops on service in Central India and in Madras and Bombay in 1818-19, led some of the officers to consider it contagious. The official reports drawn up by the Medical Boards of Calcutta and Madras, in 1818, concluded that cholera was not contagious, though there were some dissentient voices to this view. The Medical Board in Bombay, on the other hand, considered that it was contagious. In Paris, where I first saw the disease, in 1832 it was declared by the leading physicians to be non-contagious. The same opinion prevailed in England in 1848 and 1854. There were always individual exceptions, but officially and by the profession generally cholera was considered non-contagious. The first departure from this view in any official report, occurs in one published in India in 1856, when the con-

tagious nature of the disease was assigned as a reason for incurring the expense of removing the prisoners from the jail at Agra during a very severe epidemic. Again, in 1860, 61, 62, and 63, the same reason was assigned for incurring the expense of removing the European troops into camp during severe epidemics in those years. Similar views were supported by the Sanitary Commission in 1862, and they have since that time guided the measures adopted by the Government of India in regulating the precautionary treatment of the disease.

The non-contagious nature of the disease was the prevailing opinion of the profession in Europe until the year 1865, when the severe epidemic which desolated many countries attracted general attention, and was followed by the International Sanitary Commission at Constantinople. Since then the change of opinion has been very decided.

There are few now who do not consider the disease transmissible, either directly or indirectly, from a cholera patient to a healthy subject; and this is one of the recognised definitions of a contagious disease. The objections to the contagious view of the question are now reduced to the remark that cholera is not so contagious as some other diseases; or that it is not communicated in the same manner, but through some special channels as the air, subsoil-water, etc. That the course of the disease is influenced by the state of the various media through which the poison passes is evident; but that any one of these in a normal state is essential to the transmission of the disease is not proved, and the existence of an abnormal state has no further foundation than conjecture required to supply a deficiency in some theory.

It is thought by some, whose opinions have great weight, that the locality in which the disease occurs offers the most important channel of communication. This is the popular belief of the natives of many parts of India, as they will sometimes desert their villages when the disease breaks out, and take refuge in the surrounding jungles, thus showing their idea that there is danger in the locality.

With these preliminary remarks, I will now proceed to my subject, the channels of communication; and in the first place will trace the course of the poison from the diseased body of a cholera patient, through external agents, or media, to the healthy person in whom it excites the characteristic symptoms of the disease. I take it for granted that the symptoms of cholera are caused by the presence of a poison in the system, and that it is only when that is removed that health is restored. The restoration of health implies the elimination of the poison, either in an active or an inactive form. The ordinary channels of elimination are the lungs, the skin, and the bowels, and through one or more of these it may be inferred that the poison leaves the body. As these are the channels through which foreign bodies find entrance, it may be presumed that through one or more of them the poison enters the system. That the bowels form one channel for exit, is evident from the characteristic profuse discharge from them; and for entrance also, from the fact of the disease having been induced in a healthy person by swallowing the evacuations from a cholera-patient when mixed with drinking water or food. The opinion that the cholera poison exists in a most dangerous state in the evacuations has gained ground rapidly since the remarkable case of the Broad Street pump, recorded by Dr. Snow in 1854, and it is now universally accepted by the profession. Some authors of note suppose that the bowels are the only channel of elimination, and that, by disinfecting the evacuations, they can arrest the communication and dissemination of the disease.

That the skin forms another channel of exit appears probable from

the profuse cold perspiration, which is a symptom of the disease, and which has the peculiar or characteristic odour. Many instances are on record of the disease being communicated to healthy people who have worn, handled, or washed the clothes belonging to cholera patients. It is probable that in these cases the poison also entered by the skin; but it is possible that it may be of so light or volatile a nature that it may rise into the air and be breathed, thus entering through the lungs; or it may even adhere to the saliva in the fauces and be swallowed, and so pass into the bowels.

That the lungs are a channel of entrance for the poison is rendered probable by the fact of the disease following attendance on patients suffering from the disease, as well as exposure to currents of air from places polluted by cholera evacuations, to peculiar fogs or miasms or foetid sewage-gases. There are numerous illustrations of all these cases.

Having thus briefly alluded to the different channels through which the poison, or germ of cholera, may leave and enter the body, I will now consider the various channels or media through which it passes while outside the body.

In ancient times, it was considered that plagues and pestilences of all sorts depended on an *epidemic or abnormal state of the air*, and not on contagion; otherwise the earth would have been depopulated by the extension of these diseases. This theory of an epidemic atmosphere has been thought especially applicable to cholera, as it differs from other contagious diseases, as small-pox, etc., in the fact that recovery from one attack affords no immunity from a second. It would also explain the limited duration of these epidemics, and these simultaneously appearing or disappearing over a wide extent of country. But it has been satisfactorily proved by recent investigation that atmospheric influence alone will not account for the appearance of the disease. It is a curious coincidence, that in India almost the only believers in this atmospheric theory at present are the two statistical officers in Calcutta —Dr. Bryden, Statistical Officer to the Supreme Government, whose Tables of the Diseases of India are most valuable; and Dr. Smith, Statistical Officer to the British Medical Service, who has lately written a book to prove that ague, cholera, and sun-stroke are modifications of the same disease. Dr. Bryden's theory is, that cholera originates in Bengal, where it is endemic; that it is carried over the world by winds; and that it has a period of existence limited to three years, after which it disappears, and will not return without a fresh irruption from Bengal. The objection to this theory is, that *the disease* progresses against the wind as readily as with it, on the different lines of commerce. The observations recorded at the time of the Hurdwar epidemic show that the disease spread simultaneously with the returning pilgrims in all directions of the compass, progressing at the rate at which they travelled, whether on foot, on horseback, or by rail; and thus could in no way have depended on the wind. Again: the disease has in many cases accompanied steamers across the ocean, and appeared in the new countries simultaneously with their arrival. In reference to the supposed triennial character of the disease, this is not supported by the fact that, since it reached Persia in 1820, it has appeared there every year with very few exceptions. In Russia since 1830, and in Germany and America since 1832, I believe it has never been absent from some part of the country.

The *season* of the year has a very decided influence upon the disease in India. I have compiled tables showing the result of a careful examination of the statistical returns of the sickness and mortality among

the European troops in the Bengal Presidency, from the year 1814 up to the present date. The influence of season on cholera is here clearly demonstrated; and it cannot be considered accidental, as it includes a period of fifty-eight years, and a total strength of 1,095,549. The second line in the table shows the returns from the native troops since 1826, on a total strength of 3,930,546. The third line shows the returns from the native prisoners since 1854, on a total strength of 980,546; the fourth, the returns from the native population since 1867, on a total of 221,042,902. It will be observed that the season favourable to the spread of the disease differs with the locality. In Bengal, and on the coast, the season is March, April, and May; whilst in the North-West Provinces and the Punjaub it is July, August, and September. The influence of season is marked also in Madras and Bombay; the period coincides more with the season in Lower Bengal than with that of the Upper Provinces. The vicinity of the sea may exert some influence. These separate divisions of the country show variations in the season; but season has merely a modifying influence on the atmospheric channel. These tables are valuable, as showing its extent; but they also indicate that there are other powerful influences bearing on the diffusion of the disease, as some of the most severe epidemics in the Upper Provinces have occurred at the seasons which these tables indicate as almost exempt from cholera—viz., the great epidemics at Hurdwar in 1782 and 1867, which both broke out in April; and the attack in Lord Hastings's camp in Bundelcund, which commenced in November in 1817. I have also seen some sharp local attacks, and had reports of many others, during the cold season in the Upper Provinces—a time when these tables show an almost total absence of the disease. July, August, and September are the months in which cholera has raged most severely in London; but the attacks over the country are not so prescribed. As a general rule, the season for the disease in England and all temperate climates appears to be autumn, though instances of its spreading at other times are numerous. It is very probable that pure air has a disinfecting or purifying property. At any rate, articles that have been excluded from the air have retained their infecting power for weeks or months. This has been shown when people were attacked after examining boxes containing the clothes of those who had suffered from cholera some time before. The influence of certain impurities mixed with the air in increasing the multiplication and facilitating the dissemination of the poison is very marked, whilst others are supposed to restrain its action or destroy the poison. The admixture with the air of the gases produced by animal and vegetable decomposition, evidently favours the development of cholera as much as plague, typhus fever, and other epidemic diseases. Some even suppose that defective sanitary arrangements may induce cholera; but there is no proof of this, and I do not believe in the spontaneous production of specific disease. This is a point of great practical importance, as it shows the value of those precautionary means which may be employed to intercept the communication of the disease through those channels which we are now considering. It is supposed by some that even discussing the contagious nature of cholera, far more authoritatively sanctioning such a view, would act injuriously on the public; that the sick would be deserted, and panic increased. But this is far from my experience. Concealing the truth does no good; and it is already the popular idea that the disease comes from the sick person, and that there is danger in his vicinity. It creates confidence to be assured that, though the poison may be, and

most likely is, imbibed from contact with the sick, its effects may be rendered very harmless by early attention. In some cases, nature alone casts it out; while, if the *malaise* from the poison proceed to diarrhœa, this can easily be checked in its earliest stage by simple remedies. I have on more than one occasion seen a regiment in a state of extreme excitement when it was loudly asserted that the disease was not contagious; and I have seen confidence restored by measures carried out avowedly for the purpose of avoiding contagion.

That very impure air will not alone cause the disease, is evident from the fact that cholera has only of late years become known in Europe; and defective sanitary arrangements are certainly not more general now than in former years, though they may attract more attention. As in a richly manured field a luxuriant crop of wheat or barley is produced if wheat or barley be sown, so in a very contaminated air will the crop of disease be abundant if the germs of cholera be introduced. The most striking illustration of this that I have met with occurred at Chillony, a station in Assam, in 1869. The disease had been prevailing in a mild form for some weeks in the surrounding country; but the station remained free until, for sanitary purposes, it was resolved to clean out a dirty hollow which formed a natural cesspool near the jail. Clearing out the soil caused a most disagreeable smell, and was immediately followed by a severe outbreak of cholera amongst the prisoners employed in the work and the inhabitants of the vicinity. In this instance, the poison, being present, seems to have found a suitable soil in the disturbance of the cesspool, in which it multiplied excessively, and without which it might have disappeared. The laudable intention of the sanitary officers proved injurious, from being carried out at a wrong time.

There is no point on which the concurrence of professional opinion is more decided than on the danger of impure air. It is founded on observation of the fact that in cities where the air is most vitiated, the disease prevails to the most fatal extent. As there is generally a combination of other non-sanitary effects, in sewerage, drainage, and bad water, it is difficult to discriminate the individual share of each agent. Impure air has probably been the channel of communication in instances on record where an individual walking out in the open air has been sensible of a disagreeable smell previously to the commencement of symptoms of cholera; also when the disease has appeared in part of a range of barracks or buildings situated to leeward of the place where the body of a cholera patient lay, or of cesspools or privies used when cholera was raging. One side of a ward has sometimes been affected when the occupants of the opposite side have not suffered. I have records of numerous instances illustrating these points; but in no case did the range traversed by the poison extend beyond a few yards.

In the investigation of the various channels by which the poison may be transmitted from the diseased to the healthy, the first object that occurs for consideration is the body itself in which cholera has manifested its presence. When this has run its fatal course, may it be transmitted from the dead body? This point has been argued with much ability and plausibility by the best medical authorities since the appearance of the disease in 1817. With many, it is still an open question. The argument that the dead body is not a source of contagion, rests on the negative evidence that many or most of those who have performed *post mortem* examinations have not been attacked by the disease. Negative evidence of this nature is to be found without limit; and it is a fact, though not so universal as many suppose who

limit their acquaintance with the disease to the stage of collapse. For example, Dr. Jameson, one of the most influential non-contagionist writers in India, relates that the body of a surgeon who had died of cholera was examined by three medical officers, who all remarked a peculiar smell, and all suffered for the next two or three days from sickness and diarrhœa. With our present knowledge of the symptoms of the earlier stages of the disease, we should say they had imbibed the cholera-poison; and this would be an illustration of its being contracted from the dead body. There are many instances related of women employed in washing and dressing dead bodies being attacked on the following day. I may mention the case of a sepoy who in 1818 died of cholera at Goruckpore, and was carried to the grave by five of his companions. They all took the disease that night, and died. Dr. Townsend, Sanitary Commissioner Central Provinces, reports in 1869 that on May 25th three men were summoned by the police from the village of Dumwahi to bury the body of a man who had died of cholera near the police-station. The cholera broke out at Dumwahi on the next day, and lasted fifteen days. These three men died, and sixteen more, out of a population of 106. The policemen were not affected; they probably did not touch the body. I have read several reports of cholera having broken out among parties of coolies employed in road-making, after opening graves where cholera subjects had been buried some years previously. In America, where it is a common practice to transport the dead from a distance to their native towns for burial, instances of the disease having appeared after the arrival of the bodies of cholera cases are reported in the *Army Medical Reports* for 1867. The presumptive evidence from these cases is, that the dead body is a source of danger.

The living body is considered by nearly the whole profession, in the present day, to be the channel through which the poison of cholera is received, multiplied, and transmitted; and from the living body the evacuations are one of the chief channels of communication. On this point there is little divergence of opinion. It is in accordance with the history of the disease to infer that the human body has been the means by which it has been spread over the world, through the ordinary channels of commercial communication, the poison having probably been disseminated in the evacuations. The number of those who question the power of the poison to multiply when in the body is very limited.

I think Professor Pettenkofer stands alone in considering the subsoil-water essential to the reproduction of the poison in a state of activity. There is every probability that the poison is capable of passing through the various stages of its existence outside the body, in a watery medium; but it is clear that it may pass directly from the body of the sick, or from the evacuations which proceed from the body, to a healthy body, and there induce the disease without the intervention of any subsidiary aid, beyond the mechanical transmission by air or by water. In impure water the poison probably multiplies, but this is not essential to its activity. In the cases just alluded to, of infection from dead bodies, there is no indication of any intermediate state of development in the poison, in its passage to those who received it. Instances of healthy people being affected by the disease after intercourse with cholera patients, are beyond number. I may give in illustration a few instances from my own experience, as they materially influenced my opinions of the disease. In 1856, during an epidemic attack at Agra, I visited the Jail Hospital, where the disease was very severe, with Dr. F. On leaving the hospital, he complained of feeling sick, but we drove to the

Thomason Hospital, where I had several good surgical operations, at which he assisted. The excitement appeared to keep the attack in check till the operations were finished, but immediately afterwards he was seized with watery vomiting, and other symptoms of cholera. On another occasion, during the same epidemic in 1856, while passing through the cholera wards of the Jail Hospital, I visited the dead house, where I saw some fifteen bodies. On returning to the cholera wards, where the peculiar odour was very oppressive, I felt a shock in the præcordium, followed by a cold, deadly feeling, which gradually extended over the body. I immediately concluded that I was attacked by the disease, but thought, if I made it known, it might create panic amongst the attendants. I asked the native doctor if he had a good stock of quinine in store; he said he had, and I desired him to let me see it, and continued my progress through the hospital—the cold, dead feeling now extending to the extremities. On arriving at the hospital door, the native doctor brought the quinine on a paper to show me. I put a large pinch into my mouth, jumped into the carriage, and told the coachman to drive home. At first the quinine had no taste; but, as I chewed and swallowed it, I became warm, and it tasted bitter. On reaching home, I took two cholera pills, lay down and fell asleep. I awoke well. For two or three days there was want of appetite, and the bowels were torpid, but there were no active symptoms. A few days afterwards, an American missionary, to whom I had related this case, had a similar shock about two hours after attending a child who was suffering from cholera. About two hours after the shock, diarrhœa commenced; after the second or third motion, he recollected my treatment with quinine, and commenced taking it; the diarrhœa continued till morning, but collapse did not supervene. At Agra, in the epidemic of 1861, a clergyman, Mr. C., was attacked with watery vomiting while attending the bed side of a cholera patient in the Artillery Hospital; he had that morning officiated at the funeral of a gunner who had died of cholera. The captain and three men of the funeral party were attacked the same day. The captain and, I think, one man died; the clergyman, from the use of timely remedies, recovered. On that same morning, on visiting the hospital of the European Regiment, I found the officer on duty vomiting in the verandah, which was the commencement of the disease; he died two days afterwards.

There is a remarkable case noted by Staff Surgeon Connell in Madras, in 1818, of the wife of a conductor, living in the Artillery Lines, being attacked with cholera. Her friend, Mrs. Gray, attended her for a couple of hours; she was seized with the complaint, and died the next morning. Her son, a boy six years old, living in the same house, was attacked the day after the death of his mother.

It is also reported by Dr. Cardew, Deputy Inspector-General of Hospitals, that the first case of cholera that occurred at Lucknow in 1884, before the epidemic appeared in the city, was in an officer of the Royal Artillery, followed, within twenty-four hours, by the attack of two other officers who were residing in different parts of cantonments; but both these officers had been in constant attendance on the first patient till he died.

I consider these illustrations of the direct communication from the sick to the healthy, by means of the air certainly, but to a limited extent, and not requiring a specific or epidemic state of the atmosphere, which shall extend continuously thousands of miles, as implied in Dr. Bryden's aërial theory.

We now come to the consideration of the solid or earthy media, to

which the poison may become attached, and so be made a source of danger. The dead body comes under this denomination, but it has already been discussed. The clothes and bedding used by the sick come next under notice, and that they are dangerous is very generally believed; and, if so, this goes far to establish the contagious nature of the disease. In India, in 1869, under the orders of Government, I collected the opinions of the medical officers in that country on the chief points in the history of cholera. The result is a valuable table, showing the views of 505 members of our profession, who have all had experience of the disease. By 492 of these, the clothes and bedding were recommended to be burnt, while only three opposed this view. That danger arises from merely washing these articles, is shown in many instances. It was reported from Besabat, in 1867, that the inhabitants were attacked by cholera two days after the clothes of some pilgrims had been, contrary to orders, washed in the village tank. It may have been from the clothes of the pilgrims that the inhabitants of Jough Kelan were infected the same year; but the sick pilgrim remained on the banks of the tank during the day, and he most probably bathed and used the water for other purposes. At Constantinople, in 1865, an instance occurred which proves that these articles become contaminated. The clothes and mattresses of the sick were washed in a fountain, and, the water-pipe being unfortunately broken, the foul water communicated with the clean. In a single day sixty people died in the suburb which is supplied by the affected stream. It is a common practice in India to wear the clothes of the deceased, and this may be one of the causes of the widespread mortality. There they wash their own clothes; but, in Europe, there are special people employed for the purpose. The Report of the College of Physicians in England, in 1849, shows many instances in which washerwomen were attacked, and it was amongst them that the disease first appeared in 1854, after the clothes of a cholera subject had been washed. In America, many similar cases are recorded. Instances are numerous of people being attacked by the disease after unpacking and handling clothes that have been worn by cholera patients. One of the most remarkable anecdotes I have met with, illustrating this, is related by Dr. Simpson, who states that a young lady, in 1832, was attacked with cholera after wearing a cap which had been worn by her aunt, who had died of cholera nearly a year before. The cap had been set aside in a drawer, which was not opened until that day. Similar cases are mentioned as occurring in England and in Germany, by Professor Pettenkofer, of Munich, 1854, by Dr. Sibert, of Frankfort, 1856, and Dr. Flauvel, in France, 1868.

That the cholera poison becomes attached to places, and, remaining there, must be a source of danger, is the next point for consideration. On this the opinion of the medical officers in India, given at the time to which I have alluded, is less decided than on some other points, though it is very clearly defined. Of 505 officers, 432 think the disease communicable from the place, whilst 19 say that it is not; but, as only 4 of these object to removal from the place as a desirable remedy, their practice does not agree with their theory. That the ground may become affected is proved by instances of troops or travellers being attacked after encamping on ground recently vacated by parties suffering from cholera. The period during which the open ground, exposed to the influence of the sun and air, may remain dangerous, is uncertain. I have seen no injurious effects arise from ground which had been occupied by cholera camps a year previously, when troops have again moved on to it. But I had reason to suppose that old buildings, in the vicinity of

Agra, which had been occupied by cholera patients the year before, were injurious to the new comers, as attacks in them were more numerous than they had been in the previous year, or than they were in the open camps in the neighbourhood. I may here allude to the danger which arises from latrines. I have noticed that they are a fertile source of the dissemination of the disease, particularly in the European barracks and native jails. The accompanying table, which I bring foward to show the influence of season, also demonstrates the effect of the use of latrines. There is a marked difference in the extent to which the European soldiers and native prisoners suffered, as compared with the native soldiers and civil population. The military duties of the European and native soldier are the same, and the native soldiers and civil population are of the same race as the native prisoners. The sanitary arrangements for the troops and prisoners are equally efficient, while those of the towns are known to be very defective. There must, therefore, be some powerful channel of communication in force, with the English troops and native prisoners, which is wanting with the native troops and civil population, and this, when discovered, should be guarded against in all countries. The circumstances common to the English troops and native prisoners are, that they reside in large rooms or barracks, and that they use public latrines, whilst the native troops and civil population live in separate houses, and have no common latrines.

In the Indian reports which have come under my notice, instances are innumerable of a first case of a severe epidemic appearing in a village in some resident who had returned from a visit to a distant town, or to another village, where the disease was raging. Similar cases are common also in European and American reports. I have myself observed attacks to follow visiting buildings, or other places, to which the poison may have been attached; living patients were also present, and from them the poison may have been transmitted, and not from the building.

The question whether there was danger in admitting cholera patients into general hospitals was much discussed in India when the disease appeared in 1817. It was decided that there was not, and this opinion prevailed till very recently. It was promulgated in official reports in France, Russia, Germany, and England, and acted on up to 1865, since which time these views have generally changed. There are, however, a few who maintain that there is little or no risk, and such cases are still admitted into general hospitals in many of the cities of Europe wherever especial cholera hospitals are not yet established. If the contagious nature of cholera were once authoritatively admitted, this would not be permitted. Even when there are special cholera wards, the danger is brought very close to the other sick in any hospital. Large wards for the treatment of cholera patients are very unfavourable. In no case is it more important to keep up the spirits of the sick, and the depressing effect is very great of seeing their companions in agony sinking and dying rapidly around them. Small tents, or huts, form, in my opinion, the best cholera hospitals. They should be erected on open ground in the vicinity of trees, if practicable; the sites for these hospitals should be selected, and all subsidiary arrangements settled, before the disease breaks out. By this means time is gained, confusion obviated, and panic averted, by the public seeing that the authorities have foreseen the danger, and know how to meet it. The arguments in favour of the non-contagionist views were very strong. It was reasoned that if the disease were contagious, the physicians, hospital attendants, and other patients present must suffer—

indeed, could scarcely escape attack; but that in many instances these people did not suffer more than others. Some went so far as to say that, from habit, being in the hospital created an immunity from attack. Many old Indians will assert that the disease cannot be contagious, because they have attended friends suffering from cholera without contracting it themselves.

My own experience furnishes many instances illustrating immunity from attack. The native hospital attendants rarely suffered. The class of *shampooers*—natives employed to rub or "shampoo" cramped European sufferers—were in no instance affected during four epidemic attacks at Agra, where seven hundred were employed. The reports from the General Hospital, Calcutta, show that none of the subordinates, or native attendants, and few of the sick, have been attacked in the hospital; and similar reports are numerous in reference to other European hospitals in India. I see no reason to doubt the accuracy of these reports. But, on the other hand, as a *general rule*, the number of patients in hospital, and orderlies attending on their comrades when in hospital, who are attacked, is above the average of the rest of the corps.

Dr. Brown, the present Inspector-General in Bengal, reported that, of the cases in the hospital of Her Majesty's 89th Regiment at Umballa in 1861, one-fourth occurred in patients in hospital for other diseases, and one-sixth in the orderly attendants. Dr. Poole, Civil Assistant-Surgeon, notes in a report in 1859 that it is worthy of record that every one of the regular attendants on the sick were severally attacked, and he considers that the further progress of the disease in the hospital was only checked by daily doses of quinine, taken by himself, and ordered for the native doctor and all the other attendants. Dr. Wilkie, Deputy Inspector-General, Meerut Circle, in his report of 1861, remarks that in the European hospital at Meerut, twenty-seven men and three women patients were attacked, and six orderlies and attendants. Surgeon J. MacGregor, of Her Majesty's 39th Regiment at Bellary, in the Madras Presidency, writes in 1859 that there were a great many attacks in the hospital, both in men who had been long under treatment, and in men recently admitted for fever, in whom blood-letting had been practised. In this weak state cholera invaded twelve patients, and proved fatal to eleven. The Cholera Commission in India in 1862 reported that the sick and attendants in hospital were attacked above the average proportion. This is the conclusion at which I myself have arrived, after thirty-eight years of careful observation of the disease. That it is the decided opinion of the profession in India, is shown by the table I have before quoted. The adverse opinions on the four leading questions bearing on this point—viz., removal from the locality, isolation of the infected, special hospitals, special attendants—only amount to sixteen; whilst the opinions in favour of these measures amount to 1,943, which is a proportion of more than 99 per cent. I do not deny, on the contrary, I am of opinion that the body gets accustomed to, and from habit tolerant of the poison; somewhat as it gets accustomed to the use of alcohol, opium, or arsenic, of which an *habitué* can with pleasure take a dose which would render insensible, or kill, one not accustomed to their use. This view is strengthened by the fact that visitors to an infected locality are peculiarly liable to attack, and suffer in a greater proportion than the residents. I and others have observed this in the Upper Provinces, and it is peculiarly remarked in Calcutta.

It follows as a natural inference, from the facts here stated, that removal from an infected locality is one of the best means of avoiding danger from the disease. In military life this is practicable, and in

India has been carried into effect with the greatest advantage, by removing the troops into tents on the first appearance of the disease in an epidemic form. This is not feasible with a civil population or a crowded city in the same efficient manner; but those who have the means of leaving the place should do so, as their departure would not only benefit themselves; but be of advantage to those who must remain.

On board ships at sea, the disease appears to be influenced by similar causes as in houses or barracks on land. There have been numerous instances of cholera appearing in emigrant ships and proving very fatal some days after sailing from England. I may mention two instances. The steamer *England* sailed from the Mersey on the 26th May, 1866, with 807 emigrants on board. Five days subsequently cholera appeared; there were 500 attacked, of whom 250 died. The *Virginian* sailed from the same port six days after, and on the eighth day cholera broke out, and there were 105 deaths. The disease was not prevalent in the Mersey when these vessels sailed, but some of the emigrants had lately come to England from foreign ports where cholera was raging. There have been many attacks on board the ships in Calcutta clearly traced to using river-water, especially when cholera was prevalent in the city. Those vessels lying opposite the public sewers were particularly liable to attack; but this anchorage has now been forbidden. In some instances the disease has appeared in the emigrant ships some days after leaving Calcutta, and caused considerable mortality among the coolie emigrants.

Water, as a channel of communication, was not alluded to in the numerous reports which I have perused written on the first appearance of the epidemic among the European troops in 1817-18; nor was it suspected as such in Paris, in 1832. In the report of the Board of Health in England, in 1848-49, water is not alluded to; it is there stated that the disease is not, in the common acceptance of the term, contagious, but spreads by atmospheric influence. Attention was drawn to water as a means of communication in 1854, and its agency was forcibly shown by Dr. Snow, in his remarkable account of the Broad Street well, when it was found that nearly all the persons who had the malady during the first few days of the outbreak drank of the water from this pump, and that very few who drank of it escaped the disease.

It is not now questioned, in the profession, that water is one channel through which the poison may be transmitted; some even go so far as to consider water as the only medium. Instances are very numerous which illustrate the communication of the disease through this channel. None are more convincing than those reported to me during the Hurdwar epidemic in 1867, two of which I will relate. Mr. Blyth, Deputy Commissioner Bengal Civil Service, reports that a pilgrim was taken ill of cholera at noon on the 28th April, 1867, at a well in the village of Joogh Kullan, and he died next day. His soiled clothes were washed in an adjoining pond. Other parties, who afterwards visited the well and the pond for water and ablution, caught the infection. The disease broke out two days after, on the 30th, and up to the 15th May 53 were attacked, of whom 27 died. There had been no cholera in this village for twenty years before. Assistant-Surgeon Gardner reports that, at the village of Besabat, two men, who had returned from the Hurdwar Fair, died of cholera on the 30th April, 1867, and their clothes were not burned, according to orders given, but were washed in a pond which was inside the village, the water of which was used

for domestic purposes. On the 1st and 2nd of May, sixteen attacks occurred in this village. There is an analogous instance in a report on the subject by Mr. Anthony of Madras. The part of the tank at Ramnad which is nearest the chuttes, and where the clothes of the first patient must have been washed, is the part from which nearly fifteen-sixteenths of the cholera patients had taken their drinking water. That there is a popular belief in water as a channel of communication, is shown by the disturbances which took place in America (in the South) from the rumour that Government had poisoned the rivers. In Europe, there have been some political demonstrations, under the idea that the wells had been poisoned during cholera epidemics. In India, the suspicions of the people have only been directed to the medicines issued by Government, which they thought were mixed with substances which would destroy their caste.

The influence of the admixture of animal or vegetable matter to water, in facilitating or retarding the propagation of the poison, is a point of great practical importance. It is generally supposed that the poison multiplies more readily in water which contains animal or vegetable matter; but water, in all states of impurity, has been used without inducing cholera, whilst pure water, in any way contaminated by the admixture of cholera evacuations, will almost surely induce the disease. This is proved beyond doubt by the case given by Dr. Macnamara, when water in a drinking jar was so infected, and imparted the disease to those who drank it, as well as by Dr. Snow's observations. A small quantity of cholera poison in ordinary drinking water, may soon render it capable of widely disseminating the disease in a most fatal manner. There is no evidence that this poison can be removed by filtration, or by the addition of any disinfectant which will leave the water fit for use; it is, therefore, of vital importance to prevent the poison gaining access to the water. The military cantonments which have suffered most severely from cholera in the Punjaub are supplied with water from open canals. That this was the channel of communication in Peshawur, in 1867, is highly probable, as a large party of Hurdwur pilgrims passed through Peshawur, on the 19th of May, on their way to Cabool. Their road, for some miles, lay near the canal. The disease broke out all over the military cantonment on the 20th, and in the city on the 22nd and 23rd. That the station of Peshawur was attacked on the 20th is a fact, and there is strong probability that the cause was through the canal water, as on no other occasion has the disease appeared at this season. This view is strengthened by the result of the removal of the troops. One-half of the 42nd Regiment was moved into camp on the 20th, and the remainder during the three following days. The whole regiment changed ground on the 24th, and moved on to Cherat. Out of a strength of 674 men, 105 had been attacked up to the 25th, of whom 54 died; 23 were attacked in the five following days, and, on the 31st, the last case occurred, making a total of 129 cases, of which 72 died. The 77th Regiment was attacked on the 21st. On the 23rd, four companies were moved into camp, and the rest of the regiment were also sent into tents in the vicinity of the station, as soon as carriage could be procured. The last case in this regiment occurred on the 24th June; the disease, therefore, existed in it three weeks after it had ceased in the 42nd Regiment.

The subsoil water may often be connected with the diffusion of the disease, but it is in no way essential to it, as supposed by Professor Pettenkofer. This is clearly shown by the progress of the disease bearing no relation to the rise and fall of the subsoil level in India, more

especially in the Central Provinces, where it has been carefully observed by Dr. Townsend, and by the continuous course of the disease, which accompanied the stream of pilgrims from the Hurdwur Fair, in 1867. There can be no influence from subsoil water in cases where the disease appears in ships some days after sailing from a port, in which cholera did not rage, and amongst emigrants who had never even seen the country where it did prevail, and from which it must have been brought by those fellow-emigrants who had lately left it. That the dampness of the soil in connection with chemical or vital action on the surface may influence the course of the disease, is very probable. There are many instances of epidemics breaking out after sudden storms or heavy falls of rain. The Hurdwar epidemic of 1867 was preceded by a heavy fall of rain. Instances are not uncommon of epidemics subsiding after severe storms. The disease has occasionally been preceded by severe dust storms, as at Kurrachee in 1845. The influence of the season, which is so clearly shown in the tables I have referred to, I attribute to the natural action of the elements on the surface of the soil. I do not think there is anything specific in the atmosphere when the disease is spreading, but merely that the state of the air and water at that period is well suited to the growth and development of the poison which causes the disease. The tenacity of life of the poison under all circumstances, and its excessive fecundity in some, indicate the difficulties of restraining the course of the disease. The development of the poison outside the body may be checked by the use of certain agents, called disinfectants. The nearer to the focus of the disease, the more effectually can they be applied. The evacuations from the body being the greatest source of danger, every possible means should be employed to disinfect them, and prevent their entering the public sewers, or getting near the sources of water supply; everything soiled by them is a source of danger. Every energy should be directed to the effort of restraining the multiplication and dissemination of the poison when it has appeared; but our first care should be to ward off its entrance. There are difficulties in excluding so subtle an enemy, and the means are vexatious and expensive, but not always useless. When we witness the great mortality radiating from a single point, and when we see how one focus may be contaminated by the casual visit of a sick man, and consider that the exclusion of that individual would have averted the explosion that followed his arrival as surely as the explosion of a magazine follows the admission of a single spark of fire, we cannot but wish that that exclusion had been enforced.

Great benefit has arisen in all countries, and at all times, from attention to sanitation. It is a means of saving life from all epidemic disease, and in none is its aid more valuable than in cholera; but it would not be safe to neglect other precautionary measures, and rely solely on this. In India, I have seen some of the most fatal attacks take place in cantonments and jails, where all practicable sanitary rules were in full force and most zealously carried out. The foul air from drains, cesspools, and dirt of all kinds, is the nurse, not the mother of cholera.

In Europe sanitation has received the most careful attention, and is most thoroughly studied, but the origin and source of cholera has not been so perseveringly investigated as in India, where the Government has enforced many valuable regulations for restraining the dissemination of the disease, and averting danger from its attacks by early treatment and precautionary measures. Our knowledge that the poison passes through certain channels and is transported by human intercourse indi-

cates the possibility of restricting its course, and experience has shown that by quarantine regulations many cantonments and jails in India have escaped when the disease raged in the surrounding country. The disease has been arrested in the quarantine station at Staten Island, in America, in nine out of thirteen instances in which cholera subjects were landed from ships. In England, during the last two years, there have been about twenty instances where the disease, having appeared in the crews of vessels which had left infected ports in the Baltic, has been prevented from spreading by quarantine regulations in English ports. Sanitary arrangements unquestionably form a valuable auxiliary and are the groundwork on which plans for the restriction and cure of the disease must be founded, and without which neither quarantine nor medical treatment could be successful, but they will neither prevent the transmission of the disease over the world, nor cure it when present. The subtlety of the enemy, and the rapidity with which he multiplies, indicate the difficulty of restraining his action, but that he should be occasionally arrested is a position gained, saving, perhaps, a wide-spread mortality. Whilst his occasionally eluding this arrest is matter for regret, and is occasioned only by the imperfection of the measures used, or neglect in carrying them into execution.

A REPORT

ON

EMIGRANT SHIPS.

BY

THE SANITARY COMMISSION OF
THE "LANCET."

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be comprised in the said resolution; thence to New Orleans, Louisiana, where he will take his station, and make the necessary visits and inspection on the Gulf coast. On the 1st of November next he will return to this city and make his report to the Secretary of War, through the Surgeon-General.

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By order of the Secretary of War:

E. D. TOWNSEND,
Adjutant-General.

A REPORT

ON

EMIGRANT SHIPS.

A VERY profuse and somewhat sensational communication has recently appeared in the columns of the *Daily News* from "An Amateur Emigrant." The production is a mild imitation of our old *Pall Mall Gazette* friend the "Amateur Casual;" and we are bound to believe that the imitator in this case was impelled to record his personal experiences for the benefit of future emigrants. Anyhow the "big gooseberry" has not been exhibited without effect. We have already as a result looked over a good half-score of letters on the subject, and there can be little doubt that at least treble that number will find their way into the waste-basket of our contemporary, most of which will contain much that is true and not new, as well as much that is new and not true.

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By order of the Secretary of War:

E. D. TOWNSEND,
Adjutant-General.

It is, however, our intention to view the matter in a properly serious aspect. Our readers may or may not be aware of the fact that 102,452 English, 19,232 Scotch, 71,067 Irish, 53,246 foreigners, and 6438 persons whose nationality was not classified, left the United Kingdom last year as emigrants. Of these 198,843 went to the United States, 32,671 to British North America, and 12,227 to the Australian colonies. Liverpool is, and always has been, the chief port of embarkation, no less than 166,166 of the above total number having embarked thence last year. It is a duty to discover how this quarter of a million of persons fare on their passage across the ocean, for the sanitary aspect of the question occupies a very prominent position. The state of health in which emigrants embark, the quarters assigned and the food supplied to them, and the treatment they receive on board, are all matters that bear as directly on the health of the community at large as drainage, disposal of sewage, and anything else that aids to effect prevention of disease and maintenance of health.

Not much more than twelve months ago we gave a synopsis of the Thirty-first General Report of the Emigration Commissioners, and we propose, in a short series of systematic reports, to show how far

facts justify the statements made on that occasion; whether the cry that is being raised against emigration arrangements afloat is unreasonable, justifiable, or necessary; and, if necessary, whether means exist to modify or to remove the evils that are complained of so loudly and so emphatically.

As we write the *Atrato*, one of the largest vessels in our merchant navy, has been compelled to land 550 emigrants at Plymouth in order to repair machinery. Her living freight is destined for Melbourne; and as this is the first trip of a newly formed line of large emigrant ships, it is all-important that this subject should, specially in the interest of Australian voyagers, be most thoroughly sifted and investigated.

There are some few categorical particulars relative to emigration that may fitly serve as a prologue to the reports on emigrant ships that we propose to lay before our readers.

The Passenger Amendment Act of 1855, which is still the text-book of the Commissioners of Emigration, enjoins that every statute adult shall have on the "upper passenger deck" of any emigrant ship fifteen clear superficial feet of deck, and on the lower passenger deck eighteen superficial feet. The amount of cubic space is not

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Adjutant-General.

actually specified, but it is provided that six clear feet in height shall exist from deck to deck (*not* from deck to beam); so that, according to rough calculation, each adult must have at least ninety cubic feet of space between decks. And it is further provided, that every person shall have five superficial feet clear for exercise on the upper deck or poop. Thus much for space. The 26th section of the Act gives to the emigration officers concerned very general instructions as to ventilation, but, as we are informed, further rules and recommendations are laid down by the Commissioners in a special code of instructions drawn up for the guidance of their medical inspectors. It suffices here to say that ports or scuttles, hatchways, windsails, and cylindrical ventilators with moveable cowlheads, have been for many years, and still are, the only means whereby the air is changed between the decks.

The 31st section of the Act is devoted to a general description of the kind, quality, and quantity of provisions and water ordered to be shipped for the use of emigrants, but we have obtained from the Emigration Office a list of all articles required, and the weekly totals for each person are as follows. (1) Animal food—*i.e.*, beef

16 oz., pork 24 oz., preserved meat 16 oz., suet 6 oz., and butter 6 oz.; (2) bread-stuffs—*i.e.*, biscuit 42 oz., flour 56 oz., oatmeal 16 oz., peas $\frac{3}{4}$ pint, rice 8 oz.; (3) preserved vegetables—*i.e.*, potatoes 8 oz., carrots 8 oz., and onions 8 oz.; (4) groceries—*i.e.*, raisins 8 oz., tea 1 oz., coffee (when roasted) 2 oz., sugar 12 oz., and West India molasses 8 oz. Three quarts of water are allowed daily, "which is, as far as possible, to be distilled water," and the ship's surgeon may issue a larger allowance in the tropics "provided the supply will admit of it." Pickles, mustard, salt, and pepper are also issued, and lime-juice, "out of the tropics, may be issued or not, at the discretion of the surgeon." The medical comforts are vast in their variety, and include, among other things, yellow soap, "approved" stout, Braham's "excelsior" soap, oatmeal grits, and quicklime. The medicine chest is furnished according to a scale provided by the Commissioners, which need not be particularized in this place, although it is, according to reports that we have lately received, a fruitful source of perplexity to druggists who are called upon to provide the requisite articles.

The 38th and 39th sections provide for the carrying of a passenger steward and passenger cooks, but

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no stewardess is mentioned. The 41st section enjoins that a surgeon shall be carried if more than eighty days are occupied on the voyage in the case of sailing-vessels, or more than forty-five days in the case of steamships, the number of passengers on board exceeding fifty, and also in any case whenever the number of persons on board (including passengers, officers, and crew) exceeds three hundred.

We have here recapitulated as briefly as possible those points of the law bearing specially on the subject on which we are about to report, and, for obvious reasons, abstain at present from any sort of criticism thereupon. No less than 571 private passenger ships left the ports of the United Kingdom last year under the superintendence of Government emigration officers, as well as three vessels that were chartered by the Emigration Commissioners. These ships carried (including crew and passengers), 281,317 persons, and, as we take it, there are sufficient means placed at our disposal to decide: (1) whether the emigration laws are proper and sufficient in a sanitary point of view; (2) whether the emigration officers exercise their powers fully and faithfully; (3) whether the shipowners treat their living freights liberally, scurvily, or according to the exact letter of the law.

In pursuance of the subject, it is well to mention that there are at present five chief lines of steamships carrying emigrants between Liverpool and the United States, named respectively the British and North American Company (or Cunard's); the Liverpool, New York, and Philadelphia Company (or Inmans'); the National, the Guion, and the White Star lines. These companies own unitedly from fifty to sixty of the finest oceanic steamers, of from 3000 to 5000 tons each; and about six of these ships sail every week from the Mersey, with freights of emigrants varying in number from 300 to 1000 persons. The Pacific and "Allan's" Companies also carry emigrants; but as the vast bulk of those who start from our shores are carried to New York or Boston, we elected to confine our attention to the five companies above-mentioned, and had from all their representatives every facility given for the proper conduct of a complete tour of inspection. Thus we were enabled to visit one vessel in dock, a second when just out of dock, and two others several hours before the time of departure, when all the emigrants were on board and were being officially inspected by the emigration officers. Lastly, our inspection was most valuably supplemented by a journey to Queenstown in a fifth ship,

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which belonged, as we were informed, to the youngest of the five companies above-mentioned.

As the object of this inquiry is not immediately connected with the physical condition of emigrants on embarkation, but with accommodation given and treatment received, we shall describe briefly (1) general arrangements; (2) ventilation and sanitary arrangements; (3) scales of diet, and the way in which the food is served.

1. *General Arrangements.*—These vary considerably, and are not, in fact, exactly alike in the ships of any two companies. All the vessels have a chief steerage, as well as a lower or "orlop" deck, which latter is not always used. The main steerage deck varies from 7½ to 8 feet in height, and is in most ships divided completely across by one or more fixed bulkheads, thus forming an aft and a fore-steerage. The married couples, with their children, and the single women, are, in some cases, placed together in the aft-steerage, the married people being on one side of the ship, and the single women on the other. In some ships married people, single men, and single women are all separated from each other, a boarded division being built up across the ship; in others, no division at all exists. The berths (or, in shore parlance, bed-places) are arranged in lots of from 20 to 24

each, on two levels from deck to deck, with one or two scuttles to each of these compartments, and have a passage between them averaging in width 3 feet. Each berth is at least 6 feet in length; and among the five ships inspected, varied in width from 20 to 26 inches. In those vessels where the single and married compartments occur indiscriminately, the walls of each are completely closed, and hinge or sliding doors are provided, so that in such cases the compartment is as exclusive as an ordinary room ashore; but privacy is purchased at the cost of fresh air, although some small openings exist between the beams above, and also under the lower berths. In those ships where the married couples and single women are separated from the rest, or where all three classes are separated from each other, the berths are built generally with open or trelliswork; though, as we observed in one instance, this was so arranged as to exclude observation from without pretty completely. The tables are placed fore and aft between the rows of berths, and in most cases are, when not required, fixed up above close to the beams, so as to leave the space amidships clear. In several ships, where width permitted, fixed seats were also put up fore and aft along the outside of the berths.

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The system of closet and urinal arrangement varies very much. In the older ships both are sometimes fixed on the upper deck, and thus all emigrants are required to come up the ordinary ladder in order to use them. In others they are arranged on the main deck, but in the newest ships there are distinct sets of closets for each class of persons, each set being approached by a ladder from the steerage, so as to be easily entered in all weathers by the women and children.

Lavatories, with hot, cold, and salt water laid on, and with the same sort of entrance from below, are provided in all the new ships; but many of the old vessels have none at all, so that women and children are compelled in all weathers either to wash on the upper deck or make a mess in their own quarters below.

The hospital arrangements are most curiously different in nearly every line of ships. In some—and those too of the newest class—a small compartment, containing from four to eight beds, is fitted up at the end or in the centre of each division, often in the most inconvenient part of the deck, and of course in the immediate vicinity of the general body of emigrants. In some vessels of an earlier date of build all hospitals are fixed on the upper deck, and

in one case we were shown a third small compartment in the same part of the ship which was reserved for a midwifery or any other special case.

Such are the general arrangements of these emigrant ships; and according to our own knowledge and belief, there are remediable faults in all. It is our decided opinion (1) that married couples with their children, single women, and single men should each have a separate division, with separate hatchways; or if this is not practicable, that the married men should be compelled to be separated from their wives and children at night, so as to have, at all events, one division for each sex; (2) that if either of these arrangements obtained, no indecency could be charged to the universal adoption of trellised compartments, and, as we shall show in our next article, the ventilation of the steerage must as a necessary consequence be improved in a very marked degree; (3) that the fixed seats fore and aft, though seemingly a small matter, are a great comfort to the women and children, and should be always put up; (4) that urinals, closets, and lavatories should always be provided for the women and children, and should be so arranged that they can be used without necessary access to the upper deck; (5) that, as a sanitary matter, it is most important to abolish all

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hospitals from the steerage — until, indeed, the arrangements for these passenger ships are far more ample than at present—and the upper deck seems to be the only proper position for them.

It appears to us that, as far as general arrangements are concerned, all these suggestions might be combined and adopted in one ship without any great amount of difficulty.

Ventilation and Sanitary Arrangements.—No patent or special plan of ventilation appears to be adopted in any of the Liverpool emigrant ships with which we are concerned. The outlets for foul and the inlets for fresh air comprise—ports or scuttles along the sides of the vessels (none of these ships have stern-ports); hatchways, varying in area very considerably; ventilators of two kinds, called respectively up-cast and down-cast; and windsails. The ports and scuttles are, comparatively speaking, of little use as ventilators, for they must be always closed when the wind is fresh; and it is also to be noted that, except in the newest ships, the orlop or lowest deck is, as a matter of necessity, unprovided with them. The hatchways are of course a chief means for the ingress and egress of air to and from the 'tween decks. One at least of these must exist over each division of the steerage, and their area, as

a matter of working convenience, is in all cases as large as the size and build of the ship will allow. The 26th section of the Passengers Act of 1855 recites that over every hatchway "there shall be erected such a booby-hatch or other substantial covering as shall, in the opinion of such emigration officer, afford the greatest amount of light and air and of protection from wet as the case will admit." The practical result is, that in most emigrant ships substantial coverings are provided: sometimes in the form of what are called storm-hatches—*i.e.*, large wooden hoods over the hatchways, with hinged lids; and sometimes a simple grating, covered if necessary with a tarpauling. It is a rule, and we believe one very fairly observed, that these hatchways are never closed at all except from stress of weather; but as, particularly in Atlantic voyages, this circumstance must occasionally occur, other means of ventilation are necessary; and so a varying number of fixed up-cast and down-cast ventilators are provided; and here again a vast variety of arrangements exists in the different ships examined. No particular principle appears to be followed in the number or position of these ventilators. The down-casts are, at their upper ends on deck, furnished with a moveable cowl, which is, or should be, trimmed

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strictly, and altered as the wind changes. The lower end is in some ships carried to within a few inches of the deck, in others about waist-high, and in others higher. The upper coverings of the up-casts are usually fixed; and the position of their lower ends as to the level of the deck, or the points at which they are situated, is very various, even in the case of ships the build of which is almost exactly alike. The total number of these ventilators does not appear, as a rule, to have any fixed relation to the size of the ship or the area of the steerage deck, but varies from ten to twenty, with a total area of from a hundred to a hundred and fifty square feet. Ventilation by windsails consists, as most of our readers may know, of suspending canvas ventilators with the upper openings head to wind, over any hatchway or other convenient communication that exists with the steerage or orlop decks. These, if properly attended to, are most useful, but want constant watching and careful trimming.

The supplementary means of ventilation comprise lateral spaces left over and below the bulkheads and the berths, which vary in vertical measurement from six to ten inches, and are in most cases carried along the entire width of the deck.

We may now recapitulate our experiences as to

temperature during a passage of about eighteen hours from Liverpool to Queenstown. On making a round in company with the medical officer of the ship at 10.30 P.M., with a strong head wind blowing, and the ship at full speed, our thermometer stood at 75° Fahr. in the main steerage, at 73° Fahr. in the main orlop (or lower) deck, and at 66° Fahr. in the fore orlop deck, no material difference in temperature being observable in any particular parts of the deck. At this time all the ventilators were in working order, with one exception, and that on the main orlop deck, to which we directed the attention of the officers. All was perfectly still, although there were no less than 463 steerage passengers on board; any indications of sea-sickness were so slight as to be scarcely audible or visible. We visited the decks seriatim immediately after twelve at midnight: the same temperature as noted previously existed on all, the same ventilator was palpably out of gear, and the same quietude existed. At 8.20 A.M. the next morning, when the wind was somewhat more on the beam, and having observed that the cowl heads of the ventilators had been trimmed accordingly, we went below to the steerage for the third time, when the occupants were in the midst of breakfast, and no upward movement had

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commenced on the part of any one. The temperature of the deck in which the married couples and children were located was 61° Fahr., and on the lower deck it was 67° Fahr. All were then up and busy at their morning meal, and although we shall now proceed to speak of food, we may say parenthetically, that nothing appeared to have affected the appetite unfavourably. The same ventilator still refused to do duty, and it is well to record in this place that the builder of the vessel, who on this occasion chanced to be on board (overlooking a process for lighting the ship with gas), pleaded guilty to having diverted the current of air of this particular ventilator from the lower steerage deck into the gas-room, which latter is an off-shoot of the engine-room.

The sanitary arrangements, apart from those already described, may be very briefly told. It is not considered diplomatic in these ships, whether the weather be fine or stormy, to insist upon any general elimination of passengers from the steerage deck before breakfast is fairly over; so that, as a rule, the space below is not clear much before ten o'clock in the morning. It is then the duty of the under-stewards and helpers to sweep 'tween decks thoroughly, to see that the berths are clean, and afterwards to scatter

about some sort of deodorizing powder, carbolic acid being most generally used. The ports or scuttles are opened if wind and weather permit, and the men, women, and children are as far as possible kept on the upper deck till dinner time, which occurs between twelve and one o'clock midday. The medical officer of the ship and the purser make a general round of the ship after the morning clearance is over, and from past as well as recent experiences we believe that this morning tour of inspection is diurnally and systematically performed.

In summarizing the above record and remarking critically thereupon, it is plainly manifest that the designers and builders of ships are still very much at sea as to any simple and efficient scheme of ventilation. No uniform plan exists, it being as we believe a main point in the mind of the designer not to spoil the appearance of the spar deck by the exhibition of too many ventilator-heads or cowls. Hatchways in steamships are usually down-casts. Windsails, when used, must of course act in the same manner, but we have over and over again found them, even in the tropics, twisted and throttled below, probably by some interested person berthed near them. In spite of much that has been written on the subject, including the ingenious in-

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ductions and statistical particulars compiled by Dr. William H. Pearse, of Devonport (who, by the way, refers exclusively to sailing ships), we are very much disposed to believe and maintain that in a steamship moving through the water at an average speed of from eight to ten knots an hour, foul air has a tendency to collect in the after part of the steerage, where, if stern-ports existed, it would find a ready exit. The whole subject of the ventilation of ships is at present involved in obscurity, because the matter has not been sufficiently or systematically studied, and because the plans hitherto proposed are far too complicated for general adoption. But, as it appears to us, there are several conditions that should, and do not, universally obtain on all these Liverpool steamships—(1) that plenty of fixed up-cast ventilation should be provided at the forward end of the fore-steerage, and at the after end of the aft-steerage; (2) that more down-cast ventilation should, as a rule, be provided at the fore end of the fore-steerage, particularly on the lower deck; (3) that the lower ends of all ventilators, ~~whether up-casts or~~ down-casts, should in all cases be brought to within a foot or eighteen inches of the floor of the deck, to prevent excessive draughts or mischievous meddling; (4) that no ventilator of any

kind provided for use on an emigrant deck should be diverted from its original purpose before another of equal calibre is provided.

We prefer to postpone at present special criticism on the temperatures above recorded, and leave it to our readers to consider whether, according to their own knowledge and belief, the condition of things between decks on this occasion may or may not be correctly described as more "stuffy" than the bedrooms of many of our upper and middle classes, the occupants of which enjoy a large amount of cubic space and a liberal supply of light.

Scales of Diet, Cooking, and Serving Arrangements.—Our preliminary remarks recapitulated the scale of diet drawn up and enforced by the Emigration Commissioners in accordance with the Passenger Amendment Act of 1855. The particulars of that scale need not be re-written, because we shall describe *seriatim* the meals of a steerage passenger, and because, too, it is universally allowed that no limit as to quantity is made in any of these ships. We have, however, compared the scales of diet in use in the Royal Navy and in troop-ships with those of the Emigration Commissioners, not by any means to the disadvantage of the latter, although it is manifest that the scale requires revision. It is

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not, however, necessary to discuss that question; for it appears that in no single instance is the official list adhered to in its entirety.

There are from two to four steerage passenger cooks in each vessel, or an average of one to every 100 emigrants. The galleys of the vessels that we visited were in very fair order. The meat is stored in a large ice-house forward, as are also the vegetables and other fresh food, all of which is taken indifferently for the use of steerage and saloon passengers, though it is natural to suppose that the prime joints are sometimes chosen for the latter class. We inspected, however, three ice-houses, and found the quality of their contents, animal and vegetable, very good, though packed in a heterogeneous and unmethodical way, indicative of certain waste and probable deterioration. All the biscuit examined, whether out of store or out of the hands of emigrants, was good, and the bread was as good, but not better, than it should be. As to the latter article, it is sufficient to say that, having eaten it at the commencement and at the end of a voyage in one of these emigrant ships, as well as in mid-ocean, we believe that sourness is the exception rather than the rule. The butter taken at random from an emigrant's plate on the steerage table was as good

as that furnished in the saloon; the potatoes were fairly good; and the soup in all cases was better in quality than we have seen served out to the inmates of any large "service" or other establishment ashore.

Now as to the *modus operandi* of meals in the steerage. At all meals, and we believe on all ships, an *ad libitum* scale obtains. Breakfast commences at 8 A.M., and consists of coffee, sugar, fresh rolls and butter, or oatmeal porridge and molasses. The coffee is brought from the galley in large tins, and is served out to all in turn by the steward and his assistants. We have tasted better and worse. At 8.20 P.M. we visited all the steerages, "grubbed about" among the people, tasted their provisions, and talked to several, including a good specimen of the sea-lawyer, who had "crossed" several times before. We went round at the dinner hour (one o'clock) on board two ships, and in both cases inspected the provisions before they were sent from the galley. The soup, as we have above indicated, was very good, and is indeed always a strong point in nautical cookery. It was taken from the galley in large tin cans, and was sufficiently hot when it reached the steerage deck. The meat was cut up in the galley, and sent down in large metal dishes.

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The carving was rough, and the result to a delicate appetite was not inviting; but the meat was sound, wholesome, and fairly cooked. One steward served it out to each passenger with a large fork, and another followed with potatoes. All had an opportunity of being helped again. Salted ling was given on Friday, in deference, as it is said, to the religious proclivities of Irish emigrants, this being, indeed, the only fish day of the week. We doubt very much the wisdom of this proceeding. It is difficult to cook fish properly in large quantities, and the result on board ship is not particularly successful. We did not, however, hear any complaints, except from the father of one family, who objected to the absence of animal food, and who was at once provided with a meat dinner by the purser. In all vessels pudding is given on Sundays, and in most "mess" (*i.e.*, salt) beef and pork occasionally, as Swedes, Norwegians, and Germans are said to prefer it to fresh meat. Supper is served at six o'clock, in the same fashion as at breakfast, and consists of tea, sugar, bread, and butter, with oatmeal for those who like it. Biscuit is always at hand, and the allowance of water, though fixed by the Government scale at three quarts per day for each person, is practically unlimited.

In summarizing the above brief description we may at once say that the sort of machinery employed in serving out rations on board emigrant ships is very much the same as that in existence at most of our large public institutions. There is of course less order, but this is to a great extent necessitated by want of room, and by the absence of that discipline which as a matter of course obtains in all large establishments ashore. And lastly, the sight or smell of food of any kind is, as we all know, excessively distasteful to a sea-sick stomach. There can be no doubt that the cooking is to a certain extent clumsy, in true accordance with the fashion of Britons whenever large quantities of food are prepared for consumption. But until a better system obtains under similar circumstances ashore, we can hardly expect much reformation afloat; and speaking from close personal observation, made during the various stages of tropical as well as Atlantic voyages, we can testify to the fact that three-fourths of the emigrants "take to their victuals" very kindly indeed, and eat abundantly, if not enormously. It must be recollected that these passengers are carried across the Atlantic in from ten to twelve days at the rate of about a half-penny a mile, including provisions, and that last

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year 212,423 such passengers were landed at their respective destinations with a mortality of only ·06 per cent. A very cursory glance at the occupants of an emigrant ship when she is fairly out on the Atlantic will soon convince the observer that for the vast majority, the much quoted "middle passage" has very few horrors.

Those of our readers who have looked over this Report will possibly come to the conclusion that, if the condition of things set forth therein really obtains, very little reformation is needed. But this is not so. It is our duty to show that many minor and some radical changes are required in the entire system of the emigration business as carried on at Liverpool. And first as to the arrangements made for the accommodation of emigrants afloat. It appears to us that some definite system of berthing, such as is already adopted in some ships, should be officially planned, and adhered to unvaryingly. And without entering into details, it is plain that if the women and children were berthed together, or the married people and single women put into separate compartments, ventilation would be greatly aided, and that with very little sacrifice to what is called decency, by building the berths with a minimum of bulkhead

work—as open, indeed, as those on board the *Serapis* and other Anglo-Indian troop-ships. Lavatories should be provided for the women and children on board all ships, and should, as also the closets and urinals, be accessible, under cover, from the steerage deck. No hospital of any kind should exist on the steerage deck, as such a plan is fruitful of all sorts of sanitary mischief. And a steerage stewardess, well up to her work, ought to accompany every vessel, whose business it should be as much to keep the single women in order as to assist the married women and children.

The ventilation question is difficult of solution, and those who have studied the matter know that in this, as in other things, it is easy to preach and hard to practise. It seems to us, however, that in most ships too little attention is paid to the necessity of providing plenty of down-cast ventilation forward and of up-cast ventilation aft on the steerage deck. In the absence of any definite system, the points where fixed ventilators are most required can be ascertained by experiment without difficulty during the first voyage, and no shipbuilding considerations as to sacrificing symmetry or the like should be allowed to interfere with their construction. It is specially necessary, too, that all down-

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cast ventilators should be brought to within about eighteen inches of the steerage deck, and that both up-casts and down-casts should be examined periodically night and day by a person told off specially for the duty. The whole subject of ship-ventilation requires, however, far more attention than it has hitherto received, for the arrangements now in vogue on board our smartest ironclads and other QUEEN'S ships are still very imperfect; and it would be well if DR. MACDONALD, who has been recently appointed Professor of Naval Hygiene at Netley, were associated with the Constructor's Department at the Admiralty to advise on so important a question.

We have very little to recapitulate on the question of diet. There is enough, and indeed much more than enough, for the waste is enormous, and, as we think, utterly inexcusable. Most of those who go across the Atlantic in the steerage feed better there, both as to quantity and quality, than they ever did before, and better too than, for some time at all events, they will do again; and, as we remarked before, clumsy cooking is a "privilege" that your genuine Briton has not yet laid aside.

All cargo should be on board before emigrants embark, and it is surprising that the Emigration

Commissioners have not insisted upon this rule long ago.

It is, however, hardly to be anticipated that the reforms above mentioned will be inaugurated and adhered to properly and completely unless some radical change is made in the system of official supervision. The work of the Emigration Commissioners will, at the beginning of next year, be transferred by Act of Parliament to the Board of Trade, and indeed steps have been already taken to give effect to the decision. Hence an opportune moment occurs for commencing changes of administration. We take it that the chief desideratum is an increase in the duties, powers, and responsibilities of emigration medical officers. At present their work is absolutely limited to a hasty and perforce unsatisfactory inspection of the emigrants just before sailing, and of the medicines provided for their use. They know nothing officially about ventilation, hospitals or sick bays, closets or urinals, food or medical comforts; for all these matters come exclusively under the cognizance of the emigration officer (a naval man), no reference being made to the medical inspector in the capacity of "sanitary authority." It is reported that this plan was adopted, and so continues, to facilitate

be comprised in the said resolution; thence to New Orleans, Louisiana, where he will take his station, and make the necessary visits and inspection on the Gulf coast. On the 1st of November next he will return to this city and make his report to the Secretary of War, through the Surgeon-General.

* * * * * * * *

By order of the Secretary of War:

E. D. TOWNSEND,
Adjutant-General.

work, or, as shipowners would phrase it, "quick dispatch." But we much doubt the wisdom of the proceeding. The emigration officers at Liverpool and elsewhere are particularly smart and concise in the execution of their duty, and appear to know the nautical items exceedingly well. But it requires no sophistry to show that many important sanitary matters that directly affect health should be placed under the inspectorial control of the medical officer. It does not appear that they are at present either overworked or overpaid at Liverpool, although the Board of Trade have already advertised for a third inspector. But these inspectors should be called upon to examine all intending emigrants before embarkation (this opinion was enunciated in the *Lancet* some months ago), and afterwards make a general inspection afloat. They should also be responsible, in conjunction with the lay officer, for the ventilation, hospital, closet, and lavatory arrangements, and for the quality of the rations and medical comforts provided; for these are all medical and sanitary questions, and directly affect the health as well as the comfort of emigrants at sea. We are bound, and indeed very glad, to record our strong sense of the active and intelligent way in which the present

system is worked, both by the Government officials and by the shipowners; and we tender to both many acknowledgments for much courtesy. The result, however, of this tour of inspection and of former practical experience afloat has strongly impressed us with the belief that, on the part of the shipowners, the changes above indicated (all of which, it must be recollected, exist in some *one* line of ships) are imperatively needed; and that, on the part of Government, the medical officer should be far more utilized, and saddled with a far greater amount of responsibility.

THE END.

be comprised in the said resolution; thence to New Orleans, Louisiana, where he will take his station, and make the necessary visits and inspection on the Gulf coast. On the 1st of November next he will return to this city and make his report to the Secretary of War, through the Surgeon-General.

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By order of the Secretary of War:

E. D. TOWNSEND,
Adjutant-General.

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42D CONGRESS, 3d Session. } SENATE. { EX. DOC. No. 9.

E. S. Parker

Compliment of Harvey E Brown
Asst Surgeon U.S.A.

LETTER

FROM

THE SECRETARY OF WAR,

COMMUNICATING,

In obedience to law, information in relation to quarantine on the Southern and Gulf coasts.

DECEMBER 9, 1872.—Referred to the Committee on Commerce and ordered to be printed.

WAR DEPARTMENT, *December* 6, 1872.

The Secretary of War has the honor to transmit to the United States Senate and House of Representatives, in compliance with the requirements of a joint resolution, approved June 6, 1872, providing for a more efficient system of quarantine on the Southern and Gulf coasts, the inclosed copy of the report of Assistant Surgeon Harvey E. Brown, United States Army, who was detailed to make the investigations called for.

WM. W. BELKNAP,
Secretary of War.

WASHINGTON, *December* 2, 1872.

SIR: I have the honor herewith to present a report of the work performed by me, in obedience to Special Orders No. 139, current series, from the War Department, of which the following is a copy:

[Special Orders No. 139.—Extract.]

WAR DEPARTMENT, ADJUTANT-GENERAL'S OFFICE,
Washington, June 17, 1872.

On the recommendation of the Surgeon-General, Assistant Surgeon Harvey E. Brown is hereby detailed to make the inspections and reports required under the joint resolution of Congress approved June 6, 1872, "providing for a more effective system of quarantine on the Southern and Gulf coasts," and will proceed, via New York City, to Norfolk, Virginia; thence to such other towns or ports on the Atlantic coast as may be comprised in the said resolution; thence to New Orleans, Louisiana, where he will take his station, and make the necessary visits and inspection on the Gulf coast. On the 1st of November next he will return to this city and make his report to the Secretary of War, through the Surgeon-General.

* * * * * * *

By order of the Secretary of War:

E. D. TOWNSEND,
Adjutant-General.

The joint resolution referred to in the foregoing order is as follows

JOINT RESOLUTION providing for a more effective system of quarantine on the Southern and Gulf coasts.

Whereas experience has proved that the present system of quarantine on the Southern and Gulf coasts is inefficient to prevent the ravages of yellow fever in the cities and towns of that section: Therefore,

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That the Secretary of War be, and is hereby, directed to detail one or more medical officers of the Regular Army, who shall, during the coming season, visit each town or port on the coast of the Gulf of Mexico and the Atlantic coast, which is subject or liable to invasions of yellow fever, and shall confer with the authorities of such port or town with reference to the establishment of a more uniform and effective system of quarantine, and who shall ascertain all facts having reference to the outbreaks of this disease in such ports or towns, and whether any system of quarantine is likely to be effective in preventing invasions of yellow fever, and, if so, what system will least interfere with the interests of commerce at said ports; and shall make, also, a detailed report on this subject to the Secretary of War, through the Surgeon-General, on or before the assembling of the third session of the Forty-second Congress, in December, eighteen hundred and seventy-two.

Approved June 6, 1872.

In obedience to these instructions I have the honor to report that I have visited the various ports on the Southern, and Gulf coasts which have been in the past the seat of epidemics of yellow fever. In each place visited I have examined the quarantine laws in operation, conferred with the local boards of health when any such existed, conversed on the subject with physicians of experience, and with those whose long residence or commercial or professional position would qualify them to give an intelligent opinion on the subject; and have endeavored, so far as was practicable in the brief space of time allotted me, to ascertain the facts connected with the various epidemics of yellow fever in the ports and towns visited, both by examination of publications on the subject and by personal conversations with those most likely to be conversant with such facts; and have endeavored to arrive at a correct opinion as to the value of quarantine restrictions against yellow fever, and to what extent such restrictions can be enforced without material interference with commercial interests. The result of my inspections has been such as to convince me—

1st. That in the vast majority of epidemics, if not all, that have occurred in the United States, the germinal principle of the disease was imported from elsewhere, and was not due to local causes.

2d. That a system of quarantine can be organized which will prove effective in preventing invasions of yellow fever.

3d. That a properly-organized system, so far from interfering with the interests of commerce, will prove really beneficial to the commercial prosperity of ports where established.

4th. That the present quarantines at the South, being established by either State or municipal authority, lack that uniformity which is absolutely necessary to their efficiency, are not founded on rational views of the pathology of the disease, and are generally defective in their administration, and that, consequently, a system of a different character must be adopted to attain the end desired.

5th. That yellow fever needs to be planted in a favorable soil in order to become epidemic, and that, consequently, the danger from the disease is measurably lessened in proportion as attention is paid to the local hygiene and sanitary condition of those parts of the ports or towns which, by reason of the character of the inhabitants or other causes, are usually the habitation of the earlier cases.

I am aware that, in enunciating the first four of the above propositions, I am in opposition to the views of some of the ablest and best

minds at the South, who have had the largest experience in this malady; and it is with great diffidence that I feel obliged to differ from such distinguished gentlemen as Professor E. Geddings and W. F. Wragg, of Charleston; Doctors E. A. Anderson, of Wilmington, North Carolina; R. D. Arnold, of Savannah; and Warren Stone, of New Orleans; but, believing that the history of the epidemics in the United States warrants the conclusions to which I have arrived, and that a very decided change has taken place of late years in the minds of the profession on the subject of the portability of the yellow-fever poison, and the consequent value of quarantine measures, I propose, before entering into a consideration of the subject of quarantine, to present a brief abstract of the history of the epidemics that have occurred in this country, so far as any such can be obtained. To do this intelligently demands some notice of the early appearances of the disease in the West Indies, and of the great epidemics in New York, Philadelphia, and other northern ports, between 1790 and 1822; for, although these are not properly embraced in the scope of my report, yet they have such an important bearing on the subject, that some reference to them is necessary to do justice to the question at issue. I proceed, therefore, to an account of the epidemics that have appeared in various northern ports, before giving the details of those in the southern sections of the country, and along the Gulf coast.

HISTORICAL SKETCH OF YELLOW FEVER.

The earlier historians of the West Indies ascribe the origin of the yellow fever to the East. An insurrection having broken out in Siam, the French colonists at Bannock were driven from the country, and embarked on the ship Oriflamme and two other vessels in the latter part of the year 1690, and sailed for Martinique. The Oriflamme touched at Brazil, where yellow fever had been prevailing for seven or eight years, and soon after the disease broke out on board, and by the 5th of January, 1691, over a hundred deaths had occurred on the vessel. The disease spread to the shore, and was carried by other vessels to many of the other islands. Moreau de Jonnès, in his work on yellow fever, argues, however, that the disease had existed in St. Domingo for many years previous to that time, and had prevailed in the first settlements formed by the immediate followers of Columbus, (pp. 14–60.) However this may be, it is certain that the disease was known for many years as "Mal de Siam," and the fact of its importation by the Oriflamme received general credence with the medical writers of the eighteenth century. Another account ascribes its introduction to slave-ships from Africa in 1669, in which year a fatal epidemic prevailed in St. Domingo.

It seems certain from the narratives of some of the Jesuit missionaries, as well as the historians of the period of Columbus and his immediate successors, that fevers of a malignant type, and accompanied with a yellowness of the skin, were peculiarly fatal to the newly-arrived Europeans. Such fevers prevailed epidemically in St. Domingo in 1494, in Porto Rico in 1568, at Darien in 1514, in Guadaloupe in 1635, at Martinique in 1640, at Barbadoes in 1647, and at St. Croix in 1652, and, as before mentioned, at St. Domingo in 1669, and in 1686 the local authorities in the last-mentioned island, passed ordinances to prevent the introduction of contagious and malignant fevers by means of slavers. (Moreau de Jonnès, op. cit., pp. 58, 59.) It seems most probable that these earlier epidemics were of yellow fever, but that after the arrival

of the Oriflamme in 1690, the disease acquired a peculiar degree of malignancy, which it did not before possess, and which led the inhabitants to imagine that a new disease had appeared among them. Chisholm fell into this same error in reference to the disease, which was imported into Granada in 1792, on board the ship Hankey, from the abandoned colony of Boullam, coast of Africa. This epidemic made its appearance after the West Indian islands had enjoyed a complete immunity from the disease for twenty years; and differing materially in its type and the degree of malignancy from former outbreaks, it was regarded by Chisholm (Essay on the Boullam fever, p. 89) as a disease previously unknown, and he dates the origin of the true yellow fever of West Indies from this year.

'Probably much of the obscurity that hangs around these earlier epidemics arises from the confounding of the genuine "vomito prieto" with the malignant remittent of the West Indies (all these fevers being designated by the common name of "yellow" previous to 1690,) on the part of the earlier writers, who were, most of them, non-professional men—an error which is made in more recent times in every southern city with the first cases of an epidemic, and sometimes, throughout its whole duration, physicians of learning and experience are unable to determine, positively, whether they have had any yellow fever among them or not, as was the case, a few years since, at Jacksonville, Florida, and last year in Charleston, South Carolina, as will be shown on a subsequent page.

The first appearance of the disease in the United States was in 1636 at Boston. Previous to this time, "pestilent" and "malignant distempers" are reported as prevailing at various early periods among the Indians in New England, and attempts have been made to convey the idea that this sickness was yellow fever; but, as La Roche remarks, "We cannot be censured for undue skepticism if we entertain doubts as to its identity with true yellow fever, especially as it is acknowledged to have raged in winter," (vol. 1, p. 47.) Numerous towns suffered during the last half of the seventeenth century from epidemic disorders, but it is more than probable that most, if not all of these, were fevers of malarial origin, due to the felling of the forests and the constant upheaval of the soil, which would take place in young and growing communities in settling a new country.

In 1693, an English fleet, under command of Sir Francis Wheeler, which had been sent the previous year to effect the reduction of Martinique, and had spent a month at Barbadoes, returned to Boston with yellow fever on board. The mortality had been so great among the troops and sailors, that over one-half of the whole number of persons in the fleet had died, and the disease is stated to have been introduced into Boston, and prevailed with great severity. Its next appearance was in Philadelphia and Charleston in 1699. It was asserted by one writing long subsequently, that the disease was imported from Barbadoes in a vessel laden with cotton; but none of the contemporary chroniclers mention this circumstance, although the fact that it was known by the inhabitants as the Barbadoes distemper, would seem to give a warrant to the assertion.

In 1702 it appeared for the first time in New York, and was said by contemporary writers to have been imported from Saint Thomas, West Indies, in a bale of cotton. It was called the "American plague" and "the great sickness." Although the population of the city at this time was not more than eight thousand, over five hundred persons died. Griscom's History of Visitations of Yellow Fever at New York, p. 2;

Currie, Diseases most prevalent in the United States of America, p. 63.) The disease did not appear again in New York until 1745, although Webster, in his book on Epidemics, speaks of "malignant infectious diseases" in 1732 and 1743. In 1741 it made its second appearance in Philadelphia. There were at this time a large number of German immigrants in the city, who suffered severely. It prevailed until the approach of cold weather, and caused about two hundred and fifty deaths. Carpenter (Sketches of Yellow Fever, p. 12) gives Lind as authority for the assertion that it was introduced about the 1st of June "by means of a trunk of wearing-apparel belonging to a gentleman who had died of the disease in Barbadoes." (Lind on Fevers and Infections, p. 107.) A rather doubtful outbreak of disease, thought by some to be yellow fever, occurred in 1744; and in 1747 it was again epidemic. It was the universal opinion that it was due to foreign importation from the West Indies, as it prevailed chiefly in those parts of the city near the wharves, in sailors' lodging-houses, and among the classes of people most likely to be about the shipping. Like the previous epidemic, the clothing of a person who had died in the West Indies was asserted to be the source of infection.

Dr. Thomas Bond ascribed it to the presence in port of infected ships, upon which the disease broke out after their arrival. No person seems to have thought of giving the disease a local origin. (Bond, North American Medical and Surgical Journal, vol. 4, p. 271; La Roche on Yellow Fever, vol. 1, pp. 60, 61.) In 1762 it again prevailed in Philadelphia, commencing about the middle of August and terminating the last of October. Dr. John Redman, in his account of this epidemic, (pp. 12, 13,) says:

> Its first and greatest ravages were about the new market and the square to the eastward of it, in which, after some considerable search and tracing it, it was found to have originated in a number of small back tenements, forming a kind of court, the entrance to which was by two narrow alleys from Front and Pine streets, and where sailors often had their lodging, to which a sick sailor from on board a vessel, from the Havannah, (where it then raged,) was brought privately after night, before the vessel had come up to town, to the house of one Leadbetter, where he soon died, and was secretly buried; and, I believe, Leadbetter, with most of his family, and many others in that court, soon after fell a sacrifice; and from thence it spread rapidly, first affecting the houses nearest adjoining, in Front and Pine streets.

Public opinion was universal in ascribing it to foreign importation, from the West Indies.

La Roche lays great stress on the fact that, in all of these epidemics, contradictory statements are made by different writers as to the especial vessel, or other source, from which the infection spread; but this is not to be wondered at, when we consider, as stated by that distinguished author himself, that we have no accurate professional accounts of these epidemics, being dependent for what we know of them entirely to letters, &c., written by eye-witnesses, who would be likely to indite what was the prevailing opinion, without any minute investigation of the subject. Moreover, there is no reason why they should not all have been correct, and the fever introduced by two or three different channels in the same season. From this time until 1791 the country was free from the fever, an immunity which Carpenter shows (op. cit., p. 45) was due to the destruction of the West India trade by the passage of the series of acts of Parliament known as the "commercial monopoly," and by the subsequent war of the Revolution, which, of course, destroyed all trade. With the return of peace, however, came a renewal of the West India traffic, and with it the long respite which the coast had known from yellow fever came to an end, it making its appearance in

1791 in New York, and in 1793 in Charleston and Philadelphia. About the middle of August, 1791, it broke out in the neighborhood of Peck Slip, New York, a locality well fitted by its surroundings to propagate the disease—"a part of the city thickly inhabited; its houses generally small and badly ventilated; many of the inhabitants were in indigent circumstances, which is a frequent cause of the want of cleanliness." (Addoms, Inaugural Dissertation on Yellow Fever, p. 7.) For some time it confined itself to this locality, but at length began to spread, being carried to other parts of the town by those who had nursed the sick. It continued to rage until about October 15, and, on the approach of cold weather, disappeared. (Medical Repository, vol. 1, p. 305.)

The epidemic in Philadelphia, in 1793, has acquired a permanent interest, in the eyes of those interested in the subject, not only by reason of the great mortality, but on account of the bitter controversies which arose relative to its origin, the doctrines of a local cause for the disease being now advanced for the first time. The weather was very variable during the season, the spring being pleasant; July very hot, August moderate, and September hot. From August until October there was a great drought. Fevers of the exanthematous type had been prevalent during the season, but disappeared soon after the outbreak of the fever. The disease made its appearance about the 15th of August on Water street, in the vicinity of the docks. It was confined for a time to this locality, but toward the last of the month spread up Water to Vine street; thence along Front; and by the 15th of September was generally prevalent throughout the city, as well as in Southwark and Kensington, then separate villages. It raged with great violence until the 10th of November, when a severe frost put an end to it. "It did not escape observation that, during the whole course of the epidemic, the greater number of cases occurred in the vicinity of the Delaware River; that there also, as in close alleys and small streets, the disease assumed its most aggravated form, and proved most usually fatal; and that its severity lessened in its progress westward, and toward the districts." (La Roche, op. cit., p. 68.)

The population of the city was over forty thousand, of whom four thousand and forty died, and upward of twelve thousand fled from the city. As regards the origin of this epidemic, the College of Physicians gave it as their opinion, in an official report to the governor of the State, that it was imported from the West Indies in July, many vessels having arrived from infected ports in that month. Some, however, of the members, including Dr. Redman, the president, Dr. Benjamin Rush, Dr. Jos. Hutchinson, and Dr. Foulke, dissented from these views, and an angry controversy ensued, which has been handed down from epidemic to epidemic, and maintained on either side, with varying success, until the present day. The local causes were to be found in the filthy condition of the streets and wharves, the want of any adequate police arrangements, and the exposure of a lot of damaged coffee, on a wharf, near Front and Water streets, the smell of which "was putrid and highly offensive," so that persons living in the neighborhood were obliged to keep their doors and windows shut to exclude the fetor.

Mathew Carey, in his account of this epidemic, ascribes it to the arrival of numerous vessels from the West Indies, and among them one from Cape François, which brought a large number of refugees from the island of Saint Domingo, whence they had fled to escape the horrors of the revolution. There was no attempt at a quarantine; the vessels came freely up to the wharves; and in some cases sick persons and infected articles were landed in the city. Dr. Currie gave to it the same origin.

(Carey on Epidemics of 1793, pp. 69–70; Currie, Treatise on Synochus icteroides, pp. 1, 84; Haygarth's Letter to Dr. Percival, April 9, 1801, pp. 169–170; Rush, Medical Inquiries, vol. 3, pp. 17–26.) In 1794 the fever prevailed moderately in Philadelphia, and was ascribed by Dr. Rush (Medical Inquiries, vol. 4, p. 63) to the exhalations from the gutters, and to the stagnating ponds of water in the neighborhood of the city. The following year it appeared in New York, prevailing, as before, in the streets bordering on the East River, as far up as the swamp; a part of the town "remarkably distinguished by peculiarity of circumstances and situation, seemingly well calculated for the accumulation and decomposition of all kinds of perishable animal and vegetable substance." (Dr. V. Seaman, Medical Repository, vol. 1, p. 306.) The disease was unquestionably imported this year in a vessel from Saint Domingo, which arrived at the Dover-street wharf on the 19th of July, having lost one man from yellow fever on the passage. On her arrival she had three persons ill of the fever, and one died on the 20th July. On the 22d the health-officer, Dr. Treat, was taken sick, and died on the 29th. On the 25th a man by the name of Valentine, who was employed about the vessel, took the disease. At this time the ship William from Liverpool was lying at the same wharf as the infected ship; and on the 25th four cases occurred on her, all of whom died within a week. The next cases were from the ship Connecticut, lying about two hundred yards from the other; then cases began to occur on shore among clerks and employés of the store-houses on the docks, and soon the disease spread through the surrounding neighborhood, causing seven hundred and thirty deaths, most of whom were recently-arrived foreigners. (Dr. V. Seaman, Medical Repository, vol. 1, pp. 316–318; Dr. Colin Chisholm's Letter to John Haygarth, M. D., on Yellow Fever, pp. 58, 61; Rev. J. McKnight, in American Medical and Philosophical Register, vol. 3, p. 203.) The disease was carried from New York to Huntingdon, Long Island, where many died. (Dr. David Hosack, American Medical and Philosophical Register, vol. 3, p. 191.) There were a few cases in New York in 1796–'97, due, according to the statement of Griscom, (op. cit., p. 7,) to the detritus collected by the building of a great dock at the foot of Whitehall street, and to the emptying of a sewer at the foot of Broad street; the slip being exposed at low tide to the action of the sun upon its muddy bottom. In the first of the above-mentioned years it was imported into Knowles's landing on the Connecticut River by means of the brig Polly, which arrived there from Cape St. Nicholas, St. Domingo, in August, and had on the passage lost several of her crew from yellow fever. About this same time a number of cases occurred at Chatham, Connecticut, near Middletown, the first victims being two young men from that village, who visited Knowles's landing and slept in the cabin of the infected vessel. (Miner and Tully on Fevers, pp. 359, 363; Tully's Letter to David Hosack, M. D., New York Medical and Philosophical Journal, vol. 1, p. 153.) In 1797 Providence, Rhode Island, suffered from an outbreak of the disease, which caused forty-five deaths. It prevailed almost entirely along Water street, and other streets and alleys near the water's edge, a very dirty and filthy portion of the town, where the tide ebbs and flows beneath the docks, leaving mud and oyster-shells exposed at low tide, and being the receptacle of all the waste and offal of the town. The first cases occurred about the 15th of August, and the disease disappeared about the last of the month. (Wheaton, Short Account of Yellow Fever, Medical Repository, vol. 10, p. 329.)

This year there was another severe epidemic in Philadelphia, where

it broke out about the 1st of August, prevailed extensively during that month and September, and was checked by a frost on the 15th of October, having caused about thirteen hundred deaths. There was the usual controversy as to its origin. The College of Physicians, as before, traced it directly to the arrival of two infected ships, in July, one from Havana and one from Port au Prince, while the Academy of Medicine called attention to the putrid exhalations from the gutters, privies, and docks, which were, if possible, in worse condition than in 1793. Strobel (Essay on the Transmissibility of Yellow Fever, p. 120) quotes the late Professor Chapman as asserting that his recollection was positive that this epidemic was imported from the West Indies. The fever this year was much more contracted in its area then before; a result, doubtless, owing to a policy of isolation adopted by the authorities, by which all communication was cut off with the infected districts during the epidemic. (Dr. Felix Pascalis on Contagious Epidemic Yellow Fever in 1797, pp. 8–29; Philadelphia United States Gazette, September 13, 1798; Condie and Folwell, on Pestilence of 1798; Rush, on Epidemic of 1797, &c.) This year the disease prevailed as an epidemic in Baltimore, and, as in 1794, it was confined to the vicinity of Fell's Point, a low, swampy, unpaved locality, in the immediate vicinity of the shipping. The first case which occurred in the city had just arrived from Philadelphia. (Davidge, Treaties on Yellow Fever, p. 17; Potter's Memoir on Contagion, p. 20.)

The year 1798 introduced the most wide-spread epidemic ever known in the history of the country.

At Boston, Massachusetts, it caused about two hundred deaths, prevailing, as it had previously, in 1796, about the docks, near the water's edge, along Front Street, and in the vicinity of Market Square, the victims being those persons who passed most of their time about the shipping. From here it was wafted by the wind to higher localities. Dr. Samuel Brown believed the disease to be entirely local in its origin. (Yellow Fever in Boston, pp. 21–37.) Portsmouth, New Hampshire; Norwalk, Hartford, and New London, Connecticut; and Salem, Massachusetts, also were visited by the disease.

In New York, the disease broke out about the 28th of July, on Front street, near Coenties Slip, then in the neighborhood, and during August spread along New Slip, Cliff, John, and Water streets, all localities renowned for their filth and general offensiveness. To those causes must be added the arrival of infected ships from the West Indies.

The epidemic lasted without intermission until October, when a frost suddenly caused its cessation, but not until two thousand and eighty-six persons had died. (Dr. James Hardie, Account of Yellow Fever in New York, pp. 2–6; Currie, Memoirs of the Yellow Fever, 1798; McKnight, American Medical Philosophical Register, vol. 3, p. 293; Seaman, Letter to Dr. Miller, Medical Repository, vol. 4, p. 250.)

In Philadelphia there were three thousand five hundred deaths between the 1st of August and the 1st of November. Its introduction was most positively traced to the arrival of the ship Deborah, with yellow fever on board, from San Domingo, on the 18th of July, and to the subsequent arrival of other vessels.

The disease, as before, began in the vicinity of the wharves, and prevailed chiefly along Walnut, Spruce, and other streets at first, but subsequently spread all over the city. The ship Deborah, which introduced the disease, was removed to Kensington for repairs, and in a short time the fever broke out in that village, first among the persons who had been on the vessel, and then among those who had visited or nursed the

first cases. (Condie and Folwell, op. cit., pp. 31–53; Currie, op. cit., pp. 130–135.) Immense numbers of the inhabitants of Philadelphia fled from the city as the fever became generally diffused, and some of them carried the disease to other places. At Chester and Marcus Hook, Pennsylvania; Wilmington, Delaware; Bridgeton and Woodbury, New Jersey, it was introduced by refugees from Philadelphia, and carried off large numbers. (Currie, op. cit., pp. 136, 138; Dr. James Tilton's Letter to Dr. Miller, Medical Repository, vol. 3, p. 128; Dr. George Monroe, Medical Repository, vol. 3, p. 136.) During the progress of this epidemic, the ship Nestor, of Portland, left Philadelphia for Petersburgh, Virginia. She had four deaths on the passage, and the disease soon spread to those employed in loading her, and others who visited the ship, and many died, both in City Point and Petersburgh. (Currie, op. cit., pp. 109, 113.)

Two points are worthy of note in connection with this epidemic—one, that the authorities of Philadelphia had become frightened at the yearly appearance of the disease, and had placed the city in excellent sanitary condition compared to what it had been previously, by a careful and vigorous system of police, and yet entirely neglected any quarantine measures; and the other, that the authorities of Baltimore established a quarantine, not only against vessels coming from the West Indies, but also against persons and baggage from Philadelphia, and entirely escaped the disease.

In the next year there was a mild epidemic in Philadelphia, which the College of Physicians, as before, traced to importation from the West Indies. In 1800 it visited Providence, Rhode Island, and Baltimore, Maryland, prevailing, as in previous years, in the immediate vicinity of the wharves and shipping. It broke out this year in New Bedford, Massachusetts, where a vessel arrived from Demerara with the fever on board. She also contained a large quantity of damaged coffee. All the persons attacked had either been on board of her, or lived in the vicinity in dirty and ill-ventilated quarters.

In 1802–3 and in 1805 there were moderate epidemics in Philadelphia, after which the disease entirely disappeared from that city until 1819. There was more or less of the fever in 1802–5 in Baltimore, Maryland; Boston, Massachusetts; Alexandria, Virginia; New Haven, Connecticut; and Providence, Rhode Island. In 1803, in New York City, it made its first appearance in some houses on Coffee House Slip, and prevailed chiefly, as before, in the dirty, filthy streets, and among the wretched inhabitants living nearest to the river. The season was an unusually hot one, and Griscom says, "Proofs of its domestic production greatly accumulated this year;" but it is difficult to reconcile this statement with the assertion of Dr. P. S. Townsend (Observations on Yellow Fever, p. 368) that, "about this time" (*i. e.*, the first appearance of the disease) "several vessels, having sick on board, were ordered from the Coffee House Slip to the quarantine ground." The total number of deaths was between six and seven hundred. From New York it was carried to the village of Catskill, on the Hudson, where a number died. The next year there was a mild epidemic in New York, prevailing along the streets bordering on the East and North Rivers, and causing about three hundred and fifty deaths.

A few sporadic cases were noticed in 1806 and 1807, but from this time New York was free from any serious epidemic until 1822. In 1809, however, the neighboring city of Brooklyn suffered to some extent. Early in July the ship Concordia arrived from Havana, at which port yellow fever was epidemic. The first case was a sailor on this ship, and all the

subsequent cases were living in the immediate vicinity, (a very low, filthy portion of the city,) and were directly traceable to infection from this ship. There were about forty deaths. (Gillespie, Yellow Fever in Brooklyn, American Medical and Philosophical Register, vol. 1, p. 101.)

In 1811 a similar outbreak occurred at Perth Amboy, New Jersey, which was directly traceable to the ship Favorite and brig Ocean, infected vessels, which had recently arrived from the West Indies. The first cases were of persons who were either connected with these vessels or had been on board. (Hosack and Bayley's Report to Board of Health, American Medical and Philosophical Register, vol. 3, pp. 94, 106.)

In 1819 there was a good deal of fever at quarantine, New York, and cases occurred in September, at its old haunt in the vicinity of Old and Coenties' Slips, but there was nothing approaching an epidemic, there being less than a hundred cases in all. (Drake, Yellow Fever in New York, Medical Repository, vol. 6, p. 125.)

In this year the fever appeared in Baltimore, being immediately preceded by the arrival of an infected ship from Havana; communication having been held between her and the city. (Revere on Fever in Baltimore in 1819; Carpenter on Yellow Fever, p. 18.)

1820, *Middletown, Connecticut.*—The fever was introduced early in June by the sloop Antelope from New York. This sloop had on board a man from the schooner Milo, from Savannah, who was sick from yellow fever. The captain of the Antelope was then taken sick and recovered, but the first case died. About this same time the brig Sea Island arrived from St. Iago de Cuba, having lost two men on her passage, and with others sick. A man named Harrington, who went on board of her, died, and all the earliest cases were traced to this infection. (Dr. John B. Beck's Report to New York Board of Health, New York Medical and Physiological Journal, vol. 1, p. 158.)

In 1820 it prevailed epidemically, and, as before, broke out subsequently to the arrival of infected ships from St. Iago de Cuba and other places. The first case was on the 24th of July, in Water street, near Race, and nearly simultaneously to these were cases on Hodge's wharf, where a vessel, recently from Cuba, was lying. About the 9th of August it broke out near Walnut-street wharf, also occupied by infected ships, and spread from there to the adjacent street. In both of these cases its progress was arrested by sanitary measures. It prevailed afterward, to a limited extent, on Mulberry, Front, Water, and other streets, but the total number of cases was very small—about one hundred and twenty-five, of which eighty-three died.

After this the authorities of Philadelphia became fully awakened to the importance of measures to prevent the introduction of the disease, and devised a rigid quarantine law, as well as local sanitary measures. The result was, that the city enjoyed an immunity from the disease until 1853, a period of thirty-three years. Then the disease re-appeared, from the want of a vigilant quarantine, being clearly due to importation, as follows:

On the 12th of July, 1853, the bark Mandarin arrived at the Lazaretto below Philadelphia from Cienfuegos, Cuba, which port she left June 25, with a cargo of molasses. There was more or less fever prevailing at her port of departure when she left, and two of her crew died on the passage, of fever. Although all on board were healthy, the vessel was detained one day for fumigation and disinfection, and all the effects of the deceased sailors destroyed. The next day she came up to the city, and stopped at South-street wharf. Three days later she was moved to the next pier above, at the foot of Lombard street, where she

discharged her cargo. On the 20th she was again moved to the second pier below, at the foot of Almond street, where she remained until the 26th, when she was sent back to the Lazaretto by the board of health. After the discharge of her cargo, and especially while lying at the Almond street wharf, it was noticed that there was a very offensive smell about the vessel, and especially when the bilge-water was pumped out. On the 19th of July a case of "suspicious fever" occurred in a man named Sharp, who occupied the head of the South-street wharf as a stand for business. It proved fatal on the 26th. On the 20th another case occurred, in the captain of a brig lying next to the Mandarin, in the Lombard-street dock. He died on the 23d, at a boarding-house on shore, called the Champion House. The same day a German and his wife, who kept the South Street Ferry-House to Red Bank, took sick; one died on the 26th, and the other on the 27th. The same day a young man named Koehler, who was daily on the dock, took sick, and died at the Champion House on the 27th. On the same day the mate of a bark lying just below where the Mandarin discharged her cargo was taken, and died on the 22d. On the 21st the mate of the Mandarin was taken, and died in hospital on the 23d. Three cases now occurred within the next week at the before-mentioned Champion House, and a large number of others in the immediate neighborhood. These cases all presented well-marked symptoms of yellow fever, and most of them had the black vomit.

The hygienic conditions of the vicinity of the docks were bad; a large sewer, emptying into the South-street dock, and there being foul wharves and dirty houses on shore. Dr. Wilson Jewell (from whose paper read before the College of Physicians August 5, 1853, I have obtained the foregoing details) says:

> No yellow fever existed in our city until six days after the arrival of the Mandarin; it broke out immediately abreast of the wharf where she first hauled to, and although there were existing causes in the vicinity—on shore—for the production of disease, there were "plague spots" in other parts of our city remote from South-street wharf, when, had the question been asked, we should have unhesitatingly located the first appearance of fever of a malignant type, independent of the suspected existence of a foreign focus of infection competent to exercise its morbific influence, on an atmosphere already tainted.

The disease prevailed more or less until the beginning of October, confining itself, however, almost entirely to the first infected locality. In all, there were one hundred and twenty-eight deaths.

In 1870 the brig Home arrived at the quarantine station, below Philadelphia, from Jamaica. She arrived on the 29th of June, having lost her captain with yellow fever on the 24th, and had the cook sick with the same disease. On arrival there was no sickness on board. She was an old vessel and excessively filthy, so much so that the crew, while loading her, could hardly endure the foul emanations of her hold; and planks had to be laid over the bottom of her hold, to keep her cargo of logwood from being drifted by the mass of filth and water which had accumulated there. Besides this, she was found to have on board a large quantity of old rags, which had been purchased in Jamaica by the captain; these circumstances justified the Health-officer in detaining the vessel at quarantine. The rags were burned, and stevedores and lighters sent down from Philadelphia to unload the brig, while the crew were removed to the Lazaretto, from which, however, the second mate deserted on the 30th of June; a passenger on board was allowed to go to Philadelphia.

On the 30th of June, the pilot, Stephen Bennet, left the brig for the Delaware break-water, but was taken sick at Wilmington, Delaware,

and returned to Philadelphia, where he died of yellow fever. July 2, a sailor, named Elliot, from the brig, was taken at the quarantine station, and recovered. On the 3d of July, the second mate, who had, as remarked, deserted from the quarantine, was taken sick in the city, and died on the 6th, of yellow fever. Soon after, a French sailor, named Pierre, was taken sick in the city, and threw up black vomit, but recovered. One of the stevedores was taken sick on the 17th of July, and two days later the wife of the captain of a lighter, engaged in discharging the vessel, his son, and a man employed on board, were reported sick with the fever. Subsequently to this, quite a number of cases occurred at the Lazaretto and in the vicinity. In all, there were twenty-nine cases, and eighteen deaths. (La Roche on Yellow Fever in 1870, pp. 20, 26.)

In 1822 there were an unusually large number of arrivals at the New York quarantine of vessels from the West Indies. No less than seventy-six vessels and over fifty cases of fever reported there before the 1st of July. Some of these ships that were loaded with sugar had their cargoes lightered up to the city, and between the 1st and 9th of July there were four cargoes thus landed at the foot of Rector street from vessels that had yellow fever on board. On the 10th of July, two children, daughters of a Mr. Reder, living on the corner of Washington and Rector, next to the river, and a young man named Thomas, clerk in a store on the opposite corner, were taken with yellow fever. This man Reder was a cooper, and had been engaged in repairing the vessels and boxes containing the landed sugar. Thomas died on the 16th, and one of the children on the 15th; both with black vomit. On the 15th, a brother of the girls was taken, and died on the 22d. The next case was a little girl by the name of Rose, who had been continually in company with the Reder girls; she also died. Four other cases soon occurred in Mr. Rose's house; from this time it spread up Rector street, along Washington and Franklin, and soon became generally epidemic. The disease ran its course until the 5th of November, causing two hundred and thirty deaths. (Griscom, op. cit., p. 19; Carpenter, Sketches of Yellow Fever, pp. 24, 25.) It should be borne in mind that Rector, Greenwich, and Washington streets were not then, as now, occupied by German lodging-houses, sailors' boarding-houses, and a low and vicious class of the population living in filth and misery. On the contrary, these were then the fashionable localities of the city; the houses, occupied by the wealthier classes, were roomy and cleanly, and contained nothing which could, by any possibility, be seized upon by the most ardent non-contagionist as evidence of the local origin of the outbreak. All observers coincide in attributing its appearance to the landing of cargoes of sugar from infected ships, and to that alone. (Hardin on Yellow Fever in New York in 1822; Townsend on Yellow Fever in New York in 1822.) This is the last visitation of the fever to New York City in epidemic form. It has, however, prevailed several times in the vicinity, presenting facts well worthy of note. In June, 1823, the ship Diana arrived at quarantine, New York Harbor, from New Orleans. She was detained thirty days, and then allowed to come to the wharf at Brooklyn to unload. A few days after her arrival, cases of yellow fever occurred in a family living near the wharf, who had communicated with the ship, and the disease spread generally throughout the village. An investigation showed that the Diana had cases of yellow fever on board, both before and after her arrival at Brooklyn. (Dr. P. S. Townsend's letter, quoted by Carpenter, op. cit.) The disease has prevailed in epidemic form on Staten Island three times, viz, in 1799, 1821, and 1848, and each time the first cases were directly traceable to infected ships lying at the quarantine

anchorage, or to the cargoes landed from them. (Report of Dr. Elisha Harris to New York quarantine commissioners, January 27, 1858.)

In 1856 occurred the well-known outbreak of the disease at Fort Hamilton, Long Island. The season had been characterized by an unusual number of arrivals of infected ships at the quarantine. "Forty-seven infected vessels, from twelve ports, from the 1st of April to the 1st of August, 1856, sent into the marine hospital fifty-eight cases of yellow-fever. Forty-seven vessels in 1856 reported having had in their ports of departure twenty-seven cases of yellow fever and sixteen deaths; * * * thirty-nine cases of fever on their passage to this port." (Report of Dr. R. H. Thompson, health-officer, August 4, 1868.)

The cargoes of a number of these ships were lightered from quarantine to the Atlantic dock, in Brooklyn. One of these vessels, the ship Jane H. Gliddon, had old rags for a portion of her cargo.

The first case of yellow fever, other than those who arrived in the infected ships, came to the marine hospital, Staten Island, from No. 12 Oak street, New York City. He was an Irish emigrant who arrived on the 3d of July in a vessel that had anchored over night in the midst of the infected fleet.

On the 14th two men were taken on Staten Island who had been employed in unloading the Jane H. Gliddon, and the next day a lighterman engaged in transporting cargoes to the Atlantic dock.

It made its appearance on Governor's Island on the 29th of July; the first case being that of a young woman, who had just returned from Fort Hamilton, and who lived in Rotten Row, in that part of the island nearest to the Atlantic docks, in Brooklyn. August 1, three other cases occurred in Rotten Row and the adjoining South Battery. "And in these two places the disease fixed its abode, as it were, almost exclusively, and continued until the number of cases amounted to sixty-three, fifteen of whom died." "With regard to its existence in this garrison, there is satisfactory evidence of its not having been generated on the island, but that its origin was in our vicinity, either on shipboard or on land, and that its causes, whatever they were, being thus eliminated in our neighborhood, were wafted to us by the wind." (Report of Assistant Surgeon L. Guild, United States Army, Army Medical Statistics, 1856, pp. 13, 17.)

Before these cases had happened, however, two men in the employ of a Mr. Bergen, on the Long Island shore, near Greenwood, gathered some straw which they found on the beach. This was on the 11th of July, and in a few days they both died with black vomit. Two men who were hired in their place and occupied their room soon sickened and died, and by the 27th of the month there had been fourteen cases in this vicinity, of which ten were fatal.

On the 1st of August the disease broke out at three different points in the vicinity of Fort Hamilton. Infected ships were anchored in Gravesend Bay, and the southwest wind, which prevailed, blew directly over these vessels toward the shore. Besides this, matresses and bedding were thrown overboard and drifted up along the beach. There was no reason to doubt that this was the source of infection; the hygienic surroundings of the locality being excellent. Cases occurred during the season from Bergen Point, and several were sent to the marine hospital from Brooklyn. In all, there were five hundred and thirty-eight cases, and one-third died. It was a curious fact connected with this epidemic that, although it extended for four miles along the shore, at no time did it extend more than three hundred yards from the shore-road. (Report of Dr. Elisha Harris, quoted by Griscom, op. cit.) Dr.

Harris, in a letter to Dr. Griscom, dated July 13, 1857, reports a striking case of the importation of the fever in the person of a sailor who was employed as ship-keeper on the bark Lucy Heywood, just arrived from the West Indies with yellow fever on board. This man had just arrived from Bangor, Maine; and, after sleeping on board for seven or eight nights, was seized with symptoms of yellow fever, and died of black vomit in a few days. Harris says, "There could have been no other source for the origin of the fever in that man, who had just arrived from Maine, than such infection as inhered in the cargo and cavities of the Lucy Heywood."

In September, 1868, ten cases occurred at Fort McHenry, Baltimore, Maryland, concerning which Surgeon Charles McDougall, U. S. A., remarks, "We believe the yellow fever at Fort McHenry came from vessels infected with the disease, at quarantine, near by." (Army Medical Statistics, 1856, p. 85.)

In August, 1870, an epidemic disease broke out on Governor's Island, New York Harbor, in the same part of the island which had suffered in 1856. The disease was not at first recognized, but in the latter part of September, when it had become generally prevalent, it was pronounced to be yellow fever by experienced physicians, and the sick were removed October 1 to the West Bank Hospital, at quarantine. This did not, however, stop the course of the epidemic, which continued until October 26, when the last case occurred. The disease was confined to the southeast portion of the island, and was undoubtedly due to the arrival at the Atlantic and Empire docks, at Brooklyn, of vessels which, being infected, had evaded the New York quarantine by entering at Perth Amboy, New Jersey, and coming thence to Brooklyn to discharge. The tide in the channel, between the island and Brooklyn, is such that articles thrown overboard from these ships would readily be carried to the shores of the island. The people of Brooklyn did not become infected, because no susceptible person was permitted near these vessels, they being unloaded by their own crews. There was a number of cases which occurred in New York and Brooklyn of persons who had visited their friends on the island, but they did not communicate the disease. The population of the island was seven hundred and twenty-two, of whom one hundred and fifty-seven had the disease, and forty-nine died. (Report of Surgeon Charles Page, United States Army, December, 1870; personal information from Assistant Surgeon G. M. Sternberg, United States Army; report of Dr. Carnochan to quarantine commissioners, October 4, 1870.) This completes what I have thought it advisable to note in reference to epidemics occurring in towns situated farther north than the extent of coast over which I was required to make my inspections.

I proceed now to give a detailed account of the facts, as far as they have been accessible to me, of the various outbreaks of yellow fever in the southern ports and towns, commencing at Norfolk, Virginia.

Yellow fever first made its appearance in Norfolk in 1795. In the succeeding years there were a few cases, until 1800, when there was a most severe epidemic. During the summer, the weather had been unusually hot, with very frequent rains, and the sanitary condition of the city was very bad, as we learn from the report of Doctors Selden and Whitehead. (Medical Repository, vol. 4, pp. 329, 337.) About the middle of July, several vessels laden with fruit arrived from the West Indies. This fruit had rotted on the passage, and was left in a putrefying condition for many days on the wharves. The vessels themselves, as might be expected, were foul almost beyond belief. On

the 26th of July, two fatal cases occurred, of persons who lived in the immediate vicinity of the wharf, where this fruit was exposed, and who had been employed in picking it over and repacking it. A previous case had occurred, in a sailor by the name of Carter, on July 22, also fatal. Dr. Schoolfield, in his account of epidemics in Norfolk and Portsmouth, (Report of Portsmouth Relief Association, 1855, p. 98,) says, quoting from Doctors Selden and Whitehead, that others had died a few days before the 26th; but, though he argues from this fact that the epidemic had a local origin, and was not due to importation by the fruit-vessels, yet he gives us no particulars, and no dates, to substantiate his views, except in the case of Carter, who died on the 22d, after the arrival of one of the first vessels. And as other cases soon occurred, all in this immediate vicinity, it is certainly more rational to ascribe its origin to this cause than, with the imperfect information we possess, to insist on its local origin. The disease soon became epidemic, and raged until the 30th of October, when it ceased; having carried off about two hundred and fifty persons, among whom were a large number of negroes.

The fever prevailed again in 1801, 1803, and 1805, but there was no considerable epidemic until 1821. In that year, the schooner George Armistead arrived on the 20th of July from Guadaloupe; yellow fever prevailing at her port of departure when she left. She had several cases on board during her voyage, and two men died on the day of her arrival. She came immediately to dock, and, having discharged her cargo, was transferred to another wharf, where they pumped out her bilge-water, which was "so putrid and offensive as to render it expedient that the doors and windows of a neighboring house should be closed, in order to exclude the effluvia arising from it." On the 1st of August, a clerk in a warehouse on the dock, but a few yards from the vessel, was taken ill, with decided symptoms of fever, and died. The same day, a negro woman living in the house above mentioned took sick, and on the 4th there were two other cases in this house, and a few days subsequently two more. Two persons who had assisted in pumping out the bilge-water were next taken, and both died; and from this time the disease became epidemic. (Committee's Report on Origin of Yellow Fever in Norfolk, 1855.) Dr. Robert Archer, who wrote a history of this epidemic, (Medical Recorder, vol. 5, pp. 60–73,) and who was ignorant of the fact that yellow fever had prevailed on the vessel during her passage, or that two men had died of the disease the day of her arrival, ascribes the origin of the disease to the foulness of the vessel, and says, "It is evident that the effluvia arising from the bilge-water was the immediate cause of the disease in every instance that has been recorded; for every individual had been exposed to its influence, and almost every one who had come within its atmosphere was more or less affected by it."

In 1826 the disease prevailed again, this time being traceable to the unloading of a cargo of damaged coffee, and subsequent cleansing of the vessel, at one of the public wharves. (Committee's Report, p. 14.) From this time until 1852, Norfolk was not again visited by the fever. Long immunity from the disease had rendered the authorities very careless in regard to quarantine regulations; and on the 20th of July, a Spanish bark, the Tascio, from Havana, arrived, and, without being subjected to any health-inspection, discharged her cargo and pumped out her bilge-water at one of the wharves. She had two men sick on her arrival, whose cases were pronounced by a homeopathic doctor—an uneducated man—to be typhoid fever. On the 7th of August, a death

from yellow fever occurred in a row of tenement-houses about a hundred yards from the vessel, there being no intervening houses. Several other cases were soon heard of in this row, and before long it became decidedly epidemic in the lower part of the town; but the weather was unusually cool, and the cases were of a mild character. In this year, for the first time, the disease made its appearance at Portsmouth, several deaths happening at the marine hospital and in the vicinity. (Committee's Report on Yellow Fever at Norfolk, 1855, p. 16.)

In 1854 the French frigate Chimère arrived at Norfolk with the fever on board; fifty-four of her crew were removed to the naval hospital, and the vessel placed in quarantine. While at her anchorage, a large number of cases of spoiled provisions were thrown overboard, and, drifting on shore, were collected by people living in the vicinity, and taken to their houses. In one house, about a mile and a half from Portsmouth on the river-shore, a lady and her son, who had been occupied with cleaning these jars, sickened, and both died with undoubted yellow fever. Soon after this, some cotton floated ashore from two New Orleans vessels anchored in stream, and being gathered up, was spread out to dry at a house not far from the other. In a few days four cases of yellow fever occurred at this house. During this season there were three fatal cases in Norfolk, but in neither that city nor Portsmouth did the disease become epidemic, and I merely mention them to show the unquestioned transmission of the disease by the jars and cotton, or if not transmission, then certainly a very remarkable coincidence. (Schoolfield, Portsmouth Relief Association Report, 1855, pp. 93, 94.) In the next year, the great epidemic, which attracted such general attention over the whole country, took place; and I shall go somewhat into detail in describing the commencement of this epidemic, as the facts have the most direct bearing on the general subject upon which I am called upon to report. Early in the season, March 19, 1855, the United States frigate Columbia arrived at quarantine from Saint Thomas, West India, with fever on board. Sixty-three cases were transferred to the naval hospital; of these five died, and there seems to have been no spread of the disease from this source.

The earlier months of this year presented no remarkable meteorological phenomena, and up to June the condition of the population as regards health was unusually good. June was very wet, and rather hotter than the average, while the remaining months of summer were very hot. The condition of the streets was comparatively good, and there were no local causes likely to bring on a pestilence which had not existed for a series of years. "On the 6th of June, the steamer Ben Franklin, Captain Byram, arrived, in distress, from St. Thomas." As yellow fever was prevailing at that island, she was quarantined by the Health-officer, having previously sent her passengers by the Baltimore boat, in the lower bay. She had lost two men on the passage, neither of them from yellow fever, as the captain positively asserted. The vessel remained at quarantine until the 19th of June, but the crew were allowed to come up to the city. The captain of the steamer admitted to the health-officer that he had two deaths on the passage, but denied that they were from fever; but subsequent testimony, obtained by the Norfolk committee, conclusively proved, not only that these were cases of yellow fever, but that there had also been a third, which recovered; and that, while in quarantine, another man died with undoubted yellow fever, who was secretly buried in the woods on the neighboring shore. On the 19th of June, the captain having pledged his word to the health-officer that the hold should not be broken into or the bilge-water

pumped out, and again positively certifying to the health of all on board, the vessel was permitted to come to Page and Allen's ship-yard, at Gosport, near Portsmouth, for repairs. And yet, at this very time, he had one case of yellow fever; for, on the 21st, a man named Palmer was removed from the vessel to the marine hospital, and died of black vomit on the 22d, having been sick since the 15th. The sanitary condition of the ship-yard and the neighboring localities was very bad. The wharf was rotten, the mud exposed at low tide, and the ground covered with rotting vegetable *débris* several inches deep. In the immediate vicinity was a row of tenements occupied by a large number of Irish, whose habits were filthy to an extreme. Nearly all the workmen in the ship-yard were unacclimated New Englanders.

After the arrival of the steamer at the yard, the captain deliberately violated the promise given but a day or two before. The bilge-water was pumped out, a portion of cargo and ballast transferred to the dock, and a large number of workmen employed in the hold and about the machinery.

On the 30th of June three cases of fever occurred in a house on Water street, fronting the ship-yard; and on the 3d of July a man by the name of Jones, who had been employed for a week on the steamer, was removed to the hospital, where he died of black vomit on the 17th. A day or two later another man, Carter or Courtright by name, also employed on the steamer, was taken sick and died on the 8th. The authorities now became alarmed, and ordered the vessel back to quarantine, where several more cases occurred, among them two negroes from the country, who passed a night on the vessel, and were subsequently attacked at their homes, neither of them having been in Norfolk or Portsmouth at all.

On the 8th of July the fever broke out in the low Irish row before spoken of, and from thence spread rapidly through Gosport, reaching Portsmouth about the 1st of August, where it speedily became epidemic. It should be mentioned that Doctor Schoolfield reports one case as having been seen by him on the 24th of June, in a house on the river-shore, about a mile and a half from Portsmouth. This was less than a mile from where the Franklin lay at anchorage at quarantine, being in the same house where some cases were seen in 1854, (above noticed as having derived the infection from the French frigate Chimère.) It is more than probable that this case received the disease in the same way, though Doctor Schoolfield thinks it positive evidence of the local origin of the epidemic. In the mean time there had been unrestricted communication between Norfolk and Portsmouth by means of a steam ferry-boat, which landed close to Barry's Row. The first cases in Norfolk were about the middle of July, in Barry's Row, (Irish tenement-houses on Church street, in the usual filthy and neglected condition of such places, and built on ground reclaimed from the river by filling in within twenty years.) Here the disease found a congenial habitation, and a large number of cases occurred. The authorities barricaded the streets and destroyed the obnoxious houses, but this did not prevent the epidemic from spreading in every direction until the whole city was involved. From the last of August until the middle of September was the period of its greatest intensity, after which it declined, finally terminating in the last week in October, in Norfolk, and somewhat earlier than this in Portsmouth. (Williman on Yellow Fever in Norfolk, Charleston Medical Journal, vol. 11, p. 169.) The population of Norfolk at this time was about 15,000, and of Portsmouth about 10,000. Of these, very large numbers fled to escape the disease.

Of those who remained there died, in Norfolk, 1,807, out of an estimated number of 10,000 cases, and in Portsmouth about 1,000 out of 4,000, who remained and took the disease.

The disease has not appeared at Norfolk since that time, though it has twice certainly, and perhaps oftener, made its appearance on vessels at quarantine. In 1869 a French frigate arrived in Hampton Roads with the captain and two surgeons sick, both of whom died. She was anchored off Sewall's Point and closely watched by the military authorities at Fort Monroe, and no importation of the disease took place. In May, 1871, a three-masted schooner arrived from Charleston with the captain, first officer, and two sailors sick, all of whom died. She was anchored at the Willoughby Spit light-vessel, and the dead buried on the shore above Fort Monroe, and there were no cases on shore. (Personal information furnished by Colonel W. F. Barry, United States Army.)

Wilmington, North Carolina.—There was a slight epidemic in 1796, but I have not been able to obtain any particulars in reference to it; but Dr. A. J. De Rossett, a venerable practitioner of that city, who witnessed it, was of opinion that it was entirely due to local causes, the disease being but an aggravated type of the common autumnal bilious remittent. (Medical Repository, vol. 2, pp. 153, 155.) From this time the city was free from the disease, except, perhaps, a few sporadic cases (of very doubtful authority) in 1819 and 1820. In 1821 occurred an epidemic of a very severe character. Wilmington at that time was a small town of from twenty-five hundred to three thousand inhabitants. Its sanitary condition is described by an eye-witness to have been the worst possible, and the fever was regarded by Dr. Nathaniel Hill (Medical Recorder, vol. 1, p. 86) to have arisen from the great amount of decaying vegetable matter about the wharves, which was daily washed by the tides, leaving it exposed to the sun at other times; and also to the low, damp, and filthy cellars, which were so wet as to require to be baled out. These foci of disease had, however, existed many years without producing an epidemic, while, August 9, 1821, the brig John London arrived from Matanzas, Cuba, West Indies, having had cases of the fever on board, and the first cases on shore were in the immediate vicinity of the wharves above spoken of, and which would be most likely to be the portion of the town where the sailors from the newly-arrived vessel would congregate. At this late day we are without any positive *data* to connect the arrival of this vessel with the fever, except the statement of old residents, that those first attacked had been on board; but the facts that the epidemic soon followed her arrival, and that the local causes that have been claimed as causing it existed equally in previous years, will not, I believe, be disputed by any. In 1854 nine deaths occurred as a direct result of importation from Charleston, but, it being late in the fall, the disease did not become epidemic.

After this Wilmington enjoyed an exemption from epidemics until 1862.

Her people escaped the great series of epidemics that extended from New Orleans to New York, 1853-'56. In 1862 the most severe visitation the town has ever known took place. The city at the time was in a favorable condition for the development or spread of the disease. The larger portion of the male population had been taken away to enter the rebel service, and there was, consequently, a very general neglect of the most ordinary hygienic precautions; the authorities being so anxious to repel the enemy from their front by the erection of great lines of works that they overlooked the necessity of guarding against

the more insidious foe that was soon to cause a greater mortality and more distress and woe than an invading army. The stringency of the blockade had caused a general want of all the comforts and many of the necessaries of life; and there was much destitution and suffering among the poorer classes. Dr. W. T. Wragg, of Charleston, thus describes the condition of the city during the height of the epidemic:

> On entering the city at the railroad depot I noticed the disagreeable odors which filled the air, and these became more evident as I advanced into the town. The hotels were all closed, and when admission into one of them was obtained I was struck with the same want of care as had been noticed in the street. When daylight appeared, the want of cleanliness displayed, both in the house and on the premises, accounted for the foul air around. In the yard were seen piles of offal, consisting of old bedding, rags, scraps of vegetables and, in short, the refuse matter of all sorts usually about the habitations of man, when the demands of cleanliness and care have been lost sight of. * * * Along some of the principal streets I saw the cellars flooded with water. In the yards I was called into I saw on every side the accumulated refuse of the house, the kitchen, and the stable. * * * The heavy rains of the late summer and early fall had created a state of things well calculated, it seems to me, to generate disease. Through many parts of the town, in those (suburban) portions, ponds of water, stagnant, and covered with green vegetation, existed. Some of these ponds, I was told, had been drained in former years because they occasioned sickness, and had always been kept carefully dry till this season, when from scarcity of labor they had been allowed to refill by the choking up of their vents. In many places new ponds were formed by the unusual quantity of rain.

This statement refers to the condition of affairs toward the close of the epidemic; but Dr. W. G. Thomas, of Wilmington, shows (New York Journal of Medicine, December, 1869) that at the outbreak of the epidemic no such state of neglect existed; that while the sanitary condition of the city had been neglected for years to some extent, still the vast accumulations of offal and vegetable *débris*, of which Dr. Wragg speaks, were the result of entire neglect of cleanliness from the consternation of the people *after the epedemic commenced* and not before.

There must, therefore, have been some other cause for the appearance of the disease than either atmospheric disturbances or neglect of sanitary measures. After a careful consideration of the very able articles on both sides, above mentioned, as well as of information kindly furnished me by Dr. E. A. Anderson, of Wilmington, I am inclined to the opinion that the following is the correct solution of the question:

During the summer, yellow fever prevailed extensively at Nassau. On the 2d of August the blockade-runner Kate left that city, arriving at Wilmington on the 6th. She had one case of fever on board on her arrival, who died on the 8th, at the marine hospital. Six deaths occurred in a few weeks, all of them of persons who had been patients in the same ward where this man died. In the mean time a man by the name of Smith, who had spent the day on the Kate, on her first arrival, died on the 13th of August with black vomit. On the 17th of August a seaman died in a house on shore, and within a few weeks a large number of cases and several deaths took place in this house and in the neighborhood.

On the 3d of September a man by the name of Swartzman died, having been frequently on the Kate. So with another, by the name of Crapen, who died on the 19th, his wife having previously died on the 10th; and with Hyer, who died on the 13th, having been constantly about the vessel. From this time cases rapidly multiplied until it was pronounced epidemic, some time between the 14th and 24th of September.

It is proper to state that Dr. E. A. Anderson, a gentleman of the highest professional standing, asserts positively that he had cases of yellow fever in his practice previous to the arrival of the Kate, and Dr. Wragg narrates eight cases said to have occurred previous to that

time; but as Dr. Thomas shows an error in dates in a number of these, and that others were under the care of an irregular practitioner, (who would be very apt to make a mistake in diagnosis,) it may be assumed that the weight of testimony is in favor of its introduction by the Kate. Even Dr. Wragg states:

> In my first interviews with the resident physicians, I learned that their unanimous opinion was that yellow fever had been introduced into Wilmington by the steamer Kate, from Nassau, New Providence, which port she left on the 2d August, arriving at the wharf in this city on the 6th at 2 o'clock p. m. It was stated that, at the time of her arrival, she had a sick man on board, and that others of her crew were taken sick soon after, and, being carried to boarding-houses, spread the fever among those who nursed them or were near them. It was also stated that among the earlier cases occurring amid the resident population were those persons who had the most direct intercourse with the vessel for trading or other purposes.

I may add that in my late visit to Wilmington I conversed with many leading gentlemen, professional and otherwise, all of whom, with the exception of Dr. E. A. Anderson, concurred in this view.

As stated before, the disease became epidemic about the 20th of September. It attained its acme about the middle of October, and then gradually declined, until the third week in November, when it disappeared. Wilmington had at this time about five thousand inhabitants; of these fifteen hundred and seven had the fever, and four hundred and forty-six died—a percentage of mortality greater than that of almost any other epidemic. (Report of Acting Assistant Surgeon D. W. Hand, March 15, 1866; Dr. W. T. Wragg, Yellow Fever in Wilmington in 1862, New York Medical Journal, August, 1869; Dr. W. G. Thomas, Review of Dr. Wragg's Report, New York Medical Journal, 1869; Dr. E. A. Anderson's Examination of Dr. Thomas's Review, New York Medical Journal, September, 1872.) From that time to the present, Wilmington has been free from the disease in an epidemic form, though sporadic cases have been seen several years in the practice of leading physicians.

The disease, however, failing to spread, no especial pains were taken to ascertain its origin, and this, I may remark in this place, will be found to be the case in almost every town coming under my observation. Sporadic cases occur nearly every year in almost all the southern cities, and are assumed to be of local origin, and I am far from denying that they may be and often are so; but still, granting this, it is quite probable that a careful examination of these cases (such as always takes place at the time of a great epidemic) would develop some communication, in a proportion, if not a majority of the cases, with extramural sources of infection.

At Smithville, North Carolina, at the mouth of the Cape Fear River, a number of cases occurred. These are asserted to have originated as follows: A man by the name of Brown, belonging in Smithville, died in Nassau, New Providence, of yellow fever, and his trunk was sent home to his wife. It remained unopened for about two months, when it was opened, and the clothes spread out on the piazza to air. The widow who unpacked the trunk took the disease, and there were other cases in the family and neighboring houses. But, assuming that this story is not entirely to be depended upon, yet, as Dr. Anderson states, there being daily communication between the infected city of Wilmington and Smithville during the summer and fall, it is probably not necessary to go to an infected trunk to account for the appearance of the disease in the latter place.

It should be added that since the close of the war, Wilmington has changed vastly in her sanitary condition; an excellent health ordinance, improved drainage and sewerage, and a more general attention to

hygienic laws, have greatly increased the general health of the city, and will probably do much to prevent a return of yellow fever in epidemic form.

Coast of North Carolina, 1864.—At New Berne there was a most violent epidemic. The first cases were taken sick early in September, and the epidemic lasted until the first week in November. New Berne was very much crowded with soldiers, civilians in army employ, and refugees, and the percentage of mortality was very large; out of about three thousand cases there were nearly seven hundred deaths. No vessels had arrived at the port during the season from any place from which they could have brought the disease; and nothing was found in the history of the first cases to sustain the idea of extramural infection. It was, indeed, considered probable that some of the infected goods, introduced within our lines by Dr. Blackburn and G. J. Hyams, might have been conveyed to New Berne; but Dr. Hand, who carefully investigated this matter, found no evidence to warrant such a supposition. (Official report, April 1, 1866.) This being the case, the medical officers on duty felt bound in duty to discover some cause for so unusual an outbreak in a town which had been exempt for fifty years, and instead of one they found a dozen, any one of which they considered amply sufficient to account for the phenomenon. Dr. Hand says the streets were well and thoroughly policed, but the back yards were dirty, yet "the general sanitary condition of New Berne, when the fever broke out, was better than that of most southern cities." Unusually low tides in the Neuse and Trent Rivers, which left exposed a strip of mud a hundred yards broad, was one cause assigned; the presence of filth and dead rats underneath old buildings, which had been in the same condition for twenty years; the presence of an unusual quantity of mud in the streets, &c. Privies, that from the statement of the writers had been uncleaned for a quarter of a century, suddenly became endowed with powerful septic capacity, and nursed in their vaults the germs of the disease. A large quantity of stable-manure had been thrown into the docks, though whether any had been brought from other places for this purpose, or whether any more was thrown there than ordinarily existed in such a town, is not stated. One writer ascribes it to the felling of forests on the southwestern outskirts of the city, so that the sun, acting on the soil, developed malarial influences which penetrated the city; and another, with equal gravity, ascertained that the trees in New Berne had not been trimmed for several years, and that consequently the foliage had grown so dense that the rays of the sun could not penetrate to dry up the mud. All concurred in ascribing to it a local origin and nothing but that. (Official report of Colonel Peter Pineo, medical inspector, United States Army, December 31, 1864; Surgeon C. A. Cowgill, United States volunteers, November, 1864; Surgeon D. W. Hand, United States volunteers, April 28, 1865, April 1, 1866; and letter of Dr. J. M. Davis, Ninth New Jersey Volunteers, June 24, 1865.) That some of these numerous and somewhat conflicting causes may have had an effect as affording resting-places for the disease, no one will question; but Assistant Surgeon J. J. Woodward, United States Army, whose experience in studying the medical history of the war entitles his opinion to the highest consideration, thinks that a more thorough investigation would have shown that the disease was introduced by means of the numerous refugees who were daily coming into New Berne in great numbers, and some of whom doubtless came from Charleston, at which place yellow fever was prevailing. This seems to me much the more rational view. Certainly it is

more in accordance with what we see of the history of yellow fever in other places.

The disease was carried from New Berne to Beaufort, North Carolina, where it broke out on the 24th of September. The first cases were of persons who had been in New Berne within a few days of their seizure; the next, persons who lived in adjoining houses. The epidemic lasted until November 17, causing sixty-eight deaths. (Official report of Surgeon U. P. Rice, United States volunteers, November 18, 1864.)

Charleston, South Carolina.—The yellow fever made its appearance at Charleston at a very early period in the history of that city, and has since that time visited it more frequently than any other place in the country. The following abstract of the epidemics I copy from the valuable census of Charleston by Doctors Dawson and DeSaussure:

Yellow fever first prevailed as an epidemic in this city in 1699 or 1700; the exact date cannot now be ascertained. According to the account given of it by Dr. Hewitt, it prevailed in that year very extensively, carried off a large number of the inhabitants, including many of the chief men of the province, and was called the plague. The same disease recurred in 1713, and caused as fearful a mortality as at its previous visitation. From this period, no mention is made of its occurrence until 1728, when it again recurred, and was then first called "yellow fever;" although from the description given of two former epidemics, there can be no doubt of the identity of the last with the two preceding. In this year also it caused a great number of deaths. In the year 1732 it commenced its ravages in May, and continued until September or October; during the height of the epidemic from eight to twelve whites were buried daily. It appeared again in 1739, 1745, and 1748, and is said to have raged as severely in these years as in 1732. The number of deaths which it caused in those years cannot now be ascertained. From 1748 to 1792 no epidemics of yellow fever occurred, although it is stated that in 1753 and 1755 a few sporadic cases were seen. In 1792 a new series of epidemics occurred; it raged in that year, and in 1794, 1795, 1796, 1797, 1799, 1800, 1801, 1802, 1804, 1807. * * * From 1807 to 1817, a period of ten years, there was nearly a complete exemption from epidemics of this disease. In this latter year it re-appeared and caused two hundred and seventy deaths, a large number of whom were children. In 1819 it again prevailed epidemically, causing one hundred and seventy-six deaths. In the next year it prevailed slightly, but did not become epidemic. In 1824 another serious epidemic occurred, which carried off two hundred and thirty-one persons. From this period, 1824, to 1838, no serious visitation of yellow fever occurred, although several cases of it existed in each of the years 1827, 1828, 1834, and 1835. In 1838 the most serious epidemic occurred which has been known in the annals of Charleston, and causing the largest mortality which has ever resulted from the disease in this city. In 1839 it was again epidemic, but to a much less extent than during the former year. From 1839 to the present time, (1848,) nine years have elapsed, during which the city has been exempt.

In 1849 it again made its appearance, but in a very mild form, and has since that time prevailed with varying severity in the following years, 1852, 1854, 1856, 1858, 1864, and 1871. Besides this, there have been unquestioned sporadic cases during other years, but no approach to an epidemic.

The local hygienic conditions of Charleston have always had a most important bearing upon the spread of the disease, and, as believed by many of her ablest practitioners, upon its origin. (See Simons on Yellow Fever in Charleston, Charleston Medical Journal, November, 1851, p. 792.) The city, situated at the junction of the Ashley and Cooper Rivers with the bay, is on very low ground; the average fall in drainage being not more than five feet. In early times, and to some extent still, there were numerous salt-marshes extending from the rivers toward the town and, to a considerable extent, within the limits of the city. These were washed twice a day by the tides, and as long as they were left alone did not seem to be deleterious to the general health; and, previous to their being filled in, the yellow fever was confined to what is known as Old Charleston, in the southeastern part of the city. Gradually, however, as land became more valuable by the natural extension

of the town, many persons owning lots on these salt-marshes filled them in, thus destroying the natural drainage in the vicinage. The material used for this purpose was often of a highly injurious character, consisting of old wood, offal, the detritus from the drains, and filthy dirt-heaps, thus making a vegetable compost of highly putrefying capacity, to be exposed to the hot sun. In every case where this has been done, yellow fever has extended its area over the newly-filled localities, until now there is hardly any portion of the city that can be considered exempt. Full details of the subject will be found in a very interesting letter of Dr. W. T. Wragg, (Report to Common Council on Epidemic of 1858, pp. 36–45.)

For the last seventy years the condition of the drains, privy-vaults, latrines, sewers, and yards around the houses has been such as to call for the severest comments from all that have interested themselves in the health of the city. Offal and garbage, and the detritus of cow-yards, have been openly placed in the streets; the drains have not unfrequently been opened in summer to be cleaned, and their contents strewed over the surface to fill up vacant lots. "Another and a greater evil, alike favorable to the formation of an unhealthy state of the atmosphere, are houses crowded, or rather packed, from basement to attic, with human beings; and the yards or lots of small dimensions, equally crowded, with horses, cows, goats, and, in some instances, hogs and dogs. The filth of all these animals, biped and quadruped, is frequently, in one mass, placed in the street, for the scavenger's cart to remove." (Report to Common Council on Epidemic of 1858, p. 19.) Still another source of disease is the large number of burial-places in the most thickly settled portions of the city. These are very much crowded, and often in the heat of summer exhale the most offensive odors. This was the state of affairs in 1858; and Dr. Robert Lebby's report to the mayor in 1872, as well as my own conversations with many physicians in the city, and personal observations during the past summer, all show that the condition of affairs has not materially improved since; perhaps, even for a year or two after the close of the war, it was worse than it had ever been, owing to the disorganized condition of public affairs caused by the war.

We are without detailed accounts of the earlier epidemics. Dr. John Lining says, (letter to Dr. Robert Whytt, of Edinburgh, December 14, 1753:)

Within these twenty-five years it has only been four times epidemical in this town, namely, in the autumns of the years 1732, 1739, 1745, and 1748, * * * and lastly, whenever the disease appeared here, it was easily traced to some person who had lately arrived from the West Indian Islands, where it was epidemical.

During this period the commerce of Charleston with the West Indies had attained considerable proportions, and there were many arrivals at the port from the West Indies in every year. Toward the last of this period this trade had been gradually growing less, in consequence of the unjust restrictions placed on the trade of the colonies, by Parliament, in the various navigation acts, and other measures, tending toward a commercial monopoly. By 1750 the trade had almost entirely ceased; and it was not revived until the independence of the United States was secured, and affairs settled down once more on a quiet basis. By a singular coincidence, yellow fever disappears from the annals of Charleston during the whole of this long period, and only makes its appearance again in 1793, when the trade had again become large with the West India ports. (Carpenter on Yellow Fever, p. 45.)

A reference to the preceding quotation from DeSaussure and Daw-

son will show that it was epidemic nearly every year after this until 1808, when it again disappeared from the city until 1817. Dr. Carpenter (History of Yellow Fever, pp. 45,46) shows that this period of exemption was exactly coincident with the suppression of traffic with the British West India Islands, consequent on the embargo act and the subsequent war with Great Britain, and that it was not until that traffic had been revived by the peace of 1815 that yellow fever again visited Charleston. The first cases this year (1817) occurred among sailors in the lower portion of the city, and from them it spread over the greater portion of Old Charleston. During the continuance of this epidemic a vessel left Charleston for Beaufort, South Carolina. The authorities of that place having no belief in the contagious or infectious character of the disease, admitted her to the wharf, without quarantine examination. "Two days after, one of her passengers complained of sickness, and was placed under the charge of a colored nurse. The fever was soon characterized; the patient died. The nurse was the next taken; she died; then some of her family, and from this point the fever extended over Beaufort, and destroyed one-sixth of the inhabitants." (Hume, "Report to City Council of Charleston on Origin and Source of Yellow Fever," Charleston Medical Journal, March, 1854, p. 154.)

Dr. S. H. Dickson says of the epidemic of 1819, (Charleston Medical Journal, November, 1856, p. 744.) that it was the most terrible year of relative mortality I have known," and of that of 1827, that it was "the least fatal in proportion." During subsequent years, there was no serious visitation of the disease, until 1838, when it caused three hundred and fifty-three deaths, two hundred and eighty-one of which were adult males. On the 4th of July of that year, the British bark Lord Glenelg arrived from Demerara, and lay at Boyce's wharf, having at the time several cases of yellow fever on board. At this time, the ship Medora, from Liverpool, lay at another wharf, about one hundred yards distant. The first case of fever among the citizens was the wife of the steward of the Medora, who had frequently been on board to see her husband. This case occurred on the 1st of August. The next case was a sailor from the Medora, who was taken to the hospital on the 4th of August. Both of these cases died, and the disease soon spread to other houses, and became epidemic. (Hume, op. cit., p. 154; Strobel, Essay on the Transmissibility of Yellow Fever, p. 124.) It is not known that there was any communication between the Lord Glenelg and the Medora; indeed, Dr. Thomas T. Simons denies that there was at any time any yellow fever on the Lord Glenelg, and finds a sufficient cause for the fever in the great fire which swept over the city in the spring of that year, and which left a great mass of filth, decaying and putrifying material, old rags, all kinds of offal, of every description, exposed to the solar rays. To these causes, he thinks, must be added the influence of a large number of unclimated mechanics from the North, who came to Charleston to rebuild the burned district, and who lived in crowded boarding-houses, under unfavorable hygienic conditions. (Simons's Reply to Wm. Hume, M. D., before the city council, Charleston Medical Journal, May, 1854.)

The following year, 1839, the disease again prevailed epidemically. During May, June, and July a large number of vessels arrived at the city from the West Indies, laden with fruit, much of which was rotten, and all the vessels very filthy. Strobel says:

> From the 1st of May to the 30th of July, inclusive, there were no less than thirty-six arrivals from infected ports in the West Indies. Now, we consider each of these vessels a source of infection, for they arrived in our harbor with the epidemic atmosphere of the places from which they came hermetically sealed up in their holds; not only so, but many of them were laden with fruit, vast quantities of which had rotted in their holds prior to their arrival, the effluvia from which must have added greatly to the virulence of the poisonous atmosphere with which they are freighted. (Op. cit., pp. 171, 172.)

But the vessel to which the first cases were clearly traced was the ship Burmah, which arrived on the 7th of June from Havana, having recently had two deaths among her crew from yellow fever, and having three sick on her arrival, who were immediately sent to the hospital. The Burmah lay for some days in stream, in the vicinity of a number of other vessels that were either in stream or at neighboring wharves. On the 17th of June five men were admitted to hospital from some of these ships, three being from the Leonore, from Boston; one from the Chatham, from Boston; and one from the Elizabeth Bruce, from New York. Cases were soon admitted from other vessels, until they became so numerous that there was scarcely a vessel uninfected. The first case in the city was on the 26th of June, in the person of a man named Humphreys, who arrived on the 19th of June from Havana, and was boarding on Market street. On the 7th of July a case occurred a few doors west of this boarding-house, and soon numbers of other cases occurred, and the fever was pronounced epidemic. The Medical Society of Charleston appointed a committee to investigate the origin of this epidemic, and they came to the conclusion that there was no warrant for supposing its introduction to be due to the Burmah, but that there were "local and general causes" within the city itself amply sufficient to give rise to it. Simons says that "many of the lots which had been exposed by the fire of 1838 remained unoccupied, and continued to present the same surface to the deleterious influences of the sun and rain, and many strangers, also, who remained in the city, engaged in the occupation of rebuilding." (Op. cit.) He also says, in another essay, that Dr. Robert Lebby, post-surgeon at Fort Johnson, had cases under his charge previous to the arrival of the Burmah. This epidemic lasted until some time in October, and caused one hundred and fifty-three deaths.

It prevailed next on Charleston Neck, in 1843, but did not become epidemic in the city. Hume traced the infection in this case to the arrival of a man by the name of Hewes, from Havana, sick with fever; but as Hewes arrived on the 27th of July, and the first cases thereafter were not until the 10th of September, the time seems too long to trace any connection between them.

The city was now free from disease until 1849, when again the fever became epidemic. The history of its introduction seems to be as follows, though, as usual, it gave rise to the most violent controversy between the advocates of local origin and the importationists. As this is found to be the case with every epidemic in Charleston, I merely present the facts, and will let inferences be drawn by others: The first case was taken to the hospital on the 6th of August, being a sailor from the bark Numa, then lying at the Union wharves, having arrived from Havana on the 18th of July. This case proved fatal the night of his admission; and the next day another case made its appearance from the steamer Isabel, which arrived from Havana on the 25th of July, and was at the same dock as the Numa. On the 20th of August the captain of the ship Huron, lying at the next wharf below the other vessels, was taken with yellow fever. On the 28th the captain of the British ship Queen Victoria, lying next above the infected ships, was taken sick and died of black vomit; and in a few days five of the crew of this last ship were taken. Cases began to occur in the houses in the

vicinity of the wharves by the 3d of September. Shortly before this a man by the name of Holman arrived by steamer from Savannah and died on the 29th of August at the poor-house. On the 2d of September a German, who had been on the infected ships to buy cigars, died of the fever on the corner of East Bay and Elliot streets. Other cases soon occurred in the vicinity, and by the middle of September it was decidedly epidemic. In October it was very severe, commenced to decline in the early part of November, and by December 1 had disappeared. In all there were one hundred and twenty-three deaths; but two of these occurring among natives. (Hume, op. cit.) The local causes that had their influence toward the development of this epidemic (as has been the case in almost the entire history of yellow fever in Charleston) were a high degree of temperature, a moist atmosphere, and the excavation of earth in various portions of the city. "In 1849," says Simons, "an extensive drain was opened in Hasel street, excavating the most filthy and offensive materials; and likewise an extensive drain on Market street from Church street to the wharf. Yellow fever occurred earliest in these localities, and was more fatal. Again, this earth, so filthy and offensive, was transferred to King street from Horlbeck's alley to Hasel street; and in this particular spot there were sickness and mortality among a class of persons who are generally exempt."

In 1852 the first case was the wife of a shipmaster named Cole, who died on her husband's ship, at one of the wharves, on the 16th of August. The ship lay at the dock, in the immediate vicinity of three vessels, recently arrived from the West Indies, and she had on board a quantity of damaged cotton, taken from a vessel that had been destroyed by fire a short time before. It is not known whether either of these circumstances had anything to do with the production of the disease. They are mentioned by contemporary writers (Hume and Simons) as having a possible connection with it. In January, of this year, excavations had been commenced, with the view of laying the foundations for the new custom-house, and this work was continued during the summer; about two hundred unacclimated Irish laborers being employed, most of whom had arrived in the city during the year. The earth taken from this excavation was sold, and distributed throughout the city, and used in filling up low and swampy lots. Dr. Simons regarded this as a powerful predisposing cause of the epidemic, in which Hume differs from him, who shows (Charleston Medical Journal, November, 1854) that in 1847, 1848, and 1853, much more extensive excavations and distributions of earth took place than in 1852, and yet these were seasons of complete exemption from disease. Still, it can hardly be doubted, that this excavation did act as a factor in this epidemic, and that in the other years mentioned other circumstances required to produce an epidemic were wanting. This much may be admitted without justifying Dr. Simons's assertion that the excavation originated the disease. A more rational view would be that the excavation and presence of unacclimated laborers favored its epidemic development; its origin being looked for in the holds of the West India ships lying at contiguous wharves to the ship when Mrs. Cole, the first case, died.

The disease became epidemic the last week in August, raged with violence during September and October, and declined and terminated during November. In all, there were three hundred and ten deaths, of which two hundred and seventy were of Irish and German laborers; at this time the population of the city, including the suburbs, was about forty-five thousand. But five of the deaths were natives of the city.

Early in the season of 1854 the steamer Isabel arrived from Havana and Key West. She came to the wharf on the 11th of May, and on the 14th a passenger was sent to hospital with black vomit. He, however, recovered, and there were no more cases from this source of infection, either in the hospital or on the ship. This man had been some weeks in Key West, where yellow fever was prevailing. On the 11th of July the steamer again arrived from the same ports, and an Irishwoman on board was taken sick to the Pavilion Hotel, and removed the next day to the lazaretto, where she died of black vomit. Still there was no propagation of the disease from this case. (Dawson, Report of City Register of Charleston.) On the 22d of July the Spanish vessels Columbus and Concha came to the wharf from quarantine, their port of departure having been Havana. Four days after each vessel sent a sick man to the lazaretto, one of whom died with black vomit. On the arrivals of these vessels, the ship Sullivan, just arrived from Liverpool, was lying at the dock to which they came, and on the 7th of August a sailor was taken with yellow fever from this ship, and on the 10th another case occurred from the same source. Dawson (op. cit.) thus narrates the next cases that occurred among the shipping:

> The brig Emily arrived at Accommodation wharf from Boston on the 8th of August. A seaman was sent to the marine hospital on the 18th, having been three days sick before admission, and died on the 20th with black vomit.
>
> The schooner Monterey, from Philadelphia, arrived at Central wharf (next to where the other vessels were) on the 7th of August; on the 17th one man was sent to the marine hospital, and is now convalescent.
>
> The Susan Cannon, from Baltimore, arrived at the same wharf on the 8th of August; on the 17th one man was sent to the marine hospital, who is still under treatment.

On the 8th and 9th there were other arrivals of northern vessels with similar results. Nor was this the only source of infection, for on the 13th of July the bark Aquatic arrived at quarantine in distress. This vessel was from Matanzas, bound to Dublin, and on the voyage, all the crew being either dead or sick with yellow fever, she was run ashore, but subsequently got off, and brought to Charleston. At the quarantine, all of her original crew were sent to the lazaretto, and she was brought up to the Union wharves in charge of persons living in Charleston. At these wharves was the bark Vesta, from Boston. On the 7th of August a case occurred on the Vesta, another on the 11th, and on the 12th another, this latter on shore, in the person of a man who had worked on the Aquatic. The first case on shore was at a hotel, in the person of a Spanish gentleman, who visited the Concha repeatedly. He was taken on the 7th of August, but recovered. An Irish servant girl, who washed the bed-linen of this man, was next taken on the 20th, and soon after other cases occurred in this house. From these various centers the disease diffused itself throughout the city. (Frost, Review of Weather and Diseases, 1853, 1854; Charleston Medical Journal, January, 1855; Hume, Yellow Fever in 1854, *idem.*)

This was the most widely-diffused epidemic that had ever been known in Charleston. All classes, races, and conditions alike suffered attacks, though, of course, the mortality was greatest among the unacclimated. The fever was extremely general among the black population. It was noticed that there was an unusual number of second attacks. (Wragg's Roper Hospital Report, p. 19.) The fever was decidedly epidemic by the middle of August, and lasted as usual until some time in November, causing six hundred and seventy-five deaths.

There was no epidemic in 1855, but in 1856 it made its appearance on the 14th of July, and eight other cases occurred between this date and the 5th of August. Previous to this time there had been numerous

arrivals at quarantine of infected vessels, and in some cases these vessels had been permitted to discharge their infected cargoes at the city wharves within a day or two after their arrival; those engaged in unloading the cargoes went freely between the vessels and the city, and in some instances the crew and passengers had free access to the city. One of the earliest cases was that of John Abbott, who was engaged in lightering the bark St. Andrew, which arrived from Havana, on the 18th of July. He was taken sick on the 25th, on King street, near Broad, and was removed to the lazaretto. "Nearly all the earlier cases have occurred to seamen from vessels at the wharves, where the cargoes of the infected vessels were landed." (Charleston Medical Journal, November, 1856, p. 845.) The epidemic was not general throughout the city, being confined chiefly to the hospitals, and to the vicinity of the wharves and sailor boarding-houses. There were a number of cases, which occurred in patients in the hospitals, who had not been out of the wards for months previous to the outbreak of the disease. The majority of the sick were among the Irish and those who lived in crowded tenement-houses in the midst of filth. The disease was epidemic until the latter part of November, causing, in all, two hundred and forty-six deaths. Commenting on this epidemic, the editor of the Charleston Medical Journal remarks:

> We would, in conclusion, recommend a more stringent quarantine system, which would exclude all vessels from infected ports from our harbor. The petty traffic during the summer with the tropical ports usually infected with yellow fever should not be allowed to continue, as millions of dollars are lost to the city when the fever is introduced, and hundreds of lives are wantonly sacrificed.

In 1857 there were seventeen deaths reported as yellow fever, but it did not attain the proportions of an epidemic. Not so in 1858, when a most severe epidemic occurred, and one very instructive in its details. Up to the 30th of June of this year there had been thirty-three arrivals of vessels from West India ports and five from New Orleans, most of them loaded with sugar and molasses, fruit, and cigars. On the 18th of May the bark Nueva Rosalia arrived from St. Iago de Cuba, with one case of yellow fever on board, who was removed to the lazaretto, where he recovered.

On the 28th of June, the steamer Catawba arrived in three and a half days from Havana, and was quarantined, although having no sickness on board. Her passengers were sent to the railroad the next day, and her crew detained until the 7th of July, when they were discharged and came up to the city. Among them was the steward, by name Garcini, who went to a lodging-house on Tradd street between King and Orange. On the 9th of July the first case of yellow fever occurred in the person of a man named Abbott, who lived in this house, and who died on the 18th. On the 12th of July the steward, Garcini, was taken sick, and died with the black vomit on the 15th. He was a very intemperate man, had been drunk ever since he had been on shore, and was reported to have had the fever before in 1856. In the mean time the cook of the Catawba, a German named Schwacterer, who lodged at No. 16 King street, near Tradd, and in close proximity to the house of Ahrens, where the other cases were, was taken sick on the 10th of July, and returned to the steamer, from whence he was sent to the lazaretto, where he died on the 16th with yellow fever. The next two cases were those of policemen living in the vicinity, and other cases soon followed, so that, by the 12th of August, it had been gradually diffused through all that part of the city and soon became epidemic, being most severe in September, declining gradually through October, and rapidly in November. There was one death in December. (Hume, Charleston Medical Journal, vol. 15, pp. 1–32.)

The report of Dr. I. L. Dawson, city register, conclusively shows that the premises of Tradd street were in a most filthy condition. There was a large heap of manure festering in the sun by the door, which had been brought from a neighboring cow-yard, the odor from which was insupportable; a cellar had recently been excavated in the house, and the fresh earth heaped up on one side of the house, and the privies were filthy and very offensive. The sewers along Tradd street were filled with mud and offal and choked up, as were those of many streets in the vicinity. In fact, the sanitary condition of the whole city was as bad as could be, and presented the most favorable conditions for the development or spread of epidemic disease. The committee of the common council say:

> The filth and putrefaction found in the subterranean cesspools were alone sufficient to engender the most malignant fevers that could possibly affect a city.

The committee further say:

> The conclusions to be deduced from the foregoing facts are that if yellow fever be introduced by importation it cannot become epidemic, except by common causes, i. e., the atmosphere of Charleston must be in the same condition as the atmosphere of Havana, (or elsewhere,) from local causes, to produce or favor an epidemic; and if not in this like condition, no epidemic can possibly result from such importation. (Report of committee of city council of Charleston on epidemic yellow fever of 1858, p. 45.)

Shortly after the fever became epidemic in Charleston it broke out in the garrison of Fort Moultrie, on Sullivan's Island. The first case was a man by the name of Jones, Battery E, First Artillery, who died August 15. This man was orderly to the commanding officer and was in the habit of associating freely outside the garrison, the island being then crowded with persons from Charleston. On the 20th a second case occurred, and on the 22d a third, the servant of the commanding officer; the fourth was on the 26th, and many others subsequently. The number of deaths in the city during the season was seven hundred and sixteen. (Report of Surgeon B. M. Byrne, United States Army, post surgeon, Fort Moultrie.)

There were a few sporadic cases in 1862, most of which occurred in the vicinity of the wharves, and were thought by Surgeon D. W. Hand, United States Volunteers, who investigated the subject, to be due to infection received from blockade-runners, "several of them having been quarantined that fall with yellow fever on board." In 1864 there was a general prevalence of the disease. "The first case was that of a Mr. Dallas, living on Calhoun street, near the citadel. He was taken sick July 27, and died nine days after, with every symptom of yellow fever. No more cases occurred for two weeks after this. Then it appeared in several parts of the town, and finally, August 24, came back to Mr. Dallas's house, where three persons then had it. Soon after this General Jameson, on duty at the citadel, sickened and died with the same disease. It now became epidemic and spread to all parts of the town. * * * It is thought that less than two thousand persons in all had yellow fever at this time, and that the mortality did not exceed 35 per cent. among all classes. Most of the physicians believe that the epidemic was this year originated in Charleston. Previous to the sickening of Mr. Dallas, no vessel had arrived in the port; but two or three days after Mr. Dallas was taken sick, the steamer Druid came into the harbor with three cases of yellow fever on board. Several other steamers came in during the month of August with the same disease on board, and it was known that yellow fever prevailed as an epidemic at Nassau."

(Report of Acting Assistant Surgeon D. W. Hand, United States Army, March 8, 1866.) It seems most rational to suppose that there was an error in diagnosis in the case of Mr. Dallas, (an incident which every one that has seen yellow fever at the south knows is of common occurrence at the beginning of epidemics,) and that the first cases really received their infection from the Druid. Dr. Robert Lebby, health-officer of Charleston, (letter of July 21, 1872,) speaking of this epidemic, says:

> Fever existed or prevailed in Charleston in 1854, during the war. It was then said it was imported in the Siren, a blockade vessel from Nassau or Jamaica. Dr. Kinlock and myself saw cases in Calhoun street, northeast of the citadel, on the low lots, before the vessel arrived; she did have yellow fever on board, and it may have extended in an atmosphere prepared and compounded of atoms favorable for its propagation and extension.

The epidemic in 1871 was remarkable from the great blending of other types of fever with the true yellow fever, rendering the diagnosis difficult and often impossible; yet there were undoubtedly many cases of the latter in the city. Dr. Lebby, the health officer, also notes it as an extraordinary fact that, for the first time in many years, no infected vessels arrived at the quarantine during the season. Common report attributed the outbreak of the disease to infection received from the bark Teresa, a vessel which had been quarantined at Bull's Bay with yellow fever on board; but Dr. Robert Lebby (Report of Port Physician for 1871) and Dr. Manning Simons (Transactions of American Medical Association, 1872, pp. 316–20) both show very conclusively that this could not have been the case, no communication being possible between the ship and the first case which occurred within the time specified. Nor do there seem to have been any other possible sources of foreign importation. Dr. Simons finds sufficient causes for the prevalence of the disease in the filling of sunken and low lots with the garbage and offal of the town, in the construction of a new drain on Market street, (where the disease was chiefly located,) and in the caving in of a drain in Rafer's alley, near Market street. It is proper to add that the character of this epidemic was disputed by many of the physicians of Charleston, some going so far as to insist that there had been no yellow fever in the city at all.

The disease prevailed this year in Beaufort, South Carolina. "The first case occurred on August 6, in a man who was brought from Bull River, sick with fever, and died on the 17th, with marked symptoms of yellow fever. The disease ran its course from this time until November 21, when the last case terminated fatally. There were forty-one cases among the whites and one colored; seven of these (whites) died." (Transactions American Medical Association, 1872, p. 331.)

In 1862, when a large army was concentrated at Hilton Head, South Carolina, there being about ten thousand troops there at the time, the steamer Delaware, having troops on board, left Key West for Hilton Head, where, after a short detention at quarantine, she landed her passengers on the 8th of September. It is believed that she had cases of yellow fever on her passage; but, however that may be, soon after her arrival the fever broke out among her passengers, and, by the 17th of the month, eight had died. On the 9th of October an employé of the Quartermaster's Department was taken sick, and soon after a number of officers and soldiers on duty in the vicinity of the wharf where the Delaware had landed, but the disease did not spread generally among the troops. The total number of cases was forty, of whom twenty-five died.

Savannah.—The yellow fever prevailed at this place in 1820, but I have not been able to put my hands on a copy of Dr. Waring's pamphlet describing the epidemic, and am consequently unable to give any particulars of it. In 1827–'28 there were a few cases of black vomit, occurring during a general prevalence of "dengue," or breakbone fever, but no epidemic of yellow fever. (Waring on the Epidemics of Savannah, North American Medical and Surgical Journal, vol. 10, p. 145.) The city was not visited by yellow fever again until 1854, when a severe epidemic occurred. The first case occurred on the 5th of August, on the corner of Lincoln and Broughton streets. Between that date and the middle of the month cases increased in number slowly, after which it became epidemic. It exhibited its greatest violence from the 20th of August to the 20th of September, commencing in the northeastern section of the city and advancing to the southwest. Professor R. D. Arnold, M. D., who attended the first cases, informed me that there was no reason to doubt the local origin of the disease. The weather was unusually hot, and the summer so dry that the ponds all dried up, and he considers this sufficient to account for the epidemic.

The causes assigned by Hon. John E. Ward (at that time mayor of the city) were—

1. The removal of mud from the Savannah River, by dredging, and its deposition on the wharves of the city. Lieutenant Morton, United States Engineers, who was in charge of this work, denied that any had been so deposited.

2. The conditions of the rice-lands in the vicinity. These, during the height of the epidemic, were in good order, but toward the close of the season a heavy storm broke the banks surrounding them, and they became overflowed. This was, however, too late to affect the course of the epidemic.

3. A peculiar epidemic condition of the atmosphere.

4. The arrival of the brig Charlotte Hague, which put into Cockspur Road about the 29th of June. (Mayor's Report of City of Savannah, 1854.) This vessel was found off Tybee Island in distress, and was brought into Cockspur Road, with her captain and two men sick with fever. Dr. Hume (Charleston Medical Journal, vol. 10, p. 31) asserts that these were cases of yellow fever, and that they were brought up to Savannah and treated in a private hospital on Broad street, and were the cause of the city becoming infected, while Dr. R. C. Mackall (Health officer) and Dr. W. M. Chartres deny that these were cases of yellow fever at all, but of mild bilious remittent. (Charleston Medical Journal, vol. 10, pp. 150–153.) During the epidemic there were five hundred and eighty deaths among the white population and fourteen among the colored people. After this there were no more cases of yellow fever until 1858, when there were a few deaths, and since that time the city has been exempt.

The little town of Saint Mary, Georgia, suffered from yellow fever for the first and only time in 1808. The season was very rainy, causing stagnant pools of water to be formed about the town, and the water in the wells became offensive to the smell and taste; yet there was no change in the general health of the inhabitants until the 5th of September, when the schooner Polly arrived from Savannah with two men sick with yellow fever. These were carelessly allowed to be landed and taken to a house in the center of the town, where they both died. Two men who nursed these cases died on the 9th and one on the 10th of September. The whole family in the lodging next took sick, and the disease soon became epidemic in town. The malignancy of the disease was undoubtedly intensified by the extremely filthy condition

of the town. (Letter of James Seagrove to Dr. Hosack, American Medical and Philosophical Register, vol. 3, p. 417.)

An epidemic which occurred in 1839 at Augusta, Georgia, gave rise to much discussion, the advocates of the local origin of the disease claiming it as clearly an instance of its spontaneous development. These pointed to what was called the "trash-wharf, a slide or inclined plane, which was erected in 1834 for the purpose of throwing the filth of the city, including dead animals, into the river. This mass of animal and vegetable matter having accumulated to upward of 200,000 cubic feet, it was resolved by the authorities of the city to have it removed, and accordingly, during the months of May and June, its interior was exposed to the action of the sun. Having penetrated the exterior crust, the heat evolved was so great that the workmen, although wearing thick shoes, were compelled to desist from their work for two hours at a time, so as to suffer it to cool." (Army Medical Statistics, 1819–'39, p. 249.) Dr. Frank Robertson, of Charlestou, informed me that the river was unusually low that year, so much so that a crop of turnips was planted on what had been the river-bed, and stated his most positive belief that the disease originated from these two causes. On the other hand, Monette shows (Yellow Fever at Natchez and the Southwest, p. 128) that the town was unusually healthy for two weeks after the fever was epidemic in Charleston and ten days after it was so in Savannah; that Augusta was not only the terminus of the railroad from Charleston but of steamboat navigation from Savannah, and that very many persons had fled from these places to Augusta. The first manifestations of the disease were among those who had fled from these places. Strobel, who was in Charleston at the time, has no hesitation in affirming its importation from that place. On the subject Monette forcibly remarks:

> Why did not yellow fever manifest itself in Augusta at the same time it made its appearance in Charleston and Savannah? Why was the epidemic in Augusta deferred until Charleston and Savannah were almost deserted by their inhabitants and the transient population? Why did it not commence in Augusta first, if it proceeded from local causes? Did the arrival of the people from those infected cities excite the extrication of pestiferous miasm from local causes which had lain dormant up to that time?

Monette is, however, in error, when he speaks of the disease as being epidemic in Savannah in that year. There were a few cases, and many of the inhabitants fled from the city for fear of its general prevalence, but it did not become epidemic, as it did in so many other towns in that year.

Saint Augustine, Florida.—Yellow fever is said to have visited Saint Augustine for the first time in 1807. It was known to have been especially severe in Havana that year. Florida was at that time under the Spanish domination; there were few or none of the inhabitants that were American, and the records of this visitation, if any such there are, are unknown to the profession in the United States. It did not appear again until 1821, the year of the cession of Florida to the United States. This political event had the most important bearing on the course of the subsequent epidemic. As a consequence of the change of flags, a large garrison of United States troops, all of whom were entirely unacclimated, were quartered in the town. Besides this, the city was crowded to an unprecedented degree with adventurers and strangers from all parts of the United States. On the 10th of July the exchange of flags took place, and immediately thereafter the Spanish garrison was sent in transports to Havana. Three of these vessels returned to Saint Augustine about the middle of August, with yellow fever on board, one, the schooner Alexander, having lost her entire crew and being navigated into port by two passengers, named Miller, who lived in Saint Augustine. The cook of the Alexander, being the only one left of her original complement of men, died the day after her arrival, and his bedding, clothes, and other effects were thrown overboard. These floated, with the tide, up the river, and were picked up by a lawyer named Fitch, who was then going in his boat from the city to his plantation, at a considerable distance from the city. Arrived home, he had the bedding washed and dried, intending to use it. A few days afterward (it is impossible to give the exact dates) a negro woman who had washed these things took sick, as did also Mr. Fitch, and returned to Saint Augustine, where the latter died on the 10th of September. From these cases the disease spread to the whole of Mr. Fitch's family, all of whom died. In the mean time two young men, recent arrivals in the city, had gone on board one of the returned transports, and returning to town, soon after sickened and died. The disease spread slowly at first, but in the month of October it became violently epidemic, and nearly all the strangers in town were attacked. In all, one hundred and thirty-two cases died, besides forty among the troops, out of a mean strength of one hundred and twenty. No Spaniards, Minorcans, or other natives were attacked, and but three of the deaths were those of negroes.

The local causes which intensified this epidemic, and were said by some to have originated it, were as follows: The police of the town, during the Spanish occupation, had been extremely bad. Every one who has seen any of the New Mexican houses, or those on the Rio Grande frontier, can form a very good idea of its condition, without any special description. The streets were narrow and crooked, filled with filth and offal, the back-yards and privies never cleaned. Add to this that many of the houses, which had been closed in consequence of the removal of the Spanish residents to Havana, were occupied by the newly arrived strangers, often without proper means to render them habitable or remove the previously existing nuisances.

A second local cause was the closing of a bar, about twenty miles from town, at Little Matanzas Inlet, by which a broad bay extending from the bar toward the city was converted into a fresh-water marsh. In consequence, all the salt-water vegetation and the oysters and other shell-fish died, and putrefactive decomposition went on to such an extent that we have the authority of General Hernandez, a very old resident, for saying that it "gave rise to exhalations of malaria so offensive that the stench could scarcely be borne." The prevailing winds from the southeast blew directly over the marsh toward Saint Augustine. Bilious remittent fevers had prevailed to an unusual extent previous to the advent of the vomito; the season had been hot and wet, and there was much sickness, especially dysentery. (Strobel, Essay on Yellow Fever, pp. 129–147; Dowler, Epidemics of New Orleans, p. 18.)

After this year there was no more fever at Saint Augustine until 1839. Early in the summer of that year, as will be remembered, yellow fever broke out in Charleston. On the 15th of August a family by the name of Abel, who had been living in a house on Meeting street, Charleston, in which were cases of yellow fever, arrived in Saint Augustine and took board at the house of a Colonel Johnson, fronting on the bay. Several of this family were slightly ill within a few days after their arrival; but it does not seem clear, from the account given by Strobel, that these were cases of undoubted yellow fever. About the 1st of September

their trunks, boxes, &c., were unpacked, and the contents, consisting of bedding, mattresses, and clothes, hung out on the balcony and rear fence to air. On the 4th of September a case of sickness occurred in the house of Antonio Andreo, living next door to Colonel Johnson, which closely resembled yellow fever. On the 8th of September Mr. Andreo was similarly attacked, and, two days after, three of his children. All of these cases recovered. The next cases were in the house of a Mr. Circopoly, who lived directly behind Colonel Johnson, their back-yards adjoining. Here a child was taken sick on the 10th of September, which was speedily followed by four more, in the same family, all of which recovered. The disease next broke out in the family of a Mrs. Brown, living about a block west of Circopoly's. Up to this time there had been no deaths, and the disease had been almost entirely confined to children, natives of the city, and it seems to me that these were all cases of dengue, or breakbone fever, although, if (as Arnold of Savannah thinks) dengue and yellow fever are identical, (Arnold on the Identity of Dengue and Yellow Fever, 1859,) this statement is of no consequence. Now, however, a new element arrived to complicate matters. On the 10th of September about forty Seminole Indians, who had been confined at Fort Moultrie, Charleston Harbor, arrived in Saint Augustine, accompanied by a guard of soldiers, and were transferred to Fort Marion. Some of the troops had the fever at Castle Pinckney, before leaving Charleston, and there were cases among them after their arrival at Fort Marion. Whether it was due to their arrival or not, it seems certain, from the evidence, that after this time the prevailing disease assumed more decidedly the characteristics of yellow fever. A German died of black vomit in Colonel Johnson's house, and other fatal cases soon occurred in the vicinity. The disease gradually extended to the westward, across the town, the prevailing winds being from the northeast. Those who affirmed the local origin of the epidemic ascribed it to the filling in of a gully, behind the sea-wall, (running along the front of the bay,) with shavings, old clothes, garbage, and other filth of the city, producing a large amount of decaying animal and vegetable matter to fester in the hot sun. Another local cause was that a rank growth of weeds had sprung up in many parts of the town in the place of the orange-groves, which had been destroyed by a frost in 1835.

In 1841 the disease prevailed to a certain extent, there being fifty-six cases and twenty-six deaths among the troops at Fort Marion. Dr. Porter, United States Army, from whose report I obtained this information, does not state how it originated. (Army Medical Statistics, 1839-1855, pp. 310, 311.)

An incident connected with the importation of yellow fever into Jacksonville, Florida, which was kindly furnished to me by Dr. A. S. Baldwin, of that place, may be mentioned here. I will give it in the doctor's own words:

> In July, 1859, I was called to attend two children of a neighbor; one, a little girl was sick for a few days and recovered; the other, her brother, developed regular black vomit, and died. This case was seen by several of my professional brethren, who had no hesitation in pronouncing it to be a genuine case of yellow fever, and a post-mortem examination still further confirmed our diagnosis, and settled the question beyond doubt, for the box-wood liver was a marked characteristic. The origin of these cases is a matter of interest in connection with the transmissibility of the disease. The uncle of these children had, a few days before, returned from a visit to Cuba, and the day before he left Havana had been through the hospitals, that for yellow fever included. The only article of woolen clothes he had on was a coat, and this his aunt sponged with alcohol after his return from the hospitals. In going through the hospitals he passed by the beds of those who were throwing up black vomit. After his return home these children were with him when opening and taking out the contents of his trunk, and handled the articles, and they alone of the family were present, and they were both taken sick together a few days after, and, so far as could be ascertained, there was no other means by which they could have contracted disease, and no cases besides theirs occurred in the community.

Pensacola, Florida.—The first visitation of yellow fever to this city was in 1765. An English regiment was sent to garrison the town, coming from home via the West Indies, and was quartered at Barrancas. Shortly after their arrival they received supplies of food, clothing, &c., from the West Indies, where the fever was then prevailing. They took the disease, and lost one hundred and twenty men; but there is no record of its having spread among the citizens of the town. (Williams, Florida, p. 15; Lind on Diseases of Hot Climates, pp. 36, 37, 179.)

It did not appear again until 1811, and was then ascribed to some extensive ditching which had been made from the town to a swamp in its rear, thus exposing a large quantity of fresh soil to the influence of the sun's rays, which were uncommonly hot that year. Large numbers of the inhabitants perished. (Drake, Principal Diseases of Interior Valley, North America, p. 227.)

In 1821 occurred the transfer of Florida from the authority of Spain to that of the United States, and immediately thereafter there was a great rush of emigrants from all parts of the country to Pensacola. Most of these were adventurers of dissipated habits, and with but little money, who lived crowded together under highly unfavorable sanitary conditions. More or less trade immediately sprung up with the West Indies, and in the summer of 1822 there were several arrivals of vessels from those islands. Among these was the cutter Alabama, from Havana, which arrived on the 12th of August, having, during her passage, lost two of her crew from yellow fever. The day after her arrival the captain, feeling sick, went to a boarding-house on shore, where in a few days he died of black vomit. His case was followed by that of the landlady and her daughter, both of whom died. The boarders scattered all over town, but most of them took the disease, and several died; and cases soon occurred, not only in adjoining houses to the boarding-house but wherever these boarders fled. (Carpenter, op. cit., pp. 35, 36.)

While these events were taking place the brig Franklin arrived at the town. This vessel had left a northern port for Havana with a cargo of codfish, but on her arrival at that place, some of the fish being spoiled, she was ordered away by the authorities, and came to Pensacola, where she lay at anchor for some eight or ten days near the town. During this time most of her fish had been sold and distributed about the city, thus introducing a new source of infection.

The vessel was now ordered away by the authorities, but in departing went ashore at Fort Barrancas, where she was condemned and sold, and the remainder of her cargo unloaded by some artillery soldiers at the fort. The disease soon broke out among them, and a number died. (Army Medical Statistics, Report of Assistant Surgeon McMahon, United States Army, 1819–39, p. 36.) The disease became epidemic about the 19th of August, and lasted until the 27th of November, and during the season upward of two hundred died out of a total population of about one thousand. Of the sanitary condition of the town at this time Assistant Surgeon McMahon, United States Army, remarks:

> From an attentive examination of its natural position I am induced to believe that this city holds out a fine prospect for salubrity. Exposed in front to the sea, and sufficiently remote on every other side from swamps or large bodies of fresh water, it possesses so much elevation as to have a constant and free ventilation, while the soil of the city, as well as of the vicinity, is one unvaried bed of sand mingled sparsely with a rich vegetable mold. The streets are wide and spacious, intersecting each

other at right angles. * * * The houses, generally built of wood, are many of them in a state of decay. The lots and yards, containing the accumulated filth of years, are extremely offensive. The privies, being very much neglected, are abominably fetid. The soil of the gardens, being in a great measure artificial, vegetation is very productive, and, as the leaves and offals are suffered to remain on the surface, decomposition, formed by heat and moisture, rapidly ensues. To these prolific sources of miasmata may be added a total neglect of cleanliness in the personal habits and domestic economy of the inhabitants. An unusual degree of heat and moisture during the summer has tended effectually to bring those exciting causes of disease into operation.

I have quoted somewhat at length, because this description will apply to almost every town and every epidemic that has occurred in the southwest. Everywhere, in studying the disease, we find bad drainage, vegetable decomposition, intense heat, superabundance of moisture, filthy privies and yards, old decaying houses, and dirty people present as elements to make up a factor in the production of an epidemic. It was remarked, during the epidemic of 1822, that there was a singular mortality among the brute creation, nearly all the dogs in town having died and foxes in the neighboring woods. (Drake, op. cit., p. 228.)

The next appearance of the fever was in 1825, in which year, Monette says, it was introduced, and prevailed with great fatality; but I can find no details of its occurrence. Drake, however, refers the accounts of this epidemic to the year 1827, when it again prevailed, though in much milder form than hitherto. I have not been able to find any details of its origin. In 1828 it again made its appearance, at the navy-yard and hospital, but not in the city, being brought to the navy-yard by the arrival of vessels of war having the fever on board. The same occurrence took place in 1834.

In 1839 a sick person arrived from New Orleans, and stopped at the house of the naval surgeon there stationed. This man died five days after his arrival. Next the doctor took sick, then his negro servant, then other members of the doctor's family, and thence it spread through the whole yard. (Dr. J. A. Wedderburn, United States Navy, in Carpenter, op. cit., p. 27.)

It was epidemic, also, in the city of Pensacola, the first cases being in the persons of unacclimated refugees from Mobile, who came with the *semina* of the disease in their systems. Subsequently it affected the resident population. (Drake, op. cit., p. 234.) During this season the French frigate La Gloire arrived at the navy-yard with twenty cases of fever on board. She was placed in quarantine, and all intercourse cut off between her and the city, her sick being removed to the naval hospital. There was no extension of the disease from this source to the city. (Monette, op. cit.)

In 1841 the fever broke out on the sloop of war Levant, lying in the Bay of Pensacola. The sick were first placed under a shed on shore, and, after two weeks, moved to the hospital. The disease did not spread so long as they were isolated under the shed, but, on being removed to the hospital, they had to pass the marine barracks. After this, cases began to occur in the yard, first in the person of a negro woman who lived near the shed, then among the marines, and then generally throughout the yard. It was introduced, after this, every year to the navy-yard, by the arrival of infected ships, until 1848, and again in 1853. (Dr. Isaac Hulse, United States Navy, Maryland Medical and Surgical Journal, vol. 2, p. 391; Dr. S. C. Lawrason, United States Navy, Maryland Medical and Surgical Journal, vol. 3, p. 393; Dr. Wedderburn, United States Navy, in Barton's Report of Sanitary Commission, pp. 124, 125.)

In 1853 the disease once more appeared at Barrancas barracks and the navy-yard. Early in July a detachment of recruits arrived at the navy-yard from New Orleans. On the 9th of that month five of them were admitted to the hospital with yellow fever. Several of the attendants who nursed them were taken sick, but the disease spread no further. On the 24th of July the United States steamer Vixen arrived from Tampico, Mexico. A few days after her arrival, a boy who had been in the habit of going on board took sick and died, and his sister, who slept with him, also died, both with black vomit. About this time a man was removed from the Vixen to the navy-yard who had sustained a fracture, and his mattress was thrown overboard, which being picked up by a negro, he sickened with yellow fever and died in a few days. Cases began soon to multiply in the navy-yard and on the Vixen, until nearly the whole population in the vicinity suffered. From the navy-yard it spread to Barrancas barracks, and of the whole command but seven escaped an attack. It should be remembered that the sailors on the Vixen had suffered from what was called "Tampico fever" on the voyage to Pensacola and some were sick on her arrival. (Dr. John F. Hammond, United States Army, Army Medical Statistics, 1839–'55, p. 326.)

From this time until 1867 there was no epidemic of yellow fever in Pensacola. In 1863 there was, however, a most instructive outbreak of the disease on board the storeship Relief, at the navy-yard, extending from the vessel to the yard and to the other vessels in the navy. This vessel had been for more than a year off Ship Island, as a store-ship. She arrived at the navy-yard some time in the month of August, and, being at the time perfectly healthy, was allowed to pass the military quarantine without question, and came to the anchorage off the yard. Shortly after her arrival a large amount of sickness was noticed on board, which was at first diagnosed to be typhus fever. There were thirty cases, of which seventeen were removed to the hospital in the yard, and the remainder, with the ship, were sent to the quarantine station, six miles distant.

The diagnosis of the cases sent on shore was soon corrected by the death of a man with black vomit at the yard, and by the occurrence of seven deaths on the Relief at the quarantine, "all these cases being the most decided and malignant yellow fever." About the 1st of September several cases died on a merchant-ship lying off the navy-yard, and much sickness took place in the bomb-fleet, which had come from the Mississippi River and were anchored at the yard. Other cases occurred in the yard and a few in the adjacent village of Warrenton. There was a large body of troops encamped a mile and a half distant, at Barrancas. The camp was kept in the most perfect police, and a rigid "cordon sanitaire" established between the camp and the navy-yard, and the troops escaped the disease. Dr. Gibbs, of the Navy, thinks there can be no doubt that the disease originated on the Relief. He says:

There can seem little doubt that the disease was generated on the Relief. * * * Stores had remained undisturbed in the vessel over a year. During this time several hundred gallons of sperm-oil had leaked from the casks into the bilge. The ship made much bilge-water, which was exceedingly offensive in its odorous gases and very black; kegs of pickles were moldy, or in a state of decomposition; and the freshly-preserved meats, in cans, were unsound to such an extent as to give rise to an animal odor. When I first visited the ship the hatches were all closed, as the offensive odor with them opened was most intolerable. (American Journal of Medical Sciences, April, 1866.)

While these events were occurring, the frigate Colorado, on the blockade off Mobile, experienced a similar outbreak, there being about forty

cases and seven deaths. The disease was traced to stores received from the Relief. (Letter from Dr. B. F. Gibbs, United States Navy, July, 1872.) Dr. E. F. Holden, United States Navy, (American Journal of Medical Sciences, January, 1868, Causes of Certain Diseases on Ships of War,) however, considers the deleterious influences of the bilge-water in ships to be overestimated, and does not regard it as capable of generating disease of a malignant character; but Dr. Gibbs and others think that it will do so in tropical climates, especially when agitated by the motion of the waves after a long period of rest at an anchorage.

The year 1867 was, as is well known, one of general epidemic tendency throughout the Gulf region, and Pensacola did not escape. A military quarantine had been established on the 21st of May. On the 21st of June the English ship Fair Wead arrived from Jamaica, where yellow-fever was epidemic. She seemed to be clean, and had not a case of sickness on board, but was detained at quarantine until July 2, during which time one man died, suddenly, as was reported, of congestion. After coming up to the city yellow fever broke out on board, and by July 10 there had been eight cases and three deaths. On the 19th another man died, and the vessel was ordered back to quarantine. She does not seem to have communicated the disease to the shore. Some other vessels, lumber-laden, having fever on board, were ordered from the harbor to quarantine about the same time. On the 24th of July the schooner Texana arrived from New Orleans, and a few days thereafter her mate was taken sick with yellow fever and brought on shore to a boarding-house in the city. Other persons living in this house soon sickened, and the boarders, scattering to various other houses, spread the disease in several different localities. On the 9th of August the disease was epidemic. (Letter of Mayor to General Seymour, commanding United States troops.) The troops at Barrancas were thereupon, on the 14th of August, moved to Fort Pickens, leaving only a dozen men at the former place. This detachment were positively forbidden to pass to the village of Warrenton, which was separated from the barracks by a belt of woods between the two. On the 23d of August, the United States steamer Alliance arrived at Barrancas with a detachment of the Twenty-fourth Infantry, one man being sick who died, two days after, with black vomit. The men in the barracks were immediately removed from the vicinity of the hospital to the distance of two hundred yards, and there were no cases either among them or at Fort Pickens, although the disease prevailed extensively at Warrenton and at the navy-yard adjoining, approaching to the very belt of woods between the village and Barrancas barracks. (Report of Acting Passed Assistant Surgeon N. L. Campbell, United States Navy, to Naval Bureau.)

Some isolated cases that have occurred at different times have an interesting bearing on the subject under discussion. In 1867 there was but one case of yellow fever in the city. This was in the person of a little girl who died, during its prevalence, at the navy-yard. No connection could be traced with any extra mural source of infection, and at first it was considered a case of purely local origin, but inquiry showed that the child, a few days before being taken sick, had played hide-and-seek in her father's garret with her playmates, where were found some sails that had been purchased a short time before from an infected ship in New Orleans.

In 1853 a young man died in the village of Milton, a few miles from Pensacola. His clothing was placed in a trunk, put away, and forgotten. In 1855, two years subsequently, this trunk was opened and the clothing taken out to be aired, and yellow fever broke out in the house, and there were several deaths. (Dr. Herron, Pensacola.)

The medical topography of Pensacola presents a more favorable exhibit than that of any other town on the Gulf coast. With a broad bay to the south, from which blows continually a cool breeze, it is situated on a high pine barren, with sandy streets, houses widely separated, and nothing about either its situation or hygiene to warrant the local origin of the disease.

Key West, Florida.—This island first suffered from yellow fever in 1824. The epidemic is ascribed by Dr. Benjamin Ticknor, United States Navy, to the combined influences of atmospheric vicissitudes, miasmata, and bad food, fatigue, and intemperance among the population. (North American Medical and Surgical Journal, vol. 3, pp. 216–222.)

In 1853 it was introduced from Tampa, Florida, by two soldiers who arrived in August, and died of the fever soon after their arrival. Other cases followed these, and in November it became epidemic, prevailing until August, 1854, causing one hundred and twelve deaths out of about three hundred cases. The most infected locality was in the immediate vicinity of a stagnant pond, near the center of the town, which had been made the receptacle for the offal and decaying vegetable matter of the village. (Report of Assistant Surgeon R. F. Simpson, United States Army, Army Medical Statistics, 1839–'55, p. 323.) It is a little singular that, although Doctor Simpson details its mode of introduction from Tampa, he should still express his belief that the disease had its origin in local causes.

In 1862 the inhabited portions of the island were very much crowded by reason of the war. Three volunteer regiments were stationed there, besides a large number of sailors belonging to the blockading fleet and mechanics employed in refitting naval vessels, all of them unacclimated. Typhoid fever prevailed in the town and camp previous to the appearance of yellow fever, there being upward of one hundred admissions to the military hospital with this disease in April and May. On the 20th of June the bark Adventure arrived from Havana, having yellow fever on board, and was quarantined. Four patients from this vessel were taken to the Marine Hospital. Cases occurred, about this time, in the persons of some mechanics, who had been employed about the machinery and in the holds of some iron-clads that had been a long time on the blockade and were in a very filthy condition. By the last of July the disease was generally prevalent in town, and broke out among the troops. By the last of October there had been three hundred and thirty-one cases and seventy-one deaths in the command, besides a very large number in the town. From this point it was carried to the Tortugas, where there were eleven cases and four deaths. (Records of United States Post Hospital, Key West; Circular No. 1, War Department, Surgeon-General's Office, 1868; Reports of Assistant Surgeon W. F. Cornick, United States Army, October 4, 1862; Surgeon E. S. Hoffman, Ninetieth New York Volunteers, October 1, 1862; Harris, United States Sanitary Commission Memoirs, p. 241.) Local causes for the propagation of the disease were the crowded state of the island, the clearing away of large quantities of brush and undergrowth for military purposes, and the removal of a large number of dead bodies from a grave-yard near Fort Taylor, which was done by unacclimated Irish laborers, and "accompanied by an almost insupportable stench." (Sanitary Report of Surgeon E. S. Hoffman, Ninetieth New York Volunteers, September 30, 1862.)

In 1864, in April, a lot of blankets and other goods were received at

Key West from Nassau, New Providence, and exposed for sale. It was asserted that they were a portion of those sent to various places by the notorious Dr. Blackburn, with the view of introducing yellow fever. Be that as it may, the fact is that the Rev. James H. Schneider, chaplain Second United States Colored Troops, bought one of the saddles, and on the 23d April was admitted to hospital at the barracks, where he died, on the 26th, with well-marked symptoms of yellow fever. The saddle was subsequently sold to two other officers, both of whom took the fever and died. From this case others appeared in the hospital and among the troops, and the disease soon became epidemic. The population of Key West at this time was about five thousand, including about twelve hundred troops, two hundred prisoners of war, and one hundred and fifty Florida refugees. (Dr. B. S. Thompson on Yellow Fever at Key West, New York Medical Record, vol. 3, pp. 246–248.)

I have not been able to obtain any satisfactory information relative to its prevalence in 1865, though I was informed by the citizens of the town that it was epidemic in that season.

In 1867 there was no sickness up to the middle of August. On the 31st of July the Spanish frigate Francisco de Assis, accompanying the English steamer Narva, arrived in port. The latter ship had been employed in laying the telegraph-cable from Key West to Havana. Both of these vessels had yellow fever on board, and both were about being ordered to quarantine, when orders arrived from "superior authority" (so stated in the report of Dr. Cornick, from which I quote) to issue no such orders, and, in consequence, both vessels were allowed free communication with the shore. Besides this the sick on the Narva were moved to the shore, and treated, some in the marine hospital and some in private houses, where many of them died. Dr. Cornick, who reports these facts, gives it as his decided opinion (Report on Yellow Fever and Cholera, Circular No. 1, 1868, Surgeon-General's Office, p. 152) that the disease was introduced by these vessels. "Up to their arrival the island was perfectly healthy, and likely to remain so."

At Fort Jefferson, Tortugas, during this period, the disease prevailed with great severity. The first case was on the 4th of July, in the person of Captain G. W. Crabb, Fifth Artillery, who had just returned from Havana. He recovered, and left for the North on the 3d of August. The next case was on the 19th of August, (a private of Company K, Fifth Artillery,) and within a week there had been four more cases, all from this company. On the 25th the schooner Matchless arrived from Tampa with a case of yellow fever on board, and the same day cases appeared in Company L, Fifth Artillery, and among the officers' servants. From this time it may be said to have become epidemic. In all, there were two hundred and seventy cases (including fifty-eight relapses) and thirty-eight deaths. A board of officers was ordered by the commanding officer to investigate the causes of the outbreak. They ascribed it—

1. To the prevalence of yellow fever along the Gulf coast.
2. To persistent southeast winds and long-continued rains.
3. To the arrival of large numbers of unacclimated recruits during the summer.
4. To the damp and unwholesome condition of the barracks occupied by the men.
5. To the filling up of the moat with mud along two sides of the fort, (nearest the barracks of Company K,) owing to the incompleteness of the sea-wall. The stench from this source was very great.
6. Defective privies and bad drainage.

The board, after examining the whole subject, came to the conclusion that it was of local origin and due to the above causes, but they at the time of making their report were not aware of the case of Captain Crabb, (above referred to,) which clearly proves the importation of the disease. This was owing to the fact that the medical officer of the post had died during the epidemic.

The disease prevailed again with violence in 1869. Large numbers of Cuban refugees had arrived on the island, many of them poor and destitute and living under very unfavorable hygienic conditions. While I could obtain no satisfactory data in reference to the first cases this year, yet physicians resident in the town gave it as their unqualified opinion that the disease was introduced by these refugees.

But there is ample ground for the introduction of yellow fever into Key West at any time without going to any special vessel or locality to account for it. The local quarantine regulations provide (section 5) that all sponge-boats, wreckers, and other small craft sailing along the reef shall be exempt ordinarily from visit by the health-officer, but may come and go with impunity. Now, these of all others are the vessels that most need watching. Very many of them only plead the ostensible business of sponge-gathering to cover up their real one of smuggling, and make frequent illicit visits to the Cuban shores, to bring over contraband goods. Of course, where a case of fever occurs in any house having connection with any of these boats, the matter is carefully concealed, as an investigation would lead to an exposure of their illicit traffic were it to be ascertained that they had visited Havana. Hence all such cases are loudly trumpeted as of local origin, and, as persons high in position are not unfrequently interested in this species of trade, a certain kind of influence is brought to bear to favor the impression that fever always exists in Key West. That this is not so the foregoing record shows, it not having appeared at all in 1869, and previous to that not oftener than every other year.

Key West is a sand-island, resting on a coral reef, having in its interior lagoons, which frequently occasion outbreaks of malarial fever. The town is always dirty, which seems to be the normal condition of a great many of the inhabitants. There is always plenty of food for the disease, and always a convenient place for its location. I can see no reason why the disease should not originate here, but it certainly does not seem to have usually done so.

Western Coast of Florida.—The fever prevailed at Tampa in 1839, but I have not been able to find any particulars of it further than a statement in Carpenter's Sketches that "it was introduced from New Orleans." Strobel also asserts (Essay, p. 214) that the United States troops stationed at Fort Brooke obtained their supplies from New Orleans, and that the disease was brought from that city through that channel of communication.

In 1853 a steamer arrived from New Orleans, in September, with quartermasters' supplies on board, and came up to the wharf without any inspection. Lieutenants Cook, Second Artillery, and Silvey, First Artillery, went on board on her arrival and spent some time in the captain's cabin. There was no sickness on the vessel at this time, but she had several cases and some deaths from yellow fever on her passage over.

The same day, three soldiers from the garrison at Fort Brooke spent some time on board. These latter were taken sick three days afterward, and died with unmistakable symptoms of yellow fever. Two days subsequently, Lieutenant Silvey was taken ill, but recovered, and a short

time afterward General Childs, commanding the troops in the State, died, as did also Lieutenants Cook and Butler, Second Artillery, and Redfield, post surgeon. The disease spread generally in the community, and there were many deaths, both among the troops and the citizens. (Letter of Captain W. Silvey, United States Army, July 21, 1872.)

In 1871, about the middle of August, the fever broke out at Cedar Keys, Florida. The facts are not positively ascertained as to its origin, but it was generally believed to have arisen from a sick man, landed from the Havana steamer. The hygienic condition of the place was bad. "At the Keys there were some half dozen saw-mills, with considerable accumulations of saw-dust and slabs, undergoing slow but gradual decay, while the extensive pine piling of the railroad and wharf was in the same condition, besides the stumps of piling burned a few years since." (Tampa newspaper, April 27, 1872.) Shortly after the disease broke out at Cedar Keys, a white boy and a negro, who lived in Tampa, returned to that place from the Keys. They were both taken sick with yellow fever after their arrival in Tampa; both recovered. "The next case was a mulatto woman, who had removed from the country and had been living here something near a year." The next cases were a brother of Dr. Wall, of Tampa, and his wife. The latter died on the 6th of September, and a child died, the same day, in a house opposite to the doctor's. "The disease from that time spread rapidly among those who had settled here since 1867." (Letter of Dr. John P. Wall, August 23, 1872.) While these events were occurring, a physician, Dr. McCaw, from Gainesville, Florida, a village about ninety miles in the interior of the State, visited Cedar Keys to offer his medical services. On his return to Gainesville he was taken with the prevailing disease, and died; and the disease became epidemic in the town and was attended with fearful mortality.

Mobile, Alabama.—A settlement was made on the present site of Mobile about the beginning of the eighteenth century, and yellow fever is said to have prevailed there in 1705. (Drake, op. cit., p. 216.) Its next appearance was in 1765, when it was imported by a British regiment from Jamaica, and prevailed with considerable malignancy. (Roman's History of Florida, p. 13, quoted by Drake, op. cit., p. 216.) From this time there was no yellow fever there until 1819, when it appeared in epidemic form. It was said to have been imported from Havana by the schooner Patriot, which arrived at the wharves in Mobile on the 15th of August. A young man, who was a passenger on her, died of yellow fever a few days afterward, and the disease spread in this part of the town. Professor Drake (op. cit., p. 217) quotes old residents of the town as confirming this view, but a "committee appointed to investigate the causes and extent of the disease," in their report affirm positively that the disease had been in existence a month previous to the arrival of this vessel, and found a local origin for it in the filthy condition of the wharves and streets leading therefrom. (Medical Recorder, vol. 4, p. 160–163.) Such is also the opinion of Dr. Lewis. (New Orleans Medical and Surgical Journal, vol. 1, p. 284.) The total number of deaths was two hundred and seventy-four, in a population of not more one thousand. The disease spread from Mobile to various points on the Tombigbee and Alabama Rivers. The disease is said to have been epidemic in 1825, 1827, and 1829, but, except an allusion to these visitations by Drake, I can find no accounts of them. They are not mentioned by either Carpenter or Monette, who are generally very accurate. In 1837 it began about the 20th of September, and three weeks afterward became suddenly epidemic throughout the town. There does not seem to be any evidence of foreign importation. (Drake, op. cit., p. 220.)

In 1839, about the same time it broke out in New Orleans, it appeared in Mobile. "In both places the first cases were among the shipping exclusively, or among those who had frequented certain vessels." (Monette, op. cit., p. 118.) It prevailed with great violence during September and October, and after a change of weather, about the 20th of October, commenced to decline. The total number of deaths was about six hundred and fifty.

I have quoted above Monette's statement in regard to this epidemic, but Dr. J. C. Nott says (New Orleans Medical Journal, vol. 4, p. 564) that the disease "commenced on the corner of Government and Hamilton streets, half a mile from the shipping, in a clean, well ventilated, and fashionable part of the town."

In 1842 the fever prevailed in about half of the town, commencing in Spanish alley, a very filthy place, near the docks, and marching steadily in a northwestern direction. It was not generally epidemic, there being only about one hundred and sixty cases and sixty deaths. (Dr. P. H. Lewis on Yellow Fever, New Orleans Journal of Medicine, vol. 1, No. 2, p. 51; Nott, op. cit., p. 566.)

In 1843 it prevailed over the half of the town untouched the previous year, commencing in its northern extremity and pursuing a course to the southeast. The number of deaths was two hundred and forty. (Nott, op. cit.) Carpenter asserts that this epidemic was introduced from New Orleans. (Op. cit., p. 30.)

"There was a very mild epidemic in 1847, and again in 1851, but no violent outbreak before the general prevalence of the disease in 1853.

"The first cases of yellow fever which occured in Mobile in 1853, it is conceded on all hands, were imported from New Orleans on board the bark Miltiades."

The epidemic this year had this peculiarity, that it was not confined to the city alone, but spread to various localities in the vicinity which had previously acquired an enviable reputation for salubrity, and had on this account been places of considerable resort during the summer months. This I shall notice more in detail further on.

The bark Miltiades arrived on the 11th of July at Dog River Bar, twenty miles from the city, down the bay. She was from Portland, Maine, via New Orleans. At the latter place her crew had contracted yellow fever, and on her arrival below Mobile one of her crew was sent to the marine hospital, where he died on the 13th with black vomit. On the 19th a stevedore, employed on board in loading the ship, was taken sick and brought to the Sailors' Home, where he died on the 25th. Four other cases were removed from the bark on the 25th. All of these latter cases were brought up to town on the steamer Daniel Pratt, and, August 1st, the engineer of the steamer was taken sick. These men were taken to different houses in town. About the same time several cases appeared in town, which, on inquiry, were found to be of persons who had fled from New Orleans to escape the epidemic. The next that was known was the appearance of the disease among the citizens, and occurring in quite a number of different localities; and by the middle of August it had become decidedly epidemic, and continued as such until the 1st of November. The population of Mobile was about twenty-five thousand, of whom probably a third were absent from the city, and the number of deaths was eleven hundred and ninety-one.

About six miles west of Mobile, in a high, dry, sandy region, in a heavy pine forest, is the locality known as Spring Hill. It is the seat

of Saint Joseph's College, and the residence of about thirty families; the population is much increased in the summer by persons from Mobile, by reason of its well known salubrity. Dr. Nott gives the following account of the importation of the disease into this healthy settlement.

On the 12th of August, just about the time the yellow fever began to assume the epidemic form in Mobile, and one month after the first imported case, I was called to see a young gentleman, Mr. Alfred Murray, with a well-marked attack of the disease, at a boarding-house in Mobile, on Saint Louis street, near Saint Joseph, and on the 14th had him removed on a bed to the house of a brother-in-law, Mr. Wheeler, on Spring Hill, about the center of the settlement, and twenty days after he entered the house, September 5, two of Mr. Wheeler's children were attacked with the epidemic, and about two weeks afterward two other children were attacked; three had black vomit and two died. * * * * Mr. Greer moved, with his family, to the house of Mr. John R. Toulmin, on the 29th, from town, carrying a daughter convalescing from yellow fever. Another daughter sickened on the 8th; three of Mr. Fleming's children, in the same house, on the 10th, and Mrs. John Greer on the next day, &c. (New Orleans Medical and Surgical Journal, March, 1854.)

From these points the disease spread through the settlement, and nearly every person living there was sick.

At Citronelle, at the terminus of the Mobile and Ohio Railroad, thirty-three miles from town, a similar state of affairs existed, and there were twenty deaths out of a total population of two hundred and fifty. It also appeared at the Dog River Cotton Factory, five miles from Mobile, and other localities in the neighborhood of the town. (Doctor J. C. Nott, op. cit., pp. 571-585.)

1867.—It was not long after the disease became prevalent in New Orleans before it made its appearance in Mobile. On the 13th of August, Lieutenant Hezlep, United States Engineers, who had been in New Orleans on duty, died at Fort Morgan, below Mobile, of the fever. Lieutenant Breckinridge, who shared his room with him, next took it; and next Doctor Reynolds, the post surgeon. Many other cases succeeded, and by the 15th of September there had been nine deaths at the fort. The first case in the city of Mobile was on the 17th of September, when Major Tracey, Fifteenth Infantry, died in a private hospital in the city. I was informed in Mobile that this officer had been exposed to the disease either in New Orleans or at Fort Morgan. The next cases were in a sailors' boarding-house near the river, where a boy died of black vomit, a sailor having been previously sick in the house. After there were some cases in the neighborhood of the camp, and before the end of the month there had been about thirty-five cases and six deaths. In September it became epidemic and ran the usual course.

1870.—The schooner Louisa Williston arrived on the 19th of August, 1870, from Havana, and came to wharf near Otis's Mills. On the 22d August Mr. John Otis, who was employed at the mills, was taken sick, and died on the 28th. It was not suspected that he had yellow fever until after he died. The next case was John Otis, jr., who was taken sick September 5, and, on the 6th, Mrs. Otis, mother of the last case, and, on the 8th, her daughter, all with unquestionable yellow fever. There had been no fever on the schooner. The portion of the city where these cases occurred was notoriously unhealthy, being in the vicinity of what is called the Choctaw Swamp, where all the refuse and offal of the city were deposited, and where there are many acres of stagnant water, while the prevailing winds sweep from the swamp over the city. (Dr. George A. Ketchum, in Proceedings Alabama Medical Society 1871.)

In conversing in regard to this outbreak with the members of the board of health of Mobile, I found them generally of the opinion that the disease was due in this case to the Williston. She had just come from an infected port, and, though she had no fever on board, could readily have imparted the infection from her hold to those who where at work about her cargo. Still it is one of the doubtful cases.

Dr. Henry S. Levert, of Mobile, in a letter to Dr. Barton, of New Orleans, (Report New Orleans Sanitary Committee, p. 110,) remarks upon the epidemics of Mobile:

From the facts which have been forced upon my notice, my mind at last has been led to the conclusion that, if the causes existing in our midst do not generate the disease, they favor its introduction and contribute greatly to its extension.

One point I think is satisfactorily established, in connection with its appearance in Mobile upon almost every occasion, viz, that it has invariably occurred in those seasons when large excavations have been made, or extensive surfaces of fresh earth exposed to the action of the sun and air during the heat of summer; while, on the contrary, our city has been almost as invariably exempt from this scourge in those seasons in which no such causes existed.

New Orleans, Louisiana.—The city is situated on the east bank of the Mississippi River, and between it and Lake Ponchartrain, about one hundred and ten miles from the Gulf of Mexico, at the river's mouth.

The site of the city is a swamp, considerably below the level of both the river and lake, and protected from inundations by levees from seven to ten feet high. The streets nearest the levee have been gradually reclaimed from the river by the deposit of the earthy and organic matters in the water, and the condition of the swamp in the rear of the city much improved by the digging of canals, building of shell roads, and a system of drainage. The whole soil of the swamp, as well as of the ground on which the city is built, is diluvial, and, until some time in the present century, (1822,) the streets were unpaved, and, entirely uncared for, in rainy weather becoming impassable from the mud. At present on either side of the streets are open gutters, into which great quantities of filth and garbage find their way, offending the sight and smell, in the poorer portions of the town, by the most horrible combinations. Vacant lots on the outskirts of the city often contain stagnant water; the back yards, alley-ways, and sinks are unclean, and evolve deleterious effluvia. Many of the cemeteries are in the most thickly-settled portions of the city, and the nature of the soil not permitting interments beneath the surface, burials take place in tombs above ground, with what effect on the olfactories of the neighborhood can easily be imagined. The poorer classes of the population, especially the foreigners from the south of Europe, called here "dagos," live in narrow streets, in crowded houses, in the French quarter, under the most unfavorable hygienic conditions; and it is in this quarter that yellow fever has oftenest committed the greatest ravages, especially in those streets in the vicinity of the docks.

Yellow fever did not make its appearance in New Orleans until 1796. During this year, and for two years previous, extensive excavations had been made in digging the canal Carondelet, and it has been very generally declared, by the advocates of the local origin of the disease, that it was due to the great overturning of the soil in this work. Unquestionably, this was a most prolific source of sickness. All writers agree that diseases of a miasmatic character were very prevalent, and that thousands of the workmen employed on this and subsequent enterprises of a similar character died. It may, therefore, well be that the spread of the disease was decidedly influenced by it. That it did not originate the disease, we are assured by Carpenter, (op. cit., p. 13,) who quotes from the Louisiana Courier of November 27, 1820, a positive assertion that "it was traced to a vessel which had brought it." We are, unfortunately, without any other particulars as to the name of the vessel or

the origin of the first cases. This remark is equally true of 1799, when there was "what was considered proof of its importation." It appeared after this in 1801, 1802, 1804, 1809, and 1811, epidemics of which I have been unable to find any details.

In 1817 the English ship Phœnix arrived on the 18th of June, from Havana, with yellow fever on board. The disease continued to prevail after her arrival, and, by the 30th, she had several deaths. On the 10th of July, the Virgin del Mar arrived from the same port with many of her crew sick. She had some deaths on her passage, others died on her way up the river, and still more after her arrival, "all with the black vomit." The first cases in the city were traceable directly to these vessels. The disease became epidemic about the middle of July, and prevailed until the last of December, causing about eight hundred deaths. (New Orleans Gazette February 3, 1818, quoted by Carpenter, op. cit.) Among the medical men of the day the opinion was unanimous in favor of the local origin of the disease, but in January, 1818, Governor Villeré sent a message to the legislature, stating that the proofs of importation were positive and recommending the passage of a quarantine law. This was accordingly done, in a very imperfect manner, although it was sufficiently effective to cause a number of infected vessels to be stopped at the quarantine station, and there was no epidemic this year in the city. The next legislature, however, repealed the act and directed the sale of the lazaretto. (Carpenter, op. cit., pp. 17–18; Dowler's Tableau, pp. 14–16; Faget, Premier Mémoire sur la Fièvre Jaune, pp. 21, 22.)

In 1819 occurred the most severe epidemic the city had known. It broke out about the 1st of July, among the shipping moored to the levee. There had been in June a number of arrivals of infected vessels, and numbers more arrived in August, several of which had lost men on the passage with black vomit. From the wharves the disease spread to the neighboring streets, and it was epidemic by the middle of August. "It assumed a character of the highest malignity; medicine lost its effects, the skill of the physician was baffled, and multitudes were carried to the grave." (Carpenter, op. cit., p. 18.) The Surgeon-General (Army Medical Statistics, p. 10) estimated the number of deaths at three thousand, but this is evidently an overestimate, as Dowler shows that the total mortality for the year was only two thousand one hundred and ninety. Doctors De Chambéry and Baxter both consider this epidemic to be of local origin, though the latter states that the first cases he saw were among the shipping in port. (Medical Repository, vol. 6, pp. 1–19.)

Early in January, 1820, Governor Villeré again urged upon the legislature the necessity for a stringent quarantine law. He admits in his message that his views are in opposition to those of the medical profession of the city, but believes quarantine to be of the highest value as a preventive. Nothing, however, was done this year, and about the middle of July cases of yellow fever made their appearance, at first among the shipping and sailor boarding-houses. An investigation, ordered by the governor, disclosed the fact that on the 17th of June a schooner, the Gold-Hunter, arrived from Havana, having had two deaths from yellow fever on the voyage; and on the 10th of July the brig Charles Fawcett arrived, having lost some of her crew. The disease prevailed in the city until the 21st of December. (Carpenter, op. cit., p. 19; Dowler, op. cit., p. 17.) During the progress of this epidemic a detachment of troops came down the Mississippi, *en route* to Bay Saint Louis, Mississippi, and were detained some time in New Orleans. They arrived

at Bay Saint Louis on the 20th of August, with one man sick with yellow fever, who died of black vomit twelve hours subsequently. Within twenty-four hours there were five more cases in the command, of which two died, and the disease spread among the troops stationed there. (Army Medical Statistics, 1819–'39, pp. 20, 21.) In December of this year Governor Robertson, the successor of Villeré, again urged upon the legislature the advisability of a quarantine. In consequence of this, in February, 1821, a quarantine law was enacted, by which a board of health was created, armed with the most extensive powers against the introduction of disease, and a quarantine was established below the city, at English Town. The board did not, however, seem to consider it necessary to protect the city from the importation of the disease by way of the lake, an omission fraught with grave consequences; for, although 1821 proved healthy, the year 1822 was signalized by a severe epidemic. Yellow fever broke out in Pensacola that year, about the 12th of August, and caused great consternation among the people, large numbers of whom fled the city. On the 21st of August two sloops, the Ann and the Eliza, both crowded with passengers, left that town for New Orleans, and entered by way of Lake Pontchartrain and Bayou Saint John, which brought them into the heart of the city without any quarantine inspection. Some of the passengers were sick on the trip and died. The official report of the board of health says:

> The researches made by the board at the commencement of the late epidemic lead them to believe that the yellow fever was imported toward the end of August last by a vessel from Pensacola arriving at the basin of Canal Carondelet, and attention was first attracted to the disease in a family by the name of Lynch, passengers in said vessel. This family, of which every member but one fell victims to the yellow fever, had removed to Bienville street, where the disease first spread, and from here extended through the city.

Besides this source of infection, Dr. Forsythe, health-officer at quarantine, reports the arrival of a large number of infected vessels at the quarantine station, and that he had no means of preventing intercourse with those on these vessels, which frequently took place, and that vessels were often permitted to go the city without being properly fumigated. This epidemic prevailed until November, the number of deaths being variously estimated. Dr. Dowler places it at eight hundred and eight, which, from his well-known accuracy, is probably correct.

This same author states that the year 1823 was remarkable for its salubrity. "In no year since the first irruption of yellow fever in New Orleans were the cases of the disease so few as in 1823." This does not accord with the statement of Monette, who says that cases began to appear among the shipping in July, becoming epidemic in the city about the middle of August. (Observations on Yellow Fever of Natchez, p. 65.)

In 1824 the fever prevailed epidemically. Its history, as cited by Carpenter, (op. cit., pp. 55–60,) was as follows, as regards the mode of its introduction: On the 20th of July, the schooner Emigrant arrived at the Southwest Pass from Havana, and was towed up to the quarantine ground by the tow-boat Balize. The steamer was made fast alongside of the schooner until she got within six or seven miles of the quarantine grounds, when she dropped the schooner astern and towed her by a line to the quarantine station. From affidavits published by Carpenter, it is proved that there were at this time two cases of yellow fever on the schooner, and that several persons from the tow-boat visited these sick men, one of whom was throwing up black vomit at the time.

On the 7th of August John White was admitted to the Charity Hos-

pital and died the next day of black vomit. On the 10th of August there were two cases reported on the steam-tug Post Boy, and on the 11th two more from the Belize. These were the first cases occurring in the city, and Carpenter has no doubt that the epidemic was due to this cause, as he shows by further testimony it was a common practice for the tugs to tow vessels in this manner and for there to be unrestricted intercourse between the boats. Of internal causes of the disease at this time, the chief are thus noticed by a message of the mayor of the city, dated September 11, (I quote from Dowler's Tableau:)

The internal causes are: 1st, the filth daily created in a populous city; 2d, the low grounds and pools where stagnant water lies, the wooden gutters constantly wet and fermenting under the rays of a torrid sun; 3d, the want of privies in most of the populous districts, which renders it necessary to recur to the disgusting and dangerous use of tubs. The external causes are: 1st, the marshes lying north and west of the city, uncovered but undrained, and deprived by the cutting down of trees of the shelter formerly afforded to them by the shade of a luxuriant vegetation, for which the very miasms that now spread death and desolation among us were a source of life and vigor, &c.

In 1825 the legislature repealed the quarantine code and broke up the stations, it having been in operation four years, during which time there were three epidemics. There was much controversy at the time on the subject, but the general opinion of the community was adverse to its continuance, believing that the facts had shown it to be of no avail against yellow fever and an arbitrary restriction on commerce. It may be doubted whether the unfortunate result of this attempt was not rather due to defects in the law and faults in its administration than to the falsity of the general principle involved; certainly the accounts of these epidemics, as well as the confession above noted of the health-officer, Dr. Forsythe, would seem to warrant this assertion.

There was a mild epidemic in 1825, the disease making its appearance about the middle of July. It does not seem to have prevailed very generally, the number of admissions to the Charity Hospital being but eighty-nine, with forty-nine deaths.

It is proper to remark that we have only the most meager accounts of all of these epidemics from 1825 till 1839, and it is very difficult to find any positive proof as to their mode of origin. The disease prevailed epidemically in 1829, and extended from there to Baton Rouge, Natchez, and Opelousas. It was asserted by Carpenter to have been introduced by Spaniards from Mexico, large numbers of whom had fled that country on account of the political troubles. The commencement of the epidemic was coincident with their arrival. (Sketches, p. 26.)

It was epidemic in both 1832 and 1833, and evinced peculiar malignancy, but I have been able to obtain no particulars as to its origin. In 1837 there was a very severe epidemic, there having been nine hundred and ninety-eight admissions to the Charity Hospital, of which four hundred and forty-two died. The first cases occurred on board the vessels recently arrived from the West Indies. The disease extended this year to various points in the interior. In another place I have given the facts relative to its introduction to Natchez by reason of the constant arrival of sick persons. Besides this it was carried by steamboat communication or otherwise to Baton Rouge, Opelousas, and Plaquemine, Louisiana. Dr. T. A. Cooke, in an essay on the yellow fever at Washington, Louisiana, gives some interesting facts relative to the introduction of the disease into that place in 1826 by means of infected clothing:

About the year 1826 Mr. L. Lonaillier, a merchant of Opelousas, imported late in the fall from the city, during the prevalence of yellow fever, some merchandise. Three individuals who were present in opening some of these goods contracted a violent fever; one died, and all were said by an experienced physician to have had yellow fever. In 1828 the same individual, with M. Lazaretti, another merchant, imported goods from the city during the prevalence of the fever. Mr. Lonaillier, ever regretting the error he committed in 1826—for he believed in the importability of the poison or cause of yellow fever—opened his boxes and bales and ventilated his goods for three days before taking them to town, and no disease followed from contact with them, whereas of four individuals present on opening the goods of Lazaretti, brought directly to his store in Opelousas, three died with black vomit, and the fourth narrowly escaped with his life. (New Orleans Medical and Surgical Journal, March, 1854.)

The year 1839 was a celebrated one in the history of the prevalence of the fever. At no time since its first appearance did it prevail over such a wide extent of country; never before had it so generally extended to inland towns. Monette remarks that these latter were invariably infected subsequently to its appearance in the coast ports with which they had most intercourse.

The first appearance of this disease in the United States during this summer was invariably in the maritime or commercial ports, and the first cases were invariably among the shipping in port, and especially among those which were direct from infected West India or Mexican ports. In every instance the disease for several weeks was confined exclusively to the shipping before it began to spread among the resident population. This fact is abundantly established by the concurrent statements of the public press in all the infected ports. In no part of the United States was a single case of yellow fever seen even on board the vessels until after it had been prevailing with great mortality for several weeks in the West India and Mexican ports. (Monette, op. cit., p. 78.)

There was no attempt at a quarantine in New Orleans, and the arrivals of vessels from infected ports were very numerous. The first cases occurred among the shipping about the last of June, and others throughout the month of July, until, by August 1, about twenty-five cases had been received into Charity Hospital from this source. It now began to appear in the streets adjacent to the wharves. The report of the medical society says: "The first sections affected were the streets of the old levee." By the 12th of August it was pronounced epidemic, and thenceforth raged violently, there being one thousand and eighty-six admissions to the Charity Hospital and four hundred and fifty-two deaths. The season was noted as being unusually hot and sultry, with a great drought. During this season the city was unusually cleanly, and, apart from the fever, there was a remarkable immunity from disease of all kinds. From New Orleans the disease spread to a large number of the interior towns of Louisiana, Mississippi, Alabama, &c. It was carried to Biloxi, Mobile, Pensacola, Tampa, and Galveston along the coast, and to very large numbers of the towns along the rivers. "In every instance where this occurred the *first cases* of the disease were *invariably traced* to New Orleans, and *only such towns* as had free intercourse with the city by steamboat were visited by the disease. Those towns which were cut off from such intercourse by nature or circumstances, invariably escaped the epidemic, although they might have double the population of others and might be only one-half the direct distance from the city. The first individual cases in any of these towns were either persons landed from steamboats with the disease openly developed in their systems, or persons who had recently left New Orleans with the infection dormant in their systems when they landed, but which soon after was developed in its most malignant form." (Monette, op. cit., p. 91.) Two or three instances will suffice, as the facts in nearly every case are alike. Donaldsonville, eighty-five miles from New Orleans, is a clean, healthy town, and had no case of yellow fever until after the 1st of September. About this time a number of persons were landed from steamboats and taken either to the hotel or to the lodging-houses

near the steamboat-landing, where they were sick and some died. By the 15th of September cases began to appear among the resident population, and the first taken sick were those who had visited or nursed the sick strangers. The disease continued to prevail until frost, giving rise to upward of thirty deaths.

On the other side, Baton Rouge, which at the time had but little trade with New Orleans, and whose inhabitants refused to allow of the landing of any yellow-fever patients, escaped the disease entirely.

At Waterloo, a few miles above Port Hudson, some creoles, believing they were exempt from the disease, visited New Orleans in the midst of the epidemic. On their return several of them sickened and died, and others took the disease, and there were about fifteen deaths in the place.

In a manner similar to what has been narrated of Donaldsonville, the disease was carried to Plaquemine, Port Hudson, Fort Adams, Natchez, Grand Gulf, Vicksburgh, on the Mississippi, and to Alexandria, on the Red River, the disease in every case making its first appearance in the vicinity of the steamboat-landing, or among those who had recently arrived from New Orleans. (Monette, pp. 77–120.)

Besides these river-towns, several of the villages along the interior bayous of the State suffered, viz, Franklin, New Iberia, Saint Martinsville, Opelousas, &c. It was brought to New Iberia as follows:

> A young physician, Dr. Smith, died of the disease at Plaquemine, and his remains were carried to his friends in New Iberia, and were exposed in the village-church according to usage of the Catholics. Many persons who entered the chapel for the purpose of seeing the remains, or for other purposes, took the disease, and many died with black vomit, and the disease was communicated from these to others. (Carpenter, op. cit., p 28.)

At Franklin, on the Teche, a man named Thompson went on board the steamboat Tomochichi, from New Orleans, and spent some time. The boat had several cases of fever on board. Soon afterward Thompson was taken sick. A family named Birdsall nursed him, and they were all taken, and several died. The neighbors who visited the Birdsalls next suffered, and the infection thence spread through the village, causing about twenty-five deaths. (Carpenter, p. 27; Monette, p. 113.)

In 1841, in 1842, and in 1843, the disease was epidemic in New Orleans, and presents the same history precisely as regards its first prevalence on the shipping and in the vicinity of the wharves as was the case in 1839. Like that year, also, it was carried to many towns in the interior, and, without exception, the earliest cases were either persons who had come from New Orleans, or else had communicated with steamboats from that place.

There was no epidemic after this in the city until 1847. The fever prevailed very early in the season of this year at both Havana and Vera Cruz, and, in consequence of our war with Mexico, immense numbers of vessels were constantly arriving from Vera Cruz during the spring and summer. Graham estimates (Sketches of Yellow Fever, p. 36) that there were over three hundred and fifty arrivals from infected ports between the 1st of May and the 31st of July, of which over two hundred were from Vera Cruz. Many of these vessels were Army transports, and some of them had sick soldiers on board, returning to be discharged in New Orleans. The disease was positively introduced by the United States steamers Massachusetts and Galveston, arriving on the 16th of June; a man named John Strider being the first case. There were about three thousand deaths. (Fenner on Epidemic of 1847, New Orleans Medical Journal, vol. 5, pp. 194–196.)

The year 1853, when the next great epidemic occurred, forms an epoch

by itself in the history of the disease only equaled in its wide-spread devastation by those of 1839 and 1867. Fortunately, we have the most complete history of it, thanks to the labors of Fenner, Barton, Axson, and others, and to the admirable report of the New Orleans Sanitary Commission. From the great mass of testimony taken on the subject, much of which is irrelevant, and more conflicting, I condense the following statement: There was a general impression among the people that the disease was imported from Rio Janeiro, and especially by the barks Siva and Home, which arrived early in May, and both of which had more or less fever on board during the passage; but Dr. A. F. Axson, after a careful investigation of the facts, could not trace any connection between the earlier cases and these vessels. But, on the 16th of May, the British ship Camboden Castle arrived at the mouth of the river from Kingston, Jamaica. This vessel had been for several weeks in that port, where she had lost her captain and seven of her crew with yellow fever. After these cases she was washed with lime and cleansed, and had no sickness on board on her arrival. She was towed up to the city on the 17th of May in company with the ship Augusta from Bremen. This latter vessel brought two hundred and thirty immigrants, had been out fifty-two days, and had passed to the south of Cuba, but not nearer than thirty-five miles from shore. Her passengers and crew were healthy. On the trip up the river she was only separated from the Camboden Castle by the tow-boat, and there was free communication between the two vessels. On arrival the latter vessel was dropped at Post 27, while the Augusta was taken some half mile farther up the river to the foot of Josephine street.

On the 22d of May, Dr. Schuppert was called to see a sailor on the Augusta, whom he found with well-marked symptoms of yellow fever. The same day a man named James McGingan was taken to the Charity Hospital from the ship Northampton, lying at the same wharf, and he died on the 28th with black vomit. This ship, Northampton, was also an emigrant-ship, and had arrived on the 9th of May direct from Liverpool, having passed about fifty miles to the north of Cuba. The most contradictory testimony was given in regard to this ship. The captain asserted that she was much cleaner than the average of emigrant-ships, while Mr. Pashley, a stevedore employed to cleanse the ship, asserts that she was so foul that his hands became sick, and he was obliged to employ a second gang and discharge the first. The truth is probably somewhere between the two extremes.

The next case of fever that occurred was, from the Augusta, a sailor named Gustave Woette, who was taken on the 25th, and died on the 30th. Two more cases occurred on the same ship on the 27th.

The next cases occurred on some ships that were moored near the Camboden Castle at Post No. 26. These vessels, the Saxon and Harvest Queen, had arrived in May, and were loading with cotton for Liverpool, and had not been near any infected port.

Then cases began to occur on shore, two of rather doubtful character, of the 2d of June, at Gormley's Basin, a filthy locality in the back part of the city. Dr. Moses Dowler attended these cases, and pronounced them to be yellow fever. Faget was of opinion that they were attacked with swamp fever, as they worked every day in the swamp, getting out shingles. The next well-authenticated case was a young Irish girl, from a boarding-house on Tchoupitoulas street, near Race, about one block distant, and right between the Camboden Castle and the Augusta. She died on the 11th of June. The same day a young man, James Murray, died in another boarding-house, corner of Race and Tchoupitou-

has streets. Subsequently other cases occurred in this house. From this time cases occurred in other portions of the city, having no apparent connection with each other. Several of these were at a boarding-house for sailors, in the vicinity of the mint, and two or three miles from the other cases. The epidemic reached its maximum about the last week in August, and then gradually declined, there being but twenty-eight deaths in November and four in December.

Barton estimates the total population of the city as one hundred and fifty-four thousand, of whom sixty thousand were unacclimated, and the total mortality at eight thousand one hundred, and that the total number of cases in the city was rather more than twenty-nine thousand. (Sanitary Report, pp. 253–257.) The summer was an unusually hot one, and the amount of rain-fall greater than during any other year except 1839. The sanitary condition of the city was as bad as it well could be. "In the winter and spring, when the weather was comparatively dry, the streets and gutters were in as good condition as is ever observed in this city; at all times bad enough, but when summer came with its scorching sun and frequent rains, the streets, alleys, gutters, battures, privies, vacant lots, &c., were neglected to such an extent by the street-commissioner, whose duty it was to have kept them in good order, as to call down upon him the indignation and the bitter denunciations of the press." (Fenner's Report on Epidemic of 1853, p. 9.) "It is worthy of remark that the epidemic prevailed in the unpaved, and, of course, least improved, part of the city all around, long before it did in the central and best improved parts." (Ibid., p. 212.)

The history of this epidemic, as regards its extension to other towns, is but a repetition of that of 1839. As soon as it became epidemic in the city, the boats were crowded with people fleeing from the pestilence. Dowler estimates the number that left the city at thirty thousand. These scattered in every direction all through the country; many of them were taken sick *en route*, and the same wide-spread desolation ensued as in 1839. At all the towns on the Mississippi, and at many of the plantations as far as Napoleon, Arkansas, up the Red River as far as Shreveport, along the coast to Mobile, and thence up the Alabama and Tombigbee Rivers, to Pensacola, Tampa, Galveston, Indianola, and many other places in Texas and Florida, and to various towns along the interior bayous, the disease was epidemic and very fatal. The history of nearly all of these is so similar that it is not necessary to state them, and I will merely give, as an example, a short account of the disease as it appeared in Washington, Louisiana, condensed from a valuable history of this epidemic by Dr. T. A. Cooke. (New Orleans Medical and Surgical Journal for March, 1854.) About the 1st of August, four colored persons, being a portion of a family who had left Washington for New Orleans about a year before, and who had lost three of their family in the latter city of the epidemic, returned to Washington. They stopped at the house of some acquaintances, where one of them had the fever. They had brought their baggage with them, and a trunk remained unopened for some days, and it was at length determined to open it and ventilate its contents. The man who opened it complained of an offensive smell which arose from it, and the things were taken into a yard behind the store. Here a man by the name of Miller spent some time in examining the goods or clothes in the trunk. This man, the proprietor of the store, and the clerk who opened the trunk, were all taken with fever within a few days. The next case was a colored man in the next house. Then some persons in the house where the person from New Orleans had been sick were taken and there were seven cases in this house. Next a

Mrs. King, who lived at a distance, visited the sick, and she was taken, and her death was followed by the sickness of every person in her house. And so the disease spread until there were upward of two hundred cases in this small village. Commenting on these facts, Dr. Cooke remarks:

> As far as I have been able to learn, no doubt exists this year that this fever has been transmitted, directly or indirectly, from New Orleans to *every place* in which it has prevailed as an epidemic, and I believe that the opponents of the importation of it into New Orleans must admit that there was this year at least a remarkable coincidence between the actual importation of the morbific principle and the actual occurrence of the disease, and it may be here remarked, inasmuch as in hundreds of certified instances which have occurred from year to year up to this time all over the country, and embracing also New Orleans, this pestilence can be legitimately referred to an imported cause; that, in the absence of overt acts proving its importation, a strong probability of this importation should be regarded as satisfactory as if proof positive existed. In the country towns the fact of importation can be, and is always, verified, and another admitted fact, indeed a necessary consequence of the the preceding one, is that it never prevails in the towns unless it previously exists as an epidemic in New Orleans.

On the subject of the value of this testimony from the small towns Faget says, (Deuxième lettre à la Société médicale de la Nouvelle-Orléans, p. 3:)

> Dans les grandes villes les faits de cette nature sont enveloppés de complications inextricables, et il est extrêmement difficile, presque impossible, de savoir à leur sujet toute la vérité, et rien que la vérité même à l'aide d'enquêtes consciencieiseses et sévères; dans les petites localités, au contraire, les faits se présentent avec plus de simplicité, et sans trop d'efforts il est possible de les approfondir assez pour que rien d'important n'échappe aux investigations. (See, also, Williams on Yellow Fever at Rodney, Mississippi, 1847, New Orleans Medical Journal, vol. 5, p. 217.)

There was a mild epidemic in 1854, of which I have not come across any details, but after this the disease did not prevail epidemically until 1858.

On the 8th of May of that year the ship Elizabeth Ellen left Saint Thomas for New Orleans. She had been two months in Saint Thomas, where the fever was at the time prevailing. She had a number of cases and one death among her crew on the passage. Arrived at the quarantine on the 4th of June. She was fumigated, and authorized to proceed to New Orleans the same day, and reached pier 23, where, at the same time, was lying the ship Independence, from New York, but which had been for several months in port. In the immediate vicinity were two other New York ships, the Trumbull and the F. B. Cutting, both of whom had been a long time in port. Six days after the arrival of the Elizabeth Ellen, *i. e.*, June 10, a young lady named Eustis, daughter of the captain of the Independence, (who had his family living on board) was taken sick and died with the yellow fever on the 22d. On the 14th her brother was taken sick, and died on the 20th, and a few days later a nephew of Captain Eustis. On the 27th Thomas Mervin, a sailor on the Independence, was taken to hospital, and died on the 7th of July. About this same time Captain Smith, of the Trumbull, Captain Lyons, of the Cutting, his mate, and his daughter were taken; the two first died. Two sailors were admitted from the Cutting into hospital early in July, and there were other cases from other vessels subsequently. While these events were transpiring, a Captain Healey, who had assisted Captain Lyons and his mate to transfer the body of young Eustis from an ordinary coffin to a wooden one, was taken ill, and died on his way north up the river. Dr. Delery, in his "Précis historiques de la fièvre jaune," reports that he attended on the 16th of June a young Irish girl, Catharine Maxwell, under circumstances which precluded the idea of any connection with the other cases. She died on the 20th.

Between the 25th of June and the 2d of July, four Italians died in Charity Hospital of yellow fever. They were all from the bark Rosalie, of Palermo, that was located at post No. 21, second district. At the same wharf was a ship recently arrived from Rio, and at a neighboring one was one from Havana. Such was the origin of this epidemic as narrated by Dr. Chaillé in the New Orleans Medical and Surgical Journal for November, 1859, and which I have quoted from Delery's "Précis historiques," not having Chaillé's original memoir at hand. It is proper to add that Dr. Moses M. Dowler, in an elaborate review of the testimony in reference to the Elizabeth Ellen, (New Orleans Medical and Surgical Journal, July, 1859,) considers the evidence entirely insufficient to convict that vessel of having been the origin of the epidemic, but ascribes it to the usual local causes by the argument ordinarily advanced by the non-believers in the importation of the disease.

After 1858 there was no epidemic until 1867. A rigid quarantine was established by the military authorities during the war, which, conjoined with the excellent sanitary measures undertaken by the military authorities, effectually prevented any epidemic. Dr. Fenner reports a fatal case of fever in 1862, near Saint Mary's market, (Southern Journal of Medical Sciences, May, 1866,) which was imported from Key West, and another case was seen in the same year, which was brought from the Bahamas, but there was no spread of the disease. (Chaillé, Life and Death in New Orleans, pp. 7–8.)

In 1863 there were but two deaths among citizens, but a large number of cases occurred among the vessels of our fleet anchored in the river, and several of these vessels were sent to the quarantine ground with fever on board. (Chaillé, op. cit., pp. 8–9.)

In 1864 the fever broke out on the fleet anchored in the river off the city. There were about two hundred cases distributed among some twenty-five gunboats and iron-clads, whose condition is thus described by Dr. Elisha Harris:

> Filthiness, crowding, excessive heat and moisture, and utter lack of ventilation and lighting, together with the stagnation of the local atmosphere of those oven-like boats, incident to anchorage in a tideless stream, constitute the leading facts relating to the infected vessels. (Sanitary Commission Memoirs, p. 264.)

There were, in all, one hundred and ninety-one cases in the fleet and twelve on shore, among persons employed on board and in the vicinity, and sixty-one deaths. Five deaths are also reported among citizens living in the vicinity. The cases were removed to the naval hospital in the city as fast as taken, and there does not seem to be any spread of the disease beyond its limits.

Assistant Surgeon J. J. Woodward (Report on Cholera and Yellow Fever in Army in 1867, p. 37) suggests that these vessels might have been infected from the Spanish frigate Pizarro, which arrived at the quarantine station July 4, with yellow fever on board. La Roche quotes Dr. Baldwin, health-officer at that quarantine, to prove that the Pizarro was there in 1863 and not in 1864, and Dr. Woodward informs me that this error in date was not ascertained until after the publication of the circular referred to. Assistant Surgeon G. M. Sternberg, United States Army, informs me that (he was the secretary of the board of health at the time) he is sure several naval vessels arrived with yellow fever on board, and that the authorities endeavored to detain them at the quarantine-ground, but were unable to do so, and he is decidedly of opinion that the disease was communicated to the other gunboats in this way. This does not agree with the views of Dr. Harris, who says:

> No vessel infected with yellow fever arriving by the way of the Gulf of Mexico was allowed to pass the quarantine-station, sixty-five miles from the city. (La Roche, Yellow Fever in Philadelphia in 1870.)

And adds further:

> Yellow fever was unequivocally generated in a large number of filthy and unventilated gunboats and other naval vessels lying idly at anchor within a mile from the densest portion of the city. (Sanitary Commission Memoirs, p. 266.)

As I have shown elsewhere in this report, the facts are indisputable as to the development of yellow fever in the filthy holds of ships, and I see no reason to doubt the correctness of the views of Dr. Harris, even granting the truth of the alleged arrival of infected vessels, as stated by Dr. Sternberg, who spoke merely from memory and not from official documents. The facts in regard to the Virginia, one of these gunboats, which are given in detail by Faget, (Second Memoir, pp. 10–13,) would seem to settle this point conclusively.

On the 14th of October, an officer on board of this vessel was taken with yellow fever, and subsequently to this the vessel had a large number of cases on board, which were removed to the naval hospital. The vessel had cruised in the Gulf during the winter, but since spring had been at anchor in the Mississippi. Now, this vessel, up to the year 1863, had been a slaver, and had landed a cargo of negroes on the coast of Cuba in that year. Shortly after she was captured by one of our gunboats off the coast of Mexico, and taken into Key West, and thence to New York, where she was converted into a gunboat. The bottom of her hold was thoroughly cleansed, but her hull was iron, *with double walls*, and this interspace between the two walls being inaccessible, was never cleansed. There was communication between this interspace and the hold, and as a consequence, when she was taken from dock and went to sea, the bilge-water became so foul from impurities that the pumps could not be used. She, however, remained a healthy ship until the fall of 1864. In September she was hauled in to the bank of the river for repairs, and her hold pumped out dry and cleaned. In consequence the impurities between the walls, which before had been to some extent covered by the water, became exposed to the air, and thus spread what might well be a source of infection throughout the ship.

The year 1867 was remarked by another wide-spread epidemic, extending over a large extent of country, and although not as fatal as those of 1839 and 1853, yet the disease was probably more generally prevalent than had been known at any previous period. The whole southern country was peculiarly adapted to receive the seeds of the disease. The population of all the large cities had been enormously increased by the mustering out of troops, large numbers of whom had remained at the South to seek their fortunes. Consequently the ratio of unacclimated persons to the total population was greater than had ever been known before. Moreover, the political condition of the country was very unsettled. There was still a military occupation of the South, and municipal regulations were in many cases divided between the military and civil authorities, and as a consequence the latter did nothing to improve the sanitary condition of the towns, which, if possible, was worse than had ever been known before. About the 1st of June there arrived at New Orleans about five hundred of the disbanded "imperial army" of Mexico, ragged, filthy, and poverty-stricken, and with the seeds of disease about their persons; in fact, the fever broke out among them on their subsequent voyage to New York. The fever prevailed very early and with severity in both Havana and Vera Cruz. There was no attempt at an efficient quarantine, and large num-

bers of infected ships arrived from various ports. The first case of yellow fever reported was a man named John Cowarts, a seaman, who died in Charity Hospital, June 10th. This man had been working for some time in the hold of the bark Bessie, which had arrived from Havana on the 22d of April, and had been since that time in the dry-dock at Algiers for repairs. This man was admitted to Charity Hospital from 261 Circus street, where there were subsequently a number of cases. A case terminating in recovery was reported in Saint Charles street, near Julia, on the 13th, and a third on Julia, who died on the 23d. The fourth case was John Bonaman, a stock-raiser, who arrived from Texas on the steamer Hewes on the 21st of June. This man came from Indianola, and while there had seen and handled some infected blankets exposed for sale in an auction-store. (I have given the full history of these blankets in describing the epidemic at Indianola.) On the passage this man was taken sick, and on arrival went to the Texas Hotel, near the stock-landing, where he died of black vomit on the 26th.

The next case died at the Charity Hospital on the 29th, and was a man who had been employed among the shipping in Algiers.

On the 30th, Lieutenant Orville Dewey, Fourth Cavalry, died at the St. Charles Hotel. He came from Indianola a few days before, and while there had slept in the same room at the Magnolia House where the infected blankets before spoken of were stored. The next series of cases occurred on the bark Florence Peters. This vessel left Havana on the 3d of June, arrived at the Southwest Pass on the 13th, and tied up at Algiers on the 23d. On the way up the river, Mrs. Hooper, wife of the captain, was taken sick, and on arrival, Doctor Francis Barnes, who saw the case in consultation, pronounced it yellow fever. She died on the 30th of uremia. On the 25th, two days after the bark arrived, a young lady aged seventeen, sister of the last patient, was taken ill, and the case presented decided symptoms of yellow fever. She recovered. On the 26th the child of the captain was taken, and on the 29th the mate, both of whom recovered. Next the second mate was taken, and died at Charity Hospital on the 8th of July with black vomit. On the 12th of July the captain died, and on the 25th a man by the name of Seth Yorke, who resided in New Orleans, and had been frequently on the vessel in search of employment. While these cases were occurring several persons died at the Texas Hotel, near the stock-landing, where the man Bonaman, from Indianola, had died on the 26th.

In the mean time, the fright of the people of Texas had been so great that many of them had fled in all directions, and in this manner the disease was carried to New Iberia, on the Teche. On the 12th of August, Doctor George W. Shields died at 220 Common street, having just come from New Iberia. Several other cases occurred soon after in this house. By this time cases multiplied so rapidly in town that it was impossible to trace them out. It became epidemic in the city about September 1, reached its maximum on the 25th, and thence gradually declined until about the middle of November, when it entirely disappeared. The number of deaths was over three thousand; the number of cases in the city variously estimated; probably over forty thousand were sick.

As in previous epidemics, the disease was carried to all the towns on the Mississippi and Red Rivers, to Jackson, Mississippi, Mobile, Alabama, and other places. In all of these there was the most positive proof of its introduction by the steamboats, or by passengers on the railways.

There has been no epidemic of any consequence since 1867. In 1870

it prevailed more or less in the filthy localities near the French market, and in 1871 in the vicinity of the Magazine market.

The first case in 1870 was a man by the name of Dinslow, who was an officer on the steamer Agnes, which arrived from Honduras on the 16th of May. He was taken sick on the 26th, and died on the 2d of June. There were no other cases in the vicinity of where he died during the year. The next case was a woman named Dumfen, who died on the 19th of August at 285 Old Levee street. The same day a case was reported of a Swede, about a block from this last house. The next case was a German girl named Lena Richs, from this same vicinity, who died at Charity Hospital, of black vomit, on the 24th. On the 26th a man by the name of Craig, from corner of Old Levee and Toulouse streets, died at Charity Hospital. This man had left Havana on the 17th of August, stopped in Mobile on the 20th, and arrived in New Orleans on the 21st, and was sent to hospital sick on the 22d. After this a number of Italians were sick in this neighborhood, and the disease continued to spread in the vicinity of the French market.

The next cases occurred in the first district of the city, the first one being an Italian from the French market, where the disease was then prevailing. He died September 6 at 186 Dryades street. The next day two cases were reported of Italians at 481 Rampart street. From these two foci of infection, viz, the vicinity of the French market and the locality last mentioned, the disease spread over radii of several squares, but at no time comprehended the whole city. Undoubtedly the vigilant measures of disinfection inaugurated by the board of health had much to do with its circumscription within such narrow limits. The epidemic continued until early in December, causing five hundred and eighty-seven deaths. (Report of New Orleans Board of Health for 1871.)

In 1871 the first case was a flat-boat man named Charles Collingwood, who died on the 4th of August at Charity Hospital. This was a dissipated, houseless vagrant, who had been in the habit of sleeping about the wharves, and who for four nights had slept on the bark Mary Pratt, which had recently arrived from Cienfuegos with sugar, and was then discharging her cargo. All of her crew had been discharged, and only one man slept on board, and the stevedores who were discharging her were acclimated. The vessel was immediately sent to the quarantine-ground, where she was disinfected and fumigated, and on August 13 she returned to the city and laid up at the foot of Terpsichore street. "August 23 George M. Monrsé was employed as steward, and began work on the Mary Pratt, cleaning up her cabin, reported to be in a very filthy state; but was taken sick with yellow fever on the 29th, and died at the Hôtel-Dieu September 5. The vessel was again taken in charge by the sanitary inspector of the first district, thoroughly fumigated for a number of days, and no unacclimated persons allowed on board."

The next case traced to this vessel was John Hock, a sailor living at 433 Tchoupitoulas street, less than two blocks from where lay the Mary Pratt. This man had been in the habit of walking down on the levee every evening, and unquestionably contracted the contagion from the infected ship, although the board of health very singularly state, "No exposure to any infected place could be ascertained." He died on the 5th of September.

Before this time—August 15—John M. Rawlins, a custom-house inspector, was attacked, and died on the 20th, at 118 Washington street. This man had been sent August 11 to the mouth of the river to take charge of the brig Hope, just arrived from Havana without any sickness on board. This vessel was detained at the quarantine-station two days.

During this time her hatches were opened, a portion of her cargo removed, and the ship fumigated. Rawlins, although cautioned of the danger, slept during this time in a hammock on deck in the immediate vicinity of the open hatches. Two days afterward he was taken sick. On the 7th of September Mr. E. W. Pierce, a member of the board of health, was taken sick, and died on the 13th. He lived at 136 Fourth street, distant about a hundred yards from the residence of the last case, across a block of buildings, with a cottage intervening between the two houses, and with the prevailing winds blowing from the direction of Rawlins's house toward that of Pierce. It was subsequently ascertained that two children were sick with the fever in the intervening cottage. From this time cases rapidly increased in this locality, and "by the 14th of October there had been twenty-seven unmistakable cases within a circle of a radius of one hundred and fifty feet, taking the house of Mr. Rawlins as a center." Cases continued to occur, chiefly in this vicinity, until the last of October—the total being one hundred and fourteen, of which fifty-five died. (Reports of the New Orleans Board of Health for 1871 and 1872.)

Before closing this subject it may be proper to mention that in 1870 yellow fever prevailed at New Iberia and various other places along the Teche, which was clearly traceable to infection from a number of returned confederates who had been brought back from South America.

It is proper to state that one great cause assigned by those who believe the disease to be epidemic in New Orleans has been the constant excavations made in the soil of the city and neighborhood for public works. The more important of these have been as follows:

From 1794 to 1797 the great canal Carondelet, connecting the waters of Lake Ponchartrain with the city, was dug.

In 1817 the commencing of the paving of the streets was undertaken—a work which was continued from 1824 to 1832, during which years a large portion of the soil of the city was upturned and exposed to the sun.

From 1832 to 1835 the canal of the Bank was dug. It was said from six to seven thousand laborers on this work lost their lives from malarious fevers.

In 1836 the work of dredging out the canal Carondelet and of draining and ditching the swamp in its neighborhood.

From 1835 to 1839 the forest growth on the swamp in the vicinity of the city was cut down and removed.

From 1845 to 1850 the section of swamp between the two canals was ditched and drained, and other canals dug. (Fenner's Southern Medical Reports, vol. 2, p. 134.)

The following account of the epidemics of Natchez and its vicinity is condensed from Monette's essay on the subject and the writings of Perlee, Cartwright, and others, in the various medical journals. They may properly be added to this report as showing the importance of guarding those interior towns which have communication with New Orleans, whenever yellow fever is prevalent in that city. The history of Natchez in reference to yellow fever does not differ materially from that of all the other places located on the Red and Mississippi Rivers, and I have selected it as a specimen, because, thanks to the industry of Monette and Cartwright, the history of the epidemic there is very complete.

Prior to the year 1817, the commerce of the Mississippi had been entirely carried on in flat-boats and barges. In that year steamboats were first used, and intercourse between the different towns on the river became, in consequence, much easier. Yellow fever, as we have seen,

prevailed in New Orleans as early as the 30th of June; but, with the exception of sporadic cases, in previous years, had never been in Natchez. Early in September the steamer Washington arrived from New Orleans, having several persons sick on board with yellow fever. Some of these were landed. Besides this, a number of young men who went on board to see the steamer, then a great curiosity, took the disease and died. From these two starting-points the disease spread with unexampled malignity over the city, and by the 28th of September was declared epidemic. The circumstances favorable to its propagation were stagnant pools throughout the city and great filth, and above all the exposure of a large quantity of fresh earth, in excavating, so as to correct the grade of some of the streets. Many of the inhabitants fled to the surrounding country; but the disease continued to prevail until one hundred and thirty-four had died, up to the 9th of November, when frost occurred and put an end to it.

In 1819, as we have seen, the fever broke out in New Orleans about the middle of August. By this time steamboats had become so common as to excite no unusual attention, and it is, consequently, not surprising that we cannot trace the introduction of the disease in Natchez to any particular boat; nevertheless it appeared there about the 1st of September, became epidemic by the 14th, and continued to prevail until December, when, of a total population of only five hundred and sixty-eight, one hundred and eighty had fallen victims to it. This year, as in 1817, it was very wet; but the thermometer did not indicate a higher grade of temperature than usual.

In 1823 yellow fever became epidemic in New Orleans early in August, having prevailed more or less for a month previous. It made its appearance in Natchez about the 10th of August, and was considered epidemic by the 20th. Large numbers of the inhabitants fled to the country. For a month the disease prevailed with violence, when the equinoctial gales occurring arrested its course for a week or two, when it broke out again and raged until frost. The population of the city was about one thousand, not counting the runaways, and over three hundred died. The weather was very sultry, with frequent showers. Numbers of cases occurred in small places in the vicinity of Coonville, to which refugees had fled. The local causes were alleged by Dr. Cartwright to be the deposition of carcasses of animals on the outskirts of the city, and to some spoiled bacon and fish at a certain store house in the city, and he ascribed the entire origin of the disease to these nuisances. (Cartwright, Medical Recorder, vol. 9, p. 5.)

On this point Monette remarks:

> These, however, we think were only contingent circumstances, which have often occurred to the same extent in previous years, and other epidemics have since prevailed, when all these circumstances did not exist; indeed, when those which have been considered most essential were entirely wanting.

In 1825 the first cases were near the steamboat-landing, "under the hill." The persons were those whose occupations would lead them to have frequent access to passing boats. This was about the 20th of August, when the fever had been for several weeks in New Orleans. It was confined for two weeks to the town "under the hill," when cases began to appear in the upper town, clearly traceable to sick persons who had been brought from below to their residences. On the 29th of August the board of health pronounced an epidemic condition of the city, when, as usual, all those who could get away ran away. Many of these went to Washington, a small town six miles distant, in a clean and elevated locality, carrying their bedding and goods with them. Some of

these were infected, and there were over eight deaths among them, when citizens of Washington began to sicken, and an epidemic prevailed there until sixty died. Monette says:

> The first citizens of Washington were my patients, and their infection could be traced clearly to opening and handling those bales of blankets and other goods brought from New Orleans and Natchez. The disease was imported into Washington as surely as it was into Coonville in 1823. (Monette, op. cit., p. 68.)

In Natchez, the epidemic continued until a frost, the 1st of November, and caused about one hundred and fifty deaths.

As usual, the epidemic was ascribed to putrid bacon, excavations, local filth, &c.; but Monette says:

> These causes had no agency whatever in producing the epidemic, and we have shown already that such causes are harmless as generators of yellow-fever miasm. We assert, without fear of successful refutation, that whenever these circumstances have synchronized with a yellow-fever epidemic they were only incidental circumstances, and not in any manner essential to its existence. (Op. cit., p. 69.)

In 1829 there was a mild epidemic, causing about ninety deaths. It broke out about the 1st of September, some weeks subsequent to its appearance in New Orleans, and prevailed until about the middle of November. It was ascribed to the same cause as the others. There was constant communication with New Orleans by steamboat.

The disease did not appear again in Natchez until 1837, when it broke out about the 8th of September, subsequent, as usual, to its appearance in New Orleans. Cases had been landed "in a moribund state from steamboats, direct from New Orleans." It prevailed until November 25, and caused two hundred and eighty deaths. The cause of this epidemic was unquestionable. By an act of the legislature, the Natchez hospital, which had been closed for several years, was re-opened for the reception of indigent sick. "It was in full operation when the yellow fever broke out in New Orleans, and scarcely a day passed without the reception of one or more patients from ascending boats, after the 1st of June; and after the 1st of August, nearly all of these were yellow-fever cases." "As a general remark, there has never been yellow fever in Natchez, unless when the hospital was open for the reception of indigent boatmen and others from the river."

In 1839 the yellow fever, as we have already mentioned, broke out in New Orleans as early as June, and was decidedly epidemic by the middle of August. During the earlier part of this season, Natchez was unusually healthy. The weather was dry and sultry. For a month after the appearance of the disease in New Orleans, there were daily arrivals of steamers from that place, and large shipments to Natchez of cotton and woolen goods, provisions, &c., for the use of the planters. There had also been several arrivals of parties of foreign emigrants, who lived crowded together in filthy, ill-ventilated houses. Moreover, during all this period there were quite a number of persons sick with the yellow fever, transferred from passing boats to the Natchez hospital; there were twenty deaths from this source alone up to the 20th of September. Some of the Natchez people had also visited New Orleans, and been taken sick on their return. About the middle of September the disease began to spread near the steamboat-landing, and soon after at the two hotels, both of which were crowded with refugees from New Orleans. It soon became epidemic, and by the middle of November, when it ceased, had killed two hundred and thirty-five persons. This year the authorities of the village of Washington established a quarantine against Natchez, and not a case occurred in their town. Natchez this year was unusually cleanly.

The city authorities, from the frequent statements made that the sole cause of the fever was the filthy condition of the city, had caused the whole town to be thoroughly cleansed and limed.

In 1841 yellow fever again prevailed at New Orleans, but the inhabitants of Natchez, warned by the fearful epidemic of preceding years, closed their hospital, and refused to permit any boats to land, and this year, for the only time in their history, they entirely escaped.

Vicksburgh, one hundred miles further up the river, placed "no restrictions on commerce," and suffered severely. Grand Gulf, Louisiana, which had frequently suffered before this year, followed the example of Natchez in prohibiting all intercourse with New Orleans, and escaped with but two cases.

The quarantine thus established at Natchez was kept up rigidly every season that there was any disease in New Orleans, and, as a consequence, the city continued free from yellow fever until 1853. In that dreadful year of pestilence, however, a number of persons arrived from New Orleans, and were taken sick before the quarantine was established for the season. (Graham: Yellow Fever, its Causes and Consequences, p. 14.) From these cases the fever became epidemic in Natchez about the middle of August. It was singular that the little town of Vidalia, Louisiana, directly across the river, became infected this year for the first time. Monette calls attention to the remarkable exemption of this town when Natchez was so frequently visited, and shows conclusively that it was due to the fact that the steamboats from New Orleans never landed there, but always made their landings at Natchez. Both towns were situated in precisely similar topographical conditions, with the same climate. One was frequently, nay, daily, visited by steamers from an infected port; the other, never. Result: the former had frequent epidemics, the latter none. This year, however, 1853, a sick woman was transferred from the Natchez quarantine to Vidalia, and soon after a family, with several sick, crossed from Natchez. In a very short time the disease became epidemic in the village.

The first case in Natchez in 1853 was on the 17th of July, in the person of a Mr. Pearsall, who died at the hotel shortly after his arrival from New Orleans. It was followed by several others in the same house. The next cases were of persons who fled from New Orleans. (Dr. Davis in Barton's Report, Epidemic of 1853.) I have said that the disease was absent from Natchez until the year 1853. Since writing the above I have come across an account of an epidemic in 1848, about which the physicians differed much, some calling it dengue, and others yellow fever. It was certainly very mild in its character; commenced about the middle of June in the district under the hill, and lasted until November. There was so much dispute on the subject that it hardly seems worth while to refer more definitely to it. This, though a very imperfect sketch, completes what I have thought proper to record of the visitations of yellow-fever to New Orleans and along the Mississippi Valley. The disease has frequently appeared in the places along the Gulf coast between New Orleans and Mobile, but except at Biloxi in 1702, never, without it was first epidemic, in either New Orleans or Mobile. An examination of the chronology of the yellow fever by Drake (op. cit., pp. 190, 191) will show the truth of this statement.

Galveston and other places in Texas.—Galveston is built on the west side of Galveston Island, the town now extending from its original location on the west bay, across the island, to the Gulf shore. The island is composed of shingle, and so saturated with water that it is found at a depth of but a few inches from the surface. A large portion of the

city is but a foot or two above the surface of the Gulf, and the highest elevation not more than four or five feet. The streets are wide, and, except in the business portion of the town, the houses usually separated from each other by a considerable space. Originally, along what is now Market and Mechanic streets (the second and third streets from the Gulf) existed a long morass, which was overflowed by the tide. In course of time this was filled in by the dirt and offal of the city, and now constitutes the most densely-populated portion of the town. Here, many of the houses are old; the inhabitants poor and dirty; the privies and cess-pools uncleaned; and everything exists favorable to a development of yellow fever. It is in this locality that nearly every epidemic of yellow fever has first made its appearance; or, if not commencing here, has prevailed with greater violence than in other portions of the city. In the rear of the city is Hitchcock's Bayou, a morass like the one before spoken of, extending into the heart of the city, and washed by the tide, which, when it is out, leaves great masses of putrifying materials exposed to the action of the sun. There are now many houses built in the vicinity of this bayou.

The mean temperature of Galveston is about as follows: In the hottest seasons, the mercury seldom rises above 90°; the mean temperature for the year being about 74°. Heavy rains usually occur in April, May, and June, while the latter part of the year is dry, and subject to north and northwest winds, which often cause sudden falls of temperature, producing great discomfort. (Heard, Medical Topography of Texas.)

Until the epidemic of 1867, the sanitary condition of Galveston was very bad; especially at the close of the war (when large numbers of negroes rushed there from all over the State, and lived crowded together in filthy houses, and reeking with dirt) its condition was as bad as could be. Latterly, however, under its present efficient mayor and health-officer, great attention has been paid to inspecting and removing nuisances, and now it will compare favorably with most any southern city.

The first visit of yellow fever to Galveston was in 1839. At this time the city had been settled but a few years, and the inhabitants were mostly located on a street called the Strand, running along the west side of the bay, and in immediate contiguity to the wharves for shipping. The population was about one thousand. Toward the last of September a steamer arrived from New Orleans, having a case of yellow fever on board, who died of black vomit four days afterward. The same day or the day following her arrival, the cook of a vessel anchored a few yards from the steamer took sick, and died on the 2d of October with black vomit. About the same time a Mr. Tichenor, living on the corner of Strand and Twenty-second streets, was taken sick, and the day following a Mr. Lang. Two other cases occurred between the 27th and 30th, and the disease soon became epidemic, lasting until the 7th of November, during which time there were two hundred and fifty deaths. Such is substantially the account given by Dr. Ashbell Smith, (although he makes no mention of the arrival of any steamer before the occurrence of the first case.) (Account of Yellow Fever in Galveston, Texas, in 1839, pp. 7, 8.) But the Hon. Hamilton Stuart, of Galveston, informed me that the universal opinion at the time was that the New Orleans steamer introduced the disease, but he differs in his dates from Dr. Smith, placing the first cases as early as the 20th of September; the latter placing the arrival of the steamer on the 27th, the day before the three first cases occurred. Unless, therefore, we allow for a shorter period of incubation than has been generally regarded in this disease, the question of importation must remain a matter of doubt; but it is not

improbable that the universal sentiment of the population at the time was the correct one, and that the apparent discrepancy grows out of some error in dates.

The yellow fever prevailed this year, for the first time, in Houston, and was supposed to have been imported from New Orleans. (Dr. William McCraven on Yellow Fever, in Houston, New Orleans Journal of Medicine, vol. 5, pp. 231, 232.)

In 1842 a few cases of fever occurred. The season had been unusually rainy, with a high range of temperature, yet the disease showed no disposition to become epidemic. I have not been able to learn any facts in regard to the origin of the few cases that did occur.

In the latter part of June, 1844, the United States steamer Poinsett, *en route* from Vera Cruz to Pensacola, touched at Galveston for supplies. She had on board a number of persons belonging to the unfortunate Santa Fé expedition, who had recently been released from a long imprisonment in Mexico. Two of these were landed at Galveston sick, and on the 5th of July one of them died, on the Strand, at a sailors' boarding-house, as was supposed, of bilious remittent. A comrade of his, who lived with a boot and shoe dealer, persuaded the latter to go with him and lay out the deceased. In a few days the shoemaker sickened and died, and other cases succeeded in the house of the latter and in the surrounding neighborhood. Meanwhile the other sick man, from the Poinsett, had been taken to another boarding-house, and this, likewise, became a focus for the spread of the disease, several cases having been taken in that house and the neighborhood. Still the disease was not recognized to be yellow fever until about the 10th of July, when a man by the name of Street, who lived directly opposite to the boarding-house where the first case occurred, was taken sick. He was seen by a physician from Florida, who had extensive experience with yellow fever, and he pronounced it to be that disease, and that evening Street died of black vomit. The Washington Hotel, in the immediate vicinity, now became infected, and many cases occurred there. It next spread from the Strand among the shipping along the wharves, where it was especially fatal. By the 20th of July it had become generally epidemic all over town, and raged with great violence for about six weeks, when it ceased suddenly, simply for want of material to feed upon. It was noticed, however, that any unacclimated person who came into the infected district between the period of the cessation of the epidemic and the access of frost took the disease. Mr. A. Ball, of Galveston, from whom I derived much valuable information as to these visits of yellow fever, furnished me with some curious instances of the recurrence of the disease, in isolated cases, long after the epidemic had ceased. Some weeks after the last death occurred in the city, the wife of Mr. Gail Borden, who lived some distance from the infected district, and who had not been near it during the prevalence of the epidemic, came up to town to do some shopping. She remained all day in town visiting various stores, and at night returned to her house, where, in a few days, she was taken sick, and died of black vomit. About four weeks after this, there having been no cases in the city in the mean time, a Mexican woman, who lived some miles down the island, came up to market, and she likewise went home and died of yellow fever; and some six weeks later, just before the access of frost, another lady, who had been absent from the city throughout the whole epidemic, returned to the city, and very shortly died. The population of the city, at this time, was about four thousand, and the number of deaths not far from four hundred. The earlier part of this season had been wet and hot, but, during the prevalence of the fever, a peculiar

balminess of the air was noticed, with fine breezes from the Gulf, and cool, clear nights.

From Galveston the disease was carried to Houston; the first case occurring on the 10th of July, in a person who had just arrived from that place. Other cases came from there in July, and in the latter part of the month cases began to multiply in Houston, and by August 15th it was epidemic, and prevailed until some time in September. (McCraven, op. cit., p. 232.)

In 1847 the disease re-appeared, but not until very late; the first case being a man living on Hitchcock's Bayou, back of the town, who, it was asserted, had been employed in unloading vessels from New Orleans. The fever was declared epidemic October 11, and ceased about November 25. The population was four thousand seven hundred and fifty-eight, and the number of deaths about two hundred. There was much dispute about the origin of this epidemic, and the question is still an undecided one. The Mexican war was then prevailing, and there was an unusual amount of commerce on the Gulf, especially from Vera Cruz, where the vomito had raged all summer. The Galveston Civilian of October 4, 1847, says: "We have had arrivals every week during the summer of vessels from infected ports, with diseased persons on board in some cases."

The city was now free from visits of yellow fever for six years. On the 12th of August, 1853, the steamer Mexico arrived from New Orleans, with three of her crew sick with yellow fever. They were sent to the hospital, and all died. Within a few days five cases occurred among the patients sick with other diseases in the hospital, and an epidemic followed, which carried off five hundred and thirty-five persons out of a population of eight thousand. The last death was on the 28th of November.

In 1854, it again appeared; the first case being on the 8th of August, on a vessel in the harbor. By the 12th, four cases had been brought to hospital from among the sailors and laborers on the docks, and the disease soon became epidemic, there being four hundred and four deaths up to its cessation on the 5th of November.

In 1858, the disease prevailed in New Orleans, and in the early part of August, a milliner in Tremont street, near Market, received some trunks of goods from that city. These were opened and handled by a young English woman employed in the store, who was taken sick, and died in a few days, with well-marked symptoms of yellow fever. Very soon after, a man who lived directly opposite, on the same street, took the fever and died, and within a week, a young man by the name of Hanna, living a few houses farther down on the same block, (and who, it was said, had visited the English girl when sick,) died of black vomit. The fever became generally epidemic by the 28th of August, and raged until some time in November, causing three hundred and seventy-three deaths out of a population of about ten thousand.

The next year, 1859, it re-appeared, was declared epidemic on the 17th of September, and lasted until November 30. There were one hundred and eighty-two deaths, the population being about the same as last year.

In the early part of the recent war the blockade was very strict at Galveston, and, as a result, her commerce was almost entirely cut off, the blockade-runners seeking Sabine and other smaller places in preference. As a consequence, there was no yellow fever at Galveston for several years; but in 1862 it was introduced, by means of a blockade-runner, at Sabine, and was thence carried to Houston, when an epidemic

of a mild character resulted. (Heard on Epidemic Diseases of Texas, p. 14.) It was also introduced this year into Matagorda, where, out of a population of five hundred, there were two hundred and fifty cases and one hundred and twenty deaths. (Harris, United States Sanitary Commission Memoirs, p. 240.)

In 1864 blockade-running was carried on to a considerable extent at Galveston, as many as five vessels from Havana being in port at one time. Dr. Hanna, of Galveston, told me that the disease was introduced this year by a schooner from Tampico, who ran the blockade at San Luis Pass and came up the back bay, where she was visited by a young boy, who soon after died of yellow fever, this being the first case; but I have not been able to learn the name of this schooner or the date of her arrival, and am more inclined to consider the general impression the correct one, that the Havana blockade-runners brought the disease. It became epidemic by the 1st of September, and there were two hundred and fifty-two deaths out of a population of five thousand five hundred. The interesting fact in reference to this epidemic was the total immunity of the troops in the forts around the city, in consequence of a quarantine established by them. There were then four forts in the vicinity of the town, each garrisoned by a large force of confederate troops. As soon as the fever became epidemic in Galveston, the officers in command of these forts declared a rigid quarantine against the city, and totally cut off all communication. Flags were placed on staffs at a quarter of a mile distance from the fort, beyond which no person was allowed to go. Every day the teamsters, mail-carriers, &c., who brought supplies and letters from the city, came out as far as the flag, where they deposited their burdens and then left; after they were well out of sight, a detail of acclimated persons from the fort went out and brought in the supplies and mails, being forbidden to hold any intercourse with any one. This was kept up until the close of the epidemic, and not a single case occurred in any of the forts. Per contra, at this time there was a confederate regiment, the Second Texas, stationed at various points in the city. The colonel of the regiment was advised to remove them, but did not do so until several cases had occurred among his men. They were then sent down the island for several miles, but the command suffered very severely.

There was no yellow fever in Galveston after this until 1867. In the spring and early summer of that year there was no quarantine, and steamers were almost daily arriving from both New Orleans and Indianola, as well as very many sailing-vessels. Yellow fever, as has been shown, commenced to prevail about the 1st of June in the latter place. The meteorological conditions at this time are thus given by Dr. S. M. Welsh, (Galveston Medical Journal, February, 1868:)

> The month of May was temperate, showery, pleasant, and remarkably exempt from all febrile diseases, nor was there anything to be observed in the type of disease to foreshadow yellow fever. June, however, was a month of uninterrupted hot weather, the thermometer ranging daily from 85° to 90°, with a breezeless, close, and stifling atmosphere. Toward the close of the month, from the 20th of June to the 5th of July, a period of two weeks, there were heavy falls of rain daily, literally flooding the streets, and accompanied with unusual electrical phenomena. In the intervals the sun shone brightly and with intense heat. It should be remarked that our municipal authorities had shown, for several weeks previous to the epidemic, unusual activity in cleaning the city. Everything seemed to have been done which prudence and foresight could suggest to ward off the scourge.

On the 26th of June, the first case of yellow fever occurred in Galveston, in the person of a young German, named George H. Moeller, who had just arrived from Indianola, and who died on the 4th of July, in a

boarding-house in the lower part of the city. There was, however, a case of a man by the name of Elliot, from New Orleans, reported on the 22d of June, at the city hospital; but there seems to be some doubt as to his case, as Assistant Surgeon Samuel Adams, in his report, (Circular No. 1, 1868, Surgeon-General's Office, p. 83,) makes no mention of him, and Dr. S. M. Welsh (Galveston Medical Journal, February, 1868) speaks rather doubtfully about it. On the 30th of June, a man who had been sick with syphilis in the city hospital, since the 17th, and who had tenanted a bed next to the supposed case of Elliot, took sick, with well-marked yellow fever. On July 1, a seaman, from a vessel in the harbor, was admitted to the city hospital, and two more July 5. Soon after two cases were found in the boarding-house where Moeller died. After this cases occurred in various parts of the city, "without apparent connection with each other;" and by the end of July it was unquestionably epidemic. The disase appeared among the troops about the same time as in the city; the first case being a private soldier, who was taken July 1, and which was rapidly followed by others. The disease raged with great violence in August and September. Early in October the number of deaths commenced to lessen, probably from lack of material, and in November there were but few cases. The population of this city was estimated at fifteen thousand, and of these, eleven hundred and fifty died of yellow fever. The mean average strength of the command of troops was one hundred and fifty-five, and there were one hundred and ninety-nine cases, and ninety-seven deaths; among them, many officers of great distinction and promise; viz: General Griffin, Colonels Abert and Swartwout, Drs. Taylor, Adams, and Rowe, and others. Dr. Welsh estimates the total number of cases in the city at about eight thousand.

It was not very long, after the disease became epidemic in Galveston, before it appeared in Houston. In the second week in August, the first case occurred, being imported directly from Galveston. By the 20th of August it had become generally epidemic. The first case among the troops (at that time encamped a mile from the city) was in the person of one Hale, a teamster, who was obliged to leave camp every day and visit the city to bring out drinking-water for the camp. This was on the 7th of September. On the 12th, Colonel J. D. O'Connell, commanding the post, who was boarding at the Hutchins House, in the city, was taken sick, and died on the 16th, and cases rapidly succeeded in the camp. During the whole epidemic there were seventy-one cases and twenty-five deaths, (out of a mean average strength of seventy-two.) The city was unusually filthy and muddy in consequence of the long continued rains of May and June. The range of the thermometer was unusually high during July, August, and September. Dr. F. Hasenburg, in his official report to the Surgeon-General, remarks:

> That the first cases of yellow fever observed in Houston occurred in persons who contracted the disease in Galveston is proved beyond doubt. (Report on Yellow Fever and Cholera, pp. 93-95.)

It is to be regretted that the doctor contented himself with this bare statement without giving his proofs.

In August, a man by the name of Vorhees went from Galveston to Hempstead, along the line of railroad, fifty miles from Houston, where he died of yellow fever. Other cases soon succeeded, and one hundred and fifty-one deaths occurred between August 9 and November 26.

On the 8th of August, Captain J. A. Devine, Bureau of Refugees, Freedmen and Abandoned Lands, came from Galveston to Brenham, took sick, and died on the 13th. Within two weeks there were two other cases, and between the 11th of August and 31st of October there

were two hundred and forty-six cases and one hundred and twenty deaths. There were no cases among the garrison stationed there, though encamped in the center of the town, which Dr. Warren, the post-surgeon, ascribed to his having administered every day a table-spoonful of white mustard-seed to each of the men.

At Navasota, on the line of the Texas Central Railroad, the proofs of importation were equally positive. The first case occurred on the 12th of August in the person of a man who had been in Galveston but a few days before. This man died on the 15th, and the next cases were among some friends who had visited the corpse, two of whom died on the 24th. Out of a population of about fifteen hundred, nearly every one had the disease between this time and the 1st of December, and there were one hundred and fifty deaths. (Report of Dr. A. R. Kilpatrick in Heard's Report on Epidemic Diseases of Texas, p. 33.)

Ten miles from Navasota is Millican. On the 15th of October, an ostler, belonging to one of the hotels there, took sick, having been in Navasota a day or two before. Next, the proprietor of the hotel and his wife died with black vomit, and a panic succeeding, nearly the whole population absconded. In all there were fifteen cases and four deaths. (Report of Dr. N. A. East, Heard, op. cit.)

At Chapel Hill, on the Washington County Railroad, a young lady, on the 6th of August, received a box of dress-goods from Houston, which she handled freely, and spent some time in examining. About the same time a young man received a box of books from Galveston. Both of these contracted the fever, and were the first cases. It soon became epidemic, causing one hundred and twenty deaths before the 1st of December. (Report of Dr. B. C. Meredith, Heard, op. cit.)

On the 4th of September it appeared at Alleytown, on the Colorado River, the first cases being employés on the railroad, who passed daily between that place and Houston. It soon became epidemic. At La Grange it was especially malignant, the first cases occurring about the last of August in the persons of refugees from Galveston and Houston, and soon spreading to others who had communication with them, prevailing with great severity until about the 1st of November, there being about eight hundred cases and two hundred deaths out of a total population of not more than fifteen hundred. (Reports of Drs. Hicks and McCown, Heard, op. cit.)

The towns of Columbus, Washington, and Richmond, though favorably situated for the introduction of the disease, established a rigid quarantine, and each of them escaped entirely. Dr. Heard says, (op. cit., p. 38:)

> So far as I have been able to learn, the origin of yellow fever was, during the past season, in every instance, clearly traceable to infection.

The meteorological conditions in all the above-named places presented a great similarity. Heavy rains until the last day of July; a very high temperature, much above the average; and winds, varying from north to northeast, constituted the most salient features. Nearly all these towns are dirty, and no attempt is made by the authorities at keeping them clean. In several of them malarial fever of an unusually severe type had prevailed previous to the outbreak of fever.

The first case of yellow fever in Indianola was in September, 1852. In that month a Mr. Jackson arrived by steamer from New Orleans and stopped at Sloan's Hotel, where he had a well-marked attack of yellow fever, but recovered. The inmates of the house at the time were Mr. Sloan and his wife, five children, a German servant-woman, and a negro

boy. On the eighth and ninth days after Mr. Jackson's arrival the whole family were taken sick, with the exception of one of the children. The German girl went to her home, and in due season the members of her family had the disease. The negro boy (who belonged to a General Woodward) was taken home by him, and communicated the disease to all of the general's family within the next twenty days. So far as the imperfect records at my disposal show, there were no local causes operating to produce or increase the severity of this epidemic. The town is built on a sandy strip of land, almost entirely surrounded by salt-bayous, with Matagorda Bay on its eastern front. Though but very slightly elevated above the Gulf, there are no known unfavorable hygienic conditions, and the place had always been free from malarial disease. (Dr. F. E. Hughes in Indianola Bulletin, December 16, 1870.)

In 1853 it again appeared. There was frequent communication with New Orleans by a regular line of steamers, and the first cases were two men, Sheppard and Stanley, who were employed in receiving goods from the steamers, and consequently spent nearly all of their time on or in the vicinity of the boats. The disease spread from these cases throughout the season, and a considerable epidemic resulted.

In 1858 it again appeared, the first case being an arrival from New Orleans by steamer. "As at Galveston, this disease never appeared before its being epidemic in New Orleans, and imported from there." (Dr. J. M. Reuss, Heard's Report on Epidemic Diseases of Texas, p. 15.)

In 1862 (exact date not known) the steamer General Rusk ran the blockade and landed at Lavacca, on Matagorda Bay, from Havana. She had one sailor sick with yellow fever on her arrival. A number of soldiers visited the ship and contracted the disease, and by them it was carried to the camp at Green Lake, and subsequently to Indianola.

In 1867 occurred the most severe epidemic ever known in the history of the town, and as this was the starting-point from which the disease spread through the whole State, and even to sundry points in Louisiana, a detailed account of it will be advisable. Dr. J. M. Reuss (in Heard's Report on Epidemic Diseases of Texas, read before the American Medical Association in 1868) thus refers to certain local conditions that no doubt influenced the course of the epidemic:

> In the rear of the town are flats, which are also parts of the town and subject to overflow. These flats, as well as the bayous in the rear, were overflowed before the occurrence of yellow fever, by fresh water, in consequence of continued rains, the water running in from the surrounding prairies. This brackish water, as well as the clearing and filling up of streets in May and early part of June, also the making of a road by the military to their camp, and throwing up a bank and digging ditches on both sides of this road, by which very offensive emanations from the ground spread by the prevailing south winds over the city, made, no doubt, the atmosphere a very good vehicle for the propagation of epidemic disease, and it required but the germ of yellow-fever miasm to inaugurate a fearful epidemic.

For many of the subjoined facts I am indebted to Dr. F. E. Hughes, the mayor of the city, and for others to the official reports of Acting Assistant Surgeon Samuel Santoire, United States Army. On the 20th of May the schooner Marguerita arrived from Vera Cruz, which port she left on the 11th. She had a large number of passengers, persons who had left the South at the close of the war to found a colony in Mexico, and were now returning, the colony having proved a complete failure. She had no sickness on board during her passage, or on her arrival, but yellow fever was epidemic in Vera Cruz at the time. Shortly after her arrival, the bedding, blankets, and other effects of the passengers were landed and taken to the Magnolia Hotel, whence, after being kept a few days, they were transferred to an auction-store to be sold. A

drayman by the name of Hunter was employed to take the things from the hotel to the auction-rooms, and in packing the blankets on the dray he was assisted by a boy named Henry Cook, who, after the dray was loaded, got on the pile of blankets and rode to the auction-rooms. Within a few days they were both taken sick, and both died on the 24th of June with black vomit. In the mean time the deputy-collector of customs, Mr. Prouty, who had boarded the vessel on her arrival, sickened on the 10th of June. There seems to have been some doubt about this being a case of yellow fever, and the length of time between his visit to the schooner and the date of his sickness is too great to warrant the ascribing the latter event to the former. About the next cases, however, there is no such doubt. Two men from the country, named James Duke and William Andrews, who were in Indianola for the purpose of shipping cattle, seeing the blankets advertised for sale, went into the auction-rooms and handled them very freely, but noticing some stains on them, they thought them dirty, and did not purchase. They left town that evening and returned home, where the following week both sickened, and one died with black vomit, as did also a negro woman who nursed them. Other cases soon occurred in the town of persons who had handled or bought the blankets, and by the last of June it had become decidedly epidemic. "In less than a week the whole business part of the town was struck down as by lightning, there being no less than one hundred and twenty-five to one hundred and fifty cases taken during that time out of a population of less than a thousand." (Reuss.) It next attacked the troops stationed there, the first cases being of soldiers detailed on special service in town, and thence spreading to the camp about a mile distant. By the 15th of July the disease had increased in violence, and there were many deaths, and the troops were then removed to Green Lake, twenty-two miles from Indianola. After moving, two soldiers took the fever and both died, after which there were no more cases. There were twenty-four cases and fourteen deaths in the command out of a mean average strength of fifty. The epidemic seemed to have ceased shortly after the 1st of August for want of material. The total number of deaths was about seventy-five among the citizens.

The seeds of the disease were carried in various directions by persons who left Indianola soon after the outbreak. As before remarked, it was carried to Galveston by a young German named George H. Moeller, who had been traveling in Western Texas, and who passed through Indianola on the 26th of June, and died in Galveston with black vomit on the 3d of July.

To New Orleans by two cases, one, John Boerram, a stock-trader, who arrived at the Texas Hotel, near the stock-landing, from Indianola, on the 21st of June, and died on the 26th; and the other, Lieutenant Orville Dewey, United States Army, who died at the Saint Charles Hotel on the 30th of June. It is a curious fact that Lieutenant Dewey occupied the same room at the Magnolia Hotel, in Indianola, in which the bedding brought from the Marguerita had been stored.

To New Iberia, Louisiana, it was carried by two men from Indianola, as I will show in detail in describing the epidemic in that locality. It appeared at Port Lavacca, twelve miles up the bay from Indianola, on the 3d of July, became epidemic about August 1, and lasted until some time in October.

At Victoria, thirty-eight miles from Indianola, it appeared about August 1, the first cases being infected persons from Indianola. It became epidemic about the 20th of August, and raged fearfully, there

being not less than two thousand cases, and a mortality of two hundred. (Heard, op. cit., p. 32.) It also extended to Goliad, a country-village twenty-eight miles from Victoria, and prevailed epidemically. Early in August it was carried to Corpus Christi, and became epidemic there, and was thence taken, by means of an ambulance loaded with passengers, to Rio Grande City, where nearly one hundred and fifty deaths occurred in a total population of about seven hundred and fifty.

Brownsville, Texas.—Fort Brown was established by our army of occupation in 1848, and the city of Brownsville grew up around it. By 1853, when the yellow fever first occurred, the population was about three thousand five hundred. The first case of fever was on the 23d of September, in the person of a citizen, but previous to this a number of cases of a malignant fever had occurred on the steamer Comanche, at the mouth of the Rio Grande, and several persons were known to have come to Brownsville from this boat. The fever gradually extended to Brownsville during the last week in September, and in the first part of October appeared in the garrison. It was very malignant in November, and was so prevalent that nearly every person suffered an attack. The last case was on the 23d of December. During the same time the disease was epidemic in Matamoras, on the Mexican side of the river. The number of cases in garrison was two hundred and forty-five, and the deaths, fifty. The number of deaths was much increased by the intemperate habits of the soldiers. (Report of Surgeon S. P. Moore, United States Army, Army Medical Statistics, 1839–'55, pp. 353–357.)

In August, 1858, the schooner Johnson arrived at Brazos Santiago, Texas, from New Orleans, having had one death from yellow fever on the passage. Soon after her arrival two of her crew were taken sick, and both died with black vomit. Some cases next appeared among residents at Brazos, next at Point Isabel, and at the mouth of the Rio Grande, and on the 20th of August the steamboat Mentoria arrived at Brownsville from the above-mentioned places, having a case of fever on board. On the 22d of August, a Mexican living near the steamboat landing was taken sick, after which there were several cases in this vicinity, and by the 1st of September it became generally epidemic in town. It broke out in the garrison of Fort Brown early in September. The epidemic reached its acmé in the first week in October, after which it declined, and entirely ceased early in November. Among the troops there were ninety-two cases and forty-one deaths. (Report of Assistant Surgeon A. F. Watson, United States Army, Army Medical Statistics, 1855–'60, pp. 182–184.)

In 1867, as soon as it became known that yellow fever was prevalent on the coast, the military authorities established a rigid quarantine at Brazos Santiago, and the Mexicans at Bagdad, on their side of the river. The roads leading to Corpus Christi were also placed under military espionage. The town of Brownsville remained healthy until October 7, when a great hurricane almost destroyed both it and Matamoras, after which it began to be very unhealthy, and on the 12th of October a German died of black vomit. Other cases succeeded, and the epidemic continued with considerable severity until January 10, when it suddenly subsided immediately after a frost, having caused about two hundred and fifty deaths. *The garrison immediately adjoining the town was placed in strict quarantine, and no cases occurred there.* The first cases were two Mexicans, who were sick between the 10th and 15th of September. There were several other cases that month, and the first death occurred (as before stated) on the 7th of October, in a low boarding-house, occupied by Germans, and, at the time, containing some imperialist

soldiers, just arrived from Mexico. There were three deaths in the house before the 12th of October, all of Austrians, and during the next ninety days many others. There was some sickness in Matamoras, but not until after its appearance in Brownsville. (Report of Assistant Surgeon E. Cowles, United States Army, Circular No. 1, Surgeon-General's Office, 1868, pp. 101–104.) There is no room for doubt that the disease was brought to Brownsille by the Austrian refugees, many of whom had come from Vera Cruz and Tampico, and other towns on the coast of Mexico.

Sabine, 1863.—In the latter part of July, 1863, the blockade-runner Victoria arrived at Sabine from Havana. She had no sickness during the passage, but came up to her dock and commenced unloading her cargo, without any examination of the ship by the health authorities. A few days after her arrival a man and a boy, by the name of Hartley, who lived on the shore, about two hundred yards from the dock, and who had been employed on the steamer since her arrival, were taken sick and died. Soon after this a stranger in town, who lived at the dock where the steamer lay, was taken ill and died with similar symptoms. Other cases followed in the house of the man Hartley, and in two weeks seven of his family were down with the disease. At this time there was a large force of confederate troops encamped in the vicinity, in anticipation of an attack from our forces. Some of these men coming into town were taken, and, being nursed in various houses through the place, spread the disease in every direction. (At all events, the disease extended through the place until a large number of deaths occurred.) Up to this time the disease was not supposed to be yellow fever, but, the epidemic becoming so widely spread and so fatal, its true character was recognized. I have not been able to obtain any figures as to this epidemic, but it raged fearfully until about October 1, when it ceased simply for want of material, the troops having been moved away during its progress.

The sanitary condition of the town was especially bad. Probably Sabine will carry away the palm for being the filthiest hole in the United States. Used chiefly as a cattle-port for shipping the beeves of Eastern Texas to New Orleans, the street bordering on the river and in the vicinity of the wharf was almost ankle-deep in cow-manure, and never cleaned, while the houses in the vicinity are old, rickety, and the yards filled with pools of stagnant water, filthy sinks, and everything that could contaminate the air. Nevertheless, this condition of affairs had existed for years, and yellow fever had never made its appearance there before; it has existed ever since, without a second outbreak. To add to the above, this year, a man living on the shore, not far from the wharf, had constructed a levee in front of his house three or four feet high, composed of oysters which he had dredged up, and these dying gave out a most unwholesome effluvia, which no doubt intensified the local causes for the epidemic.

From Sabine the disease was carrid to Beaumont, thirty miles above, and thence by rail to Houston, where it raged with great violence, as regards the number of cases, though the mortality was small. (Information furnished by Mr. Gibney, Sabine Pass.)

A review of the foregoing pages will show that the yellow fever, while not following any definite law in its visitations, has yet been more generally prevalent in some years than in others. We find certain periods in which the disease has been of frequent occurrence in many places; certain others, in which it has been absent for a long series of years. Thus, from 1699 to 1765 there were occasional epidemics in

New York, Philadelphia, and Charleston, and one at Pensacola; after which it disappeared entirely from the country until 1791, when a great series of epidemics took place, the disease extending to a great number of small towns in the vicinity of, or in constant communication with, the larger cities.

These continued almost yearly until 1807, when, with the exception of New Orleans, the cities and ports of the United States were again exempt until 1817. A third series of epidemics now occurred, lasting until 1822; after which there was no general prevalence of the fever until 1839, when the whole southern country suffered severely, yet there was no appearance of the disease north of Charleston, except a slight outbreak at Portland, Maine. From 1853 to 1856 commenced another series of epidemics, beginning at New Orleans, and along the Texas coast in 1853, and culminating at Norfolk, Virginia, in 1855, and at Fort Hamilton, New York Harbor, in 1856. The last of these periods was 1867, when again the whole Mississippi Valley and Gulf coast suffered, as well as a large portion of the State of Texas. Now, it is noteworthy, in connection with this grouping together of epidemics, that these widely-extended outbreaks are much less frequent of late than they were formerly. From 1839 to 1853 the country was comparatively exempt from the disease; and again from 1856 to 1867, if we except the years 1858 and 1862, when there were epidemics at New Orleans, Charleston, and Wilmington, North Carolina, and since 1867 the South has suffered from no general epidemic. It is also to be remarked, that in each one of these extensive epidemics the evidence is positive somewhere that the disease has been imported. Though the evidence might be defective in one place, yet it was not in another, and, moreover, in these periods the disease is known to have prevailed with violence, either in the West Indies, the coast of Mexico, or Brazil, or else there were specific causes (to be alluded to hereafter) which led to its introduction into the United States. Moreover, it will be seen that the prevalence of the disease in towns away from the coast has always been coincident or immediately subsequent to its appearance in the sea-ports having most general communication with the section of country where the disease prevailed. I am aware that a reported epidemic at Gallipolis, Ohio, in the last decade of the eighteenth century, is often quoted as an instance of a perfectly-isolated epidemic; but after carefully reading Mr. Ellicott's account, and the arguments based upon it, I am inclined to consider the evidence defective as to this disease being yellow fever at all, and the same may be said of the reported epidemic at Woodville, Mississippi, in 1844. Certainly, the report of Drs. De Valetti and Logan (New Orleans Medical Journal, vol. 1, p. 237) is not satisfactory on this point; and Dr. Faget, of New Orleans, after examining the subject, came to the conclusion that this outbreak was of a malignant form of paludal fever.

Another point to be noticed is, that New York and Philadelphia have been free from the disease since 1822, (except in the latter city in consequence of importation by the bark Mandarin in 1853,) in which year (1822) both cities established a rigorous system of quarantine, which has been kept up ever since; while previous to this time they were the most frequently visited of all the American cities.

The influence of the commercial prosperity of the country in producing epidemics of yellow fever has been ingeniously argued by Carpenter, Hume, Faget, and others. They have shown in the most conclusive manner that from the earliest period in our history, epidemics of yellow fever have occurred in a direct ratio to the amount of commercial intercourse between our principal ports and the West Indies. Very soon after the settlement of the colonies, the importance of the commerce with the West India Islands attracted the attention of the colonial merchants, and an active trade in lumber, horses, fish, &c., was carried on. This excited the jealousy of the mother country, and a long series of acts were passed by the Imperial Parliament, commencing about the middle of the seventeenth century and ending about 1760. These, known as the "commercial monopoly," had for their object to divert the trade of the colonies from the French, Spanish, and Dutch Islands, and to turn it toward those islands owned by Great Britain. Still, in spite of these enactments, the trade flourished for many years; but after the first quarter of the eighteenth century it began to languish, and by 1765 had nearly ceased altogether. As I remarked, in writing of the epidemics of Charleston, with the cessation of this trade yellow fever disappeared from the country. The war of the Revolution followed, when, of course, we had no trade. But after the close of the war came a great demand for West India products, and a general revival of trade took place, which attained great proportions between 1790 and 1805, and it is precisely in these years that we find yellow fever most frequent in all our commercial ports. Hume shows (Charleston Medical Journal, vol. 15, pp. 22, 23) that in the earlier history of the West India trade with Charleston, it was carried on entirely with small schooners owned in either Charleston or the West Indies, and always manned by acclimated crews; and that, consequently, the appearance of the disease was comparatively rare; but that with the revival of trade, after the Revolution, and especially after the passage of the navigation acts of 1789 and 1792, the New York and New England merchants saw a wide field for enterprise, and vast numbers of northern vessels engaged in the traffic manned and commanded by unacclimated New Englanders, and which rarely came into port without bringing one or more cases of yellow fever. During the ante-revolutionary period the Gulf ports were not in possession of the United States and carried on but little trade, and (excepting its appearance at Mobile in 1702 and 1765, and at Pensacola in the latter year) yellow fever was unknown among them. After 1791 a considerable trade with the Spanish West Indies sprung up in New Orleans, and yellow fever arrived there for the first time in 1796. Norfolk, Virginia, which had a large West India trade, had the fever almost yearly, and Portland, Boston, and Baltimore derived theirs from the same source. In 1807 all these places became again suddenly exempt. Why? Because in that year the embargo was laid on all trade with the islands. This continued in force until 1811, and then the war with England followed, lasting until 1815, so that the West India trade was not re-established before 1817. The fever appeared in 1809 and 1811, in New Orleans, a fact easily accounted for, if we remember that her trade was almost entirely with the Spanish West Indies, and the embargo, in so far as it related to those islands, was suspended in 1809. With the renewal of commerce, after the war, came also yellow fever: at New Orleans, Charleston, Philadelphia, and other places in 1817, at Baltimore in 1818, throughout almost the whole coast in 1819, 1820, and 1821. In 1822 New York and Philadelphia perfected their system of quarantine, and they have remained free from fever ever since, except at Philadelphia, in 1853, when there was a small epidemic, owing entirely to carelessness in permitting the bark Mandarin, from an infected port, to come up to the city. It has been repeatedly brought to the quarantine-stations in those cities, and has spread from the stations to the surrounding country on several occasions, as witness the memorable

epidemic in 1856 at Fort Hamilton; but the cities themselves have escaped. Now, to come down to our late civil war. In 1862 the southern ports were blockaded; but Wilmington was the principal resort of the blockade-runners by reason of its convenience to Nassau. Consequently we have in 1862 one of the most destructive epidemics ever known in Wilmington. During the first two years of the war the trade of Galveston was entirely destroyed; but in 1864 blockade-runners began to enter that port, and in that year the city experienced a great epidemic. Sabine Pass is a place absolutely without any commerce whatever except of a local trading character, but when the war broke out, from its insignificance, being but slightly blockaded, it became a favorite resort for blockade-runners, and before many months the whole of Eastern Texas experienced an epidemic. New Orleans being very early captured by the National Army, her trade was not interrupted, but the military authorities established an absolute quarantine, and for the first time in its history the city was thoroughly policed and cleansed, and there was no fever there during the whole war except in 1863-'64, on the fleet, an occurrence of which I have given an explanation in another place. Charleston had the fever but once during the war, and then it was brought in by a vessel running the blockade. The close of the war witnessed not only the revival of trade throughout the South and the thronging of all the southern cities with unacclimated adventurers, but, also, in 1867, the most wide-spread epidemic known in the history of the country.

As regards the introduction of the disease by blockade-runners, they were of all vessels (with one exception, to be noted presently) the ones most likely to become infected, or to originate the disease. In the first place, their ports of departure, Nassau or Havana, were almost always infected; the vessels themselves were small, never cleansed, most horribly filthy, and during the passage, from the very necessities of their traffic, run at so high a rate of speed that firemen had been known to die of the excessive heat. As a result, heat and moisture were always present in their holds, the two most efficient factors in putrefactive decomposition, and the third, vegetable or animal *debris*, was abundantly strewn everywhere throughout the ship. Consequently, they needed only infection from the shore to become veritable pest-houses, and they carried yellow fever with them into every port they went. Faget (op. cit.) calls attention to the remarkable fact that the yellow fever has been for so many years a disease from the western side of the globe, and is entirely unknown in the Indian seas. To be sure, it was many years known as the "Mal de Siam," but it is not recorded that the disease existed there on the departure of the Oriflamme, and the fact that she stopped at Brazil on the passage renders it probable that she received the infection from that source. However that may have been two hundred years ago, the fact is unquestionable that for the past hundred years yellow fever is localized in the countries bordering on western, and is unknown in eastern seas, if we except, perhaps, the recent great pestilence at Mauritius, and it is a matter of great doubt whether the fearful mortality of that epidemic was occasioned by yellow fever or not. Now, the fact of its prevalence almost exclusively in the West Indies opens another and very interesting phase of the influence of trade on these visitations.

When the slave-trade was first established it was carried on under direction of the Government purchasing the negroes, and was surrounded by few of the horrors which have made it so infamous in later times. Carried on under Government inspection, the vessels were larger and

well ventilated, the food served was sufficient, some care was given to the unfortunate victims, and the whole trade partook rather of the nature of transport service. Moreover, these vessels once employed in the trade were never withdrawn from it, but after landing a cargo were properly purified and dispatched after another. In time, however, the traffic passed into the hands of private parties, who, actuated solely by the desire for gain, employed small vessels, into which the poor negroes were crowded and packed in the hold worse than any cattle, were insufficiently fed and watered, obliged to perform the offices of nature where they lay, and the vessels themselves, being never properly disinfected, furnished altogether a combination of foci the most favorable for the development of the germinal principle of the disease. Moreover, very often these slavers, having completed the voyage from Africa, took a cargo from the West Indies to Europe. In this way the fever originated at Passage in 1823, and on board the Virginia, at New Orleans, in 1864. Now, the appearance of the yellow fever in the West Indies, and the prosperous epoch of the slave-trade, are almost synchronous; and it was during its continuance that the most destructive epidemics have occurred in the West Indies. With the suppression of this West India traffic, through the combined activity of English and American cruisers, the slave-trade to Brazil enormously increased, and it is then, in 1849, that for the first time in a century and a half yellow fever makes its appearance in Rio Janeiro and the South American coast. Of course, it is not intended to offer this as the sole cause and origin of yellow fever—the facts elsewhere narrated forbid such a supposition—but merely to present it as one, and perhaps not the least. (Faget, Quatrième lettre, pp. 9, 10; Audouard, Annales maritimes, vol. 1, p. 289.)

This last cause is a thing of the past, and need only occupy attention for a moment as a part of the history of the disease in former years; but the operations of legitimate commerce with the West Indies are increasing every year with the increased prosperity of the country. The past shows that the dangers resulting therefrom are not imaginary. It is not at all necessary to prove that the disease is always an imported one to render precautions against its introduction either advisable or necessary. The most earnest advocates of quarantine are willing to grant that certain, nay, many epidemics have occurred in which evidence is defective to prove the fact of importation; yet because the actual vessel that brought the disease was not found, nor the connection traced between some such vessel and the first cases, it by no means follows that it did not exist. Every person who has investigated the earlier cases of an epidemic in any of the southern ports knows how very difficult it is to get at any definite facts relative to the antecedents of the sick persons. Their own condition is such that they can give no connected account of themselves, often not being seen until they are brought to the hospital in a moribund state, and those by whom they are surrounded are frequently ignorant people, in the lower walks of life, who imagine some ulterior motive in the inquiries made by the health-inspector, and either refuse to give any information at all, or else falsify the facts. Hence it follows that the testimony in favor of the local origin of an epidemic is generally of a purely negative character. The fact of importation not being proved, therefore it did not exist, and, as there is usually a convenient cow-yard, privy, burial-place, or wharf in the neighborhood which is redolent with filth and unwholesome effluvia, or uncleaned gutters, or crowded houses, or extensive excavations for building purposes, which have their share in rendering the locality unhealthy, these are considered sufficient to account for all the

phenomena, and investigation ceases just where it should have commenced. Nevertheless, granting for the present that in some epidemics the fact of importation is not proved, yet in very many more the testimony is so strong that it must carry conviction to every unprejudiced mind that yellow fever has been brought into the country in the vast majority of cases in which it has appeared here epidemically, and that it has frequently been kept out by proper precautionary measures. I shall endeavor to show, before concluding this report, that the quarantine has failed to be always effective in the United States, not from any defect in the principle involved, but from insufficiency in most of the present laws and a want of their proper administration.

EPIDEMICS IN FOREIGN COUNTRIES.

The question of the possibility of carrying the infection of yellow fever by means of ships, and its introduction into localities where it has no congenial home, is strongly illustrated by many of the visitations of the disease to other countries than our own.

A full account of such foreign epidemics would fill too large a space, and they are already so familiar to all students of the subject that only a brief allusion to them will be necessary; consequently I shall not endeavor any complete description of them, but only of a few, the history of which have been carefully investigated by others, and afford additional evidence of the portability of the disease.

Ascension Island.—Ascension is a lonely island, situated many miles from any other land; is in the South Atlantic Ocean, in latitude 7° 56′ south, longitude 14° 25′ west. On the 27th of March, 1823, the British sloop of war Bann left Sierra Leone, coast of Africa, (where yellow fever had been epidemic for some months,) for Saint Thomas, but the fever breaking out among the crew she was compelled to put into Ascension and land her sick. She arrived on the 27th of April, and by the 2d of May had buried thirty-two men, at which date Her British Majesty's ship Driver arrived at Ascension.

All intercourse between the garrison and the sick-tents of the Bann was forbidden, and as much as possible between the men of the Bann and those of the Driver. In a few days an admiralty clerk, belonging to the Driver, (sent on board the Bann to assist at a survey,) and Captain Sawmarez, and his servant, (sent to join the Bann,) were all seized with fever. About the same time the fever made its appearance among the garrison ashore, in the family of a soldier's wife who had been washing for one of the Bann. It first seized a boy, and then the woman herself, and in a few days four men belonging to the garrison were attacked. Of the crew of the Bann, consisting of about one hundred and thirty, not so many as ten escaped fever, and thirty-eight died, and on the island of Ascension, of the garrison, consisting of thirty-six souls, five only escaped fever, and seventeen died, and of about eight from the Driver, exposed to the contagion, four were seized and died. (Report of Assistant Surgeon Sinclair, Her Majesty's steamer Bann, quoted by Ferguson, London Medical Gazette, vol. 24, p. 840; Barnett's Official Report of Fever on Her Majesty's steamer Bann, pp. 3-26.)

This was the first visit of yellow fever to the island, and it did not appear again until 1837, when it was carried by the brig of war Forrester from Sierra Leone. The commanding officer of the brig died on the passage, and his wearing-apparel and other effects were sold in the garrison. Soon after the yellow fever broke out in the command, and two officers and a number of men died. (Ferguson, op. cit., pp. 840–842.)

The local circumstances which we have found so uniformly present in other epidemics were not wanting, for a large mud-pit had been filled up during the season in the vicinity of the garrison, and this had been rendered unusually filthy by remarkably heavy rains.

EPIDEMICS IN SPAIN AND OTHER PARTS OF EUROPE.

It seems to have made its first appearance in Europe in 1741, when it was said to have been introduced into Malaga by means of a trunk of goods. In 1764 it prevailed at Cadiz; some sailors from a vessel recently from America being the first attacked; next, some persons who lodged in the same house with them, and afterward those in neighboring houses. In 1800 it was again introduced into Cadiz and Seville, by several vessels from Havana, and, after causing great mortality in the city, spread over a large portion of Spain, being communicated by inhabitants fleeing from the first-named city. In 1804 it prevailed at Leghorn, under circumstances that seem to leave no doubt that it was introduced by the Spanish ship Anna Maria, which arrived at Leghorn on the 18th of August from Havana. She had fever on board during her passage, and endeavored to enter at Cadiz, but was prevented by the authorities, who, nevertheless, it is stated, gave the vessel a clean bill of health, with which she entered at Leghorn. Shortly after two of her crew died at an inn on shore, and ere long twelve lodgers at the inn had died of yellow fever. Meanwhile some bread had been carried on the vessel in sacks, which remained on board several days, and on being returned to the bakery were used as beds by the workmen, a number of whom took the fever and died. There were a number of other deaths of persons, whose connection was directly traced to the ship, being employed thereon as calkers, unloading cargo, &c. Some of the cargo was transported to a warehouse on shore, and the men who carried it died, and many cases occurred in the vicinity of the warehouse. In 1819 it again appeared at Cadiz, immediately after the arrival of several vessels from Havana, that had fever on board. But the most dreadful visitation of the disease ever known took place in the city of Barcelona in 1821. The disease made its appearance in Barceloncette, the port of Barcelona, immediately after the arrival of a large convoy of vessels from Havana, ten of which had yellow fever on board. The physicians of the health board seem both to have mistaken the nature of the disease and to have neglected all precautions against its spreading, and cases multiplied with great rapidity, extending from the port to the town, until, in four months, no less than seventeen thousand persons had died of the epidemic. From Barcelona the disease spread to many other towns, as far as Marseilles.

The French Academy sent a deputation of physicians to investigate the epidemic, and this is the conclusion to which they arrived:

La première apparition de la fièvre jaune à Barcelone a coincidé avec l'arrivée de bâtimens qui venaient de lieux où règne habituellement une pareille maladie. Il paraît même que les premiers malades appartenaient à deux ou trois de ces bâtimens. Quelques circonstances, dépendant des localités ou de l'état de l'atmosphère, ont pu favoriser l'invasion de la maladie, mais elles n'ont pu la faire naître; et s'il restait quelque doute à cet égard, les tristes événemens de la Tortose suffiraient pour les dissiper. Il est donc pour nous, sinon d'une évidence absolue, du moins d'une probabilité très forte que la fièvre jaune a été importée d'Amérique à Barcelone, comme elle l'a été en Espagne dans la majeure partie des épidémies antérieures. (Bally, François, Pariset, Histoire médicale de la fièvre jaune, p. 640.)

In reference to this same epidemic, Audouard remarks:

La fièvre jaune a été apportée, de la Havane à Barcelone, par les bâtimens le Grand-Turc et le Saint-Joseph, et par plusieurs autres probablement. Elle s'y est développée d'une manière insensible dès l'origine, tant dans le port qu'à Barceloncette, d'où elle est parvenue dans la ville. * * * Vainement, on chercherait dans les miasmes du port les causes de tant de désastres. (Audouard, La fièvre jaune à Barcelone, p. 325.)

Rio Janeiro.—The great epidemic at this city in the winter of 1849–'50 presents some very interesting features, bearing both upon the origin of

the disease by importation and the development of it by certain local conditions; and, also, by reason of the very wide difference of opinion which existed on the part of intelligent physicians, who studied the subject impartially, as to the cause of the outbreak. Before giving an account of its introduction, a few words will be necessary on the location, and certain climatic conditions which may have influenced the course of the disease.

The city is situated in latitude 22° 56′ south, longitude 43° 9′ west, at the head of a fine bay. It is surrounded by mountains, although the ground upon which the city is built is low; the soil is partially sand and partially clay. The average temperature during the summer is from 75° to 85°, and in winter from 55° to 75°.

For some years previous to 1849 it had been noticed that there was an unusual amount of humidity in the atmosphere; it had also been noticed that the amount of electricity was very much less, thunderstorms being less frequent than usual. It had also been noticed that the ordinary miasmatic fevers of the country had been undergoing a change of type, the intermittent becoming remittent, and the remittent continued; and the latter sometimes evincing decided malignancy. The hygienic condition of the city had been for a number of years very bad. Great quantities of mud were permitted to remain in the streets; the city offal was either collected in heaps at every corner, or else spread out over a large surface to dry before removal; the drainage and sewerage were by open gutters, which constantly emitted a most offensive smell; and the portion of the city near the shipping, where the disease first appeared, was crowded with sailors' boarding-houses, which were crammed with occupants, ill-ventilated, filthy, and the inmates dissipated and degraded.

In October, 1849, yellow fever broke out in Bahia, the second commercial city of the empire. How it got there is still a matter of dispute. By some it was ascribed to the arrival of an African slaver, and by others to importation from New Orleans by an American vessel. Doctors Rossi and Vidal (Annals of Medical Society of Montevideo, 1854) say:

> It was looked upon as proven that the fever had been imported by the American brig Brésil, which arrived at Bahia the 30th of September of the same year, from New Orleans, and had touched at Havana during the prevalence of yellow fever.

However this may be, it is certain that it was epidemic in Bahia when the American bark Navarre left that port on the 20th of November, arriving at Rio Janeiro on the 3d of December. The vessel was pronounced clean and her crew healthy; she was not detained at quarantine, but came up to town, where the crew were paid off and discharged. Quite a number of them took lodgings at the house of one Franck, on Misericordia street, in the low and filthy locality before spoken of. From this house, on the 28th of December, there was removed to hospital a Swede named Anderson, who had been a sailor on the Navarre. He died two days after his admission to hospital, and six after the inception of the disease. The same day a Finn named Enquist, who lodged in a house about forty feet behind Franck's, and who was frequently in his house, was taken sick, and died on the 29th, both with undoubted signs of yellow fever. On the 4th of January a seaman named Wilson, belonging to the American bark Hercules, was admitted to hospital. He had been staying at Franck's house. This case recovered. The next case was on the 5th of January, and was a sailor of the Navarre, named Baker, who had likewise been in Franck's house. He died a few hours after admission. On the 7th of January three more cases, and on the 8th another, also from this same boarding-house. This same day five cases were discovered in the house of one Wood, directly opposite to Franck's, and within a few days four cases occurred at the boarding-house of Hourdé, next door to Wood's. All these houses were much frequented by shippers and sailors, and by the middle of January cases began to appear in hospital from the Russian and Swedish ships in the harbor; and all of the earliest cases had been frequently at the houses above named. The disease now began to extend, at first slowly, but soon much more rapidly, until all the shipping in the harbor and all the streets in the city became infected. No class of the population escaped; but it was most fatal among the sailors, next among those who had recently arrived in the city; while among the permanent residents, although they very generally took the disease, the mortality was very small. It is remarkable that a number of masters of vessels direct from Europe made statements to the effect that the fever broke out on their vessels as soon as they arrived off the coast of Brazil. During the progress of the epidemic there was a number of cases occurred on different vessels in the harbor, which were anchored a long distance from each other and had held no communication with the shore. No less than ninety-five vessels in the harbor sent sick to the hospital during the epidemic, and the highest and cleanest parts of the city suffered as much as the filthiest. Nevertheless, it was noticed that a number of inhabitants fled to Petropolis, a watering-place thirty miles from and three thousand feet above Rio. Over a dozen of these died in a crowded hotel in that place, and yet in not a single instance did they spread the disease. The epidemic terminated in September, 1850, having killed from four thousand to thirteen thousand, according to estimates of different persons. (Dr. R. Lallement, "Observations on the Severe Epidemic of Yellow Fever at Rio Janeiro in 1850, Rio, 1851." Dr. Croker Pennell, "A Short Report on Yellow Fever, as it appeared in Brazil during the Summer of 1849–'50, Rio, July 1, 1850." Dr. Paula Candido, "Report on Yellow Fever at Rio Janeiro;" "Report of the Sanitary Commission of New Orleans on Epidemic of 1853.")

The following opposing conclusions were arrived at by physicians of high standing, who carefully investigated all the facts. Their articles may be found in the "Annals of the Medical Society of Montevideo for 1854." Dr. Souza, of the Brazilian navy, says:

1. Yellow fever always arises from local causes.
2. It possesses no germ of contagion, nor of reproduction.
3. It cannot, therefore, be transmissible.
4. It is always identical in the various climates in which it springs up, whether it be sporadic, endemic, or epidemic.
5. The only sanitary measures to be employed against the disease are strictly hygienic.
6. A sanitary system opposed to the introduction of imaginary virus should be abandoned as useless and frequently pernicious.

Per contra, these are the opinions of Drs. Rossi and Vidal, from the same journal of annals:

1. That yellow fever never originates from local causes.
2. That it does possess a germ of contagion and of reproduction.
3. That it is consequently transmissible.
4. That it is always identical in every climate.
5. The same sanitary measures should be adopted which are commonly opposed to pestilential diseases.
6. A sanitary system, having for its object the prevention of transmission of an infectious disease, ought to be regarded as the most sacred of duties; while it can never be or become dangerous. (Transactions of Medical and Chirurgical Faculty of Maryland, '56, pp. 68 and 74.

From that date yellow fever appears to have taken up its residence in Rio, prevailing every year with more or less severity.

In 1851 the steamer New World, *en route* from New York to San Francisco, touched at Rio. After leaving that port, the fever broke out on board, and she finally put in at Callao, having lost most of her crew by yellow fever. A similar incident happened to the steamer Quito, which arrived at Callao in January, 1852. From this time a new disease made its appearance on the coast, which the natives called "pellusa," but which was subsequently ascertained to be genuine yellow fever. (New York Medical Record, January 15, 1872, p. 519.) During the prevalence of the fever on the coast, a soldier, who had deserted, started for his home in the mountains of Peru, but was taken sick at the village of Pisco, (it was said after spending some time in examining and looking after the effects of a man who had died of the fever.) He was conveyed six leagues in an open litter to his home in Charcas. At every town and village the people crowded around the litter from motives of curiosity, and the result was the breaking out of the disease in all the hill-country of Peru. It spread from the villages to Cuzco, the ancient capital of the Incas, where, out of a total population of forty thousand, it was reported there were twenty thousand deaths.

Port du Passage.—This is a small fishing-village of about twelve hundred inhabitants, not far from Bayonne. Its location is a healthy one; the streets are clean; ventilation abundant; the occupation of the inhabitants conducive to health; and affords no local causes for the spread or origin of pestilence. In 1823 the population of the town was nearly doubled by reason of some French army operations. On the 2d of August of that year the brig Donostiarra arrived at the port, and anchored off the town. This vessel had formerly been a slaver, and had not been repaired since her last voyage as such. She left Havana about the 1st of June, and, when ten days out, lost a sailor by sudden death. After a voyage of thirty-six days, she reached Corogne, on the west coast of Spain, where she underwent a quarantine of ten days. On her arrival at Port du Passage she was admitted to pratique without detention, as she had no sickness on board, and had already been subject to a quarantine at Corogne. On the 15th of August the custom-house officer placed on board fell sick, and died on the 17th. On the 30th, a carpenter at work on the ship was taken, and died on the 22d. From this time until the 1st of September a number of other persons at work in the hold of the ship were taken, as well as some watermen who were in the habit of visiting her. It was worthy of remark that although hundreds of persons had visited the vessel (for her arrival was a matter of great consequence to the people of the little port, being the only one that year) between the 2d and 15th of August, yet it was not until the cargo was discharged, and the hold empty, that anybody was taken ill. During the first week in September, cases began to occur on shore, in the houses adjoining the wharf where the vessel was moored. On the 6th of September, the authorities, having no doubt that the disease was traceable to the vessel, caused her to be moved to a distance of several miles from town, but, notwithstanding this, the epidemic continued to increase. About the middle of September, Audouard, a surgeon of the French army, was sent to the place, and he immediately ordered the dispersion of the sick in the country, which was done on the 25th, and from this time the epidemic ceased. There can be no doubt that this was genuine yellow fever. Audouard had examined the disease closely at Barcelona, in 1821, and so pronounced it: "Tous les individus, présentèrent les mêmes symptômes; ils vomirent des matières noires, et devinrent jaunis avant de mourir." (Faget, Mémoires sur la fièvre jaune, deuxième lettre, pp. 3–5.)

In a similar manner, the disease was introduced into St. Nazaire, France, in 1861. On the 25th of July, 1861, the ship Annie Marie arrived at St. Nazaire, France. She left Havana on the 13th, there being an epidemic of yellow fever prevailing in that place. She had nine cases of fever and two deaths on the passage, and had seven men sick on her arrival, but convalescent. Ten days having elapsed since the occurrence of the last case, the ship was not quarantined, but came up to the docks, where her crew were discharged, and a gang of seventeen stevedores hired to discharge the vessel, which was loaded with sugar. Out of this party thirteen took the fever, and several died. The crew of a neighboring vessel, Le Chaston, came on board the Annie Marie while she was discharging. This vessel soon after left for Indret, on the Loire, and arrived there on the 1st of August with one of her crew of five men sick with fever, who died. Shorly after three others of her crew died. In the mean time another vessel in dock near the Annie Marie left for L'Orient, and arrived there August 10. On the 14th the disease broke out at L'Orient, and there were a number of deaths. At St. Nazaire the disease was likewise communicated to other vessels, and also to persons on shore. One of the workmen employed in discharging the ship went some miles into the country to the village of Montoir, where he resided. Here the physician who attended him was attacked with the fever, and died; and a physician at Indret likewise contracted the disease after attending some of those who were sick with it. The total number of cases was forty-four, of which twenty-six died.

Quebec, 1864.—In August, 1864, the ship Montgomery arrived at Quebec, from Nassau, New Providence. The captain stated that he had lost his wife and two of his crew from yellow fever on the passage. After her arrival a young man named McClusky, who lived but a few steps from the dock where the Montgomery lay, went on board of her, and was soon after taken sick with undoubted yellow fever. Other cases afterward presented themselves, but there was no epidemic.

This case is interesting from the existence of the disease in very high latitudes, showing the necessity for stringent quarantine regulations, not only in southern ports, but also wherever vessels may arrive from infected ports. Commenting on this case, Professor La Rue, of Quebec, says:

> I am perfectly convinced that we have had yellow fever at Quebec; perfectly convinced that the case of which Dr. Wherry speaks (the case of McClusky) is really dead of this disease; and I have every reason to think that if the Montgomery, instead of arriving at Quebec in August, when the nights commence to get cool, had arrived at our port during the excessive heat of the month of June and the first part of July—I say, I have every reason to think—that Quebec would have had to suffer a most fatal epidemic. (Faget, Considérations générales, pp. 13, 14.)

Nor is this the only case of the appearance of the disease in very high latitudes, as witness the following:

Swansea, Wales.—The bark Hecla sailed from Cuba for Wales, July 28, 1865, and arrived at Swansea on the 2d of September. On the passage she lost three of her men with yellow fever, and landed three sick on her arrival, one of whom, named Saunders, died the next day. The authorities were much alarmed, and Saunders was buried within four hours after his death; his house and neighboring ones disinfected and fumigated; and orders issued to stop the further discharge of her cargo, from the Hecla. About thirty tons of copper-ore had, however, been landed. The ship was then thoroughly disinfected and fumigated with chlorine, and after this discharged her cargo, a work which was com-

pleted by the 21st of September; after which she was removed to another dock. On the 28th of September she left this dock and proceeded to sea. On the 23d of September a death from yellow fever was reported in town, and close inquiry resulted in the discovery that there had been quite a number of cases since the 15th. There were in all twenty-two cases, of which twenty were of persons in immediate communication with the ship, or living in the immediate vicinity. A ship lying close to the Hecla lost two of her crew. It should be remarked that the season was unusually hot. In all, fifteen died, and after the removal of the Hecla not a single fresh case occurred. (La Roche, Yellow Fever in 1870, pp. 33–35.)

It can hardly be supposed that, in spite of its low alluvial position, there would have been any yellow fever at Swansea had it not been for the arrival of the Hecla.

Buenos Ayres.—The mortality occasioned by the yellow fever in this city in 1871 attracted the attention of the whole civilized world. Rarely, if ever, had such destruction been caused by any pestilence. The city is not one that has ordinarily been visited by epidemics; in fact, previous to its first appearance at Rio Janeiro, in 1849, it was unknown to the south of that city. In 1858 it became epidemic in Buenos Ayres. Toward the latter end of March, the royal mail-steamer Camila arrived there from Rio. On the voyage she had three deaths from yellow fever. Soon after her arrival, cases of yellow fever began to occur on shore, in the streets running parallel to the river, and after prevailing for six weeks disappeared suddenly, under the influence of a strong southwest wind from the "pampas." The first cases on shore were clearly traceable to the steamer. The number of deaths was between six hundred and seven hundred.

In 1871 occurred the greatest epidemic known in the history of the world, except perhaps the Barcelona visitation of 1821.

During the few years previous, there had been numerous arrivals of emigrants from Europe. The weather was extremely sultry, the mercury ranging between 96° and 104° Fahrenheit. During the latter part of 1870 there had been a great drought, and the river had considerably contracted within its banks, exposing large quantities of alluvium, to be acted upon by the sun. On the first week in January, according to the local papers, a passenger in detention at the lazaretto escaped from the quarantine and rode into town, and joined his family, where he had the fever. He recovered, but a number of his family died; then those in the next house; and from thence it spread through the whole locality known as the district of San Telmo, which was inhabited chiefly by Italians, very much crowded, and the dirtiest in the city. About this time a vessel from Genoa arrived at Buenos Ayres, having touched at Barcelona on her passage. During the voyage she had fourteen deaths from yellow fever, and on her arrival the captain evaded quarantine by concealing both the facts of his touching at Barcelona, or having had any fever on board, and, proceeding to the city, landed her passengers, "many of whom were doubtless infected." During January the disease increased but slowly, there not being more than an average of twenty deaths per day up to the middle of February; but by the end of that month it had spread to adjoining districts, and the deaths were from forty to fifty per day. In March the increase was terrific, and by the close of the month the deaths were three hundred and fifty per day.

The epidemic followed in its course the route taken by a great flood the year before, the houses devastated by which, after remaining empty for a long time, had recently been re-occupied without being cleaned, and were, as well as the streets, in a filthy condition, with the yards and streets contaminated by heaps of rotting clothing and other refuse deposited there by the flood. In March there were heavy rains, and on the 10th of April the epidemic reached its climax, when there were five hundred and forty deaths, after which it rapidly declined, probably from want of material. The number of deaths was variously estimated from thirteen thousand four hundred to twenty-six thousand; probably a mean between the two would be nearer correct. (Dr. J. H. Scrivener on Yellow Fever, in Buenos Ayres Medical Times and Gazette, February 17, 1872.

These brief accounts, selected from a very large number which have occurred in the present century, will suffice to show that yellow fever is not confined in its visitations by any considerations of latitude or longitude; that, while it has its natural home in that portion of the Western World where tropical heat prevails throughout the year, it may, and has repeatedly, been carried thence to extreme northern and southern latitudes; and that it has never prevailed in such latitudes except when accompanied by the most direct proof of its importation. Whether at Swansea, Wales, or Quebec, on the one hand, or Buenos Ayres or Barcelona on the other, the story is always the same—of the arrival of an infected vessel a short time previous to the appearance of the pestilence, and careful investigation uniformly connects the earliest cases on shore with some vessel from a port where yellow fever was prevailing.

In spite, however, of the vast number of facts of this description which are to be found in the history of yellow fever from the earliest times, the opinions of students of the disease have varied much as to the possibility of its importation.

Probably no question can be found in the literature of medicine which has given rise to such a long series of controversies, argued with such earnestness on both sides, and finding among the advocates of either view some of the ablest and most learned minds of the day.

Prior to the last decade of the eighteenth century, there does not seem to have been any question in regard to the contagiousness of the disease. The subtile distinctions which have been made by later writers between infection and contagion do not seem to have disturbed the mind of the profession of that period. It made use of the two words as synonymous, meaning simply the communication of a disease from one person to another, either by direct contact or by means of fomites, through the agency of ships, clothing, bedding, &c. Lining, as I have already mentioned, in his letter to Dr. Whytt, of Edinburgh, speaks in no doubtful terms of the "*introduction*" of the disease to Charleston in all the epidemics previous to 1753. Dr. John Redman has no hesitation in ascribing the yellow fever at Philadelphia, in 1762, to importation from Havana. One of the earliest accounts of the yellow fever which we possess was written by M. Desportes, who was sent, in 1732, to San Domingo, by the King of France, to investigate the subject. He called the disease "a pestilential one, and had no doubt of its contagious nature." (Pascalis, op. cit., p. 5.) In the North American Medical and Surgical Journal for October, 1827, will be found an introductory lecture, delivered at the Pennsylvania hospital by Dr. Thomas Bond, in 1766, in which he refers to two out of three epidemics which he had observed as having been imported; to the third he ascribes a local origin; and this seems to be the first allusion on record to anything affirming the local origin of the disease. All of the earliest historians of the West Indies speak of it as being carried from one island to another. Dr. Thomas Dancer says (Medical Repository, vol. 7, p. 247)

that most of the practitioners in the West Indies concurred with Dr. Chisholm in his views as regards the contagiousness of the disease. In fact, it was not until the great series of epidemics which began in 1791 that any doubt was expressed by medical writers on the subject. From 1793 to 1798 Drs. Nathaniel Potter and John B. Davidge, of Baltimore, Jean Deveze and Benjamin Rush, of Philadelphia, and others, enunciated the idea that the disease was of local origin in the United States. The official report of the College of Physicians of Philadelphia on the epidemic of 1793 was dissented from by Rush, Hutchinson, and others, and their views, which ascribed the disease to putrid exhalations from the streets and docks, and to decaying coffee and other articles on the wharves and in store-houses, soon gained ground, and found an official utterance in 1798, when Rush, Physick, and others, who had become incorporated as the Academy of Medicine of Philadelphia, made a report to the board of managers of the hospitals on "The origin, progress, and nature of the pestilence of that year," and repeated their former opinion that the first cases "appeared to originate from the putrid exhalations of alleys, gutters, and docks, and from the stagnating water in the neighborhood of the city." Nevertheless, even they qualified their opinion by the further statement: "We derive the late rapid increase of the fever from the foul air of several ships lately arrived in the port, and from some damaged coffee which arrived in a brig from Jamaica on the 29th of July." In a letter to Governor Mifflin, of Pennsylvania, dated December 3, 1798, this same body remarks:

> We are the more confirmed in the opinion we have delivered that the yellow fever is a native disease of our country by discovering that the same opinion is held by most of the physicians and citizens of our sister States.

And they conclude their letter by deprecating in the most solemn manner the continuance of a belief in the supposed importation of the fever.

As before, these views were strongly opposed by the College of Physicians, who insisted that the disease was exotic, and that there was nothing in the condition of the streets, alleys, or docks to warrant the assumption of the local or domestic origin of the disease; but, on the contrary, the disease could be unequivocally traced to the vessel that introduced it.

The controversy, thus begun, soon extended beyond the limits of the bodies in which it originated. The volumes of the Medical Repository and other journals, from 1800 to 1820, are filled with able articles advocating one side or the other of the question. On the side of the importationists were to be found Currie, of Philadelphia, the Hosacks, Bayley, Townsend, Francis, Seagrove, and many more; while the domestic origin of the disease was maintained with equal energy and ability by Jackson, Miller, Mitchill, Seaman, Pascalis, Brown, Wheaton, &c. Abroad the physicians who had seen the disease generally believed that it was of foreign derivation. Dr. Robert Jackson says that "the Spanish physicians seem, all of them, to believe in contagion as the cause of this disease." (Yellow Fever on Coast of Spain since 1800, p. 16.) Bally, François, and Pariset, (as well as Audouard,) who made official reports to the French government on the epidemics at Barcelona and Passage, had no doubt that it was imported into those towns; but on the whole, in our own country, the doctrines of a domestic origin for the fever found most advocates. The southern physicians, as a rule, adopted these views, and after 1822 the controversy seems to have died out, until it was revived by the publication of Strobel's work in 1839. This was very soon followed by the brochures of Monette, of Natchez, and Carpenter, of New Orleans; and before many years by an able series of articles by William Hume, in the Charleston Medical Journal, all of which, taken in connection with undisputed facts in relation to the epidemics of 1838 and 1839, and subsequent years, once more attracted attention to the doctrine that yellow fever was an imported disease. The history of the series of epidemics during the years 1853–'56 so strongly pointed to foreign importation as the source of the pestilence that this opinion once more rose in importance, and has, I think, continued to increase in prevalence since, being confirmed by the history of the late war, and of the great epidemic of 1867. During this period, however, the exhaustive work of La Roche supported the endemic origin of the disease. His views, however, seem to have been greatly modified since that time, if we may judge from his article on "Yellow fever in 1870 at the Philadelphia lazaretto." At present, so far as my observations and conversations with southern physicians enable me to judge, they may be divided into several sects or parties on this subject, viz:

1. Those who retain the views, held in the early part of this century, as to the endemic origin of the disease.

2. Those who believe that it can be imported, but also that it occasionally originates in the southern towns.

3. Those who believe that it is always imported, and that sporadic cases are to be accounted for, either by the hibernation of germs from a previous season, or else by the supposition that these cases may not be yellow fever at all but cases of malignant paludal or bilious fever.

Very soon after commencing the visits of inspection called for by the joint resolution, I foresaw the advisability of obtaining, in some definite form, the opinions of the profession at the South on this subject and that of the quarantine. I accordingly caused circulars to be printed, which were addressed to several physicians in each of the towns I visited, inviting them to give their views as to the portability of yellow fever, and the value of quarantine restrictions. This was done more as a matter of justice to these gentlemen than for my own information; for, as I had conversed with most of them, or had read their ideas in the medical journals, it was not necessary that they should address me for the latter purpose. To my regret, however, very few of those whom I addressed took any notice of my communication. If, therefore, I do them any unintentional injustice in my summary of their views, they are debarred the right to complain, as they were given ample opportunity to place themselves on the record had they so desired.

I think that a majority, especially of the younger physicians, believe decidedly in the transmissibility of the yellow-fever poison; but they hesitate openly to express their views, partly because by so doing they would find themselves in opposition to men who may be said to lead the profession at the South, and who, with the people at large, are considered to speak almost with infallibility; and partly because the acknowledgment of the importation of the disease carries with it necessarily a belief in the value of quarantine, which, when no epidemic threatens, is an unpopular idea with the mercantile community. I am led to this opinion from an incident which happened in New Orleans. The mayor invited a number of leading men to meet me to discuss the question. The distinguished Professor Warren Stone was in the chair, and made an eloquent address in favor of the domestic origin of yellow fever, and denouncing all quarantine measures whatever. But one gentleman ventured to differ from him, and he was not a physician; but after the adjournment of the meeting, several assured me privately that

they did not at all agree with Dr. Stone in his views, and would take occasion to express themselves in writing to me on the subject, but this they never did.

Thus far I have endeavored merely, in the words of the joint resolution of Congress, "to ascertain all facts having reference to the outbreaks of the disease" in the southern ports and towns; and to present what proofs may exist of its importation, in so far as they may bear upon the question of the benefits to be derived from quarantine measures. But there is another aspect in which the subject must be viewed. "The question of the mode of existence of contagious matter," says Dr. Burdon Sanderson, "lies at the foundation of all scientific inquiry as to the means of obviating or counteracting contagion." Here we leave the domain of ascertained facts, and enter upon that of mere hypothesis. The subject is one full of difficulty, and I cannot hope to do more than, by availing myself of the investigations of those who have made the question of contagion a special study, to offer such a view of the origin of the disease as will harmonize best with the most recent ideas on the intimate pathology of contagion.

If, up to the present time, the search after these means has appeared unsuccessful, and the results obtained by their application have been unsatisfactory, the explanation of the fact is to be found, not in any want of knowledge of the properties of the agents employed, but in the ignorance which prevails as to the physical characters of the substances which it is our object to destroy or render innocuous. (Twelfth Annual Report of Medical Officer of Privy Council, p. 229.)

If we look through the writings of the earlier authors on yellow fever, and, indeed, some of later date, we shall find that they regarded the disease as a malignant type of bilious remittent; and due to the same pestiferous miasm. Certain epidemics which prevailed in New England and Virginia in the early part of our history, being accompanied by a jaundiced skin and the vomiting of altered bile, were so closely assimilated in their phenomena with the true West India fever that the differential diagnosis between the two diseases was not clearly made out, and the subtile distinctions between them overlooked in the prominence of certain symptoms which they had in common. And yet a glance at the habits and habitats of the diseases would have shown how little it is possible for them to have a common origin. Yellow fever is essentially a disease of large cities and sea-port towns. It never originates in the country, even in the most swampy and malarious districts; and when carried there by refugees from any town where it prevails, it fails to spread.

The rice-swamps of Georgia and the alluvial districts of Alabama and Louisiana are saturated with malaria in its most concentrated form. Pernicious and congestive fevers, high grades of bilious remittent or intermittent, are the common lot of those whose business takes them to such localities, yet yellow fever is unknown from this source; but, on the contrary, it is found that very soon after the outbreak of an epidemic in New Orleans or Mobile, the remittents and intermittents partake more and more of the type of the imported disease, until at last the more potent poison overwhelms the feebler one, and malarial diseases disappear entirely for a time. Moreover, the attack of yellow fever is generally protective against a second one, which, as is well known, is not true of any form of malarial disease. Yellow fever finds its subjects chiefly among the unacclimated, while remittent fever attacks equally those who have lived their whole lives at the South. The colored people are peculiarly susceptible to the influences of marsh-poison, while, though yellow fever frequently prevails among them, it is by no means so fatal as among whites. Yellow fever has prevailed as an epidemic at places (such as Pensacola) where the surrounding country is a high, dry sand-barren, without any malarial influences to produce disease. In fact, it requires but a close examination of the history and clinical phenomena of the yellow fever to show that it is a disease *sui generis*, and not dependent on marsh-poison, or allied to any of the forms of paludal disease that are so common in the southern country, away from the cities.

What the peculiar nature of the yellow-fever poison may be, we have thus far failed to ascertain; yet it may be permitted to generalize a little on the subject, and endeavor to see whether recent researches into the pathology of contagion will serve to elucidate the question. The fundamental deduction arrived at by recent observers is, that every kind of contagion consists of extremely minute particles of organic character, capable of growth and multiplication to an indefinite extent; and that the contagious fevers are due to the introduction of such particles into the blood. They differ materially as to the nature and origin of such particles. Hallier, of Jena, considers the organic forms found in contagious fluids to consist of fungi, composed of single cells, which reproduce themselves with extreme rapidity, and are capable of being developed into higher organisms than themselves. These are called by him "micrococci," and by Bechamp "microzymes." Beale, on the contrary, regards the contagious particles as the result of retrogression of a normal germinal principle, or bioplasm, from which all the tissues of the bodies are developed. All the complex tissues and organs of man are dependent on the changes which take place in a transparent, colorless, living matter, called "bioplasm." This germinal matter has the faculty of dividing into other bioplastic masses, each mass going to fulfill some special purpose in the economy, and not being capable of any other. These bioplasms are analogous to certain forms of low organism found in nature, called "amœbæ," and like them they have certain remarkable powers of locomotion.

After a time some of these bioplasms cease to multiply, though they still live and take up food. The living matter of which they are composed undergoes change, and is converted into tissue. There are, however, some masses of germinal matter in the adult which continue growing and multiplying through life, as they did in the embryo; among these are the white corpuscles of the blood. Under certain circumstances these masses of bioplasm may become degraded and incapable of performing their higher functions, while still possessing the power of reproduction and multiplication to an enormous extent. Actively living, degraded bioplasm of this kind may retain its vitality for a long time, although entirely removed from the body. Beale consequently defines a disease-germ to be "a particle of living matter derived from direct descent from the living matter of man's organism."

With the abstract truth or falsity of these two theories of disease we are not at present interested. They both agree in ascribing the origins of contagious disorders to organic living particles, and whether these are called microzymes, bacteria, or bioplasts; whether they are of fungoid growth, obtaining entrance to the body as parasites; or whether germinal masses, "derived from normal cells, and due to a series of changes in existing matter under new circumstances," is of little consequence to the inquiry before us. Without, therefore, entering further into this discussion, it will be proper to inquire into the relations that these contagious particles may bear to the subject of the spread of yellow fever. Assuming for a moment the fact of their existence, they may

escape from the organism in which they have been developed in various ways. As Cohnheim has demonstrated that bodies as large as the white corpuscles of the blood may migrate through the walls of the capillaries in inflammation, it requires no credulity to believe that these particles, which are infinitely smaller, may also thus escape into the surrounding tissues; especially when the surface capillaries are congested during an attack of fever. Once outside the vessels, it is an easy matter for them, aided by their amœboid movement, to make their exit from the body. Reaching the surface, they may be expelled with the expired air, pass away with the discharges from the body, or be thrown off with the epithelial scales from the cuticle. Perhaps they may be eliminated from the blood by the action of secreting and epithelial cells without the vessels. In whatever way they escape from the body, their vitality is so great that they will live and multiply for many days when so removed. In a partially-desiccated condition they may float in the air; they may impregnate water and food, attach themselves to clothing, bedding, or other articles around a sick room, and, existing thus under so many and such different conditions, have unlimited facilities for gaining access to the bodies of other persons favorably situated for their reception. Sanderson and Chanveau, in their experiments on the cattle-plague, have shown that the amount of contagious material necessary to produce disease in another individual is extremely small. "A mere trace of serum is sufficient to propagate the cattle-plague." This is further manifested in the very small amount of morbid matter required to infect a person in the case of a dissection-wound. Consequently, assuming that such morbid particles are given off from a person sick with yellow fever, it is easy to conceive how extensive may be the morbific influence arising from a single patient. In times of epidemic prevalence, the air will become filled with these floating particles of contagium. In consequence, where a ship leaves an infected port, and her hatches are battened down after her cargo is stowed, she carries within her hold, hermetically sealed, a portion of the air of the infected town, heavily charged with these floating particles. Many of these sink to the bottom and impregnate the bilge-water, or become incorporated with the filth which exists on the sides and bottom of the vessel. Individuals who leave an infected port carry with them in their clothes millions of these little particles, which grow and multiply in whatever position they are placed, and may even acquire increased virulence after their exit from the body in which they were developed.

It has been objected to this theory that if these particles of contagium are so numerous and exist so universally in an infected town, how is it that anybody escapes an attack? Perhaps, in the present condition of our knowledge, it is hard to give a satisfactory answer to this question, yet it is no more than occurs in life under other circumstances. In battle but a very small number are killed or wounded in comparison with the number of shots fired. The air seems filled with missiles, but yet comparatively few are hit. Many conditions may be required in the organism beyond the simple reception of the germ to enable it to develop disease. Of the seed scattered over a field but a small proportion finds a congenial soil in which to germinate; the rest perishes

The fact that of a number of persons equally exposed to the influence of contagium some will contract the disease while the majority will escape, may be accounted for by supposing either that, in the latter case, the particles do not really penetrate the vascular wall at all, or that they are in some way destroyed as soon as they traverse the wall of the capillary and come in contact with the blood. (Beale on Disease-Germs, p. 84.)

There may also be certain circumstances connected with locality or population which will favorably influence the reproduction of contagion. Peculiar meteorological conditions, defective drainage, poisonous gases, rotting vegetable *débris*, can no more originate yellow fever than they can small-pox, but they may so vitiate the vital capacity for resisting disease as to render the organism peculiarly receptive to the influence of a morbid poison.

The vital and all-essential fact in the etiology and geography of yellow fever is that which relates *to the implantation of the epidemic germs, or the fomites*, in a locality where temperature, humidity, and personal conditions favor their pestilential propagation." (Harris, United States Sanitary Commission Memoirs, p. 242.)

Among such personal conditions may also be considered the influence of fear; the psychical depression caused by dread of an epidemic operating probably more powerfully than any other predisposing cause in affording a congenial resting-place for the germs of the disease. It should also be borne in mind that, of the total population of any southern city, but a small portion is really susceptible, the balance being composed of acclimated persons. The number of cases is apt to be in direct proportion to the number of unacclimated residents.

While it has been uniformly found that a temperature below 32° Fahrenheit has stopped the progress of an epidemic, it is probable that the commonly-accepted belief that the poison of the fever is thus killed is not altogether true. The vast majority of the germs unquestionably perish, but some may continue to exist under a lower condition of vitality, and passing the winter in a hybernating state become revivified by the renewed warmth of the following spring, or under certain circumstances they may slumber for a much longer period, until roused into action by circumstances favorable to their development. It is found, however, that most commonly in such cases these particles have lost a portion of their contagious vitality, and are no longer capable of originating other germs that can propagate the disease. Hence the cases which occur under these circumstances are sporadic, and the disease does not assume an epidemic character. This is not always the case, for occasionally outbreaks of the most severe type have occurred when it was probable that a long time had elapsed since the original germs of infection had been received. In this way we may account plausibly for the appearances of the disease on ships at sea, which have always been seized upon as favorite examples by those who advocate its spontaneous development.

The germinal principles of contagious diseases differ remarkably in their power. Some are vigorously infectious, others very feebly so. It is equally probable that, under different circumstances, the same contagion may possess much more virulent properties than at others. The germs may retain their vitality for a longer period, or develop a greater capacity for reproduction, and hence, in one year, we have a wide-spread epidemic, in another the germ of the disease may be introduced, but the only result be a few sporadic cases.

A phenomenon which has been frequently observed during the progress of an epidemic is its intermission or sudden cessation immediately after a violent gale. This is accounted for by the dispersion of the contagious particles beyond the city limits and the renewal of the atmosphere; a view which is confirmed by the fact that generally, after an intermission of a week or two, the disease breaks out again with increased virulence, only sufficient time being requisite for the fresh air to become charged with germinal atoms; and further, by the observation that in the most sultry seasons, when the air is comparatively mo-

tionless, the fever has confined itself sometimes to certain sections for several weeks, and afterward, under the influence of a gentle breeze, has appeared in several detached localities at once.

THE QUESTION OF QUARANTINE.

The joint resolution calls for an opinion "whether any system of quarantine is likely to be effective in preventing invasions of yellow fever." I believe that this question may be unqualifiedly answered in the affirmative, and that the war on quarantine has been partly the result of ignorance of the facts, or illogical views of the origin of the disease, and partly because the love of gain, outweighing the natural feelings of humanity, has distorted the unavoidable inconveniences, attendant on any quarantine system, into a barbarous cruelty, which has come down to us from the Middle Ages, and has no part in the enlightened civilization of the nineteenth century. It is, I think, possible to show that the cruelties and barbarities of quarantine need have no existence under a properly-organized system; that its restrictions on commerce have been greatly magnified by those who are interested in the West India traffic; and that the general trade of any city, so far from being injured, would be immensely benefited by a quarantine which would be absolutely protective, such as, it is believed, it would be possible to institute.

A strong argument in favor of quarantine may be found in the varying aspects of public opinion at the South. "À la longue, rien ne résiste à l'autorité des faits, et le simple bon sens du vulgaire l'emporte souvent sur les hésitations et les sophismes de l'intérêt et du savoir." It is found that during periods of health the sentiment of the southern people is strongly opposed to quarantine; but should there be a danger of the approach of the disease, it is astonishing what unanimity there is on the subject. The press makes fervent appeals to the boards of health to increase the efficiency of quarantine; leading men, both in and out of the profession, use all their influence to urge its establishment; and should the disease unfortunately break out, the authorities are denounced in the severest manner for their neglect. The medical profession may prove, to their own satisfaction, that the disease is of local origin, but the community does not believe it, and in times of pestilence they see what dreadful losses accrue to them from the prevalence of the disease, and all are quarantinists.

The objections raised against quarantine measures may be either professional, personal, or commercial, and may be summed up as follows:

1. It is urged that inasmuch as yellow fever is a disease of domestic origin in the United States, and due entirely to local causes, or else to the prevalence of what is called an "epidemic atmosphere," a quarantine can, by no possibility, be of any value in warding off an epidemic.

This objection is best answered by inviting attention again to the facts as set forth in the histories already narrated of the epidemics of yellow fever. These accounts show that in the majority of epidemics of which we have any detailed history, its outbreak has been preceded by the arrival of one or more vessels from a port known to be infected; that the first cases are generally either persons who came to port in such vessels or who are known to have been on board of them, and that those next taken sick have had communication with the first; or if the proof be not so direct as this, it is found that the vicinity of the wharves and docks, and the streets and alleys bordering the harbor, are

the localities first infected. The earliest cases are in sailor boarding-houses, or warehouses where foreign goods are stored, and these cases are in the persons of sailors, stevedores, or workmen about such wharves or warehouses. The filthy condition of such places, always insisted upon as sufficient of itself to produce the disease, does not afford a satisfactory explanation of its existence, because such localities are always the most neglected in a city, and the population such as defy all considerations of personal cleanliness or local hygiene. They are in no worse condition in years of sickness than in those of health, and on the assumption that overcrowded tenements or accumulations of putrefactive *débris* will produce an epidemic, it should prevail in such localities every year, which is not found to be true.

Let it be particularly noted that the conditions productive of fatal disease are in all cases subject to little or no variation, and therefore, where the causes are perennial, there ought to be no respite from the ravages of deadly maladies. The facts, unfortunately for the theories, are perverse; instead of a confirmation they supply a refutation, to my mind irresistible. (Pratt on Origin of Fevers, Medical Press and Circular, March 6, 1872, p. 206.)

It has already been remarked that the absence of direct evidence of the foreign importation of the disease is no proof that it might not have existed, and been overlooked in the difficulty of tracing the previous history of the first victims, who are frequently strangers without friends that are acquainted with their movements before the attack. It is, in fact, a matter for surprise that we have been able to trace the disease to foreign importation in such a large number of instances rather than that the evidence should be defective in a few.

It is further shown that when the disease leaves the sea-board it has generally followed the great routes of travel or trade. Natchez was not visited by yellow fever before steamboats commenced their trips on the Mississippi River in 1819; after that year the town was ravaged by the disease whenever it prevailed in New Orleans, and never at any other time. Vidalia, Louisiana, never suffered previous to 1853, in which year, for the first time, steamboats stopped there. Sabine and Matagorda, in Texas, having only a coastwise commerce, were always exempt until the severity of the blockade at Galveston and the possession of New Orleans by our forces made them convenient resorts for blockade-runners. Houston and the interior towns in Texas have only been visited by the disease subsequent to its appearance on the coast. Many similar instances could be cited.

Moreover, the direct facts in favor of quarantine are equally cogent. New York established a rigid quarantine in 1822, and although the disease has been brought almost yearly to the port, it has since that time only twice appeared within the quarantine lines, viz, at Fort Hamilton in 1856 and Governor's Island in 1856 and 1870; both instances being plainly traced to an evasion of the quarantine law. Philadelphia presents a similar record, having been exempt from yellow fever for fifty years, except in 1853, when the bark Mandarin was improperly allowed to pass the quarantine.

In 1798, when almost every sea-port town at the North was visited, the authorities of Baltimore established a quarantine, and forbade all intercourse with the infected city of Philadelphia, and alone, of all the great commercial cities of the North, escaped, though repeatedly ravaged by epidemics in previous years. From 1840 to 1853 the authorities of Natchez quarantined all steamers arriving from New Orleans, during an epidemic season, and in those years they had no fever. In 1853, when they relaxed their quarantine, the disease again made its appearance. In 1862, when the city of Galveston was crowded with

the sick and dying, there was not a case among the ten thousand troops in the neighboring forts, all communication between the soldiers and the inhabitants of the town being forbidden by military orders. In 1867 the disease appeared at every town in Texas, along the railroads diverging from Houston, except Columbus, Washington, and Richmond. These three places established a quarantine against Houston and Galveston, and they escaped entirely. In 1868 and 1869 the military authorities exercised a supervision over the quarantine along the Texas coast, and there was no appearance of the disease, although it was epidemic at Vera Cruz, Havana, and Key West, and several vessels arrived at the various quarantine stations with cases of fever on board.

These, and many other facts which might be adduced, go far to show that the favorite theory of an "epidemic constitution of the atmosphere," bidding defiance to all restrictive measures for preventing the entrance of the disease, has no foundation in fact, except as dependent on infection by means of fomites, and an acceptance of the hypothesis of the invariable spread of the disease as a result of the multiplication of its germinal principle, (this principle being generally, if not always, an importation in ships, cargoes, or personal property,) will afford the strongest possible argument why a properly-constituted system of quarantine can be of efficacy, and that the southern towns can only be rendered free from visitations of yellow fever by such means. Yet it is not to be considered that the value of quarantine depends on the acceptance of any hypothesis of contagion; the actual historical facts are amply sufficient to prove the great service it has been in the past, and to warrant further investigations into the subject, with the view of making it more efficient in the future. It can, moreover, be urged with propriety that even if it is granted that some epidemics are not to be explained except by assuming the domestic origin of the disease, yet even then the facts prove that a large number of others are certainly due to importation, and therefore, by preventing such importation, we can materially reduce the number of epidemics, even if we fail in entirely keeping off the disease, which is certainly "a consummation devoutly to be wished."

2. Another objection is, "All epidemic diseases are present in the country, and disorder the health of a people before they are manifested in their peculiar and recognized forms."

This may be true of influenza, and possibly of some of the exanthematous diseases, but it is certainly not the general rule as regards yellow fever. Malarial fevers have often been very common in the spring and early summer, preceding an epidemic of yellow fever, (as was the case at Rio Janeiro in 1849;) but this coincidence can always be accounted for by local or meteorological causes, and only those who regard the latter as a paludal disease, or a more aggravated form of bilious remittent, would trace any connection between the two. Any one who regards yellow fever as due to a specific poison must reject the idea of such an influence of that poison on the general health as could exhibit itself in an increase of other diseases not allied to it in pathological or etiological phenomena. Typhoid fever was prevalent at Key West in 1862 before the appearance of the vomito; but it may be presumed this was owing to the presence of an unusually large number of troops, and the great increase in the civil population of the town, from causes resulting from the war. The yellow fever being positively traced to importation by the bark Adventure in the latter part of June, it is simply absurd to suppose any connection between the prevalence of the two diseases.

3. That the experience of the past proves quarantines to be worthless as safeguards against yellow fever. The instances already cited of its value at New York, Baltimore, Philadelphia, on the coast of Texas, and elsewhere, are a sufficient refutation of this assertion; but as the opponents of quarantine point to particular instances in which it has proved ineffective, it will be proper to examine some of these cases, and ascertain with what justice the charge has been brought. One of these instances may be found in the fact that the legislature of Louisiana established a quarantine in 1821, which was continued until 1825, during which time the city of New Orleans suffered from two severe epidemics, viz, in 1822 and 1824. What are the facts as regards these years? The board of health established a quarantine on the river below New Orleans, but none on Lake Pontchartrain, and, in consequence, in August, 1822, two sloops (crowded with persons fleeing from the fever which raged at Pensacola) passed through the lake, and entering the Bayou Saint John, without any inspection, landed their passengers at the basin of the canal Carondelet, in the center of the city. A family by the name of Lynch that arrived on one of these sloops were the first victims, and from their residence on Bienville street the fever extended throughout the city. Moreover, the health-officer, Dr. Forsyth, reported the quarantine law so defective that there was no restriction placed on intercourse between persons from New Orleans and those detained on infected ships at quarantine; and that vessels were permitted to leave the station and proceed to the city without any proper fumigation. Such are the facts of the epidemic of 1822, and it would be difficult to select an instance which more directly proves the value of a proper quarantine, instead of militating against it; for, can any one suppose that if the board of health had used efficient measures in guarding all the avenues of approach to the city there would have been any importation of the disease from Pensacola? As regards 1824, there is equally positive proof of the utter inefficiency of the health authorities. The disease was introduced by a steam-tug, which had towed the infected schooner Emigrant to the quarantine station. There had been free communication between the tow-boat and the schooner on the passage up the river. Sick men on board the latter were visited by hands employed on the former; yet the schooner alone was detained in quarantine, and the tug proceeded to the city, sending, in a few days, several cases from among her hands to the Charity Hospital with yellow fever.

Commenting on these facts, Carpenter remarks:

> In 1821 a mockery in the shape of a quarantine was established, which, *in consequence of its total inefficiency*, had no effect in preventing the introduction of the disease. It had, moreover, a most pernicious effect in prejudicing many well-intentioned persons against the establishment of such an institution by leading them to regard it as a useless restriction upon trade. The fact is well known that the inadequacy of that institution was solely attributable to the imperfection of the law, *and not to the fallacy of the principle upon which quarantine is based*. (Op. cit., p. 47.)

Of the quarantine in 1867, at New Orleans, (which has been repeatedly pointed to as an illustration of the worthlessness of such institutions,) Dr. Francis Barnes reports:

> The Florence Peters' history illustrates the viciousness of the system of quarantine in operation here, *which does not deserve the name, being a sham, a delusion, a make-believe*, in place of one which would be efficacious if properly carried out. * * * The quarantine here is notoriously a failure in sending any protection to the city, while that of New York is a success. (Circular No. I, Surgeon-General's Office, 1868, p. 120.)

Assistant Surgeon Samuel Adams, United States Army, writing of the epidemic at Galveston, Texas, in the same year, says:

> The entrance of the disease was entirely the result of gross negligence on the part of

the city authorities in failing to take any measures to establish a quarantine for the protection of the city. (Ibid., p. 83.)

A further point in regard to this same epidemic may be mentioned. Of the towns situated on Matagorda Bay, Texas, only one (Matagorda) established a quarantine in 1867. This place escaped entirely, while Indianola, Lavacca, &c., suffered severely.

The prevalence of the fever in the vicinity of the South street wharf, in Philadelphia, in 1853, affords testimony of the same character; being unquestionably due, not to the worthlessness of the principles upon which quarantines are established, but to the neglect of the authorities at the lazaretto, in permitting the bark Mandarin to come to her dock without proper detention and fumigation. And it may be remarked that this will be found true of every instance where quarantines have been denounced as incompetent to keep out yellow fever; the fault has always been either in a defective law or in maladministration of its provisions; and it may be positively asserted that in our own country, at least, every absolute quarantine conducted with rigid impartiality has proved successful. The first quarantine convention held at Philadelphia, in 1857, composed of some of the ablest hygienists in the United States, came to the following conclusion, after mature deliberation:

> Efficient sanitary measures, including quarantine, will in most cases prevent the introduction of these (importable) diseases. * * The present quarantine regulations in operation in most of our States are inefficient, and often prejudicial to the interests of the community. (Proceedings, p. 40.)

4. "That quarantine, instead of guarding against and preventing disease, fosters and concentrates it, and places it under conditions the most favorable that can be devised for its general extension."

This objection, made by the General Board of Health of Great Britain, (Report on Quarantine, 1849, p. 61,) can only, like the last, apply to a defective administration of a lazaretto. It is hardly to be conceived how it could be true, were ordinary common sense exercised in the management of either infected vessels or persons. Yet, if the account given by the board of the quarantine arrangements at Hull, England, be a fair sample of those in general operation in the United Kingdom, it is not to be wondered at that they arrived at the opinion they express. They state that this quarantine was in Whitebooth Roads, eight miles from Hull, a locality so stormy as only to be accessible by steamers; that sick and well persons were confined together on board of the detained vessels; and that the quarantine hulk at the roads had no medical officer attached to her. Further, Mr. Robert Hardey, medical superintendent of quarantine at Hull, states that he considered it doubtful whether he had any authority to visit sick persons detained in quarantine; and, although he had been in his official position for seven years, he has "no recollection of receiving either printed or written instructions from the authorities of the customs appertaining to the duties of his office, and does not consider that at present his duties are defined with sufficient clearness."

At another station, at Stangate Creek, the surgeon reported "that his orders were positive and strict not to go on board any vessel in which there was a case of cholera; that he could not, therefore, on any consideration, go on board such vessel." (Report, pp. 130–138.)

It is no wonder the poor unfortunates, subjected to such inhumanity as this, should denounce quarantine as a relic of barbarism, worthy of the Middle Ages; but that a board of men could only see in such a state of affairs an argument against the principles upon which quarantines are founded is creditable neither to their honesty nor intelligence.

There is no reason whatever why a rationally conducted quarantine station should "foster or concentrate disease." Let all persons be removed from an infected ship; the sick to a commodious hospital, the well to a separate building, only to be detained so long as may be requisite for their proper fumigation and disinfection; let all the inhabitants and officials at a quarantine station be acclimated persons, and there need be no cause to fear the extension of the disease. The records of all the well-managed quarantine hospitals in the United States contain ample proof of the truth of this statement. But if even for the sake of argument we assume the possibility of the occurrence, yet I hold that it would be no valid objection to the institution of quarantine. If it comes to a choice between the infection of a lazaretto, containing at most but a hundred or two persons, or the spreading of contagious disease broadcast through a large town, few will be found to question which is the preferable of two evils. On a par with the foregoing, and of as little importance as a valid argument, is the complaint of the inconveniences to which passengers are subjected by detention at quarantine. No doubt it is extremely unpleasant after a long voyage, and when almost in sight of home, to be refused admission to pratique; but, in the first place, were a uniform system of quarantine in operation throughout the United States, the certainty of such delay would be well understood, and persons would refrain from coming from infected ports during the fever season except in cases of necessity; and, in the second place, it is unreasonable that a large city should jeopardize the lives of thousands of its inhabitants to avoid inconveniencing the few passengers who may arrive from foreign ports. Moreover, leading quarantinists now are of opinion that it is unnecessary to the efficiency of a quarantine that healthy persons should be detained any longer than may be necessary to render it certain that they have not within them the seeds of disease, and to destroy all germs of contagion which might have infected their clothing or baggage. The *point d'appui* of the quarantine of the future is to be the disinfection of the ship and her cargo, and the care of the sick, and the well will be subjected to detention only so long as to insure their safety and avoid the possibility of their infecting the place of their destination.

5. It is further alleged that the difficulty of obtaining accurate information as to the health of the ports of departure is an insuperable obstacle in the way of an efficient quarantine. This may be granted under the present system, controlled entirely by State or municipal authority; but it would not be true of a quarantine managed by the General Government, which could instruct its foreign consuls in all ports where yellow fever prevails endemically to forward frequent reports of the condition of the places where they are resident. The lines of telegraph now are so widely extended that there would be few towns of any consequence from which a daily report could not be received. Some ports, as Havana and Vera Cruz, may be assumed to be always infected; while others may always be considered as healthy, unless positive evidence to the contrary is received. Every health-officer should be required to obtain all the information possible as to the ports from which vessels are likely to arrive, and, aided by the reports obtained from consuls, there is no reason why the most exact knowledge should not be had relative to the health of the whole yellow-fever zone.

6. The last and most important objection to be noticed is, that quarantine places unnecessary restrictions on commercial enterprises by interference with the transit of goods by the detention of ships, by the losses of perishable articles, such as fruits, and by the additional

expense attendant on increased freightage and insurance charges from delay. These charges seem very plausible, and require to be examined with some care to expose their fallacy. It is not to be denied that under the present unequal system much inconvenience results to those engaged in foreign trade; but it is believed that many of the present restrictions can be so modified, by a wise law, as to meet the acceptance even of those engaged in the West India traffic. In a subsequent portion of this report, occasion will be taken to notice the abuses and defects of the present want of system in the southern quarantines, and to suggest proper remedies; at present, I desire merely to show that the charge above mentioned cannot be fairly sustained as a reason why quarantines should be abandoned altogether. The mistake in this case grows out of the assumption that the only persons whose interests are to be considered are those who are, directly or indirectly, engaged in commerce with West Indian or Mexican ports. But this is not a fair view of the question. "Commerce," remarks the distinguished chairman of the New Orleans Chamber of Commerce, "is as much the life of the city merchant as of the ship-owner." The only vessels that need be subjected to delay at quarantine are those coming from West Indian or Mexican ports, and not even all those, but only such as arrive from places where yellow fever is actually prevailing. The business done by these vessels constitutes but a very small portion of the trade of any of the southern towns. Let there be an epidemic in New Orleans, Charleston, or Galveston, and all their immense traffic with the interior is immediately cut off or greatly diminished. Country merchants will not visit the cities to buy goods during the progress of an epidemic, not only on account of the personal danger to themselves, but also because the goods they buy may be infected, and thus the disease be introduced into the country towns. This actually happened at Washington, Louisiana, in 1826, at Opelousas in 1828, and at Galveston in 1858. Moreover, New York, and all other places at the North, enforce quarantine against the southern sea-ports as soon as yellow fever appears at any of them; and thus the presence of the disease in the latter operates as a restriction on the great trade between the two sections of the country; a matter of far greater importance to the South than the temporary interruption to her West India commerce, which would be caused by an absolute quarantine, conducted on rational principles. Even if such a system had the effect of entirely extinguishing the West India commerce, it would still be a pecuniary gain to the South in the increased trade which would come to her ports from other directions, as a result of the feeling of security accruing from the certainty that these places would be kept healthy.

Should the restrictions on the commerce with the West Indies by quarantine amount to a suspension of the whole trade, for six months each year, the evils to the city will be less than it now suffers from the annual apprehension of the fever, and the pecuniary loss of a hundred years by the quarantine establishment cannot equal the ruin and desolation of a single season of the pestilence. * * * Is the health of the city to be placed in competition with a few cargoes of sugar and molasses, introduced without care or caution, so as to afford a luxury to our people at the least possible expense of money and at the greatest cost of human life? (Hume, Report to City Council of Charleston, Charleston Medical Journal, vol. 9, pp. 150-151.)

The editor of this journal further remarks:

We would recommend a more stringent quarantine system, which would exclude all vessels from infected ports from our harbor. The petty traffic, during the summer, with the tropical ports usually infected with yellow fever, should not be allowed to continue, as millions of dollars are lost to the city when fever is introduced, and hundreds of lives are wantonly sacrificed. We trust that in future the city authorities will look to the interest of the many, and enforce the quarantine law now in existence, or enact others which may prove efficient. (Ibid., vol. 11, p. 850.)

It is admitted that much inconvenience would result to the mercantile community from the strict enforcement of the aforesaid measures, (of quarantine,) but it is believed that the loss of property in the aggregate would be much greater if our citizens shall be compelled annually to flee from the pestilence than would result from a system of strict quarantine, duly observed and rigidly enforced. (Dr. A. F. Vaché, Letter to Committee of House of Assembly of New York, November 5, 1845.)

Vexatious and inconvenient as quarantines may be, yet even under the worst circumstances, their restraints fall upon a very small portion of any community, and those the ones best able to bear them. An epidemic of yellow fever affects all classes and conditions. It brings desolation into every household, paralyses trade, stops immigration, and causes an enormous waste of money and the material resources which go to make up the prosperity of any city. Especially in its present condition the South needs men, yet those who would add to its material prosperity will not go where they cannot have a permanent residence for themselves and their families, but are obliged to leave every summer to escape the pestilence. It may safely be asserted that the southern cities would soon double their population were it not for the dread which universally exists of these epidemics of yellow fever. Replace this feeling by one of security, such as will be given by a well-regulated quarantine, and the unequaled advantages which the southern sea-port towns offer for commercial enterprises of every kind will attract the capital and labor which more than anything else they require. The public mind is not disposed to theorize on such questions. "It demands protection against the importation of infectious disease, and will not sanction the abolition of quarantine inspections." The moral effect of an efficient quarantine on the community at large outweighs in its importance all the inconveniences resulting from the restrictions placed on a portion of the southern trade; and when to this is added, as it justly may be, its absolute protective influence, the objections made to its continuance will not for a moment stand the ordeal of investigation. The whole argument may be summed up in the following words of wisdom from the pen of Professor John T. Metcalfe:

To abandon quarantine is to put a price on human life and barter it for trade. (Essays of the United States Sanitary Commission, p. 272.)

It being then conceded that if the Southern and Gulf ports are to be protected from periodical visits of yellow fever, an effective quarantine must be maintained, the question next arises whether those now in operation at the South are sufficient to accomplish the desired end. To this there can, unfortunately, be but one answer. They neither commend themselves to the medical profession nor to the community at large. While agreeing in their main features, they lack that uniformity which is especially desirable to secure efficiency. A brief abstract of the laws now in operation in the various States will present their deficiency in this regard more forcibly than any description.

In Virginia it is left entirely optional with the authorities of any town whether they will establish a quarantine or not, but even if they decide to do so, the consent of the county court must first be obtained before they can use any land without the limits of their town. They (the town-council) are authorized to prescribe all rules and regulations for the management of such quarantine. The health-officer is appointed by the town-council, and has authority to direct what vessels are to be quarantined, and may cause *all* persons arriving in such vessels, or handling cargoes, to be removed to a hospital, at his own discretion.

All expenses incurred by the quarantine are to be paid by the master or owner of the vessel, or by the person on whose behalf such expense is incurred.

Penalties for violations of quarantine range from five to five hundred dollars fine, and may be collected before any justice of the peace. Persons coming into town by land, if infected with dangerous disease, may be detained by the health-officer, and restrained from traveling, *unless it is to return to the State from which he came.* Pilots are directed to conduct all vessels to quarantine that *they find* have infectious disease on board, for which they are allowed an extra fee of $7; and if, in consequence of his having gone on board of a vessel having contagious disease among her crew or passengers, any pilot is detained in quarantine, he is allowed $3 per day for each day's detention. Both of these extra fees are to be paid by the master or owner of the vessel.

With these exceptions, no provision is made for the collection of fees, leaving it to be presumed that the amount of such fees is optional with the town-council or the health-officer. (Code of Virginia, 1860, pp. 447, 448, 483.)

The law of the State of North Carolina provides only for a quarantine at the mouth of the Cape Fear River. It is presumed if any other port in the State desires a quarantine, that the municipal authorities will take such action as they see fit. The health-officer (designated in the law as "medical quarantine officer") is appointed by the governor, and is authorized "to prescribe such regulations as may be necessary for the protection of the inhabitants from contagious diseases." He is required to make out a monthly report of all receipts and disbursements, and to pay over all moneys received to the treasurer of the State. The State furnishes the boat for the use of the health-officer, and establishes the quarantine hospital; but all persons employed at the station are designated by the health-officer. Vessels subject to visit and inspection are required to pay a fee of $5, and every sick person taken to hospital $3 a day, the vessel being held responsible for all such expenses. The medical officer alone has authority to declare what ports are infected.

Pilots are required to bring vessels to the quarantine anchorage, and any pilot violating the law forfeits his commission.

Masters of vessels and other persons refusing to obey the quarantine regulations forfeit and pay a fine of $200 for every day they so refuse, and all persons violating any regulation prescribed by the health-officer forfeit $200 for each offense. All penalties and forfeitures so incurred "may be recovered before any jurisdiction having cognizance of the sum due, and applied, *one-half to the informer*, the other half to the payment of the expenses of the quarantine establishment."

The health-officer is authorized to issue his warrant to any sheriff or other officer, commanding him to arrest any person violating the quarantine. The compensation of the health-officer is fixed at $600 a year, and of the boat's crew at $20 a month. (Laws of North Carolina, 1868, pp. 46–48.)

By the law of the State of Florida the mayor and aldermen of every incorporated town in the State, upon or adjacent to any bay, river, or harbor, or in the absence of such incorporation, the justices of the peace of any justice's district, are constituted a board of health, who are authorized to take such measures as they may deem necessary for the prevention of the spread of contagious diseases, the expenses to be a charge on such incorporated town, or, in the absence of any incorporation, upon the county, (except as subsequently provided for in the act.)

Such board of health is authorized to appoint "port-inspectors," whose duty it shall be to board all vessels approaching the port from the high seas, and ascertain the condition of health on such vessels, and, if they consider them infected, are authorized to order them into quarantine. *Pilots may be appointed port-inspectors.* The board of health is required to appoint a port-physician, who *may* be required to examine into the condition of any vessel ordered into quarantine.

No vessel in quarantine is permitted to approach the port or town unless the master thereof has a certificate from such physician to the effect that no danger to the public health is likely to occur thereby; and if disease break out on any vessel while in port, the board of health is authorized to remove her to the quarantine-station.

The location of the quarantine-station, and all rules and regulations for its governance, and the number and character of the employés, are fixed by the board of health. Sheriffs and constables are required to aid the board in enforcing these rules.

Violators of the law are liable to a fine not exceeding $300.

The governor of the State is authorized to issue his proclamation declaring what ports or localities are infected.

Masters, pursers, and owners of ships refusing to obey the orders of a port-physician or port-inspector may be fined not to exceed $1,000, or imprisoned until such fine and the costs be paid; and any vessel may be attached or may be sold for satisfaction of such fine and costs.

The mayor and aldermen of the town prescribe the compensation of the port-physician and port-inspector; the former is paid out of the treasury of the town, and the latter by the master or owner of the ship placed in quarantine. There is no section fixing the amount of the fees to be collected. Unless the vessel has contagious disease on board, quarantine is limited to twenty days. Whenever practicable, the duties of port-physician and port-inspector shall be performed by one person. (Laws of Florida, 1869, pp. 15, 23, 26.)

The law of the State of South Carolina is based upon that of New York, and is very complete in its details. It provides a quarantine for the ports of Charleston, Georgetown, and Hilton Head.

The health-officers at each of these ports are appointed by the governor, and hold office for two years, unless sooner removed. The one at Charleston receives a salary of $1,500 per annum, and those at the other places $1,200, payable quarterly out of the treasury of the State. In addition, each health-officer is entitled to $15 per month for boat-hire and incidental expenses. The law prescribes what vessels shall be quarantined, as follows:

1. All vessels coming from infected ports, or having any case of contagious or pestilential disease on board during the voyage and arriving between May 1 and November 1, are quarantined for at least thirty days after arrival and twenty days after their cargo has been discharged, and undergo such further detention as may be considered necessary by the health-officer.

2. All vessels from any place in Asia, Africa, or the Mediterranean, or from the West Indies, Bermuda, or Western Islands, or from any place in America, in the ordinary passage from which they pass south of Hilton Head, and all vessels having had any sickness on board during their voyage arriving between the same dates, shall be subject to visitation by the health-officer, but shall not be detained unless they have had contagious or infectious disease on board during the passage, in which case they shall undergo such quarantine as the health-officer may prescribe. Vessels navigated by steam are not to be detained

longer than may be considered necessary by the health-officer. The health-officers and city officials have power to remove any vessels from the wharves to the quarantine-grounds, and may send thither all goods, stores, or persons arriving by such vessels, and such vessels or persons are not permitted to return to port without the written permission of the health-officer of said port. Vessels arriving at quarantine may put to sea again, provided they do so before breaking bulk. The duties of pilots, in connection with the arrival of vessels, are carefully defined. They are to inform themselves of the probable condition of the vessels they hail, and whether they are subject to quarantine or examination by the health-officer; and if they find that they are so, they are to bring such vessels to anchor within the buoys marking the quarantine anchorage, and to be careful to see that no violations of the quarantine act are committed while the vessel or vessels are under their charge. They are further required to subject themselves to such detention for fumigation and purification as may be considered necessary by the health-officer. Health-officers are required to reside at or near the quarantine-ground, and must visit every vessel subject to examination immediately on arrival, between sunrise and sunset; to inquire into the health of all persons on board, and the condition of the cargo; inspect the bill of health, manifest, and log-book; examine on oath any or all persons on board, so as to enable them to judge as to the disposition to be made of the vessel. Whenever any vessel is placed in quarantine, the health-officer is required to report all the facts to the mayor. Health-officers have entire control over affairs at their stations, and may cause vessels to be anchored at such places as they may think the safest for the public health; may cause persons or cargo to be removed from any vessel, and said vessel to be disinfected or purified in such manner as he shall direct; and may destroy any bedding, clothing, or cargo as they may judge necessary to be so destroyed to prevent infection, provided that in the case of the destruction of cargo, the concurrence of the mayor of the city shall first be had. They are also authorized to administer oaths and take affidavits in all examinations prescribed by law. Boats are forbidden to pass through the range of vessels lying at quarantine, and lighters forbidden to unload such vessels without permission of the health-officer. All expenses incurred at the quarantine-stations shall be paid by the master, owner, or consignee of such vessel, and all such expenses shall be a lien on the vessel, and the health-officer may detain such vessel at the quarantine-station until such expenses are paid. To oppose or obstruct the health-officer in the performance of his duties is a misdemeanor, punishable by a fine of not less than $100 nor more than $500, or by imprisonment in the penitentiary from three to six months. Masters of vessels who refuse or neglect to obey the provisions of the quarantine law, or give false information relative to their vessel, or the health of the places from which they departed, or any other person violating any of the provisions of the act, are guilty of a misdemeanor, and may be punished by a fine not exceeding $2,000, or by imprisonment not exceeding twelve months, or both. Persons who are aggrieved by any decision or order of the health-officer may appeal to a board composed of the governor, attorney-general, and comptroller-general, who shall have power to reverse or modify such order or decision of the health-officer. The governor may, from time to time, declare by proclamation what ports are to be considered infected within the meaning of the act. There is no provision made by this law for the collection of fees for visiting vessels or fixing the amount of such fees. (Act approved September 26, 1868.)

The quarantine law of Georgia provides that the corporate authorities of any town may prescribe all rules and regulations for the preservation of the public health. In the establishment of quarantine grounds, the jurisdiction of the corporation of Savannah shall be held to extend to all vessels entering at any port or inlet from Ossabaw Sound to Tybee. All persons coming into the State from an infected port, or otherwise violating the quarantine regulations, may be indicted in any county when found, and, on conviction, may be fined not exceeding $500, or imprisoned at the discretion of the court. Any justice may issue his warrant to any sheriff or constable, commanding him to arrest persons guilty of a violation of the law. Health-officers are appointed by the corporate authorities of the town, and have power to remove any vessel to quarantine, or detain them there as long as they may deem necessary. He is authorized to examine all persons on board, under oath, and any person refusing to answer may be find $100. Persons coming into any town by land from an infected place, or suffering from any infectious disease, may be compelled to perform such quarantine as the health-officer may consider necessary. Pilots are required to make themselves acquainted with the condition of vessels they board, and of the ports from which such vessels came; and, when subject to quarantine inspection, to conduct them to the proper anchorage. When a vessel is detained in quarantine no person is permitted to land therefrom, under a penalty of fine and imprisonment. When any vessel is admitted to pratique, the health-officer must give the master thereof a certificate, stating that the vessel has performed quarantine.

The governor of the State may, at his discretion, issue his proclamation, declaring what ports are infected. All fines collected under the provisions of the law are to be paid into the county treasury, and to be expended in carrying out quarantine and sanitary regulations. The health-officer is authorized to collect a fee of $2 for inspecting each vessel. (Revised Code of Georgia, pp. 254–258.)

In Alabama, any town may establish a quarantine, but if located without the limits of said town, the consent of the county commissioners must first be obtained. All needful regulations for its management are prescribed by the corporate authorities of the town, and persons violating such regulations, after five days' public notice shall have been given of their establishment, shall be fined not less than $50. Health-officers are appointed by the authorities of the town, and have power to remove all infected or foul vessels to the quarantine anchorage, there to undergo such detention as may be considered necessary for the public health. Any master, seaman, or passenger on board such vessel refusing to answer all questions put to him under oath, relative to the condition or health of the ship or port from which she came, may be fined not less than $100. Masters of vessels refusing to deliver their log-books, manifests, or bills of health to the health-officer for his information, or to repair with their vessels to the designated anchorage, must be fined not less than $200. Any justice may, on complaint of a health-officer, issue his warrant to any sheriff, constable, or marshal commanding the arrest of any person charged with violation of the law. Persons coming into town by land from an infected place, may be detained in quarantine, and, on escape therefrom, are subject to a fine of $100. All expenses are to be paid by the vessel or person in whose behalf such expenses were incurred; and all fines are to be paid into the treasury of the town where the quarantine is established. (Revised Code of Alabama, pp. 230–232.

The legislature of Mississippi has passed no general quarantine law, but

has simply enacted that the board of police of any town, or the municipal authorities, shall have power to pass such ordinances as they may see fit, to prevent the introduction of infectious or contagious diseases.

The State of Louisiana directs that a quarantine be established on the Mississippi River, not less than seventy miles below the city of New Orleans, and subordinate stations at the Rigolets, (entrance to Lake Pontchartrain,) and on the Atchafalaya River. These stations are to be under the control of a board of health, to be composed of nine competent citizens of the State, three of whom are to be selected by the city council of New Orleans and six to be appointed by the governor of the State, and all to be selected with reference to their known zeal in favor of a quarantine system, and to hold office for one year. This board is to meet once a month, from November till June, and once a week between June and November. This board appoints the resident physicians at the Rigolets and on the Atchafalaya, and all nurses, boatmen, and other employés at every station. They have the power to fix the number of days' quarantine for vessels, provided it be not less than ten; to determine how such quarantine shall be performed; to make all legal arrangements necessary to carry out a proper system; and to issue warrants to any constable, sheriff, or marshal, commanding him to arrest any person charged with violation of the quarantine law.

The resident physician at the station on the Mississippi River is appointed by the governor of the State, and receives a salary of $5,000 a year, and is entitled to one assistant at a salary of $2,000 a year. He is required to visit every vessel approaching New Orleans by way of the Mississippi River, and is entitled to charge fees as follows: On vessels of one thousand tons burden, $30; ships less than one thousand tons burden, $20; barks, $15; brigs, $10; schooners, $7.50; steamboats, $5: provided that the fee for steamships coming from any port in Alabama, Florida, or Texas, shall be $10; and for all other steamships, $20. The health-officer has power to detain vessels, with their crews, passengers, and cargoes, whenever they come from an infected port, or where such vessels have any case of contagious disease on board; and may keep them at quarantine for such time as he may consider necessary, provided it be not less than ten days. He can compel the captain of any vessel so detained to land the sick at the quarantine ground, and may cause such vessels to be cleansed and fumigated. All expenses, including $5 for every person so landed, shall be borne by the master, owner, or consignee of the vessel. The board of health may, from time to time, delegate such other powers to the resident physician as may be necessary for the performance of his duties.

The governor is required to issue his proclamation on the advice of the board of health, declaring what ports are to be considered as infected, after which all vessels arriving at any of the quarantine stations from such ports are to be subject to all regulations prescribed by the board of health. Masters of vessels refusing to obey any of the quarantine regulations may be fined not exceeding $2,000, or be imprisoned for twelve months, or both. All persons who neglect or refuse to obey the orders of the board of health may be punished by fine and imprisonment, at the discretion of the court. Resident physicians must report to the attorney-general all violations of the statute, and it shall be the duty of this official to prosecute the offenders. During the summer tow-boats are to be subject to the same inspections as vessels coming from sea. Any person going on board a vessel detained at quarantine, without permission from the health officer, is subject to a fine of $50. When a vessel is admitted to pratique, the health-officer must

give the master of such vessel a certificate, which shall be delivered to the harbor-master at New Orleans on her arrival.

Vessels out ten days from infected ports having clean bills of health, and having had no sickness on board during the voyage, will only be detained at the quarantine-station long enough to undergo fumigation.

The health-officer shall also have power to grant permits to acclimated persons to proceed to the city without detention.

In cases of emergency the board of health shall have power to issue a proclamation of quarantine, and to enforce the same, without reference to the governor of the State. (Acts approved March 15, 1855, and March 18, 1858.)

In Texas the governor is authorized to issue his proclamation declaring quarantine, whenever in his judgment it shall be necessary; and for such length of time as he may deem essential to prevent the introduction of any infectious or contagious disease. As soon as practicable after such proclamation is issued the corporate authorities of every town and city on the coast shall establish a quarantine-station, and appoint a competent physician as health-officer, and furnish him with everything necessary for the proper discharge of his duties.

Health-officers are required to make a rigid examination of every vessel coming from an infected port, whether she has a clean bill of health or not. They are authorized to take the affidavits of the master as to the health of himself and crew; may detain the vessel at quarantine for the length of time designated in the proclamation of the governor, and may use force when required in order to discharge their duties.

When there are no town or city authorities, the governor shall appoint the health-officer and prescribe regulations for quarantine.

Any vessel arriving at a quarantine-station without a clean bill of health shall be taken possession of by the health-officer, and the master thereof shall be deemed guilty of a misdemeanor, and upon conviction thereof shall be fined, not less than $500 nor more than $5,000; and said vessel shall be held by the health-officer until such fine and costs are paid or such vessel replevied; provided that the payment of the fine and costs shall not operate as a release of the vessel from undergoing the usual detention at quarantine. Every master of a vessel that passes, or attempts to pass, the quarantine-station without permission from the health-officer is deemed guilty of a felony, and, on conviction thereof, may be imprisoned in the penitentiary not less than one year nor more than five years, or fined not less than $500 nor more than $10,000. Persons belonging to a vessel, when placed in quarantine, who shall go ashore without permission of the health-officer shall be fined not less than $50 nor more than $500.

Any person who lands, or permits to be landed, any goods, wares, or merchandise, from such vessel, without the written permission of the health-officer, shall be fined not less than $50 nor more than $1,000 for each article of merchandise so landed. The health-officer shall collect fees as follows: for each vessel of one hundred tons or under, $5; for every vessel of over one hundred tons, $5; and also a further fee of 1½ cents for each ton. These fees are to be reported to the corporate authorities of the town where quarantine is established, and all fees and fines collected shall be used to defray the expenses of quarantine. On the cessation of the quarantine in each year the town or city authorities shall make a statement of receipts and expenditures to the comptroller of the State, and, should the expenditures amount to a greater sum than the receipts, they shall draw from the treasury of the State, upon an order from the comptroller, a sum sufficient to meet the defi-

ciency; and should the receipts exceed the expenditures, the excess shall be appropriated to the maintenance of marine hospitals in their respective towns or cities. Any town or city may establish such additional regulations for quarantine as the authorities thereof may judge advisable. (General Laws of Texas, 1870, pp. 3, 5, 75–77.)

Besides these general laws, the municipal authorities of most of the southern cities and towns have passed ordinances defining rules and regulations for the management of their respective quarantines, and these differ from each other to the same extent that the general laws do. It needs but a glance at the foregoing abstracts to be convinced how impossible it is to maintain an effective system of quarantine based on such heterogeneous enactments. In some cases the health-officer is appointed by the governor, in others by the municipal authorities; in some, the administration of sanitary measures is confided to a board of health, in others to the municipal councils, and in others to the health-officer alone. Sometimes the fees collected are to be paid into the State treasury, at others into the city; and sometimes no provision at all is made for their imposition. The amount of fees and penalties imposed for violation of law differs in every State to such an extent as to produce confusion. The authority of the health-officer in some cases is almost absolute, in others he is nearly powerless. In no case is proper provision made for the disinfection and fumigation of vessels and cargoes; the mode of doing so being left entirely to the discretion of the health-officer. In more than one instance the duty of inspecting vessels is confided to pilots, a class of men whose education does not permit them to be competent judges of the nature of sickness on a ship or of her sanitary condition. Unquestionably, the best of all these laws is that of South Carolina; yet this is defective in many particulars, as, for instance, in not defining and classifying cargoes so as to declare which may be fomites for the transmission of the disease and which are harmless. Beyond one or two vague expressions relative to "competency," there does not seem in any of these laws any requirements insisted on defining the qualifications required in the health-officer; and with the exception of New Orleans, the salaries paid are such as to render it impossible that physicians of experience in hygienic laws can be obtained, unless they are permitted to devote a portion of their time to other pursuits, which in itself would be a fatal bar to their efficiency.

The want of uniformity in regard to the duration of the quarantine will also be noticed. In some cases it is dependent on the proclamation of the governor, at others on the will of the board of health or the visiting physician. Unless issued after the receipt of accurate information as to the condition of all the West Indian and Mexican ports, which is not the case, the proclamation of the governor is an absurdity. For instance, the governor of Louisiana issues his proclamation, specifying certain ports which he declares infected, on and after the 1st of July, utterly irrespective of what their actual condition of health may be; and all vessels arriving from any of those ports are obliged to undergo a certain number of days' detention, without regard to their condition or the health of their crews and passengers. If a ship should have left Havana on the 30th of June, she comes from a healthy port and is not quarantined; but if her departure takes place the next day, she is detained for ten days.

A second objection to the present system of quarantine at the South is in its administration. Defects and abuses exist fatal to efficiency, which can only be removed by the adoption of an entirely different organization.

The first relates to the character of the health-officers. While some of these, notably at the larger ports, are men of character and experience, most owe their appointments to political influence, personal popularity, or, especially unimportant places, to the fact that, owing to the paucity of salary, no one else could be found to accept the place. I could not learn that provision is ever made for the examination of health-officers as to their qualification for such an important trust; nor are inspections ever made of their stations, except perhaps at the important commercial centers.

A second defect grows out of the neglect of the authorities to provide suitable quarantine accommodations. The station on the Mississippi River, below New Orleans, is an exception, but it is the only one with which I am acquainted. A complete quarantine-station should consist of a hospital for the sick, a commodious house for the temporary detention of the well, a residence for the health-officer, and store houses for the care, fumigation, and disinfection of cargoes.

Some stations, as at Galveston, have none of these; others have a building which may be used for a hospital; but generally there is no provision made for the storage of cargoes or the residence of the health-officer. In consequence of this last omission this official resides in the town for which the quarantine is established, and only visits his station when called on to inspect a vessel. Apart from the constant communication thus kept up between the station and the town, which may in itself be the means of introducing disease, it is manifest that efficiency must be sacrificed where the health-officer resides miles away from the scene of his duties, and must consequently intrust a large share of his responsibility to subordinates.

A third defect is found in the manner in which the health-officer performs the duties. There is a great deal in the various laws about inspections of log-books, manifests, and bills of health; mustering of passengers and crew; examinations of masters and others under oath; but practically all these requirements are seldom enforced. What actually takes place is for the health-officer to come on board as soon as the vessel heaves to off the quarantine-ground, hurry to the captain or purser's office, and, if she come from a non-infected port, simply inquire if all are well on board, receive his fee, and depart as soon as possible. If from an infected port, he directs the master to bring his ship to anchor, and does not concern himself further about her unless she have sickness on board. During his brief stay on board, the steward of the ship is generally engaged in loading the health-officer's boat with fruit, ice, or other delicacies, which are received as a matter of course, and, indeed, any omission in this particular has been known to subject certain lines to great inconveniences. Lest this may be thought an exaggeration, I may mention that the health-officer of one of the southern ports told me some years ago that he saved all his salary, as he was entirely supported by the great variety of articles he received in this way. As at present administered, the visits of inspection are simply a farce, the only object being to collect the fee and receive the *douceurs*, which are the perquisites of the health-officer.

The present system of collecting fees is a serious abuse, which goes a great way to dissatisfy the mercantile interests with quarantine.

At some of the southern ports, especially those on the coast of Texas, the fees are placed so high as to yield a considerable revenue, and a large portion of them is collected from vessels in the coastwise trade, that should not justly be brought within the jurisdiction of the quarantine law at all. Thus, at Galveston, Texas, there is a large trade in

lumber from Sabine and Calcasieu, which is carried on with small schooners, ranging from twelve to forty-five tons burden. Each of these little vessels, coming from ports less than a hundred miles distant, is required to pay $5 quarantine tax on her arrival at Galveston. She unloads, and taking in a cargo of groceries or "store goods," or, quite as often, only in ballast, returns to Sabine, where she is again mulcted of $5. Many of these vessels make one or two trips a week, and, their average net profit not exceeding $40 per round trip, they are thus required to pay about one-fourth of this amount as a tax to the State. Further, I was informed by an intelligent citizen of Sabine that these boats are hardly ever inspected by the health-officer. His boat is sent out, with the "permission to pass" duly made out and signed, and, coming alongside the inward-bound craft, the boatman passes up the certificate, the money is handed over, and not a question asked.

The steamers of the Morgan line run from New Orleans to Galveston, thence to Indianola, then return to Galveston, and thence back to their port of departure. Other steamers of the same line run to Brazos Santiago, via Rockport or Aransas Pass. At each of the Texas ports they have been required to pay fees, amounting to $20 for each vessel, and, on returning to New Orleans or Berwick's Bay, $10 more. I was informed by Mr. A. C. Hutchinson, agent for the line in New Orleans, that it paid over $9,000 quarantine fees in 1871. No examination of the ship or muster of the crew is ever made. This one line has, as is well known, paid fees more than sufficient to support the quarantine along the whole Texas coast. Early this year the agents of this line sued the health-officer at Galveston for the recovery of this tax, and, pending a decision, applied to Justice Joseph P. Bradley, of the United States Supreme Court, for an injunction restraining the health-officer from collecting any more fees. Justice Bradley, in granting the injunction, remarks:

> In this case we think that the charge which by act of the legislature is authorized to be exacted from all vessels coming to the quarantine-ground, namely, $5 for the first one hundred tons, and 1½ cents for each additional ton, is a "duty of tonnage" within the meaning of the Constitution; and that it is also an unconstitutional tax upon the commerce of the port. The States have a right to establish quarantine regulations for the preservation of the public health; but they have no right to place toll-gatherers at the gateways of commerce, and lay indiscriminate exactions upon all vessels that enter thereby.
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> We think, therefore, that the tax-duty or fee which is complained of is unauthorized and void, and while the complainant might, by successive actions at law, recover the amount exacted, this court, as a court of equity, in order to prevent a multiplicity of suits, has a right to entertain a bill for an injunction. We therefore grant the injunction as prayed, with liberty to the complainant to enter a final decree for a perpetual injunction, if he elects to accept the answer without replying thereto, unless the defendant desires the usual time for final hearing.

Since the rendering of the above decision no fees have been collected at any of the Texas ports. While on this subject, it may be remarked that it is very questionable whether the present system of supporting quarantines by fees collected from vessels is not altogether unadvisable. At most ports the whole expense of maintaining a quarantine is paid by a few ship-owners, who derive no benefit therefrom, but are rather embarrassed by an inquisition which is solely for the protection of the community at large. Where a vessel at quarantine requires fumigation and cleaning, or where hospital accommodation has to be provided for the sick, it is but proper that the extra expense attendant on such measures should be borne by the owners of the infected ships, who would thus be rendered directly responsible that their vessels were not the agents for the introduction of disease. With this exception, a direct tax on the

community at large that reap the benefit of immunity from pestilence would seem more just. Not only the unacclimated but every class benefits by freedom from epidemics, and there is no reason why all should not bear a share of the expense of the maintenance.

Still another defect in the present administration is found in the manner in which vessels are compelled to undergo quarantine. The custom at most of all the stations is to detain vessels coming from an infected port for a certain number of days (usually ten) from the date of their departure from such port, or (if she should have had any sickness on board during the passage or on arrival) for ten days from the disappearance of the last case. Now, the period of incubation of yellow fever being variously stated from two to forty-three days, (Gazette médicale de Paris, September 28, 1872, p. 477,) it is manifest that either this period must be too long or too short, and that any detention founded on what is yet a matter of mere speculation must be empirical. It is, moreover, objectionable as leading to unnecessary restrictions on commerce by delaying the days of sailing of regular lines, and by subjecting merchants and shippers to vexatious expense, exciting a strong prejudice against quarantine. Further than this, it is of not the slightest use to detain a vessel at a quarantine station, even for six months, if her cargo be not broken out. It is in the hold of the ship that the morbid poison is carried, and the air at the bottom of the hold is certain to be most impregnated with it. It is impossible to disinfect this cavity by merely raising the hatches and making fumigations without the removal of the cargo, and yet if disinfection is practiced at all (which is very seldom) this is the mode adopted. Secondly, no discrimination is made between different kinds of cargo. All hygienists know that some articles are more apt to retain the infection of a contagious disease than others: these are cotton and woolen goods, hides, rags, coal, &c. Others may become dangerous by undergoing putrefactive decomposition after shipment, as sugar, coffee, and fruits; while many others are under all circumstances harmless. No distinction is made at the majority of stations between these cargoes; they are alike detained, and, for want of proper accommodation on shore, are kept in the hold of the vessel—an entirely useless procedure.

Thirdly, the manner in which passengers are treated is not in accordance with the most enlightened views on the subject of the origin of epidemic diseases. Sick and healthy persons are detained together on ship-board, or at the station on shore, if there be any building for the purpose—a course which is extremely likely to increase the amount of sickness, and thus develop new germs for the propagation of the disease; so that the quarantine-station soon becomes a "pest embankment," which may infect other vessels passing it or temporarily detained in the vicinity.

The rational management of a vessel required to undergo quarantine should be as follows:

First, her passengers and crew should be removed from on board, the sick to an hospital for treatment, and the healthy to a separate building. The latter should be required to change their clothes, and to submit to such cleansing of their persons and other disinfection as the health-officer may judge advisable; their clothing and personal baggage should be thoroughly disinfected by means of the most approved modern appliances, and then those who are acclimated should be permitted to pass the quarantine, while others may be detained in a quarantine of observation; no longer, however, than is necessary to prove that they are free from the morbific elements of the fever. Secondly, the cargo

should be broken out and assorted, and, if of a character to transport infection, should be placed in warehouses in the vicinity of the quarantine grounds, whence it should not be removed until the occurrence of frost, while, if harmless, after due fumigation, it might be transported direct to its destination. Cargoes of fruit, sugar, coffee, canned articles, provisions, &c., should be thoroughly inspected, and whatever portion found to be tainted or spoiled immediately destroyed; but there should be no detention of articles of a perishable nature beyond the period necessary for such inspection. Dr. Elisha Harris, of New York, suggests that samples of cargoes might be specially disinfected by artificial cold, and then safely offered for sale. Probably the use of superheated steam or of Wells's apparatus for the use of steam and carbolic acid spray could also be made serviceable with all cargoes that are not of a character to be injured thereby. Thirdly, the cargo being removed, the vessel should be properly ventilated, fumigated, and disinfected; not in the hasty and imperfect manner usually adopted, but so completely as to render it impossible that any part of her hold, forecastle, steerage, or cabin shall remain in a foul condition. The baggage, clothing, and bedding of any person who has died on board should be destroyed by fire; the ship constantly ventilated by means of wind-sails; the bilge-water pumped out two or three times a day, and water from outside pumped in, and every portion of her cavities subjected to disinfection and fumigation. This need not take more than two or three days, after which it is useless to detain the vessel, and she should be permitted to proceed to her destination. It is true that cases have been known where the fever has broken out on ships, after the most complete disinfection, (notably on the United States frigate Susquehanna some years ago,) and it is not claimed that this course will procure absolute security, but that it will be all that is necessary in the majority of instances, and that no length of detention, after proper breaking out and disinfection, adds anything to the security. On the subject under discussion, Harris remarks:

> The accumulation of a large amount of infected materials without complete ventilation, or the close aggregation of a large number of infected vessels with their cargoes in the absence of other ventilation than that attainable by the usual means on shipboard, would, during any of the summer months, constitute a *pest embankment*, dangerous to all persons and vessels coming in near proximity thereto. Therefore, we must conclude that a safe anchorage, completely removed from the track of other vessels and the ordinary paths of commercial intercourse, with ample provisions and appliances for securing the storage and complete ventilation of cargoes, &c., constitute most essential requisites for a quarantine establishment.
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> The lighterage of cargoes directly from infected vessels to the city or elsewhere should be prohibited, and in its stead a commodious warehouse system should be provided near the anchorage on the most isolated portion of the quarantine grounds. To those warehouses all cargoes should be transferred from infected vessels immediately after arriving. (Reply to Quarantine Commissioners of New York, January 28, 1858, Assembly Document No. 69 of 1858, p. 27.)

The last inquiry made by the joint resolution is, "What system will least interfere with the interests of commerce at said ports?" Having already given my opinion that quarantine can be made an effective protection against the invasions of yellow fever, and that under a wise administration of its provision it need not interfere to any great extent with the commercial prosperity of the southern ports, and having shown in the preceding pages that the quarantines under the present State and municipal regulations are not to be depended upon to accomplish the desired end, it follows that some other system must be looked to to secure immunity from epidemics. On this point the opinions of those with whom I had an opportunity of conversing was nearly unanimous that

only under the direction and control of the General Government can any reasonable degree of protection be afforded, conjoined with such competency of administration as will afford the largest liberty to the interests of trade, consistent with public safety.

The board of health of the city of Pensacola state that—

> This board does earnestly solicit that Congress may take such further action upon the subject as shall place the whole matter of quarantine under the exclusive jurisdiction and control of the authorities of the United States.

Dr. Jerome Cochran, health-officer of the city of Mobile, writes:

> I am thoroughly satisfied that a quarantine under the control of the General Government would be far more efficient than any other plan that can be adopted. We need a complete sanitary cordon all along the Atlantic coast, and especially all along the coast of the Gulf of Mexico, and this will be very difficult under the action of the various State and municipal governments. The fever-poison follows the lines of commercial intercourse, and the power which regulates commerce should be extended also to the regulation of quarantine. (Letter, September 28, 1872.)

Professor J. T. Gilmore, of Mobile, writes:

> Believing that the yellow fever can be imported, I, of course, am an advocate of efficient quarantine, and I am satisfied that this cannot be done except by the Federal Government. Local and private interests always so vitiate the efficiency of a local quarantine that it is rendered useless. (Letter, September 10, 1872.)

Mr. J. H. Oglesby, president of the New Orleans Chamber of Commerce, is of opinion that—

> The laws imposing quarantine from foreign infected ports ought to be uniform throughout the United States. In giving expression to this opinion it is not necessary to discuss the extent of power legally to be exercised by the Federal Government. It is sufficient to know that the power to regulate commerce enables Congress to impose a uniform tax on all commodities imported, and so prevent one city from intercepting the commerce of another by an offer of more favorable duties on such imports. The effects of the quarantine system, as adopted by the several States, enables one city to cut off all communication with another, except under certain conditions fatal to the trade. Congress should, therefore, organize an appropriate bureau of public health. With the chief of this department should be reposed the power to make, at his discretion, proclamation of the existence of infectious or epidemic diseases at any foreign port, and thereupon to declare such foreign port interdicted (except on conditions defined by law) with all ports within the jurisdiction of the United States, and to make such interdiction at his discretion. The health authorities at all such American ports should be appointed or approved by the bureau of public health. The salaries of all such officers and employés so appointed, with all other expenses properly attending the enforcement of the law, should be paid out of the Federal Treasury, and in no other manner. * * * The Federal Government can better preserve the general health *and protect the general commerce* than can be done by the competitions or even combinations of rival cities. (Letter, July 20, 1872.)

Many assurances of a similar character were given to me in conversation with persons, both professional and otherwise, in every city visited. It is true that some made the objection that it would be adding to a supposed tendency to centralization on the part of the General Government; but even these acknowledged that, apart from this political consideration, the quarantine would be more efficiently and, at the same time, leniently conducted under national auspices than at present.

There is much truth in the remark of Mr. Oglesby, just quoted, that only by the General Government can those defects in administration be obviated which grow out of the commercial rivalry of different cities, which has often been a serious obstacle in the way of united effort to prevent the introduction of disease. Only under a common authority can a uniform code of regulations be established, which, as already shown, lies at the foundation of all efficiency.

A uniform system of quarantine, operating alike in all our sea-ports, we are persuaded is not only desirable, but is highly essential if we would remove many of the existing

embarrassments to commerce. This prevailing and abused inequality in the laws of quarantine to which our commerce is made subservient operates both oppressively and unjustly; for while those of some States are unnecessarily rigid when enacted, others again are so deficient in wise and proper regulations, or so defective in their enforcement, as to be rendered almost a nullity. (Wilson Jewell, Proceedings First Quarantine Convention, p. 8.)

At many ports that have but little commerce there is at present either no quarantine or only a nominal one, because the revenues from fees are not sufficient to defray the expense of its management. This would be obviated under a general system by the transfer of the surplus accruing at the larger cities to pay such deficiency. Many of these unimportant ports, especially those situated near the Mexican frontier, carry on a trade in small vessels which, being seldom cleansed or repaired, become more dangerous as transports of the germinal principle of the disease than the regular traders to the larger sea-ports. One of these craft introduced the fever into Indianola, Texas, in 1867, whence, as we have seen, it extended over a large portion of the southern country. The facilities for illicit traffic are very great in such places, and the class of vessels so employed are of all others the most difficult to keep under a lax quarantine.

It would obviate, to a great extent, the necessity for detaining vessels on their arrival if precautionary measures were adopted at their port of departure, and during the voyage to insure their being kept in good sanitary condition. Arrangements should be made by international treaty with all friendly powers to insure that the giving of bills of health should not be, as it is now, a mere matter of form, by requiring that all vessels shall be refused clearance until the masters thereof present a certificate from the American consul or commercial agent that they have been inspected by a competent physician and pronounced clean, and that the passengers and crew are in good health on the day of embarkation. In addition, all masters of merchant-vessels coming from ports within the so-called yellow-fever zone should be required to keep on board a supply of the simpler disinfectants, and to use them frequently about their vessel during the voyage, and to inform themselves as to the proper hygienic measures to adopt in the event of the appearance of the disease among the passengers or crew, and especially of the importance of the isolation of the sick as a precaution against its spread. These and similar means would do much to diminish the number of infected ships; but they are of such a character as only to be practicably enforced by the National Government.

The importance of obtaining accurate information of the condition of health in the West Indies and Mexico during the yellow-fever season has already been alluded to. Beyond a few chance inquiries made to masters of vessels, the southern health-officers take no pains to gain information on this point; but if they did, it is unlikely that, as merely the officials of a State or municipality, their request, if made to foreign authorities, would meet with the attention the importance of the subject would demand. This information, however, could always be obtained from our consuls abroad, through the State Department, were the Government to assume the care of the public health. The accuracy of the bulletins furnished by the Signal Bureau of the War Department has been found of the greatest service to commerce; a similar system, giving information each week of the health of every port having commercial transactions with the United States would be of vast importance, and equally beneficial both in its effect on commercial interests and public health.

It would no longer be necessary to assume every port to be infected,

and to declare them so by proclamation, and in consequence to subject many vessels to uncalled-for detention; but, on the contrary, health-officers would be enabled to act on positive information furnished them from their official superiors, and would be required to detain but a very small proportion of the vessels arriving even from tropical ports. Thus one great objection to quarantine as a restriction on commerce would be nullified.

If the meteorological phenomena upon which storms depend and their effect upon commerce are deemed of sufficient importance to merit the attention of a government, how much more so are the phases of a disease which renders a large portion of the coast of the United States uninhabitable during several months, which in its periodical visitations sacrifices thousands of valuable lives, paralyzes trade, diminishes immigration, and retards the progress of some of the most important cities on the continent?

A further argument for a national quarantine may be based on the decision of Justice Bradley in the case of Charles Morgan *vs.* George W. Peete, before referred to. This decision, if confirmed, will have the practical effect of preventing the collection of fees for quarantine visitation, and will render the Southern States, especially in their present impoverished condition, averse to maintain any quarantine at all when the expenses have to be paid from State or city treasuries; and it will certainly result in abandonment of quarantine by the smaller towns, where it is of the greatest consequence. The National Government, moreover, is directly interested in the maintenance of an efficient quarantine from the numbers of persons in her employ whose duties require their residence in ports liable to outbreaks of yellow fever.

At present the troops stationed in southern cities are moved into the country every summer at great expense, while the officers of customs, of the courts, &c., are continually exposed to epidemic disease, and often the most important duties of their respective offices seriously embarrassed by their sickness or death. The possible abandonment of even the imperfect quarantines now in operation becomes, therefore, a matter calling for serious consideration by Congress, that a system may be devised which will be both effective and permanent.

The necessity for a vigilant quarantine at the mouth of the Rio Grande, in Texas, furnishes an additional reason why action should be taken by the General Government. This *débouchure* is under the jurisdiction of two nationalities. The quarantine by the State of Texas at this point does no good, because any vessel detained at Clarkesville, Texas, can enter at Bagdad, Mexico, and her passengers, cargo, &c., be transported to Matamoras, whence there is no obstacle to their crossing the river to Brownsville, Texas. Yellow fever has been twice introduced into Brownsville by way of Matamoras or the river, and both times from the want of an efficient quarantine. The local and State authorities at Matamoras are extremely averse to a quarantine. This probably grows partially from the unfriendly feeling existing between the two nationalities on this frontier. It is probably that the Mexican government would cordially co-operate with ours in an international quarantine, which would insure the health of the region of country bordering on the Rio Grande, while they might object to take any measures looking to such an end without such international action. Should this not be done it would be necessary to quarantine all persons coming from Mexico to any port on the Texas coast, which has been tried and found impracticable.

While, however, the assumption of quarantine by the National Gov-

ernment would be a step in advance, by affording an opportunity for the abolition of the defective systems now in operation at the South, yet even this will not insure security from epidemics, unless the administration of affairs is intrusted to the proper department of the service.

The creation of a new bureau, to be filled by office-seekers, and of political places for the reward of political partisans, would be but transferring the objectionable features of the present system to the wider arena of national politics, and would soon result as unsatisfactorily. Experience shows that safety lies only in an absolute system, and the past offers examples conclusive enough to an unprejudiced mind that in the administration of the War Department alone will be found that freedom from political influences and authoritative management which will demand absolute obedience to law, conjoined with the largest liberty to the individual consistent with public safety. The Medical Department of the Army numbers among its officers many who have, by the nature of their service, been obliged to make sanitary science a special study, and the possession of this corps of well-trained hygienists would enable the Government to assume, with the least difficulty, the direction of quarantine affairs and at an expense which would be trifling as compared with any civil *régime*. The admirable service rendered by the Signal Bureau in its weather-reports affords presumptive evidence of the fidelity with which duties of this character would be managed by the Medical Department. Nor will it be deemed irrelevant to point to the uniform efficiency and fidelity with which the War Department has conducted all public trust confided to its charge. Skilled hygienists cannot be obtained from civil life at the salaries the Government would be willing to pay. It would not be at all necessary that every quarantine station should be in charge of a medical officer; but the general management of affairs should be confided to the Surgeon General's Bureau, and be under his supervision, aided by such inspectors as he might consider necessary, who should be detailed from the officers of the medical corps.

This system was adopted after the close of the war in the southern departments, and proved not only eminently successful but entirely satisfactory to the people of the South.

The last of the five propositions laid down at the commencement of this report refers to the necessity of local hygienic measures in arresting the spread of an epidemic. It is germane to the subject to consider briefly some of these local causes which influence the propagation of yellow fever, not only because they are unquestionably of great importance, but also for the reason that a large proportion of the students of yellow fever have found in them a sufficient foundation upon which to build a theory of the domestic origin of the disease, which, not many years ago, was held as orthodox by the majority of physicians all over the world. While more enlightened views of the etiology of contagious disease have done much to render this hypothesis obsolete, yet they should not lead to undervaluation of the importance of cleanliness, disinfection, isolation, and collateral measures, which can never supplant quarantine but may efficaciously supplement it.

The local causes influencing epidemics of yellow fever may be divided into personal, meteorological, terrene, and intramural.

1. By *personal conditions* are meant the presence of large numbers of unacclimated persons in any town subject to visitations of yellow fever.

The history of the disease in the United States proves that this on more than one occasion has exercised a most important influence on the progress of an epidemic.

When Florida was ceded to the United States in 1821, large numbers of adventurers flocked from all parts of the country to seek their fortunes in the new acquisition. Most of them came without the means of rendering themselves comfortable in their new home, and on arrival at Saint Augustine and Pensacola they took possession of the houses left vacant by the Spaniards, where they lived crowded together in unclean, badly-ventilated rooms. Their habits were of the most reckless character, passing the days in drinking, gambling, and cock-fighting; they would not even take the time necessary for the decent cleansing of their persons and their houses.

Consequently, when yellow fever was introduced in 1821 into Saint Augustine, and in 1822 into Pensacola, it found the very food most appropriate for its rapid development, and most fatal epidemics followed.

Unquestionably, an important factor in the wide spread of the great epidemics in 1867 was the immense numbers of discharged soldiers of the Army who had remained at the South after being mustered out of service. It was estimated that there were not less than fifty thousand unacclimated persons in New Orleans alone, most of whom belonged to the class just mentioned. Besides this, the negroes at the close of the war flocked into all the large cities in vast numbers, and although, in general, not susceptible to the poison of the fever, yet by overcrowding the cities, and that in the filthiest and most unhealthy localities, they exercised a great influence on the spread of the epidemic which followed. At least one outbreak in Charleston owed its fatality to the arrival of an unusually large number of immigrants a short time before; and just previous to the great visitation of fever to Buenos Ayres, there had been an extensive immigration from Europe, and the town was crowded with unacclimated foreigners. At Galveston, in 1839, the fever, after raging with great severity for six weeks, suddenly ceased, not because the infection was dead, but because there was no more material upon which it could feed. Every unacclimated person had either run away from town or had suffered an attack. This was proved by the fact that as soon as the refugees began to return the disease broke out again among the newly arrived, and there were a number of deaths. Doubtless, too, the very erratic course often taken by the disease in the progress of an epidemic may be accounted for in this manner; the leaving of certain houses, streets, and districts comparatively unscathed, and its prevalence, with great mortality, in a neighboring locality; the sudden cessation of the epidemic in one district and its equally sudden development in another; or the selection of certain victims in the wards of a hospital into which the disease is brought, leaving others unharmed.

2. The *meteorological conditions* determining the origin or progress of an epidemic are not yet satisfactorily determined, yet enough is known to render it certain that they exercise a great influence. We never find yellow fever prevailing in the winter season in any part of the United States, nor, indeed, very often before June. We always find that on the appearance of frost, or the reduction of the thermometer to 32° Fahrenheit, the epidemic ceases. Hence we may put it down as a positive law that heat is one of the essential elements in its development; and a reference to the preceding sketch of the epidemics in this country will show that the severity of the epidemic has been often in direct proportion to the heat of the season. To this we may add the moisture in the atmosphere. Heavy rains in May, June, and July have almost always preceded the great epidemics. But even apart from these rains, the position of most of the southern cities is such, surrounded by swamps and intersected by bayous, that there is an immense evapora-

tion from large surfaces of stagnant water all the time, rendering the air excessively humid. There was an exception to this rule in the season of 1839, which was extraordinarily dry. "The whole region between 30° and 36° was literally parched with drought. Before the last of August the surface of the earth, to the depth of several feet, had become entirely deprived of all ordinary moisture; vegetation began to droop; creeks and small water-courses became entirely dry, or lower than they had been for many years; the cotton-plant began to shed its leaves, many forest-trees likewise began to shed their leaves." (Monette, op. cit., p. 82.) And yet this exception is rather apparent than real; for this drying-up of the creeks and pools, while it diminished the amount of humidity, intensified other elements favoring yellow-fever development, viz, the exposure of large surfaces of decomposed vegetable matter to the influence of the sun and air, heretofore rendered innoxious by being in solution in water, or so covered from the sun that no decomposition would take place. That the "general atmospheric predisposition," of which some authors write, has any foundation in fact, I am inclined to doubt; for if it did, there should be no reason why the disease should be confined to the towns, and those living in the country on plantations so generally escape. Nor do I think a sufficient number of experiments have been made to warrant the assertion made to me by a southern physician, that he was satisfied the absence of ozone in the atmosphere was the "*fons origo*" of all yellow-fever epidemics. Certainly, a large number of experiments, conducted under my direction during several years in Galveston and New Orleans, led to entirely negative results; the ozone was as often absent in healthy as in sickly seasons, and during 1867, on some days of great mortality, the atmosphere was surcharged with it, and, on others equally fatal, it was entirely absent. But apart from these theoretical considerations, there can be no doubt that very soon after the commencement of an epidemic, especially if the weather be calm and sultry, the whole atmosphere of a town will become so infected as to render it poisonous to any unacclimated person that passes through it. Very soon in the history of all our epidemics, it becomes unnecessary to look for any special infection to trace the progress of the disease, for we find cases springing up in every direction, without any connection either with each other or with the sick. To what can these be due, if not to the general atmospheric infection? It is chiefly in this manner that the holds of ships coming from infected ports so frequently prove sources of disease; when their hatches are battened down, they shut inside a portion of the air of the infected town, which is carried along to the port of destination, and, on arrival, passes out, carrying with it destructive germs, which produce their effect on any unacclimated person that may become exposed to their noxious influence.

3. *Terrene and intramural causes.*—The topography of almost all the southern towns most subject to yellow fever shows a striking similarity. Charleston is built on low ground, at the confluence of two rivers, with swamps around and in rear of it, and estuaries extending from these rivers in every direction, often with large tracts of low ground which are covered with the tide at high water, exposing to the sun at low tide an extensive surface covered with animal and vegetable remains. Savannah (until the adoption of the regulation forbidding the wet culture of rice within the city limits) was surrounded by rice-swamps almost to the doors of the houses. Mobile, on land scarcely raised above the level of the Gulf, and with the Choctaw Swamp, embracing many acres of stagnant water,

as a receptacle for all the filth and offal of the city; New Orleans, surrounded by swamps surcharged with the most deadly malaria, penetrated in various directions by bayous and canals, the city built on ground so low that levees are required to keep the waters of the river and lake from inundating the streets; and Galveston, its business portion built over an old swamp or shallow bayou, and with Hitchcock's Bayou in the rear of the city, and extending one arm almost into its heart. And the resemblance is still stronger when we consider the sanitary condition of these cities. Take up any report of the board of health of Charleston, New Orleans, or Mobile, for the last twenty years, and one may find the same dreary record of neglected hygienic laws, and in many cases a want of exercise of the simplest common sense in the prevention of the disease. In Charleston, as land became more valuable, vacant lots were filled in so as to become available for building purposes, and often large surfaces washed by the tide, and harmless in their former condition, became vehicles for the propagation of pestilence by the improvements to which they were subjected. To accomplish this filling in, the refuse material from all over the city has been used from time to time. The city offal, the contents of the tidal drains, the earth obtained from excavations made elsewhere for building, shavings, sawdust, oyster-shells, anything that produced bulk, were dumped on the lots, forming a vast mass of putrefying material, sometimes of several acres in extent, and well calculated to breed a pestilence anywhere. In New Orleans, the first epidemic was coincident with the digging of the canal Carondelet, by which vast excavations were made in the soil of the swamp, and an immense amount of decaying vegetable matter exposed for four years to the sun. In fact, the people of this city seem to have had a perfect mania for excavating the soil in every direction. There is scarcely a year in their history but what canals have been dug, or pavements laid, or ditching carried on for drainage purposes, or some similar work, by which the original soil has been overturned. In Charleston, in 1871, the tidal drains, which extend beneath the principal streets of the city, were cleaned out in the heat of summer, in direct violation of every law of common sense, and the contents used to fill up some vacant lots on the outskirts of the city. In Mobile, in former years, the vicinity of the docks was so foul from the decaying shavings, oyster-shells, dead animals, and marine organic remains, that a medical committée stated that the smell was so foul they could not remain to inspect it.

It is only within a very few years that the southern cities have become awake to the danger of intramural cemeteries. Both New Orleans and Charleston have numerous burial-places within the city limits, in which interments still are permitted. In New Orleans there are no burials beneath the ground, but tombs are constructed above the surface, in which the dead are deposited and the tomb then walled up. This mode of sepulture, necessitated by the saturation of the ground with water, cannot but be prejudicial to the health of the community, from the immense amount of animal matter undergoing decomposition that is thus always present. In Charleston, although the interments are made in the usual manner, yet all the older church-yards are crowded to excess, and a visit to any one of them will satisfy any inquiring person of their banefulness.

Still another condition, more or less of influence on the progress of epidemic diseases, is the defective nature of sewerage in most of the southern towns. This is a misfortune growing out of the flatness of their situation, there not being fall enough to carry away the wastage

of the city by natural drainage. In all these cities, the boards of health made constant complaint of the habit of emptying slops and the waste and scrapings of the kitchens into the street gutters, where they are but imperfectly removed by the scavengers and soon become offensive to the smell and the source of pestiferous exhalations. In fact, in all of these places, though much better than they were many years ago, the city police is bad, and the condition of many of the streets bordering on the docks, and the habitations of the poorer classes, such as to invite a pestilence at any time. The people live crowded together, their houses are dirty, the alley-ways, back yards, and privies unclean and offensive, and as a result these localities are often the first to be visited by an epidemic, and suffer the most severely.

Now, in considering all these points mentioned as sufficient to account for the prevalence of yellow fever without the presence of an imported germ, is where I am constrained to think the distinguished gentlemen who advocate the local origin of the disease are in error. I am very reluctant to write anything which would seem presumptuous, in placing my opinion, with my limited experience, in comparison with the great learning and wide experience of these gentlemen of whom the South is justly proud; but it seems that thus far the weight of evidence is with those who believe that the disease is uniformly exotic. The local causes above alluded to exist every year, yet the fever appears as an epidemic only occasionally, and even then, in every instance where due patience has been exercised, has been traced to importation from an infected port. As said before, it by no means follows, because no importation can be directly or positively ascertained, that none existed; and to assert the local origin of an epidemic, simply on the negative testimony of the absence of positive proofs of its foreign origin, is very weak ground upon which to stand. One positive fact outweighs all the testimony of a purely negative character that can be accumulated, and in regard to such facts, we have not merely one, but hundreds connected almost with every epidemic in the history of the country, showing beyond a doubt that the first cases are coincident almost always with the arrival of one or more ships from infected ports; that very often these first cases can be traced directly to such infected vessel, to her cargo, or to sick persons who have been landed from her; that the next cases are generally in houses adjoining or in the neighborhood of the first, or in the persons of those who have spent some time with the first; and that the disease only becomes generally epidemic when a sufficient number of cases has occurred to infect the atmosphere of the locality. We see also that, when the disease prevails at New Orleans, it invariably, after some weeks, breaks out in those towns having steamboat communication with the city, and that those towns having no steamboat communication (as Vidalia, opposite Natchez) always escape; and that those who establish a local quarantine, and refuse to prevent the landing of steamboats during its prevalence in New Orleans, also escape. We know that the great offal mound at Augusta, Georgia, about which so much has been written, existed for five years without causing any disease, until persons sick with the fever arrived in that town from Charleston in 1839, and then immediately an epidemic followed, and the dirt-heap was made responsible for it. But while thus rejecting the idea that any of the causes above mentioned can originate an epidemic, it would be a sad mistake to undervalue their agency in propagating the disease by preparing a richly-manured soil for the reception of the foreign seed. While it is unquestionably true that yellow fever has prevailed epidemically in high, dry, and clean localities, as at Pensacola, Florida, and Spring Hill, near

Mobile, Alabama, yet the vast amount of information collected by La Roche, and other historians of the disease, shows most conclusively that the disease chooses for its favorite habitats those towns and portions of towns which are badly policed; where the inhabitants live in neglect of all hygienic laws, or where overturning of the soil or defective drainage gives rise to offensive malarial emanations. The moral of this is evident: that while, perhaps, it may be difficult to prevent the importation of the yellow-fever germ, much may be done by local boards of health to prevent the spread of the disease by improving the system of drainage; by rigid inspections of all localities inhabited by the class above mentioned; by a thorough police of streets, alleys, back yards, &c.; by the introduction of the earth-closet system in place of the present pernicious style of privy, which allows its contents to permeate the surrounding soil and neighboring cess-pools, and thus rendering them saturated with human excreta; and, finally, by a careful disinfection of all dirty localities, not only those in which fever exists, but wherever there might be reason to apprehend its spread by reason of the habits of the persons living there. This branch of the subject does not strictly come within the scope of inquiry upon which I am required to report, but it is too intimately connected with the history of yellow fever to be passed by without this brief notice. With many it is regarded as of more importance than quarantine.

In concluding this report, I hope it will not be considered improper for me to refer to the kindness and courtesy evinced by every person with whom I was brought in contact during my tour of inspection on the southern coast.

As a representative of the General Government, executing an important mission, every assistance was given to me, both by the authorities and the medical profession, in the prosecution of my inquiries, and this without regard to the personal views of any as to the value of the undertaking.

Had it been possible to have extended the inquiry over a greater space of time, this report could have been more complete. As it is, it is hoped that it may be considered as but preliminary to a more elaborate examination of the subject on the part of Congress, which will lead to important practical benefits, not only to the southern and Gulf ports, but to the whole country.

I have the honor to be, sir, very respectfully, your obedient servant,

HARVEY E. BROWN,
Assistant Surgeon, United States Army.

The Hon. the SECRETARY OF WAR,
Through Surgeon-General, United States Army.

A FEW THOUGHTS

ON

Sanitary Questions,

BY

T. W. WALLIS,

Land Surveyor and Consulting Sanitary Engineer.

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PREFACE.

During the present year the Government brought in a "Public Health Act." Shorn of some of its best provisions it succeeded in escaping the usual destruction of promised Bills at the close of the session, and it became a portion of our Statute Book. When this Health Bill was passing through Parliament I wrote a few short articles on "Water Supply and Town Drainage," without entering into the whole question of sanitary reform, which is "a large subject, and has many branches." These articles were inserted in the "Louth Advertiser," under the *nom-de-plume* of "MERONES." Some of my friends early detected the identity of the writer and advised me to acknowledge the authorship and issue these short papers, with the additional letters on House Drainage, in a pamphlet form, under my own name. This I now do; and must crave the indulgence of the public for its shortcomings and defects. They have been compiled amidst a varied and busy occupation, and have no pretentions to a literary composition: but must be regarded as a modest attempt to put in a tangible form, in a few commonplace terms, the thoughts that occupy many minds.

A few words as to the name "MERONES." "Where did you get the word," I am asked; I reply it is an anagram of "Emerson," the maiden name of my mother. Nor is this original; for William Emerson, of Hurworth (from which family I have the honour to be maternally descended), an eminent mathematician of the last century, used the signature of "Merones," the letters of his own name transposed, to some of his minor writings, published in the periodicals of his day.

These "thoughts" are re-published in the form of letters as they originally appeared; to present them in the character of an essay would require the whole to be re-cast; this would demand more time than I can spare, and would not add anything to their value. In the Appendix a few more statistics are given than entered into the "letters."

T. W. WALLIS.

10, Gospel-gate, Louth, November, 1872.

SANITARY REQUIREMENTS.

INTRODUCTORY.

LETTER No. I.

"WHAT are you about here, you sluts?"—The "sluts" throw down their dusters on the floor of the *sanctum sanctorum*, the abigail seeks safety in flight, whilst Mary, the niece, timidly stood her ground: "Indeed, uncle, your room was not fit to be seen, and I just came in to see that Jenny laid everything down where she took it up."—"And how dare you, or Jenny either, presume to meddle with my private matters? (Mr. Oldbuck hated *putting to rights*) I assure you, Mr. Lovel, that the last inroad of these pretended friends to cleanliness was almost as fatal to my collection as Hudibras's visit to Sidrophel; and I have ever since missed

'My copperplate, with almanacks
Engraved upon't, and other knacks;
My moon-dial, with Napier's bones,
And several constellation stones;
My flea, my morpeon, and punaise,
I purchased for my proper ease.'

And so forth, as old Butler has it." You'll be poisoned here with the volumes of dust they have raised," continued the Antiquary; "but I assure you the dust was very ancient, peaceful, quiet dust, about an hour ago, and would have remained so for a hundred years, had not these gipsies disturbed it, as they do everything else in the world." (See *Sir W. Scott's "Antiquary."*)

In making a few preliminary remarks on the important subject which forms the heading of this letter no more fitting preface presented itself than the preceding quotation from Scott's immortal "Antiquary." For it is as true now as it was "Sixty years ago," that Mr. Oldbuck may be taken as the type of a class who, passively or actively, object to "putting to rights"; who have no sympathy with "friends to cleanliness," who have no patience with those who insist upon cleaning out sewers, drains, cesspools, and all other accumulations of refuse matter, whether in rural districts, towns, streets, or houses; arguing that if the busybodies would only let the "peaceful, quiet" dust or dirt alone, it would do nobody any harm. However, it is not intended to discuss this phase of the question in what will be here advanced on the subject of sanitary measures necessary to maintain a healthy condition of the surroundings of the dwelling-places of man. For the question of sanitary arrangements for the health of towns has so far advanced, that the "Oldbuck" class are simply ignored. And it may be laid down in the form of axioms:—

1st.—That all animal or vegetable matter, putrid or decaying; that all stagnant accumulations in cesspools, or similar places, throw off noxious vapours, poisonous gases, which, though invisible to the eye, spread disease and death as surely as that cannon-balls kill on the field of battle.

2nd.—That an abundant supply of pure water, with the most perfect known arrangement of under-ground passages or drains for the conveyance of the town refuse entirely away from the habitations of men, are essential matters in the internal government of a town having in view the health of the people.

3rd.—That it is the duty of the governing powers, imperial or local, to do all in their power to save the lives and promote the general health of the whole people over whom they are placed to govern.

An abundant supply of good water, and a complete system of house, street, and district drainage, are so intimately interwoven, that a consideration of either subject must necessarily involve reference to the other. Since the art of drainage cannot be effected without an adequate and constant supply of water obtained by a combination of natural and artificial agencies; and a sufficient supply of water to a town (having in view the general health of the people) to be used for all highway, manufacturing, and domestic purposes, cannot be effective without the establishment of a well-devised system of drainage that shall insure the removal of all waste water, the refuse of towns, and of the animal ordure created within the province of human dwellings, in such a manner that the surrounding soil encompassing wells, &c., shall not be impregnated with poisonous matters: which otherwise will surely percolate to such wells used for artificial water-supply. And, further, that the removal of the daily accumulation of dirt, whether it be animal, vegetable, mineral, or excrementitious, from the dwellings and highways of men, should be treated without any reference whatever to the presence of a river, if such reference has in view the converting of the river into a mere open sewer; and, further, that this removal of waste material, be it solid or liquid, from a town, should be accomplished through sewers whose form, capacity, and inclination to the outfall would prevent the sewers from becoming mere elongated cesspools, as is too often the case even in towns like Louth, whose natural conformation is such that the most perfect system of drainage is attainable if only an ordinary amount of skill is applied to the subject as a *whole*, instead of constructing it piecemeal without reference to a general principle to govern the various parts so as to work into one simple complete design.

In making these remarks there is one part of the subject not yet named, though it ranks only second in importance to the primary question of making the abodes of men wholesome and healthy, and that is the beneficial appropriation of refuse matters so as to make them actually productive as one of the most powerful manures—and thus make it the interests of corporations and of agriculturists, to insure the healthy uses of inland waters for which they are properly adapted. That the "utilisation of sewage" from town-drainage will sooner or later become an established fact no one can doubt; and the wonder will be, perhaps in the next generation, that the nineteenth century should pass away, if it does so pass away, without fully accomplishing so beneficial a purpose. It is true full forty years have passed since John Martin, the artist, first drew attention to this subject, and that it is still so far from universal adoption; nevertheless the signs of the times, as shown in modern legislation and public opinion, in favour of social reform, would seem to indicate that more progress will be made on this question in the next than has been made in the last four decades.

Having introduced this subject in a few general observations, its further consideration, as specially applicable to the town and parish of Louth, must be reserved for a future communication.

LETTER No. II.

WATER SUPPLY.

In Letter No. 1, some general remarks were made, some general principles laid down, which the writer deems incontrovertible. It is true their special application to the town of Louth, or to any other town, may give rise to differences of opinion in proportion as men allow themselves to be governed by prejudices, self-interest, real or apparent; or are guided by a broad intelligence which enables them to stride over the smaller motives which influence the actions and opinions of so large a proportion of mankind.

In referring to axiom 3, it may seem to some that this is too modern an idea to be received as an axiom, and that it has been too little acted upon to be placed in that category. It is true the reverse of this has been very much the practice of the great majority of Governments in all ages and in all countries; alike influencing public and private opinion, for the reader may remember a small book written by Louis Cornaro, an Italian gentleman of the seventeenth century, who, whilst giving rules to be observed to attain a healthful longevity, adds "this book was written for the benefit of the polite and learned in society; as for the common sort of people it is very little consequence whether they live or die." At any rate not only our humanity and our Christianity but our political economy has outgrown such barbarous and ignorant sentiments as these; and no man, in this country at least, would receive public support unless he *professed* to have at heart the interests and welfare of the whole people.

It is not intended to write these letters as a formal essay on the subject of sanitary reform—or to enter into details and technicalities which would give them too much the character of a professional report, but rather to put the question into a popular form so that its general bearings may be understood by all.

Now that it is an established fact that a "Louth Water Company" will shortly be prepared to place, under pressure, a full and constant supply of the very best water within the households of every one desirous of having it, and that the "Water-works" will be a thing for all time to come to be conducted either by a private company, or by a public body; we have thus assured to us, in a more perfect form than ever known previously, the one primary condition for all sanitary reform that may in future be adopted. And, before entering upon the subject of the drainage of the town and parish of Louth, some passing remarks may be made in reference to the quality, or what we may term the domestic properties, of "Silver Spring" waters, with which the town of Louth will in future be principally supplied. To the residents of towns who know rain-water only as a darkish-coloured fluid that must pass through a long settling process before it is in a fit state for even washing purposes, it may be a little startling to be told that rain-water ranks the highest in degree of purity of any known water supply, that is when uncontaminated with the soot of towns; and next in order of purity is water from surface drainage. By this is not meant surface drainage of towns which poisons so many wells: but springs whose supply is from surface *drainage*. Now,

without applying any special tests, it is easily determinable by ordinary observation, that "Silver Spring" water ranks in the highest order of purity, applicable to general domestic purposes, of any available springs in this district. It satisfies the eye by its crystal clearness, it satisfies the palate by the absence of any impure taste, and lastly the water is *soft*, and this latter quality goes far to demonstrate that it is the best attainable, inasmuch as it is comparatively free from those earthy and iron salts which constitute the hardness of water and therefore much of its impurity; for it is well known that water taken from sources in which it has long remained in intimate contact with soluble earths and other matters, is found to have acquired a corresponding proportion of these impurities: and, in the absence of any scientific tests, the quality of spring water may be ascertained by its "softness" or by its "hardness," soft spring water being the purest, and the greater degree of hardness the most impure. This hardness of water, as just stated, is caused by the presence of various earthy salts; and by the presence of chalk in solution. The present water supply of Louth is principally obtained from deep, underground sources from a bed of chalk, and its hardness, proving its comparative impurity, renders it unfit for many domestic purposes; or, if it has to be used, occasions a great additional consumption of soap in all processes of washing and cleansing, harder work to the operator, and an increase of "wear and tear" to the clothes acted upon.

As already stated, the question of drainage is so intimately connected with water supply, that we cannot discuss one without understanding the other. It is well therefore, to state, what quantity of, and for what purposes, water is necessary. The purposes for which water is required is defined as follows:

1.—Ordinary domestic uses, including drinking, washing of persons, clothes, utensils, houses, yards, watering gardens, &c. 2.—Manufactures. 3.—Supply of public buildings, baths, wash-houses, &c. 4.—Extinction of fires. 5.—Cleansing and watering of streets and thoroughfares. 6.—Supply of pleasure grounds and miscellaneous purposes. It has been found by experiment, in towns where waterworks have been established many years, that a supply of twenty gallons per diem for each head of the population is amply sufficient for the first, second, third, and fourth of the purposes enumerated; but to make sure of an ample provision for all possible casualties and increased demands, there should be a constant service of thirty gallons per diem, for each head of the population. For the remaining purposes for which water is required an estimate has to be made according to the superficial extent of the town and neighbourhood to be provided for, and what amount of this will be supplied by the rainfall of the district; but any further particulars of this part of the subject need not engage our attention at present.

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LETTER No. III.

THE DRAINAGE AREA OF LOUTH.

The question of drainage is divisible into three parts, viz. of districts and lands; of towns and streets; and of dwellings and buildings. Of the drainage of districts and lands but little will be said further than to make it perfectly clear to what extent the town-drainage is affected by the district immediately surrounding the town. The centre of the town of Louth is also the centre of the parish; and both parish and town may be described as irregular, rectangular, or oblong figures, though the town has its longest way east and west; whilst the parish has its longest way north and south; being about three miles long and two miles wide (i.e. west to east), having an area of about 2,600 acres. The area of the town considered for drainage purposes, is about 280 acres. Louth is described as being placed at the foot of the wold hills; on the east and north-east side the surface of the land gradually slopes to the sea, on the south and west the ground rises rather suddenly above the town,—being about 460 feet above the level of the sea, six miles west of Louth; and that part on which the outskirts of the town are built is upheaved with a cleft in the middle, through which the River Lud runs; but immediately beyond the upheaved part, just named, the contour of the ground again falls, leaving somewhat of a valley between the town and the wold hills. This valley may be followed from Stewton-lane, going west into Bishop-briggs; then turning to the north, crossing Horncastle-road just beyond the reservoir of the Waterworks; crossing the west end of Crowtree-lane, still going to the north; then turning to the east, crossing Grimsby-road at the end of Fanthorpe-lane, and so on into the open marsh. Thus we have described the town of Louth as being on somewhat elevated ground, with the east end dipping towards the sea and the west end having a deep depression allowing the water-course, from Hubbard's Valley in the Parish of Hallington, to course down the whole length of the town with a fall so great that, on the water being held up, six water-mills are worked by it.* It is necessary that this general description be understood and remembered, so that what is said on the special subject of town drainage (Louth) may be quite clear.

The general surface drainage of the district is effected by natural water-courses which have their rise a little west of the parish of Louth. Thus on the south side, a small stream rises in Raithby passing through Louth parish going east, crossing the London and Alford roads, taking the drainage of the valley south of Julian-bower and Stewton-lane, on to Stewton and into the marsh, where it acquires the name of "Grayfleet." The general drainage on the north side of the parish is effected by similar small streams which have their rise about Fanthorpe or Aethorpe, and, coursing eastwards, join the river Lud in the Marsh. On the west side of the town from the high lands, Tathwell, Welton, &c., small water-courses have their rise which, when united

* The natural fall of the River Lud from Hall's mill, on the west side of the Parish to Louth Park, to the East parish boundary is 51 feet, from thence it has a fall of about 35 feet to the sea, Ordnance datum.

form the river Lud. Thus, on the south-west, streams run from Tathwell, Raithby, and Hallington, and uniting, pass between Hubbard's and Fisher's hills in a north-easterly direction, is known as Hallington Beck. On the west side of Louth, say at Welton, we have the rippling streams ploughing their way through the beautiful "vale," and, travelling eastwards to Louth unite to form Crake (or Craik) Beck. Thus Hallington Beck and Crake Beck, together with "Silver stream," from springs which have their rise in Hubbard's valley, meet in Thorpe Hall grounds, and hence become the river Lud; and this river *naturally* is quite ample to take all the surface drainage on the west side of the town and from the upheaved land before described as forming the area on which the town of Louth stands. On the east side of the town, as already stated, the surface of the land has a gradual fall to the sea from the boundary of the parish, of about four feet per mile, and that this is an ample declivity to secure a perfect outfall may be judged from the fact that some of the great sewers of London, have not more than two feet fall per mile.

Having thus shown that the town drainage of Louth cannot be injuriously affected by any part of the district drainage on any side of it, we may just notice the small water-courses that have their rise in the town itself, and which form an element in the question under consideration, viz., of water supply and town drainage. It may not be known to every inhabitant of Louth that in an obscure and not very clean part of Louth there are two sets of springs which rise within a few yards of each other, different in quality and varying very much in quantity. No doubt they are as ancient as the hills, and their presence here is due to what is termed by geologists a fault, but they do not proceed from the same "fault"; nor (there is every reason to believe) does the water come from the same under-ground water-course. At any rate the analysis of the water gives different results. The most important of these springs is that of Aswell (or Haswell) which has its rise in Aswell-hole; and after passing partly under and party above ground, working one mill in its way, empties itself into the River Lud near the Priory.—*See Appendix.*

The "St. Helen's" springs rise a little way east of "Aswell" and have an open run eastward, being the source of Monk's-dike which flows through the Cockeringtons and so on to the sea. Monk's-dike, being on so much higher a level than the River Lud, plays an important part in receiving much of the rainfall drainage from the high land on the south; doing duty as a sort of catch water drain and thus relieving the low part of East-gate, near the prison, from some of the pressure it would otherwise be subject to in heavy rains.† The Lud itself (though protesting against it being converted into an open sewer) will always form an important element in considering the town drainage of Louth inasmuch as a considerable quantity of the rainfall will find its way into the stream, and so relieve the sewers.—*See Appendix* A.

† The water at Monk's-dike head is 20 feet higher than the Lud in James-street, and running nearly parallel to it. During heavy rains Monk's-dike stream is swollen about two feet higher than when in its normal state from the supply of the springs.

LETTER No. IV.

THE HEALTH OF THE TOWN.

"To know
That which before us lies in daily life
Is the prime wisdom.—*Milton.*

It has already been shown that the two subjects, drainage and water supply, should go hand in hand to be mutually effective; and that the supply of water to the town of Louth for all purposes whatever will, in future, be guaranteed to be both ample in quantity and good in quality, and farther, that the drainage of the district around Louth would not injuriously affect the town drainage. In reference to the 3rd axiom in letter No. 1, it would be as well to consider the principle of drainage in relation to the just claims of the householder; and also with reference to the limit to the *duties* of the local government, and this may be disposed of in a very few words. For the highest authorities on this question recognise the principle "that no sewage matters of any kind whatever should be allowed to be discharged in to a drain from any rooms or apartments below the surface of the ground," and that therefore the duties of a local government are limited to providing good and sufficient drains for all surface drainage, rainfall, &c.; to remove all refuse matter from roads, streets, and houses above the ground; keeping these drains, &c., in good working order, and the streets clean, well-pavied, lighted, and in good condition. The reasonable claims of the householder are expressed by the above definition of the liability of the local government. There is an idea floating in some people's minds that if builders excavate cellars beneath the ground level the local government are bound, sooner or later, to provide drainage for such cellars. It cannot be too emphatically laid down that this is a mistake. For this is neither the original intention nor should it be the ultimate object of town drainage. Consider what effect the reverse of what is here enforced would have! It would impose an enormous expense upon a town if the drainage of cellars were considered a public duty. If owners of property will extend their property downwards below the ground level it must be their own duty to provide the means of making such subterranean rooms dry and free from water, and not expect the local government to incur the immense increased outlay that would be caused by constructing the street drains about eight feet deeper throughout every street in the town that would be requisite for what is here contended is the legitimate object of town drainage.

Having thus briefly, and very imperfectly it is feared, endeavoured to lay before the reader some general principles on town drainage, bearing upon the town of Louth, we will endeavour, as far as our powers of description will enable us to do, to convey to the reader's mind some more definite views of the subject in detail. It has been already stated that the area of the town drainage is about 280 acres, but this does not include the whole of that part which has been described in these letters as the "upheaved" land in the parish, the drainage of which must find its way into and through the town; the area of this is about 500 acres, and this will have to be considered in

relation to the quantity of rainfall, less evaporation, that would annually flow from it into the sewers in addition to the refuse matters to be discharged from the town, houses, &c., consisting of sand or dirt from our rapidly wearing streets and roads, of superfluous rain and spring water, indeed of all waste, whether solid or liquid, that the decencies of life require to be removed from our dwellings entirely away from the town.

It has already been stated that the writer would avoid technicalities and anything that partook of the character of a studied report, merely wishing to give some general account of the subject as a whole. But it is felt we must dabble a little more closely with "facts and figures," to show the necessity there is for a greatly improved system of drainage in the town of Louth. At present we will not speak of those parts of the town that are not at all or but imperfectly drained, or of the structure and character of the present drains of the town, but first meet the question—Is the health of the people of Louth, as shown by its rate of mortality, such as to require improved sanitary care? Situated as this town is upon a gentle declivity, with watercourses already described traversing the town and parish from west to east of sufficient capacity to carry away *naturally* all surface drainage; with water fairly plentiful and good, when obtained uncontaminated with the impurities of the earth through which it percolates into the wells; with no trades or manufactories specially injurious to health; with these advantages Louth should rank as one of the healthiest towns in the kingdom. But what are the facts? The death rate of Louth has averaged for the last twenty years (the returns earlier than that are not readily accessible) about 24 per 1000, whereas the standard of healthy towns is 17 per 1000. Assuming, as we have a right to do, that, with good sanitary management, the death-rate of Louth should not exceed the standard of 17; and if we put the population at 10,000 we have thus a loss of 70 lives per annum, due to preventible causes; and for every one that dies it is a moderate estimate to say that twenty cases of illness occur, which multiplied by the excess of death-rate, equals 1400.* Now, without considering the humanity of the case, think whether it is cheaper to provide the most perfect sanitary arrangement for a town and so maintain the standard of a healthy condition of the people; or, by neglect of these precautions incur a loss of 70 lives per annum, and an excessive sick-rate of at least 1400 every year. And when we speak of an excessive death-rate, and sick-rate, it is not of the aged and exhausted members of society that are thus accounted for, but of those who are stricken whilst in childhood, in youth, or in the prime of life, with the loss of health and life.

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* It is an interesting fact, and Dr. Buchanan vouches for it, that the death rate is notably reduced in places where pipe sewers have been laid,—phthisics, especially, disappearing with an improved drainage.

LETTER No. V.

THE TOWN SEWERS.

"To know
That which before us lies in daily life
Is the prime wisdom."—*Milton.*

In previous letters having spoken of the effects of imperfect sanitary arrangements, we will particularise a few of those defects. At present our remarks will be confined to town drainage, properly so called, reserving any remarks that may suggest themselves on the important division of the subject of house-drainage, until a future time; although the drainage of houses holds the same relation to town drainage as the numerous rivulets and tributaries do to a principal river, they are but the ramifications of one and the same subject.

The first and great defect of Louth drainage is, that there is absolutely no outfall whatever for the public sewers. The sewage matter is not carried away from the town, but is hoarded up in our midst; polluting the air, poisoning the water, and contaminating the earth with its gross impurities. It is true, as a matter of course that the sewers empty themselves, less the silt; but where? Directly into the Lud, under our very noses! This would be bad enough if the water of the Lud had free course, and so carried away some of the pollutions poured into it; it would then have the character of an open sewer, very objectionable it is true; but worse than this the Lud is converted into a huge cesspool by the water being held up by the six mills; and, instead of a rapidly running stream, we have comparatively still water, which is constantly being fed with the liquid refuse from nearly all the town; and, further, according to an official report on this subject, there are 254 closets emptied into the sewers and thence into the Lud; and if we take the usual number of persons to a house, viz., four, the result is that the excreta of 1,000 persons, to speak in round numbers, is annually deposited in the Lud; and, according to the data given in such cases, this amounts to 438 tons annually —of which 26 tons would be solid matter. Then again there are nearly 2,000 pits or cesspools requiring to be emptied, which is done at various intervals of time, under no system, or control, with no flushing apparatus or supply of water to insure the cleansing of such places. And, as a witty sanitary reformer once said, we decidedly object to the formula W.C.—W. Then, as to the drains, or sewers, at present existing. Not much is understood about them as a whole; but nearly all the details known on the subject are of a bad character. As a rule they are too small; and, principally owing to defective workmanship and faulty design, and also in not making a full use of the natural fall or inclination of the ground, the smallness of the sewers is rendered farther defective by the deposit of silt; in some cases to nearly, and in others to more than, half the depth of the sewers, so that the capacity of what is called a main sewer is, in such cases, reduced to the requirements of a house drain and therefore wholly unfit for the purpose for which it is intended. This has to be said, and it is said without exaggeration, and what the writer believes to be a plain statement of facts—this statement

is about the sewered part of the town. But there are parts of the town that have no sewers at all; and here the unfortunate inhabitants, residing in such districts, are doomed to be surrounded with all the usual house refuse, &c., &c., with their concomitant evils, and in this respect are but little better off than those races of men who know not the arts of civilisation.

The streets, lanes, roads, &c., that require sewers in the town of Louth amount in length to between eight and nine miles; there are probably nearly two miles of this without any drains; we give this estimate under correction, but what is here stated is approximately true. And does such a statement require any comment to point out the danger existing to the health and lives of the people living in undrained streets? In its most aggravated form, in densely populated undrained districts it has been known, in our time, to cause a devastating epidemic to pass through them with scarcely less fatal results than the assaults of a conquering foe carrying fire and sword with ruthless vengeance. In one special "blind" alley in a sea-port town, having no drainage itself, nor drainage in the steeet from which it opened, every human being perished in this awful onslaught.

It is true, with the natural drainage the surface of the land has in the town of Louth, this danger exists only in a modified form. But it is contended that the general statements herein given are sufficient to prove the position that there is need for a "greatly improved system of drainage in the town of Louth." It is no part of our design to speak dogmatically upon this question, in reference to any special plan or scheme for this purpose; knowing very well that the most eminent engineers will hold different opinions upon the same subject, where many vested interests are at stake, and various courses open to choose from; therefore our task is very humbly and modestly to seek to ventilate the subject in a popular form. But legislation on this subject is advancing so rapidly, that it may soon cease to be a question with the local authorities whether they will do anything or not; it is likely to resolve itself into "As something must be done what would be the best to do!" And the answer to this question should not be given lightly. An ill-judged scheme is as likely to cost as much, and might cost more, than the most perfect system of drainage that could be applied; Of this we may be quite certain that to construct a system of drainage based upon scientific principles for the town of Louth, will cost a large sum of money; but not necessarily "dreadfully dear." Whatever is done should be done well and thoroughly, anything less than this is false economy. It requires no special wisdom to know that to adopt the best workmanship and the best materials is the cheapest mode of executing any work the nature of which requires that it shall remain, if possible, in good working order as long as the town itself exists. But here let us caution impulsive persons from rushing from one extreme to another; the drainage of Louth is doubtless wofully defective, but there is a limit to what we have a right to expect even under the best arrangement We have already contended that the town drainage should not be constructed so that it shall necessarily take drains from underground rooms or cellars. It is granted where, in laying the drainage levels a sufficient depth is taken that will admit certain cellars being drained into them, that this would be a legitimate use of them, but not otherwise; and, again, the heavy rains we

have had this year has caused great floodings in some parts of the town so that people cry out, "Oh! you must make the sewers large enough to take the rainfall during the very heaviest showers." Here again we must demur to such an extreme conclusion. If it were not foreign to the design of these letters to go into any long array of figures, it would be conclusively shewn that to make the sewers equal to what might be required in, say, one half-hour of excessive rainfall, would not only cause a vast expense in constructing works about ten times larger than ordinarily required, but that this would impair the full efficiency of the working of the sewers for their general purposes.

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LETTER No. VI.

CORRECT DATA REQUIRED.

"To know
That which before us lies in daily life
Is the prime wisdom."—*Milton.*

It is necessary to discuss the various phases of sanitary reform not only that those who are intrusted with the execution of sanitary laws may understand the subject in all its aspects, but that the general public may be educated up to a given point to see the necessity of the regulations that may have to be enforced with the authority of law. Indeed, until all classes of society show by their habitual love of cleanliness that they appreciate the proverb "cleanliness is next to godliness," the beneficial effects of sanitary regulations will lose much of their force. And although we may at the present day see many dirty skins in the immediate vicinity of good and abundant water, which, in many cases, may be had for nothing, still we cannot admit that the most neglected class of labourers of the present day are so dirty in their habits as their forefathers were, who invented the proverb (Scotch) "the clartier the cosier." Indeed if we take a retrospective view of what has been done in sanitary matters, drainage, paving, widening and lighting the streets, &c., during the present century, the question assumes a brighter aspect in anticipating what may be done in the time to come.

We may, in passing, remark that even the drainage of lands, as we now understand the subject, is quite a modern practice, little more than a century old—previous to that time the plodding farmers of those days aspired to no higher notions of drainage than throwing up the land into ridge and furrow and cutting a trench or ditch by the hedge side. Although there were theorists as early as the time of the Commonwealth who advocated that "for the drayning trench it must be made so deepe that it go to the bottome of the cold spewing moyst water that feeds the flag and rush—a yard or four feet deep if ever thou wilt drayn to any purpose." No doubt the old sturdy farmers laughed at such wild notions of "drayning" as we do in our time at any new idea broached; but often live to see realised what were once called impossibilities. We are slightly digressing, but still the drainage of districts cannot be severed from a study of the other branches of the subject without impairing our power of becoming fully acquainted with that branch we are specially considering. And with reference to town drainage, sanitary reformers

need not be disheartened at the apparent slowness of the application of sound sanitary measures, especially in small towns like Louth; for it is well known that fashions, inventions, and various clever and ingenious appliances of life are in use in cities and large towns long before they become general in sparsely populated districts; even such a simple contrivance as lighting street gas lamps without climbing a ladder at every gas lamp was in common use in Manchester at least fifteen years before it found its way to Louth. So it is with matters of greater import—such as water-supply under pressure, improved ideas and practices in drainage; the making streets wider and straighter to let sunshine and a fresh breeze into what used to be pent up places; at the same time securing warmth and ventilation in our houses. By these and many similar devices a thickly populated town may be rendered healthier than the scattered hamlets of a village used to be. Used to be? Shall we not say now? Reader, did you ever notice how recklessly villagers set at defiance all the laws of health? How they will have a midden heap in as close proximity to the living and sleeping apartments as possible. How the refuse and animal ordure is persistently hoarded up with perhaps only a badly built four and a half wall between the pantry or kitchen; the inmates thus breathing poisoned air where all might be so pure—and all this is so utterly opposed to what nature teaches, if we would only receive the lessen placed before us, that it seems strange such gross evils are practised or allowed to exist.

However with the present convictions of leading men we may be sure that the sanitary day is coming. Men are not to do as they like with their neighbour's lives. Science must triumph over ignorance in dealing with social and domestic life. But that this may be accomplished in its widest sense it is necessary that full and correct data be obtained upon everything that forms an element in the art of sanitary reform. In the absence of such data all attempts at improvement must be more or less false and imperfect. A notable instance may be here given to show how inconclusive deductions are that are drawn from incomplete or deficient data. More than thirty years ago the writer of these letters had the privilege of hearing a course of six lectures on the steam-engine, railways, &c., by the celebrated Dr. Lardner, and in his youthful eagerness for knowledge (though the subject had no affinity for his then daily pursuits) he made his way into the lecture-room long before the public were admitted, and sketched rough drawings of all the diagrams, &c., took notes, and afterwards wrote down the lectures in detail. In those lectures Dr. Lardner *demonstrated* that steam traffic to America could not be carried on commercially; that is to say, the fuel necessary to generate steam to carry a steamer to America would just occupy the entire tonnage of the vessel; hence the voyage, though it might be accomplished as a scientific curiosity, was impossible as a trade. This conclusion was doubtless true upon data then known, but Dr. Lardner lived to cross the Atlantic in a steam-boat that was employed commercially for that purpose—and the learned doctor would realise the fact that progressive science had furnished new data from which new and improved results were obtained.

And with respect to our town drainage—house drainage we will notice in a future letter—we must not judge harshly the doings of our forefathers in this matter, for there was very imperfect data to act upon even fifty years ago, and the science of sanitary legislation had barely an existence prior to the outbreak of the cholera in this country in 1832. Our "grandfathers were not such fools as some people think they were," as one of our bucolic friends remarked at a public meeting in Louth. We know our "grandfathers" were brave and noble fellows, and when we read their history how, in travels, voyages, or war, they successfully contended with famine, fatigue, foaming seas, or fiery enemies, one instinctively recalls Scott's lines:—

"Is there a man so dead
Who to himself hath never said
This is my native land."

But in town drainage their knowledge was as limited as was that of the race of farmers, already alluded to, in reference to district drainage. That is to say it was considered that every necessary condition of town drainage was attained if they could only succeed in emptying the sewage matter into a river running through the town. The result of this was (and is yet in many cases) that the river banks or walls, where such are built, are coated, loaded or impregnated with the deadliest poisons that, on exposure, are freely given off, producing plagues, pestilence, fevers, &c. It may be said without exaggeration, that there has been more "killed and wounded" from these causes than have been caused by the "fire and sword" of the blood-stained Mars, for the curse of war comes upon us in a fitful way and at intervals; whilst the poison of sewage gases, though unseen, unheard, is constant, ceaseless, chronic; cutting off annually in this fair England of ours a number probably equal to the entire strength of the British army at the present day.

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LETTER No. VII.

THE THEORY OF DRAINAGE ILLUSTRATED.

"To know
That which before us lies in daily life
Is the prime wisdom."—*Milton.*

In these letters we cannot go into details which would be only understood by reference to drawings and a regiment of "figures." But this may be stated; if it should be decided to construct a main drainage through the town having an outfall at the east, quite away from human dwellings, that this can be done very effectually and that there are no great engineering difficulties to be encountered.* The principle on which this intercepting, or main sewer, should be constructed admits of it taking all the tributary

* It is true it is not so simple a question as to be without difficulties, engineering and otherwise; and probably could not be carried out unless a law were obtained forbidding the pollution of rivers and insuring the drainage of a district to be equally perfect: making the drainage of a village or a town but a part of a large system that should insure the efficient drainage of the whole district—and hence of the whole country: or otherwise adopting some system of utilisation of sewage which should consume the refuse of towns.

sewers from both the north and the south side of the town; and that, for short periods of excessive rainfall, surface channels or subsidiary drains may be constructed at but little cost to relieve the lowest parts of streets likely to suffer from a great fall of rain in a short time. To convey to the reader's mind a popular, and it is also a scientific, view of a perfect system of drainage for Louth, we will give an illustration from a natural object. Take a leaf, any leaf will do, but say a leaf of the nut tree; place this leaf in your left hand with the leaf stalk between your middle fingers, the point of the leaf towards your wrist; now turn your face to the east (this is not intended as a High Church practice) and carefully examine your leaf; here you have a tolerably correct model of the drainage area (the 280 acres) of the town of Louth. The general form you are looking at is what botanists call an obovate leaf; that is, it is a roundish oval with the wide end furthest from the leaf stalk; with the extreme tip drawn out to a point; this may represent the end of West-gate at its junction with Love-lane, the leaf stalk between your fingers may be the highway beyond Louth Park houses, say just beyond the boundary of the Parish. Now let your eye glance along the mid-rib, and, as you hold the leaf, you will find there is a decided fall towards the leaf stalk; though with but a slight undulation either vertically or horizontally along the whole length of the line; but on either side, on the south, or on the north, you will observe risings and undulations which not inaptly represent the contour of the ground on which the town of Louth stands. Beginning at the point of the leaf (the upper end of West-gate) you will observe several veins all coursing from the edge of the leaf into the mid-rib, with sundry secondary veins making up a net-work of lines, all communicating with the central mid-rib; and these lateral veins very fairly represent Love-lane, Edward-street, School-house-lane, Up-gate, Vickers'-lane, from Mercer-row and the Market-place, Maiden-row, Trinity-lane, Commercial-road from East-gate, and minor openings on the south side; on the north, the mid-rib, which we call the main sewer, receives the veins, or sewers, from Grimsby-road, Bridge-street, Union-hill, Holmes'-lane, with Newbridge-hill and other tributaries—and the whole uniting with the leaf-stalk, or main sewer, on the public highway near to "Johnson's" mill—forms one complete outfall for the whole of the drainage of Louth.

Examine your leaf more minutely, and you will see three distinct points of interest, which coincide so exactly with the rules laid down by the most eminent engineers as being indispensable in the construction of sewers, that we are tempted to repeat the trite remark "There is nothing new under the sun." You will observe that the veins, or sewers we will call them, in your leaf are slightly sunk below the general surface, and that the lateral veins enter the main trunk on the north and south side at different points; in no case do opposite veins enter at the same part of the mid-rib. And again, scrutinise the point of junction, and you will find instead of entering at right angles, or in an abrupt fashion, as a clumsy draughtsman would represent it, it forms a beautiful curve or the fine point of a V, so that the fluid from the tributary streams is delivered very nearly in the direction of the main flow. Still another point; the smaller veins (sewers) have their upper surface coinciding with the top of the main or mid-rib, and, a glance

at the back of the leaf will show you, the larger veins (sewers) are the deepest from the top surface, and the smaller sewers (veins) therefore deliver the fluid with a fall into the principal channel which thus passes away through the outfall (or leaf stalk) you are holding between your fingers. There is still another feature in our leaf, though not visible to the naked eye, namely the existence of numerous pores; these may very properly be compared to ventilators and entrance holes combined.

If the reader has followed this leaf description—which is certainly not copied from Linnæus, Smith, or Hooker,—he will understand the whole theory of drainage. To put this in practice, however, requires much labour and thought; and constant care and supervision. And this, unfortunately, is what the drainage of Louth has not yet received. This work has been done by the "rule of thumb," and one would suppose at what is called the *cheapest*, though in reality at the *dearest* rate. Every one will admit that a good article though it cost a large sum of money, is the cheapest in the end; and to nothing will this apply with greater force than to public works; especially to those on which the comfort, health, and lives of the people so materially depend; and which are embraced under the term "general sanitary arrangements for the public health." If any reply to this by saying, "Oh! I think the town is right enough; there is, in this respect, no improvement required;" we can only rejoin—happy man! whose life-path-lines have been laid in pleasant sunshine, along neat and well-trimmed borders; and whose non-acquaintance with the grossness and impurities of the town place him in the same category as the French princess, who on being informed that the people were starving exclaimed "Starving! why I would live on bread and cheese before I would starve." Exactly.

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LETTER No. VIII.

OUR STREETS.

Let us consider our streets. For the "public health" requires not only that poisoned air should be taken away from our dwelling places, but that pure fresh air and the sun's rays shall have ready access thereto. That our streets shall be wide enough and straight enough to admit the rain and the sunshine, so that the scrubber and the scavenger may have a reasonable chance of keeping them clean. Yet nearly all old streets were laid out as though designed to defeat this very object; to shut out fresh air and sunshine; were crooked, narrow, and irregular, with overhanging three-storied attics as though with extended arms they were spreading a mantle of protection over dirt, disease, and pestilence. In large cities the evils might have been much greater at the present day, if the devouring flames had not swept much away, and so cleared the ground for improved plans. Louth is capable of much improvement in this respect. Take, for instance, the centre of attraction in Louth—St. James' Church. This is a noble and a beautiful building, which no educated eye can look upon without feeling that sort of admiration which inspired Keats to write "A thing of beauty is a joy for ever." Note how

every approach to this noble structure partakes of a crooked or spiral form, so that from no point of view can the full beauty of the design be seen. If we did not know that accident and chance has had so much to do with these things, one might suppose the spirit of mischief had ruled in laying out the streets, so that good care should be taken to conceal the beauties of our Church. To those who pass the Church every day of their lives it may seem paradoxical to be told no living person has ever seen Louth Church! That is to say, that has seen the whole building from a given point of view, at such an angle of vision from the spectator to the base and to the vane as would display its full beauty without being marred by distorted perspective. However, this in passing: for not much improvement can be expected here beyond the removal of the triangular cluster of delapidated cottages on the north side; and this much may come to pass. We hope it will.

But in other parts of the town, the old town, much might be done in the way of improvement without a great sacrifice of property; and if any work of main drainage has to be undertaken in Louth it is to be hoped the widening of streets will be accepted as a necessary feature in the plan for re-organising these matters. The widening, opening, and general improvement of the streets, will not only produce good sanitary effects by giving more freedom to our breathing places and securing greater cleanliness, but must have a good moral effect as well. Any one with an observant eye will be familiar with certain old cottages and ancient buildings in Louth whose removal either entirely away, or several feet backward from the street would produce a very beneficial effect. And we may here state that, as a rule, for a good street the carriage way should be equal in width to the height of the houses to be built in it and foot-paths on each side six feet wide; and this, supposing the houses are thirty feet high, would give a total width of forty-two feet, this should be the minimum width; for a chief thoroughfare as much more as you like. But to judge how much we fall short of this minimum standard, the reader has but to call to mind some very public thoroughfares which are less than half this width. He would be a very sanguine man who expected much to be done in these old established places; but public attention should be drawn to all lanes or roads that are being built upon and are fast acquiring the character of streets. Here there can be no doubt it is a public duty to see that the "public health" is cared for; and not allow people to build houses within a few feet of the centre of the "road" and jumbled together in such a manner that one would suppose they had been shot from some colossal waggon, like a load of bricks, each one sticking where it chanced to fall, the owner being utterly regardless whether its facade ran parallel with the street or road.

We are such slaves to custom that we are inclined to acquiesce in most long established usages, although the conditions under which such usages become established are so utterly changed, than an act which might be wise a few generations ago may be but foolishness now. For instance, to determine (some seventy years ago) upon the width of a lane outside the town being sixteen feet when it had but little traffic and no prospect of becoming a populated part of the town (to wit, Holmes'-lane) was nothing remarkable. But now when this "lane," is fast developing into a street in consequence of its

proximity to the railway (a new-fangled idea of transit which had not been born when the "award" was made)—it is highly inconvenient to maintain it at its present restricted width—a strip of land of at least twenty-five feet wide should have been purchased along its entire length and thrown to the road.

It is to be feared our grandchildren will not think their grandfathers wise in this respect.

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LETTER No. IX.

STREET IMPROVEMENTS.

In our last some considerations were urged in favour of street improvement; even in a small town this question possesses greater importance than it may seem to do at first sight. It has already been noticed that streets should be proportioned in width to the size of buildings erected in them, so that fresh air and sunshine may be admitted and cleanliness promoted. For this latter purpose, good stone footpaths and well-paved streets are essential.

It is true there are many considerations that will arise in determining of what material our streets shall be covered with. But a long (and those with corns might add a painful) experience prompts us to remark that the material of which the streets of Louth are composed is so sharp and gritty to walk upon either when repaired, or when it has been "just washed in a shower," that one would suppose it was peculiarly designed, by its rapid wearing away the soles of our boots, to be specially intended to produce a rich offering of a golden cornucopia to the ancient order of cordwainers. When fresh "metal" (not precious—but *gravel*) is put on the streets it seems but to lend an additional zest to the grinding process carried on: resulting in tolerably complete disintegration of the metal used. Then follows either oceans of mud, or clouds of dust; and now comes water-carts and "roadmen" (we are grown too polite to call them scavengers) to allay and clear away the debris: the men labouring with scraper and broom to concentrate the mud and loose gravel into a kind of sloughy embankment on the sides of the street, which, if not removed at once, becomes a snare and a pitfall for the unwary pedestrian; then dust-men and road-men with shovels and brooms and carts gather up the refuse in greater quantities than the amount of the *metal* put on, and convey it away to some mysterious place, perhaps to fill up an exhausted lime pit: to form a glomerated mass of mud and nastiness, and will be a fruitful source of disease to the denizens of future dwellings erected upon the site. Again more metal is put on the streets, and again and again the whole process is repeated with an aggregate net result of more being carted away from, than is put on, the roads.

We might put this part of our subject in the form of an arithmetical question, which some school-boy who is fond of "figures" may find amusement in working out. Thus—Given twenty acres of street surface, macadam roads; to repair which 700 cubic yards of gravel is annually used: and that 800 cubic yards of mud, disintegrated gravel, &c., is annually carted away from these 20 acres of street area. Then the question is—How much will the surface of the street be lowered under such treatment in twenty years. The answer to be in inches to the fourth decimal.

Does it not occur to the economist that this method of making and repairing streets is the most expensive and the most antagonistic to public health that can be adopted. First, as to the expense. There are three modes in common use of forming our streets distinguished as macadam, boulder paving, and square-set pavements. We shall not dwell upon the details of these respective systems further than to remark that their comparative cost of making and repairing may be represented thus:—square sets 2, boulders 7, and macadam 16. Hence macadam is eight times dearer than square sets; and in safety, freedom from dust, and in the less cost of sweeping and watering makes its economy clearly evidenced. Then for the health and comfort of the people the advantage of the square set pavement over all others, except the new costly French asphalte, is very marked. On the other hand, the macadam, especially such macadam as we sometimes have in Louth, is not only the most costly but the most injurious to health. It is always either dust or dirt, or grinding grit, forming a composition into which is incorporated the usual street filth, droppings of animals and other refuse; the whole being pounded into a mass of more or less putrifying matter. It is very retentive of moisture; and on the well-known principle of cooling a surface by evaporation, heat is given off freely; more especially in narrow streets which have a maximum of dampness with a minimum of sunshine, producing cold and chilliness which is so detrimental to health.

All real improvements are gradual; it is safe they should be so, and especially it is so in sanitary matters. We have already insisted upon the necessity of obtaining as complete data as industry and science will furnish in these matters. No doubt the Government will in their printed code of regulations give ample guidance to all urban and rural sanitary authorities of public health: but this will not relieve local authorities from the necessity of acquiring the special knowledge of their own localities. And it is well to bear in mind some few street improvements that have been made in the memory of the oldest inhabitant. To wit, getting rid of "Watery-lane," which must have been a constant nuisance and inconvenience to the people of that neighbourhood by causing the surplus water from Aswell-springs to run under instead of *on* Maiden-row and part of East-gate, into tunnels beneath the surface and so on into the river Lud. That this has been imperfectly done is pretty clear from the experience gained during the present year, and the somewhat hostile attitude taken by some of the holders of property in Maiden-row against the present state of things. That concrete form of being we call the "oldest inhabitant" will also remember the time when the Market-place was encumbered with a block of buildings of a public character, whose removal was a great boon, leaving the present open space called the Market-hill. He may also remember (possibly) that at the west end of Gospel-gate the road suddenly dipped to the depth of three feet below the present surface, the old boulder paved road is still there beneath the present one; and no doubt the usual rainfall would keep it well supplied so as to form a constant *pool* of stagnant and putrid water; and if he can remember the geese which enjoyed this pool, he will have succeeded in tracing the origin of "Goose-pool, *alias* Gospel-gate." If these and similar improvements have been made in times past may we not hope that other improvements

will be effected in various streets and in many ways, so that our streets and roads may merit a good character and bear comparison with any others. Gibbon, the great historian, says, much of the civilisation of a nation may be read in its roads. And the needs of modern society, living together in close masses as it does, makes it imperative upon the governing powers to insure us having good roads and streets; well lighted, drained, and purified: and an intelligent care bestowed upon all matters which influence or regulate the public health.

It will be understood that the best manner of paving streets can only be adopted when the underground works, the drains, gas and water pipes, &c., are far advanced towards perfection, so that the breaking up of our streets shall be reduced to a minimum.

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LETTER No. X.

HOUSE DRAINAGE.

There are two important branches of our subject, alluded to in the introductory letter, not yet treated of, viz:—house drainage and the utilization of sewage. And these may be not inaptly described as the commencement and termination of all that is embraced under the head of town drainage. The utilization of sewage is so large a question, involving so many considerations, that much would have to be written to present even a general commentary on this part of our text to the uninitiated. And as the chemical discussions at the recent meeting of the British Association at Brighton, show that the real value of sewage matter, as supplying the constituent elements of our grasses, vegetables, and cereal crops, is not finally set at rest, it would be idle and presumptuous to speak, in this brief essay, with confident conclusions on a subject that is thus confessedly surrounded with doubt and difficulties. Probably a few more years experience in these things; more experiments, and more knowledge will ultimately remove these difficulties. In the meantime we must wait patiently and see what others can do, on a wider and more varied scale than has been yet attempted, before we could seriously entertain the thought of utilizing the sewage of Louth. Indeed as this is the ultimate object in view, and its realisation depending upon a perfect system of town-drainage, yet to be inaugurated in Louth, we may fairly be permitted to remark that these considerations are postponed for the present.

But house drainage is a branch of our subject that presses for immediate and constant supervision; one that in spite of ignorance, neglect, and short-sighted economy, has the power to arrest our attention, whether we will or no, by the foul smells, nauseous floodings, and oft-time fatal effects resulting from the imperfection of the work done in what is called house drainage; which sometimes is no drainage at all, but becomes a receptacle for sewage gases that are ever ready to escape into our houses, poisoning the air we breathe and the blood that forms our bodies. In these short papers we cannot go into much detail; but house drainage is of such vital importance in connection with a complete system of town drainage, and this latter is of

course *assumed* to exist in what we have to say on the minor or house drains, that we must give this branch of our subject a somewhat more exact attention than we have hitherto given to the main question. For the sewer drainage of the town may not be re-constructed at present, but house drainage is continually cropping up and demanding repairs or re-making. We have had sufficient experience in these things to justify us in speaking with distinctness and certainty in what will be here advanced.

More than twenty years ago the writer was agent to some cottage property in a certain "yard," in which provision had to be made for the drainage of about a dozen cottages. It had to be managed at a small cost; the drains were frequently stopped, and had to be opened and cleaned out; but as nothing but the commonest materials and the cheapest or most inferior labour could be employed, the result was always more or less a failure. And is not this too frequently the case? Is it not true that even when a skilful architect is employed, the drainage of houses and public buildings is often left to a hum-drum labourer to do the work at the lowest cost? And what are the consequences? A few years pass away and it is found that not so much as clean water will run through them; perhaps it is found the fall has been laid the wrong way for part of the distance: common tile pipes have been used with joints an inch or more apart, tiles here and there tilted up, so that a bar is quickly formed in the drain, then it is only a question of time as to when we hear the cry, "the drains are stopped." And this too in some cases where the most ample fall could be obtained say one in thirty-six: but the opportunity was entirely thrown away through the carelessness or the stupidity of the drain-pipe-layer.

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CONCLUSION.

HOUSE DRAINAGE.

In our last we commented upon the inferior character of the work done in house drainage: we would further remark that they are frequently as ill-planned and designed as they are clumsily executed. When drain pipes have to be laid and it is recommended to employ a skilled workman to do it, how common it is to hear people say, "Oh! any labourer may do that." Yes, he may do it, but with what result? This non-appreciation of the importance of drain-pipe laying recalls the incident of a fond father, who being desirous of placing his son as a student under Sir Joshua Reynolds, extolled the performances of the aspiring artist, by observing, "I assure you, Sir Joshua, my son has made so much progress in the art that he could paint in your back-grounds." "Indeed, indeed," quoth the spectacled President of the Royal Academy of Arts, "indeed, sir, the back-grounds of my pictures cause me more thought than all the rest of my work." So it is, that which seems to be but the inferior parts frequently require the most skill and care. And if your house drains require attention by all means employ the man who has the most knowledge of the subject, and the best "head" for it is only such that can be entrusted to carry out the details of the work in a manner to insure the desired effect.

We may notice one or two cases which, though literally matters of fact, may be taken as typical. We have known a drain pass under a house, a house of good pretentions too, formed of common bricks, built with common mortar in a slovenly manner, square in form—in every sense objectionable. The untrapped head was a few feet below the kitchen floor, and was used to drain a slightly sunk cellar. The smells were intolerable. The sink was done away with in the cellar, and the end of the drain carefully made good with cement. But this only concentrated the nuisance in the sitting room; this was so bad at times—when the wind blew the sewer gas up the drains—that the room was uninhabitable. It was then discovered that the excreta from a closet was passed into the sewer very near the same place this house drain entered. There was but one treatment for this, as the head of the drain had been dispensed with in the house, the drain was opened in the street and destroyed; the opening to the sewer was built up with bricks and cement; the end of the drain, near the foundations of the house, received the same treatment, and not the slightest smell remained. It may be asked, "but suppose the drain under the house could not have been spared, what would you have done?" An answer to this will be given in the summary we purpose to give, as an appendix of what is essential in the construction of sewers and drains to insure healthy houses.

Take another case. "Something is the matter with the drains." The street is again torn up, and it is found there is a four-and-a-half brick-built barrel tunnel from the house to the sewer; the house-drain has sunk and is so broken that the ordinary flow of house refuse would percolate into the chalky ground on which the tunnel was built, and find its way to the cellar in close proximity. Go on a little further and you will find that not only is the junction of the drain with the sewer done in a most imperfect manner; but we discover some common drain tiles from a closet-soil pipe laid so loosely in the ground that all the joints, at the bottom of the drain tiles, will admit the hand edgeways, and one so large as to admit a man's double fist. And what was the result of this? The closet-soil found its way through the joints of the tiles and bricks, instead of passing into the main sewer, making the the whole ground around a black mass of putrid matter. Saturating the brick-work and forming a collection of sewage poison so highly offensive that the effects upon the inmates nearly proved fatal. Here extreme measures were adopted. The old badly built barrel drain was discarded; it was opened in the street and carefully built up with bricks and cement, and the whole drainage re-constructed with sanitary pipes, united at the joints with cement; care being taken that no bar was formed inside with the cement: a fall given of about one in thirty; the head of the drain, in the open garden, trapped; and the cure of this was complete.

It may be remarked that although the head of a drain, a sink in the house for instance, is properly trapped still nasty smells will at times be noticed. This arises from the fact that neither the house drains nor the sewers are ventilated; and the more completely the street gullies and all entrances to the sewers are trapped, the greater will be the pressure of the sewage gases, when acted upon by a strong force of wind at the sewer outlet; and as it must find an outlet somewhere, like confined steam, it will force its

way through the sink traps or any chinks in the pipes. All important house drains, and sewers, at their highest parts, and at the principal ramifications, should be ventilated—by ventilating shafts built for this special purpose, these shafts or pipes should be upright tubes of sufficient sectional area that would insure the escape of the sewer gas. When this is not done the sewage gas will, under pressure, find vent through the small column of water in bell traps, which is often not more than a quarter of an inch in height. There is another cause why sewer gas enters our houses; and some writers hold that this is the principal cause; namely, that the heat of the house causes a suction to act upon any cranny that may exist in the pipes, and it is found impossible to ensure permanent air-tight pipes, hence the poison, we call bad smells, enters our houses. Under these circumstances the most effectual plan to adopt in house drainage is this; use the simplest arrangements so that all pipes from baths, closets, sinks, &c., shall be on one side of the house; but whether this is possible or not, see that all the pipes unite to form one common outlet to be taken from the house towards the sewer; before entering the sewer the drain-pipe should have a double trap of a somewhat peculiar arrangement, which cannot be understood without reference to drawings or models, though it is extremely simple, and admits of being examined without being broken into or allowing the sewer gas to escape. The pipe not to be so small as to run full and possibly empty the trap by its syphon action; nor too large to cause a sluggish action by an excess of friction. If this is done in a good workmanlike manner, the special condition of this system being observed, it will not be possible for "bad smells" to get from the sewers into our houses. And the traps in the house may, if desired be dispensed with except for the pan of the closet.

In the Appendix we will place before the reader the best mode of making sewers and drains (by the former is meant the street drains, by the latter the tributaries or house drains). For this purpose some general rules may be laid down—according with the best engineering authorities, and if these are honestly carried out under a vigilant supervision, the work done will last for many generations without requiring any further attention. When drains are properly designed and executed, with a sufficient fall, &c., they effectually clear themselves of the refuse passing through them without that somewhat clumsy, and oft-times futile, expedient of flushing (commonly called flushing) the drains. The meaning of this is to send a large body of water through the sewers with a flash (that beautiful old English word—as we say a "flash of lightning,") and so clear out the deposits. This must do good, but will fail to do the good expected of it if there are inherent defects in the construction of the drains.

Without entering into the details of water supply in reference to watering the streets without water-carts, or the supply of water to cleanse rows of cottage closets that are at present entirely devoid of any such contrivance—or indeed of any means of being regularly emptied—we would remark that the sanitary arrangements of the town cannot be carried out efficiently unless the water is abundant and supplied under pressure, to every household. The time may probably come when Corporations will find it is there interest,

even if it is not an enforced duty, to have the control of the water supply of the town in their own hands, to aid their endeavours to promote the health and welfare of the people, whom they represent and whose trustees they are. In the Appendix will be found some particulars relative to the quantity of water, the rainfall, and the public springs yield. (*Appendix* A and B.)

We cannot conclude these general remarks on Sanitary Requirements more appropriately than by quoting a few sentences from a speech delivered some months ago by that gifted man, the Right Hon. B. Disraeli, who observed, "I venture to say the health of the people is a most important subject for discussion. It is, gentlemen, a large subject. There are many branches. It concerns the dwellings of the people. It concerns their enjoyments, for it refers to air, light, and water. It concerns the regulations of their industry. Cynics many call this a policy of sewage: but to the labouring multitude of Englishmen, to men who have had fever in their families, who have year after year the children of their loves stricken down, this is not a question of sewage, but it is a question of LIFE."

APPENDIX.

A.—The annual average rainfall of Louth is 29·67 inches; it is calculated that the "upheaved" land forming the full drainage area of Louth is 535 acres, and as one inch of rain equals (nearly) 101 tons per acre, we have 535 × 101 × 29·67 = 1,603,218 tons of rain in one year upon this area. This is equal to 359,120,932 gallons; or to put it in a more familiar form, it is equivalent to 94 gallons per day per head of the population of 10,500. About one half of this passes off in evaporation; we may estimate one quarter to percolate into the earth, (part of the town stands on a chalk formation, though the subsoil is chiefly clay) or absorbed by plants, or runs into the Lud, or the natural catch-water drain—Monk's-dike; leaving only one quarter of the rainfall, about 90 millions of gallons, or 14,410,952 cubic feet of water to pass through the sewers annually. Now if we suppose the 10,500 inhabitants to use 30 gallons of water per head per diem, added to the rainfall, will give 204,975,000 gallons to be discharged annually; if this could be discharged at a regular rate throughout the year, it would pass through an egg-shaped sewer 2ft. × 1ft. 3in., half full, at the rate of 73 feet per minute, supposing the fall to be 3 feet per mile or 1 in 1760, and the area of the running water would be ·87 of a foot. With a fall of 20 feet per mile the same quantity of water would be discharged at the rate of 170 feet per second in a sewer 1ft. 4in. × 0·10in., half full; area of running water ·39 of a foot. But to meet heavy falls of rain, and considering that the bulk of the water used by the inhabitants is in twelve instead of twenty-four hours, much larger sewers are required especially when allowance has to be made for the friction of bends.

B.—There are some minor springs which discharge themselves into the Lud during its passage through the town, of which no note need be taken here; apart from these are the important Springs of Aswell (or Haswell) and St. Helen's, situated in Aswell Hole, and Gatherums, mentioned in the text. The maximum quantity of water these springs yield, according to observations made up to the present time, is as follows:—

1872.		*Gallons Per Minute.*		*Gallons Per Day.*		*Per head per diem* (10,500).
Nov. 1st.	Aswell.....	459	...	660,960	...	63
	St. Helen's...	89	...	128,160	...	12
,, 23rd.—Aswell.....		660	...	950,400	...	90

The rainfall for this year will be about 30 per cent. above the average of the last nine years. In a dry season the yield of these springs would probably be about one-half of the above quantity.

GENERAL RULES IN DRAINAGE,

CHIEFLY EXTRACTED FROM SUGGESTIONS BY MR. RAWLINSON, C.E.

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1.—Natural streams not to be arched over to form main sewers.

2.—Main sewers need not be of capacity to contain the flood water of the area drained: they should be laid in straight lines and true gradients; if the gradients are steep they should have full means of ventilation at the highest points, and should be capable of conveying all sewage to some common outlet without retaining any sediment.

3.—House drains should not pass direct from sewers to the inside of houses, but all drains should end in an outside wall and have means of external ventilation. Inlets to all pipe drains should be properly protected.

4.—Earthenware pipes make good sewers up to their capacity. Pipes must be truly laid and securely jointed with clay or cement, as the case may require. If compelled to lay a drain *through* a house, it must be bedded in concrete and made perfectly impervious.

5.—Brick sewers should be formed with bricks moulded to the *radii*; and, in all cases, set in hydraulic cement.

6.—Main sewers may have flood water overflows wherever practicable, to prevent such sewers being choked during heavy rains.

7.—Sewers should not join at right angles. Tributaries should deliver sewage in the direction of the mainflow; all junctions and curves should have extra fall to compensate for friction. All the material used should be sound, and the workmanship carefully attended to.

8.—Drains outside a house should have lipped or capped drain tiles used at stated intervals, so that the drain can be examined without being broken. All sewers and drains should have arrangements for full ventilation at such points, and in such manner as not to cause any nuisance.

9 and lastly.—The owner of a house should insist upon receiving from his architect, or builder, a correct drainage plan, with the different sized pipes, the position of bends, junction, &c., and the depth from the surface carefully marked on. A copy of this should be given to a tenant when taking a lease of a house: and any alteration made in the drains should be duly entered on the plan.

ADDENDA.

When these letters were commenced (in August last) the current opinion was that the proposed Public Health Act, would stand over till next year, and that if it did become law in 1872 no action would be taken in Louth; it was said nothing could be better or cheaper than our present drainage into the Lud. But events have outrun our thoughts. The "Public Health Act" became law; a Government Inspector has, by invitation, visited the town, and consulted with the Town Council on the 21st November. He gave a decided opinion that if a complaint were made to the Local Government Board of the nuisances caused by the sewers draining into the Lud that the law would require the Corporation to find another outlet for the sewage; and as this would be compulsory he advised them to consider the question at once, and call in an engineer to advise upon the best course to pursue. A few days afterwards the Council decided that the Drainage of the Town should be carried out in the spirit of the Public Health and Sanitary Acts; and that an engineer should be called in to give an opinion on the whole question. Thus that which seemed visionary but a few months ago is now in a fair way to become a reality.

The principal things to be attained will be the construction of an intercepting sewer through the lowest levels of the town, and providing some means of deodorising the sewage at the outfall beyond the east side of the parish—passing the liquid into the Lud and utilising the residue. Such a sewer should be of an egg-shape, straight, true in gradient, and of the best workmanship; it probably need not average more than 3ft. 6in. by 2ft. 4in. as the fall is good—being 40 feet from the top end of West-gate to the parish boundary on the East—a distance of one mile and a half; therefore the average rate of inclination is 1 in 198. If the reader refers to letter No. 7. he will see the general idea of the whole tributary sewers entering the main drain from the north and south sides of the town. It may be useful, more to invite discussion than to propound a finished scheme, to say the course of this main sewer would be, after passing along West-gate, and the north side of the church (removing the cluster of cottages there), on through Chequer-gate, and either widening the street from Healey's Court to Engine-gate, or the sewer to pass under the houses so as to take nearly the lowest levels, and

onwards in a straight line through sundry yards, &c., into and along James-street, crossing the Lud unless a straight cut be made from the factory water-wheel to the back of the Priory, instead of allowing the present *detour* of the stream by Ramsgate to remain. The sewer would then continue on the south side of the Lud across the Priory and Prison grounds, bearing southwards until it reached the highway at the River Head, near to Mr. Norfolk's house, and thence beyond the parish boundary, near which the deodorising process would have to be carried on.

Such is a general idea of what will have to be done, care being taken to utilise the existing town drains as much as possible; though it may be expected many lateral sewers will have to be made to join the main one.

T. W. W.

December 2nd, 1872.

CONTENTS.

		Page
	Preface	3
Letter 1.	—Sanitary Requirements, Introductory	5
,, 2.	—Water Supply	7
,, 3.	—Drainage Area of Louth	9
,, 4.	—The Health of the Town	11
,, 5.	—The Town Sewers	13
,, 6.	—Correct Data Required	15
,, 7.	—The Theory of Drainage Illustrated	17
,, 8.	—Our Streets	18
,, 9.	—Street Improvements	21
,, 10.	—House Drainage	23
,, 11	—House Drainage, Conclusion	24
	Appendix	28
	General Rules in Drainage	29
	Addenda	30

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Ueber

die pharmakologischen Eigenschaften

von

Eucalyptus globulus.

Inaugural-Dissertation

zur

Erlangung der Doctorwürde

bei der

medicinischen Facultät der Rhein. Friedrich-Wilhelms-Universität

zu Bonn

eingereicht und mit Thesen vertheidigt

am 31. Juli 1873

von

Theodor Siegen

aus Kolshoven.

Namen der Opponenten:

Carl Falkenberg, Dr. med.
Vincens Grisar, Dr. med.
Heinrich Vogel, Dr. med.

Bonn,

Druck von Carl Georgi.

1873.

Meinen lieben Eltern.

Schon lange war es Allen, die sich mit Klimatologie beschäftigten, aufgefallen, dass Australien trotz seiner im Ganzen mehr tropischen als subtropischen Lage ein so gesundes Klima geniesst. Während der Aufenthalt in den südlichen Theilen dieses Continents auch dem Nordländer sehr zuträglich erscheint, ja, dieselben dort ein hohes Alter erreichen, ihre Colonien aufblühen und keines Zuzuges aus Europa zu bedürfen scheinen, um die noch uncultivirten Strecken ihrer zweiten Heimath zu bevölkern, verschlingt die französische Colonie Algier, trotzdem sie unter denselben Breiten liegt, trotzdem die übrigen Verhältnisse hier wie dort dieselben sind, jährlich eine Menge dorthin gesandter Ansiedler, ja übersteigt die Zahl der Sterbefälle die der Geburten so sehr, dass in kurzer Frist, ohne Zuzug aus Europa, die Colonisten dort aussterben würden. Dasselbe zeigt sich für Aegypten, für die unter gleichen Breiten liegenden Staaten der Union. Diese günstigen hygieinischen Verhältnisse werden von den europäischen Ansiedlern, vielleicht mit richtigem Instincte, ganz besonders der massenhaften Verbreitung des sogenannten Veilchenbaums, Eucalyptus globulus, zugeschrieben.

Die Entdeckung dieses Baumes datirt aus dem letzten Jahrhundert. Am 6. Mai 1792 wurde er zuerst von Labillardiere gesehen, und konnte dieser sein Entdecker kaum Worte finden, die Schönheit des Wuchses und das Aroma, das er um sich verbreitet, zu schildern. Bis in die letzte Zeit jedoch war der Baum in Europa nur für Bota-

niker von Interesse. Die beste Beschreibung von ihm hat Dr. Müller, Director des botanischen Gartens in Melbourne, geliefert, und gebe ich dieselbe hier im Auszuge wieder.

Der Veilchenbaum ist eine gigantische Myrtacee, und nimmt, was die Höhe anlangt, den ersten Platz unter den Eucalyptusarten ein. Seine Blätter sind in der Jugend herzförmig und gegenständig, später alternirend gestellt, lancettförmig und von gefiederten hervorspringenden Nerven durchzogen; sie zeigen gegen das Licht gehalten eine Menge durchsichtiger Oeldrüsen. Die Blüthen sind achselständig, stehen zu je zwei oder drei zusammen ohne oder mit kurzem breiten Stiele. Die Blumenkrone ist pyramidal, die Griffel sind sehr lang, die Staubgefässe endigen oval. Die Früchte sind halbkreisförmig oder platt gedrückt und haben 3—4 Fächer. Dieser Baum wächst in Australien in den Thälern und dichten Bergwäldern von der Apollobai bis jenseit Cap Wilson. Ebenso häufig findet er sich auf Vandiemensland. Er hat ein rapides Wachsthum und kann zu den Kolossen des Pflanzenreichs gezählt werden, denn er erreicht häufig eine Höhe von 180—200 Fuss. Dabei ist der Stamm sehr elastisch zäh, das Holz dauerhaft, sodass es getrocknet (wobei das in ihm enthaltene Harz fest wird) zu den besten Nutzhölzern gehört, und z. B. in Australien grosse Seeschiffe aus ihm gezimmert werden.

Jedoch das, was dem Baume seinen Ruf verschafft hat, sind seine Blätter oder vielmehr das Oel, welches jene exhaliren, und das dem Baume bei den Ansiedlern seinen Namen (violet tree) verschafft hat. Von allen dort lebenden Europäern wird diesem aromatischen Oele die Fähigkeit zugesprochen, die Luft von Sumpfmiasmen zu befreien, Malaria und ansteckende Krankheiten zu verbannen.

Bisherige Beobachtungen.

Der mannigfache Nutzen, den man sich sowohl von seinem Holze als von seiner fiebervertreibenden Gegenwart versprach, war der Grund, dass man ihn in den von Fiebern heimgesuchten und an Holzmangel leidenden Gegenden Südeuropa's (besonders Spaniens und Südfrankreichs) sowie in Algier anpflanzte, wo er herrlich gedieh. Jedoch während er in Frankreich noch in der Normandie, wie mir Dr. Wolter mündlich mittheilt, gut fortkömmt, und seine Blätter dort als heilkräftig sehr geschätzt werden, gedeiht er schon an den Ufern des Rheines im Freien nicht mehr, und muss z. B. ein Exemplar dieses Baumes im hiesigen Botanischen Garten jeden Winter im Warmhause untergebracht werden. Geleitet von der Ansicht, dass in dem weingeistigen Extract der Blätter jener miasmenfeindliche Stoff enthalten sei, wurde zuerst in Spanien, dann in Frankreich, zuletzt in Ungarn und Ostpreussen die Tinctura Eucalypti zur Bekämpfung der Malaria und anderer Krankheiten angewandt und wurde der Versuch von glänzenden Erfolgen gekrönt. Es mögen hier einige der beachtenswerthesten derselben folgen, die ich aus der Litteratur zusammenstellte.

Lorinser cultivirte einige Exemplare, die aus dem Samen gross gezogen wurden, in der Stube und bereitete aus den frischen jungen Blätter ein kaltes alcoholisches

Extract (ʒii ad ʒVI), hiervon gab er einem Phthisiker und einem Pyämiker Morgens und Abends ʒj; das Fieber sank bei der jedesmaligen Anwendung; der letztere Kranke genas, der andere erlag der Phthise. Die Wirksamkeit dieses Chininsurrogats gegen Intermittens wurde in der Malariagegend der Theissufer in sehr befriedigender Weise erprobt; zwei Kaffeelöffel der Tinctur mit oder ohne Purgans vor dem Anfalle gegeben, coupirten denselben regelmässig und verhüteten die Recidive. (Wiener medic. Wochenschrift 1869. Nr. 43.) Diese Beobachtungen fallen vielleicht noch deshalb in's Gewicht, weil sie von einem anerkannten Skeptiker der kritischen Wiener Schule herrühren.

Gimbert aus Cannes wandte das ätherische Oel zu 3—10 Tropfen mit Erfolg bei träger Circulation in den Gehirncapillaren (?), bei gleichzeitig vorhandenem Cor adiposum an. Ebenso fand er die Tinctur und die gepulverten Blüthen bei Asthma humidum, intermittirenden Neuralgien und äusserlich zum Verbande schlecht granulirender Wunden von grossem Nutzen. (Gimbert, l'Eucalyptus globulus. Paris 1870.)

Prof. Mosler hatte vom Chinin nachgewiesen, dass es in eclatanter Weise die Milz derber, resistenter mache und deutlich verkleinere. Dasselbe Resultat erhielt er, wenn er endweder eine aus Wien bezogene Tinct. Eucalypt. globuli oder das aus einheimischen Exemplaren bereitete alkoholisch wässrige sehr concentrirte Extract subcutan oder per os einführte; die Menge entsprach etwa 20 Gramm der Normaltinctur. Die Milz wurde vor dem Beginne eines jeden Versuches herausgeholt, und gemessen; schon 1 Stunde nach der Einfuhr des Mittels zeigte sich in vier Versuchen das Organ derber, die Oberfläche granulirt; nach 3—4 Stunden war die Oberfläche stahlgrau mit rothen Wärzchen bedeckt, der Umfang sichtbar und messbar verkleinert.

In der contrahirten Milz konnte man ein continuirliches Summen auscultiren, vielleicht von den contrahirten Gefässwänden fortgeleitet. (Centralblatt Nr. 36. 1872.)

Dr. Strube (Demmin) hat die Tinctur, veranlasst durch die Untersuchungen Moslers, bei acht Fällen von Intermittens angewandt, und bei allen Heilung erzielt. In einem Falle hatte das vorher gebrauchte Chinin seine Wirkung total versagt. (Berlin. Wochenschrift Nr. 52. 1872.)

Dr. Keller hat mit Dosen von 2—7 Drachmen der Tinctur 310 Intermittensfälle behandelt mit günstigem Erfolge. In 202 Fällen reichte eine einmalige Dosis hin, in den übrigen musste eine zweite genommen werden. Die Fälle gehörten den perniciösen Formen an, wie sie in den ungarischen Sumpfgegenden vorkommen. (Wien. med. Wochenschrift refer. Lancet 2. Nov. 1872.)

Chéron hat nach der Inhalation von Eucalyptol Besserung und sogar Vernarbung von Lungencavernen bei chronischer Phthise constatiren wollen (??). (Gazette des hôpitaux 21. Nov. 1872.)

Primararzt Dr. Bleiweiss veröffentlichte einige Fälle von Intermittens, bei denen er die Eucalyptustinctur anwandte. Die Patienten litten an den perniciösen Wechselfiebern, die sie in Italien oder der Türkei acquirirt hatten. Es waren dies vier Quotidianen, von denen drei durch dreimalige Gabe von je zwei Theelöffeln der Tinctur geheilt wurden, die vierte aber sowohl dieser als dem Chinin widerstand und ungeheilt entlassen wurde. In zweien der Fälle war vorher Chinin in grossen Dosen ohne Erfolg angewandt worden. Ebenso glänzend bewährte es sich in zwei Fällen von hartnäckiger Tertiana, welche vorher erfolglos mit Chinin behandelt worden war. Nur einmal zeigte es sich bei einer Quartana wirkungslos, wo 0,7 Gramm Chinin die Anfälle coupirten. (Memorabilien XVIII, 3. 1873.)

Kirchberg aus Nantes hat von 19 schweren Intermittensfällen 15 »durch einige Esslöffel« Eucalyptustinctur vollständig getheilt. (Révue des sciences médic. 15. Avril 1872.) So wird wenigstens berichtet.

Chemisches.

Bei diesen Versuchen, die ich eben aufzählte, wurde fast ausschliesslich die Tinctur angewandt. Diese ist natürlich ein Gemenge verschiedener Substanzen in Alcohol gelöst: Gerbsäure, Chlorophyll, ätherische Oele. Letztere schien das Wirksame zu sein, und verdanken wir seine Darstellung und chemische Bestimmung dem französischen Chemiker Cloëz (Comptes rendues LXX 687). Er erhielt aus frischen Blättern junger bei Paris gewachsener und vom Frost beschädigter Bäume 2,75%, dagegen aus bei Hyères gepflückten und einen Monat alten Blättern 6% und aus getrockneten 5 Jahre alten Blättern aus Melbourne 1,5% dieses Oels. Dasselbe ist sehr dünnflüssig, kaum gefärbt und von kampferähnlich aromatischem Geruche. Es begann bei etwa + 170° zu sieden, der Siedepunct stieg dann rasch auf + 175°, wo er anhielt, bis etwa die Hälfte des Oels übergegangen war. Zwischen +188° und 190° destillirte eine neue Portion von Oelen ab, die sich als ein Gemenge von mehreren Oelen herausstellte und über 200° folgte noch eine kleine Menge eines anderen Oels.

Als Cloëz dann die zwischen +176 und +178° überdestillirte Hauptportion des Oels zuerst mit festem Kalihydrat und darauf mit geschmolzenen Chlorcalciumstücken in Berührung gelassen hatte und nochmals rectificirte, hatte er die Bestandtheile des Oels vor sich, das er Eucalyptol nannte und nach der Formel $C_{12}H_{20}O$ zusammengesetzt fand. Dieses Oel war farblos, sehr beweglich, hatte 0,905 spez. Gew., destillirte constant bei + 175°, rotirte nach Rechts (molecul. Drehungsvermögen $(\alpha) = +10°42$ für die Länge von 100 Mm.) und blieb bei — 18° selbst nach 3 Stunden noch flüssig. Es schmeckt, wenn man es dampfförmig mit Luft einathmet, angenehm erfrischend, löst sich nur wenig im Wasser, jedoch leicht in Alcohol, und eine verdünnte

Lösung riecht rosenähnlich. Die Löslichkeit im Wasser habe ich behufs meiner Untersuchungen annähernd bestimmt und gefunden, dass bei Zimmertemperatur das Verhältniss etwa 1 : 3800 ist.

Dies ätherische Oel schien mir das Wirksame der Drogue zu sein, trotzdem Brunel ein Alcaloid aus der Tinctur dargestellt haben will. Denn Rabuteau (Cpt. rend. 20. Oct. 1872) weist nach, dass Phosphormolybdänsäure in der Tinctur keinen Niederschlag hervorbringt, ein Alcaloid mithin nicht vorhanden sein kann.

Zu den folgenden Versuchen wurde ausschliesslich das Eucalyptol benutzt, das durch eine hiesige Apotheke bezogen eine gelbliche Farbe hatte, weshalb es von uns nochmals rectificirt wurde.

Die Destillation des Eucalyptols geht bei vorsichtigem Erwärmen und bei recht trocknem Zustande der Drogue sehr glatt vor sich. Behufs des Trocknens war das zu destillirende Präparat einige Tage vorher mit Chlorcalcium versetzt worden. Von dem Oel, was nach Erreichung von 175° in der Retorte zurückblieb, ist noch zu bemerken, dass es durch das Sieden eine gelbliche Farbe annahm. Dieselbe ist wohl der trocknen Destillation geringerer Verunreinigungen von niedrigerem Siedepunkt zuzuschreiben. Es geht daraus hervor, dass die abermalige Destillation des betreffenden Präparats nicht ohne Nutzen war, wenn auch vielleicht angenommen werden darf, dass sehr geringe Beimischungen eines andern Kohlenwasserstoffs von ähnlicher Art der Wirkung des Eucalyptols keinen Eintrag thut.

Dieses so destillirte Oel wurde auf seine Reinheit geprüft. Denn das Oel ist bei der häufigen Anwendung, die es besonders in Frankreich findet, häufig Verfälschungen ausgesetzt, und zwar sind es meistens der Alcohol, fixe Oele, Terpenthin. Erstern erkennt man daran, dass man zu Eucalyptol eine bestimmte Quantität Wasser schüttet und dann kocht; die

Volumszunahme des Wassers ist dann Alcohol. Sind fixe Oele zugesetzt, so hinterlassen sie auf dem Papiere einen bleibenden Fleck. Terpenthinzusatz lässt bei Hinzufügen von Jod eine Explosion entstehen. 1,6 Ccm. Alcohol von 90% soll 1 Ccm. Eucalyptol lösen, doch tritt dieses, wie ich fand, erst nach längerem Stehen ein.

Durch Behandeln mit Phosphorsäureanhydrid entsteht aus dem E. das Eucalypten, was Cloëz als $C_{12}H_{18}$ angibt. Es soll bei 165° C. sieden. Uns interessirt mehr, wegen der Frage der Zerstörbarkeit im Thierkörper, die Oxydation des ätherischen Oeles. Bei Gimbert finde ich die Angabe, das E. sei nur schwer oxydirbar. Ich habe in Bezug darauf zu bemerken, dass sich an der Oeffnung der Eucalyptolflasche sehr bald ein harziger Beschlag bildete, und ferner, dass das E. nach einiger Zeit Stehens in einer nicht ganz gefüllten Flasche, selbst im Dunkeln, sich mit activem Sauerstoff beladet und dann genau die bekannten Einwirkungen wie das Terpenthinöl u. s. w. zeigt. Frisch destillirt bietet es hiervon nichts dar.

Eigene Versuche.

Mit diesem so rectificirten Oele wurden nun die folgenden Versuche gemacht, bei denen mir die Arbeiten von Binz über Chinin zur Richtschnur dienten. Ich ging dabei von folgender Anschauung aus:

Wenn das Eucalyptol wirklich ein Ersatzmittel für Chinin ist, so muss es im Grossen und Ganzen diejenigen pharmakologischen Reactionen geben, die wir seit einigen Jahren vom Chinin kennen, und ferner, ist die Wirkung des Chinins auf diese Reactionen zurückzuführen, so wird ein gleiches Verhalten seitens des Eucalyptols der neuen Theorie der Chininwirkung zur wesentlichen Stütze dienen.

1.

Zuerst wurde versucht, ob dem Eucalyptol die antiseptischen Eigenschaften zukommen, wie sie Binz beim Chinin nachgewiesen hat [1]). Eine Betrachtung, die sich gerade an diese Frage anknüpft, machte ich ohne sie gerade zu beabsichtigen, in folgendem Falle: Einige Blätter von Eucalyptus globulus waren in ein im Innern feuchtes Standgefäss gebracht worden. Beim Oeffnen des Gefässes nach etwa vier Wochen zeigten sich die aromatischen Blätter an mehreren Stellen mit dichten Schimmelvegetationen bedeckt Beim Wegwischen desselben konnte man mit der Loupe deutlich die zahlreichen Oeldrüsen gewahren, auch war der Geruch des Gefässes ein intensiv aromatischer. Wir sehen hier ein Analogon mit dem Chinin, das, obwohl intensiv fäulnisswidrig, doch unter Umständen schimmeln kann.

2.

In ein wohlverkorktes Glasgefäss wurden am 30. October einige zerschnittene Blätter von Eucalyptus gebracht, dazu Wasser gegossen und das Ganze einer mittleren Temperatur von 12° C. ausgesetzt. Schon am 9. November zeigte sich auf der Flüssigkeit ein weisser Rahm, der sich bei mikroskopischer Untersuchung als aus Hefezellen ähnlichen Gebilden und einem Mycelium bestehend erwies. Die Flüssigkeit reagirte stark sauer, während sie frisch bereitet neutrale Reaction darbot. Sie roch jedoch nicht im mindesten faulig, auch waren die Blätter noch gut erhalten. Dasselbe war auch noch am 24. Januar des folgenden Jahres der Fall.

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1) Nach der Beendigung dieser Versuche fand ich in den Archives générales hierauf bezügliche Versuche von Gimbert gemacht. Derselbe spritzte Kaninchen Eucalyptol ein, wobei er sah, dass das dann gelassene Blut nicht faulte und die getödteten Thiere mumificirten, aber nicht verwesten.

3.

Es wurden in einem Gefässe eine Lösung von Eucalyptol in Wasser im Verhältnisse von 1 : 3800, in einem zweiten eine ebenso starke von Chininum chinicum dargestellt, ein drittes erhielt nur Wasser. In alle drei wurden Eiweisswürfel gebracht und sie dann einer mittleren Temperatur von 15° C. ausgesetzt. Nach drei Tagen roch das Chinin und das Wasserpräparat schon intensiv faulig, während beim Eucalyptuspräparat dies nicht so sehr hervorstach. Alle zeigten unter dem Mikroskope eine Menge Sporen, die jedoch beim Wasser und Chininpräparat grösser und in lebhafterer Bewegung zu sein schienen.

4.

Da das Eucalyptol sich nur im Verhältnisse von 1 : 3800 in Wasser löst, so wurde eine Emulsion von 1000 Ccm. Wasser mit 3,0 Gummi arabic. hergestellt. Hiervon erhielt das erste Gefäss 333 1/3 Ccm. und blieb ohne jeden weiteren Zusatz, das zweite 333 1/3 Ccm. mit 0,45 Chininum chinicum, das dritte 333 1/3 Ccm. mit 0,45 Eucalyptol. In alle drei Gläschen wurden dann je zwei Würfel rohen Ochsenfleisches gelegt, worauf sie wohlverschlossen der Zimmertemperatur ausgesetzt wurden. Vier Tage später roch das Chinin und Wasserpräparat intensiv faulig, das Eucalyptolpräparat kaum merkbar; in letzterem waren die Muskelfibrillen noch wohl erhalten, während sie im Wasser und Chininpräparat zu Detritus zerfallen waren. Die Vibrionen waren im Eucalyptolpräparat weniger und nicht so lebhaft als in den beiden andern.

5.

Es wurde der Versuch in der Weise wiederholt, dass von der vorigen Lösung in drei Gläser à 60 Ccm. gegossen und in dieselbe wohl ausgewaschene Schinkenwürfel gelegt

wurden. Dieselben hatten nach acht Tagen in der Chinin- und Eucalyptollösung ihre scharfen Kanten bewahrt, während sie im Wasserpräparate zerfallen waren. Nach 20 Tagen zeigte die mikroskopische Untersuchung der Muskelfasern, dass dieselben im Wasser- und Chininpräparate zerfallen waren, im Eucalyptolpräparate hingegen nicht. Bezüglich der Infusorien wurde im Chinin- und Eucalyptolpräparate kein Unterschied wahrgenommen.

6.

Um den Einfluss des Eucalyptols auf die Fäulniss des Blutes zu beobachten, wurden zwei Präparate von frischem Kalbsblute hergestellt, deren eins mit Eucalyptol im Verhältnisse von 1 : 300 versetzt wurde und dieselben in wohlverkorkten Reagensröhrchen hingestellt. Nach 10 Tagen roch das Präparat mit Eucalyptol noch gar nicht, das andere stank intensiv. Die Form der Blutkörperchen fanden sich in beiden Präparaten noch erhalten.

7.

Um die Wirkung des Eucalyptols auf die Umsetzung des Tannin in Gallussäure zu ermitteln, wurde eine Tanninlösung von 1 in 20 Wasser hergestellt, dieselbe in zwei Fläschchen gebracht, deren eins einen Zusatz von 5 Tropfen Eucalyptol erhielt (so dass sich das Verhältniss des Eucalyptols zu der Lösung wie 1 : 200 verhielt), und beide wohl verkorkt in die Nähe des Ofens gestellt. Nach 14 Tagen enthielt die mit Eucalyptol versetzte Lösung nur einen körnigen Niederschlag, das andere zeigte dichte Pilzhaufen.

8.

Es wurde eine Lösung Weinsteinsäure (1 : 10) hergestellt und von dieser in zwei Fläschchen je 30 Grm. gebracht, deren eins einen Zusatz von $\frac{1}{10}$ dieses Gewichtes Eucalyptol

erhielt und wohlverschlossen hingestellt. Nach 4 Wochen war letzteres noch vollständig klar, ersteres fast nur ein einziger Pilzballen.

Es geht demnach aus diesen Versuchen hervor, dass das Eucalyptol ebenso fäulnisswidrig ist, wie Chinin, dass es die Schimmelbildung in Weinsteinsäure und Tanninlösung hemmt, trotzdem auf den Blättern selbst Pilzvegetationen zu üppiger Entwickelung gelangen können. Nach diesen Versuchen prüfte ich die Einwirkung des Eucalyptols auf das Zellenleben, wozu ich die Hefenzellen wählte, da deren Thätigkeit direkt durch die Umsetzung von Traubenzucker in Alcohol und Kohlensäure studirt werden kann.

9.

Es wurden in zwei Gläser je 5 Grm. Traubenzucker und $7^1/_2$ Grm. feuchter Presshefe gebracht. Das eine erhielt noch einen Zusatz von 5 Ccm. Eucalyptol. Beiden wurde dann 100 Ccm. Wasser zugegossen und im Wasserbade einer Temperatur von 40° C. ausgesetzt. Die gebildete Kohlensäure wurde in einem Eudiometer aufgefangen und durch Kali causticum absorbirt. Es zeigte sich, dass in dem Glase mit Eucalyptolzusatz keine Kohlensäure gebildet wurde, in dem andern 80 Ccm.

10.

Der vorige Versuch wurde wiederholt, wobei $^1/_2$% Eucalyptol zugesetzt ward. Auch hier hinderte der Eucalytolzusatz vollständig die Gährung.

11.

Zum Vergleiche mit Chinin wurde der Versuch erneuert, und zwar erhielten beiden Gläser einen Zusatz von Chinin resp. Eucalyptol im Verhältnisse von 1 : 1200. (Es wurde zu diesem Versuche die Emulsion benutzt, wie sie zu

den Fäulnissversuchen gebraucht worden.) Dazu wurden $7^1/_2$ Grm. Hefe und $7^1/_2$ Grm. Traubenzucker gesetzt, und auf 40° C. erwärmt. Das Eucalyptol-Eudiometer enthielt nach einer gewissen Zeit 124 Ccm. Kohlensäure, das mit Chinin 108 Ccm.

12.

In zwei Gläser wurden je 140 Grm. Wasser, $7^1/_2$ Grm. Hefe und 5 Grm. Traubenzucker gebracht. Das eine erhielt noch eine frischbereitete Emulsion von 0,88 Grm. Eucalyptol mit 1,0 Grm. Gummi und 40 Grm. Wasser, das zweite eine gleiche von 0,88 Chininum chinicum mit 1,0 Gummi und 40,0 Grm. Wasser. Dann wurden beide wie vorher erwärmt (40° C.). Nach $1^1/_2$ Stunde enthielt das Eucalyptoleudiometer 77 Ccm. Kohlensäure, das Chinineudiometer 105 Ccm. Da die Emulsion des vorigen Versuches circa 1 Monat bei Zimmertemperatur und in schlecht verschlossenen Gefässen gestanden hatte, wodurch jedenfalls der grösste Theil des Oels verdunstet war, so ist hierdurch allein das verschiedene Resultat zu erklären.

13.

Es wurde jetzt 0,91 Grm. Eucalyptol und ebenso viel Chininum chinicum genommen, diese mit 180 Grm. Wasser, 5 Grm. Traubenzucker und $7^1/_2$ Grm. Traubenzucker gleichmässig geschüttelt, in ein Wasserbad von 40° C. gebracht und die gebildete Kohlensäure aufgefangen. Nach 2 Stunden hatte die Eucalyptollösung keine, die Chininlösung 108 Ccm. Kohlensäure gebildet.

Es geht also aus diesen Versuchen deutlich die Thatsache hervor, dass Eucalyptol die Alcoholgährung viel energischer verhindert als die gleiche Quantität Chinin.

Die folgenden Versuche betrafen den Einfluss des Eucalyptol auf die Temperatur gesunder Thiere (Kaninchen). Dieselben wurden bei allen diesen thermometrischen Untersuchungen auf dem Schoosse gehalten und mit einem Tuche vollständig bedeckt. Hierdurch wird sowohl die Erregung der Thiere als ihre excessive Wärmeausstrahlung auf der Bauchseite vermindert.

14.

Kaninchen von 1650 Grm.

3Uhr 37,4°
3,15 37,3
3,30 37,2
3,45 37,2 Puls 130.
3,55 37,2
Um 4 Uhr erhält es 0,9 Eucalyptol subcutan.
5Uhr 36,6
5,5 36,5
5,15 36,5
5,20 36.4
5,30 36,6
5,45 36,8

Wir haben hier einen Temperaturabfall von 0,8°. Die Messung geschah mit dem Geislerschen Maximalthermometer in ano. Das Thier blieb während des ganzen Versuches munter; am anderen Tage war ebenfalls keine Veränderung mit ihm vorgegangen.

15.

Kaninchen von 730 Grm.

4,45 38,8
5Uhr 39,2
5,15 39,2
5,30 39,5
Um 5,35 erhielt es 0,9 Grm. Eucalyptol subcutan.
5,55 39,5
6.15 39,0
6,30 38.5
6,55 38,2

7 Uhr 38,6
Jetzt erhält es 0,45 Eucalyptol.
7,15 37,5
7,25 37,0
7,35 37,5.

Die Messung geschah wie in dem vorigen Versuche. Wir haben hier einen Abfall von 2° zu constatiren; dass die Temperatur bald darauf so plötzlich stieg, hatte seinen Grund in der Unruhe des Thiers bei der Einführung des Thermometers; jedoch zeigt auch die Curve nach der zweiten Injection von 0,45 des Mittels höchst deutlich seine temperaturerniedrigende Kraft.

16.

Kaninchen von 730 Grm.

3,10 39,0
3,20 39,0
3.30 39,2
3,40 39,2

3,50 erhält es 1,8 Grm. Eucalyptol subcutan, wovon jedoch ein Theil wieder aus der Stichöffnung herausfliesst.

4,30 38,5
4,40 38,2
4.50 38,2
5 Uhr 38,2
5,10 38,0
5,20 37.5
5,40 38,0
6 Uhr 38,5.

Auch hier haben wir einen Abfall von fast 2°. Das Thier blieb trotz der grossen Gaben Eucalyptol munter.

17.

Nachdem ich die relative Unschädlichkeit des Mittels aus diesen Versuchen erkannt hatte, beschloss ich den Ver-

such über die temperaturerniedrigende Kraft des Eucalyptols auch bei mir selbst anzustellen, wobei ich sogleich die Maximaldose von 100 Tropfen (= 3,5 Grm.) zu nehmen beschloss, die nach Gimbert eine »Unmöglichkeit jeder activen Bewegung, bei ungestörtem Bewusstsein« [1]) hervorbringen soll. Zu diesem Behufe wurde zuerst die Normaltemperatur bestimmt.

2 Uhr	37,2	
2,30	37,4	
3	37,5	Puls 80
3,30	37,5	
4	37,5	
4,30	37,6	
5	37,6	
5,30	37,8	
6	37,8	
6,30	37,8	
7	37,8	
7,30	37,8	
8 Uhr	37,8.	

Ich sass während der Messung angekleidet auf dem Sopha in einem c. 15° C. warmen Zimmer. Die Körpertemperatur wurde mittelst eines Geisler'schen Maximalthermometers in ano gemessen. Am folgenden Tage wurde die Messung ebenso und in derselben Kleidung vorgenommen.

1,45	20 Tropfen Eucalyptol		
2 Uhr	37,7		
2,30	37,7	Puls 96	
3	37,7	Puls 90	20 Tropfen E.
3,30	37,7	Puls 80	
4	37,7	Puls 80	20 Tropfen E.
4,30	37,7	Puls 80	
5	37,7	Puls 84	20 Tropfen E.
5,30	38,0		

1) Gimbert, L'Eucalyptus globulus S. 53 u. 76.

6	38,0	Puls 86	20 Tropfen Eucalyptol
6,30	37,7		
7	37,7	Puls 84	
7,30	37,7		
8	37,5		

Aus diesem Versuche kann man deutlich ersehen, 1) dass das Eucalyptol in dieser Dosis im Stande ist, nicht nur die Abendsteigerung der Normaltemperatur (hier 0,6°) zu unterdrücken, sondern sogar noch 0,2° unter die Mittagstemperatur zu bringen, also im Ganzen zu einem Abfall von 0,8°, 2) dass das Mittel in solcher Gabe auf die Pulszahl etwas vermindernd einwirkt.

Was das Allgemeinbefinden betrifft, so war die Verdauung, trotzdem ich die ersten 20 Tropfen direct nach der Mahlzeit nahm vortrefflich; das Abendbrot wurde mit dem gewohnten Appetite verzehrt. Gegen 6,30 trat eine Art Trunkenheit auf, die gegen 8 in eine Art geistiger Abspannung und Niedergeschlagenheit überging. Dies dauerte auch noch am folgenden Morgen, bei ungeschwächtem Appetite, fort. Die Diurese war wie gewöhnlich. Der $4^1/_2$ Stunden nach der ersten Einnahme gelassene Harn hatte einen deutlichen Eucalyptolgeruch, der beim folgenden sogar ohne Erwärmen hervortrat. Der Schweiss hatte einen deutlichen Trimethylamingeruch.

18.

Der zweite Versuch wurde 14 Tage später gemacht, damit nicht etwa Gewöhnung an das Mittel das Resultat trübe. Es wurde auch jetzt wieder zuerst die Normaltemperatur bestimmt.

2 Uhr	37,3
2,30	37,3
3	37,3
3,30	37,3
4	37,4
4,30	37,4

5	37,4	
5,30	37,4	
6	37,4	
6,30	37,4	
7	37,4	
7,30	37,4.	

Ich lag während des Versuches ausgekleidet zu Bette, mit einer leichten Steppdecke bedeckt. Die nur geringe Abendsteigerung (0,1°) rührte davon her, dass ich bei diesem Versuche zu Bette lag, im ersten Versuche aber sass und schrieb, welch letzteres ja eine grössere Muskelarbeit erfordert. Folgender Tag:

12,30	37,1	20 Tropfen E.
1 Uhr	37,2	20 Tropfen E.
1,30	37,2	
2	37,2	20 Tropfen E.
2,30	37,1	
3	37,1	20 Tropfen E.
3,30	37,1	
4	37,1	20 Tropfen E.
4,30	37,1	
5	37,1	20 Tropfen E.
5,30	37,0	
6 Uhr	37,0	
6,30	36,8	
7 Uhr	36,9	
7,30	36,9	

Wir haben also auch hier nach 120 Tropfen (4,2 Grm.) Eucalyptol einen Temperaturabfall von 0,5°, der in die Zeit der Abendsteigerung fällt. Die Apathie gegen körperliche und geistige Thätigkeit, sowie die Hautausdünstung nach Trimethylamin war auch hier vorhanden.

Der Temperaturabfall nach dem Eucalyptol wird noch dadurch interessant, als das Chinin beim gesunden Menschen weniger energisch einwirkt. Liebermeister sah nach Darreichung des Sulfats in der Dosis von 40 Gran in 7 Stunden bei einem 40jährigen Simulanten trotz aller Rauscherscheinungen

keinen erkennbaren Abfall (Arch. f. klin. Med. 1867. S. 599). Jürgensen gab 120 Gran in 32 Stunden, mit etwas Säure in Wasser gelöst, ohne dass eine Herabsetzung des Tagesmittels eintrat. (Die Körperwärme des gesunden Menschen. Leipzig 1873.) Schon 1863 hätte Wachsmuth in Dorpat Aehnliches beobachtet (Arch. d. Heilkunde S. 73). Der Analogie gemäss ist zu vermuthen, dass man mit gleichen Gaben Eucalyptol nun auch mehr ausrichtet in manchen Fieberzuständen, als in den nämlichen Fällen mit Chinin.

19.

Acht Tage nach dem vorigen Versuch wurde der folgende angestellt, einmal, um den Einfluss sehr grosser Quantitäten der Substanz auf den Organismus zu studiren, anderseits um seine Spaltungsproducte im Körper kennen zu lernen.

Hierbei befand ich mich wieder auf dem Sopha sitzend, bei einer mittlern Zimmertemperatur von 15° C. Als Vergleichungscurve der Normaltemperatur kann die des ersten Versuches angesehen werden.

2 Uhr	37,7	Puls 100	25 Tropfen Eucalyptol.
2,30	37,6	Puls 100	
3	37,4	Puls 100	35 Tropfen Eucalyptol.
3,30	37,3	Puls 96	30 Tropfen Eucalyptol.
4	37,2	Puls 96	30 „ „
4,30	37,1	Puls 96	30 „ „
5	37,1	Puls 88	
5,30	37,1	Puls 96	
6	37,2	Puls 96.	

Nimmt man hier die Normalcurve des ersten Versuches, so ergiebt sich ein Temperaturabfall von 1,0° C. Nach dieser grossen (5,0 Grm.) und in kurzer Zeit genommenen Quantität trat kein Uebelbefinden noch Störung in der Verdauung ein, dagegen wieder dieselben Erscheinungen von Apathie wie im ersten und zweiten Versuche. Der Harn

roch noch nach 40 Stunden deutlich nach Eucalyptol. Im Athem war der Geruch noch nach 14 Stunden deutlich zu erkennen.

20.

Die Quantität des nach Aufnahme von 4,2 Grm. Eucalyptol am selben Tage gelassenen Harns betrug gegen 800 Ccm. Derselbe roch deutlich nach Eucalyptol und zeigte besonders am Rande der Oberfläche einen deutlichen Stich in's Braune. Um zu prüfen, ob das Eucalyptol noch als solches in nennenswerther Quantität vorhanden sei, wurden 100 Ccm. überdestillirt, angesäuert und mit Aether geschüttelt. Es ergab sich kein Rückstand aus demselben. Das noch unzersetzt vorhandene, durch den Geruch sich andeutende Oel kann demnach nur minimal gewesen sein. Der Rest von 700 Ccm. Harn wurde ebenfalls mit verdünnter Schwefelsäure angesäuert, mit Aether in grosser Quantität geschüttelt und der abgegossene Aether durch Destillation verjagt. Bei jeder einzelnen Destillation, die in einem kleinen Kolben vorgenommen wurde, zeigte sich auf dem wässrigen Rückstande des Aethers eine gewisse Quantität eines braunen Oels, das bald an einem einzigen Punkte zu einem grossen Tropfen zusammengeflossen vorhanden war. Beim Abkühlen entstanden auf diesem Tropfen sehr feine, nadelförmige Krystalle (Hippursäure?). Die gesammelten Tropfen wurden mit Wasser übergossen, und dadurch als braunes Harz ausgefällt, dann das Ganze mit Natron neutralisirt und mit Salzsäure wieder angesäuert, dann nochmals mit Aether geschüttelt und dieser dann abdestillirt. Der ölige Rückstand zeigt, nachdem er einige Stunden in kaltem Wasser gestanden, feine Schüppchen. Er riecht aromatisch, schmeckt ebenso, löst sich leicht in Alcohol, ein wenig in Wasser, dem Alkalien zugesetzt sind, und in fetten Oelen, dagegen nicht in Glycerin. Es scheint derselbe Körper zu sein, den man künstlich durch Erhitzen von Eucalyptol mit

verdünnter Salpetersäure erhält. Die Lösung wurde von noch anhängendem aus den Gummistöpseln herrührenden Schwefel befreit und dann über Schwefelsäure getrocknet. Das Gewicht des so erhaltenen Harzes betrug c. 0,6 Grm.

21.

Es wurde darauf der Versuch gemacht, das Harz, beziehentlich die darin enthaltene Säure in krystallinischer Form zu erhalten. Zu diesem Zwecke wurde das Harz in Ammoniak gelöst, und der Ueberschuss des Ammoniaks vorsichtig verdampft. Das Filtrat mit einer Lösung von Silbernitrat versetzt, wieder filtrirt, der Rückstand mit Weingeist behandelt und letzterer der freiwilligen Verdunstung überlassen. Allein trotzdem die Silbersalze solcher aromatischen Harze sich sonst leicht bilden, so gelang es hier nicht, über die amorphe Form hinauszukommen. Sämmtlicher Vorrath von Filter, Lösung und Niederschläge wurde deshalb mit Schwefelsäure sauer gemacht, nochmals mit Aether geschüttelt und aus diesem dann im Wasserbade ein grosser Theil des Harzes zurückgewonnen.

22.

Es war mir interessant, ob dies Harz noch eine Wirkung auf den thierischen Organismus habe. Freilich musste ich mich, bei der vorhandenen kleinen Quantität auf den Frosch beschränken. Von zwei gleich grossen Fröschen erhielt M 0,03 Grm. des Harzes mit 0,1 Natron carbonic. in Wasser gelöst S nur 0,1 Natron carbon. in derselben Quantität Wasser.

	Frosch M.	Frosch S.
Vor der Injection	Herzschlag 50 pr. M. Injection	Herzschlag 60 pro Minute.
20 Min. später:	Herzschlag 40 pro Minute. Allgemeinbefinden ungestört.	Herzschlag 40 pro Minute. Allgemeinbefinden ungestört.

Frosch M.	Frosch S.
Abermalige Injection.	
von 0,03 Harz, 0,1 Soda.	von 0,1 Soda.
10 M. später Herzschlag 34, Frosch athmet dispnoëtisch, bleibt auf dem Rücken liegen.	Herzschlag 30 pro Minute, Allgemeinbefinden ungestört.
30 Min. später { Derselbe Zustand wie vorher.	Beginnt jetzt auch apathischer zu werden, lässt sich jedoch nicht auf den Rücken legen. Herzcontractionen 20 pro Min.
15 Min. später Herzcontractionen 23 pro Minute. Das Thier ist etwas munterer.	Status idem.

Etwa 2 Stunden nach der zweiten Injection sind beide Frösche wieder munter, M jedoch weniger als S.

Es geht hieraus hervor, dass dem Harze lange nicht die Einwirkung wie dem Eucalyptol zukommt, wenn auch Frosch M apathischer war wie S. Das Sinken der Zahl der Herzcontractionen ist wohl dem Alkali zuzuschreiben.

23.

Um deshalb das Resultat nicht zu trüben, wurde einem Frosche (E) 0,03 Grm. Eucalyptolharz in 1,0 Grm. Süssmandelöl gelöst subcutan injicirt, ein zweiter (O) erhielt blos dieselbe Quantität Süssmandelöl.

Etwa $^3/_4$ St. nach der Injection wurde E apathisch, blieb, auf den Rücken gelegt, ruhig liegen, die Athmung ist sehr verlangsamt. Dieser Zustand dauerte etwa $1^1/_2$ Stde., worauf er spurlos verschwand. O blieb intact.

Es scheint demnach das Harz, wenn auch mit geringerer Intensität, dieselbe Wirkung auf das Rückenmark, d. h. Lähmung der Reflexerregbarkeit, wie sie dem Oele zukommt, zu haben; freilich hat von letzterem dieselbe Quantität eine raschere und länger dauernde Einwirkung auf die Reflexcentren.

24.

Dass dem Eucalyptol die Eigenschaft zukomme, künstlich an Thieren hervorgerufene Septichämien zu sistiren, schien aus der Analogie mit Chinin sehr wahrscheinlich, wenn auch noch der experimentelle Beweis fehlte. Bei der hierauf gerichteten Untersuchung hatte ich mit demselben Missgeschicke zu kämpfen, das wohl Jeder, der ähnliche Versuche unternahm, erfahren haben wird, nämlich, keine Substanz zu besitzen, die ohne störende Nebenerscheinungen nur Fieber bewirkt, und dann, die richtige Quantität der zu injicirenden Substanz heraus zufinden.

Bald machte Macerationsjauche, trotzdem sie von Vibrionen wimmelte, kein Fieber, bald starben die betreffenden Thiere, denen sie eingespritzt worden, plötzlich unter Krämpfen, ohne jedwede Temperatursteigerung. Nach mehreren auf diese Weise verunglückten Versuchen beschloss ich endlich, den Eiter, welcher einem Senkungsabcesse entnommen mit der gleichen Quantität Glycerin versetzt wurde, zu benutzen. Es erhielten von diesen zwei Kaninchen dieselbe Quantität subcutan.

Kaninchen von 1890 Grm.			Kaninchen von 1950 Grm.		
$5^1/_2$ Uhr	40,2	0,9 Grm. Eiter.	5,40	39,2	1 Spritze Eiter (=0,9).
6	40,0		6,5	39,0	
6,40	40,0	20 Grm. Eiter, 1,8 Eucalyptol.	6,50	39,7	2 Grm. Eiter.
8,40	39,7		8,45	40,0	
		Folgender Tag.			
8,15	39,5	3 Grm. Eiter, 0,9 Eucalyptol.	8,20	39,0	3 Grm. Eiter.
10,35	39,0		10,30	39,2	
11,55	40,0	3 Grm. Eiter.	10,50	39,1	3 Grm. Eiter.
2,20	40,0	2 Grm. Eiter, 3,9 Eucalyptol.	2,15	39,1	2 Grm. Eiter.
3,20	40,4	0,9 Grm. Eucal.	3,15	40,0	

Kaninchen von 1870 Grm.			Kaninchen von 1900 Grm.	
4 Uhr	40,1	Thier ist munter wie das andere.	4,5	40,5
4,35	39,5		4,40	40,0
6,5	39,6	1 Spritze Eucalyptol.	6 Uhr	40,2
7,30	39,6		7.50	40,5
9 Uhr	39.6		8,55	40,4.

Wir sehen also hier, dass das Eucalyptol, trotz der enormen Quantität injicirten Eiters doch die Temperatur nicht steigen, ja dass es sie öfters unter die Anfangstemperatur sinken liess, während das schwerere Controlthier eine Steigerung bis 1,3° zeigt. Es wurden aus Versehen dem Eucalyptolkaninchen jetzt 2 Grm. Eiter in den Peritonealsack injicirt, wodurch es heftige Krämpfe und alle Zeichen der Peritonitis bekam. Es wurde ihm deshalb nochmals 1,3 Eucalyptol injicirt. Das Thier genas in 4 Tagen vollkommen.

25.

Um die antiseptische Wirkung des Eucalyptols auch in Bezug auf nicht organisirte Fermente zu prüfen, theils um den obigen Versuch zu vervollständigen, wurde zweien Kaninchen Eiterferment injicirt und dem einen nebenbei Eucalyptol injicirt. Das Eiterferment findet sich in frischem Eiter und wurde aus diesem in folgender, bekanntlich von v. Wittich zuerst angegebener Weise dargestellt: Der Eiter wurde noch warm mit ungefähr der gleichen Menge Glycerin übergossen und mit ihm tüchtig geschüttelt. Das Ganze blieb einige Tage stehen und wurde dann durch feine Leinwand filtrirt. Das Filtrat erhielt dann eine doppelte Quantität absoluten Alcohols, der ebenfalls mehrere Stunden darauf stehen blieb und dann abfiltrirt wurde. Der Rückstand, abermals in Glycerin gelöst, wurde nochmals mit Alcohol versetzt und das Präcipitat wieder vom Alcohol getrennt. Jenes, mit Wasser geschüttelt, gab eine Lösung, die hinreichend

fein war, um durch die Nadel der Pravaz'schen Spritze hindurchzugehen.

Das so erhaltene Ferment besitzt, wie Radziejewsky vermuthet, wirklich fiebererregende Eigenschaften. Nachstehende Ziffern thun dies auf's deutlichste dar.

Kaninchen von 2400 Grm.			Kaninchen von 2300 Grm.		
3,15	39,0		3,20	39,5	
		Folgender Tag.			
9,35	39,0	6,0 Grm. Eiterfermentlösung, 2 Grm. Eucalyptol.	9,40	39,3	6 Grm. Eiterfermentlösung.
2 Uhr	38,7		2,5	40,1	
3,45	40,9	2 Grm. Eucalyptol.	3,30	41,4	
5,35	40,9	1 Grm. Eucalyptol.	5,30	41,4	
6,45	40,4		6,50	41,4	
8,30	40,5		8,35	41,0	
		Folgender Tag.			
8.50	40,0		8,55	40,0	
		Die Thiere sind ganz munter.			

Vorstehender Versuch ergibt, dass das Ferment wirklich in starkem Maasse fiebererregend ist, wie Senator es bereits von Ptyalin erwähnt und zwar zeichnet sich dieses Fieber von den durch Jauche hervorgerufenenen dadurch aus, dass es im Ganzen sich viel reiner gestaltet. Es verläuft nicht unter den Symptomen der Betäubung und Prostration, wie jenes; dann bietet es zwar eine steile Curve, aber ein im Uebrigen gutes Allgemeinbefinden dar. Die Thiere befanden sich am andern Morgen ganz wohl und blieben es auch.

Das Eucalyptol, welches in diesem Falle angewandt wurde, vermochte nur ganz zu Anfange des Versuches, hier freilich in eclatanter Weise, die Curve zum Abfalle zu bringen. Später sehen wir nicht den geringsten Effect gegenüber der Wirkung des Eiterfermentes.

26.

Da wir in der Diastase ein ähnliches Ferment haben, das Stärkemehl in Zucker umsetzt, so versuchte ich, ob auch diesem die fiebererzeugende Kraft zukomme.

Kaninchen von 2300 Grm.			Kaninchen von 2400 Grm.		
10,10	39,5	0,5 Diastase in Wasser gelöst subcutan 3,0 Eucalyptol.	10,15	39,0	0,5 Diastase in Wasser gelöst subcutan.
4,30	39,5		2,45	38,5	
4,40	39,5		4,35	39,0	
6 Uhr	40,7		5,55	39,0	
8,40	41,0		8,35	40,8.	
		Andern Morgen.			
9 Uhr	40,0		8,55	39,5.	
		Beide Thiere anscheinend gesund.			

Wie aus dem vorigen Versuche, so ergibt sich auch aus diesem, dass das Eucalyptol auf das Fieber, durch jene nicht organisirte Fermente erzeugt, von einigem Einfluss ist. Bei dem Eucalyptolthiere stieg die Temperatur nicht so hoch wie bei dem andern (Differenz 0,3°). Vielleicht spricht auch zu Gunsten dieser Ansicht der Umstand, dass das Kaninchen, welches kein Eucalyptol erhalten, 4 Tage später verstarb, während das andere munter blieb. Die Section ergab weit um die Einstichöffnung verbreitete Abscesse, die innern Organe waren normal. Dass aber das Fieber nicht von den Abscessen her entstand, geht klar daraus hervor, dass es rascher erfolgte, als Abscesse je entstehen können und dass es nicht mehr vorhanden war, als letztere entstanden waren. Beide Thiere hatten Abscesse von derselben Ausdehnung, so dass mit Wahrscheinlichkeit, weil kein anderer Grund ersichtlich war und dieser ausreicht, das Ueberleben des einen Thieres dem Eucalyptol zuzuschreiben ist.

27.

Um die Versuche mit Fermenten weiter auszudehnen, benutzte ich das Leberferment, das auf folgende Weise dargestellt wurde. Ein Kaninchen wurde vermittelst Durchschneidung der grossen Halsgefässe getödtet. Die blutleere Leber, unmittelbar dem Leibe des Thieres entnommen, wurde sodann in Glycerin untergetaucht, hier mit einer Scheere zerschnitten und dann mit Seesand zu Brei zerrieben. Das Ganze, nachdem es 24 Stunden in Glycerin gestanden, wurde mit Alkohol gefällt. Die so erhaltene Substanz reagirte schwach alkalisch.

E Kaninchen 1700 Grm. schwer.			N Kaninchen 1800 Grm. schwer.	
5,45	39,5		6 Uhr	39,5.
		Folgender Tag.		
3,20	39,5		3,25	39,5.

Es wurde jetzt 1 Grm. des noch nicht ganz trockenen Leberferments mit 12,0 Wasser verrieben und davon jedem der beiden Kaninchen die Hälfte unter die Rückenhaut injicirt. E erhielt darauf 3 Grm. Eucalyptol.

6,15	38,9	6,20	40,0
8 Uhr	39,0	8,5	40,3
9 Uhr	38,8	9,5	40,5.
		Folgender Tag.	
8,25	40,0	8,30	40,5.

Resultat: Beide Thiere zeigten, wie eine zweimalige genaue Messung ergab die Normaltemperatur 39,5, Injection des Leberferments ergab bei N binnen 6 Stunden eine Temperatursteigerung von 1° C. bei E nicht nur keine Steigerung, sondern sogar einen Abfall um fast 1°, der sich am andern Morgen fast, aber nicht ganz ausgeglichen im Vergleich mit N. Vielleicht ist noch in Betracht zu ziehen zu Gunsten meines Experimentes, dass das mit Eucalyptol behandelte Thier eines jener zarten, wenig widerstandsfähigen war, welche leichter auf solche Eingriffe reagiren. Der stattgefundene Abfall ist also noch mehr beweisend, als er es ohne diese Disposition des Thieres sein würde. Das Leberferment wirkte ganz ähnlich wie die Diastase und der Eiterextract fiebererzeugend, aber nicht betäubend.

28.

Um den Einwurf auszuschliessen, als ob die Injectionsflüssigkeit als solche, d. h. als fremder Körper das Fieber bewirkt habe, wurde der Versuch genau so wie vorher wiederholt, allerdings mit dem wesentlichen Unterschiede, dass das Leberferment vorher einige Zeit der Siedehitze ausgesetzt worden war.

Kaninchen von 2090 Grm.

10.30	39,0	
12,30	39.0	Injection von 0,5 Grm. 1 Stunde lang gekochten und dann mit 12 Grm. Wasser feingeriebenen Leberferments.
4 Uhr	39,0	
5,15	39,0	
7,30	38,8.	

Dieser Versuch zeigt also deutlich, dass die fiebererzeugende Kraft des Leberextractes durch das Vorhandensein eines Fermentes bewirkt wird, da die Temperatursteigerung ausbleibt, wenn es durch Einwirkung von Siedehitze zerstört wird. Das Ferment war unverändert dasselbe. Es hatte bisher über Schwefelsäure gestanden und bot genau das nämliche Aussehen und die nämliche Löslichkeit dar. Dieser Versuch stimmt übrigens mit Allem überein, was man auch von andern Fermenten (Ptyalin, Pepsin etc.) weiss.

29.

Zur weitern Verfolgung dieses für die Natur jener fiebererzeugenden Substanzen wichtigen Versuches wiederholte ich ihn mit Diastase: Wir hatten vorher gesehen, dass 0,5 derselben ungekocht ein bedeutendes Fieber hervorruft.

Kaninchen von 1450 Grm.

10,25	38,8	
12,45	38,8	0,36 Grm. 1 Stunde lang gekochter Diastase.
4.15	39.2	

6 Uhr	39,0
8 Uhr	39,4

Am anderen Morgen.

8,30	38,9.

Die Diastase war von dem nämlichen Vorrathe, wie die vorige; das Thier erhielt (da es nur 1450 Grm. wog, gegenüber 2400 Grm. von Versuch 26) noch mehr Diastase als das erstere. Dennoch trat kein Fieber ein, denn die besonders bei Kaninchen geringe Steigerung von 0,6° C. zur Zeit der normalen Tagessteigerung kann wohl kaum als solches bezeichnet werden. Jedenfalls ist der Unterschied zwischen diesem Thier und dem mit ungekochter Diastase behandelten sehr auffallend. Er wiegt noch schwerer durch den Umstand, dass ich zuletzt mit einem weissen Kaninchen operirte (in Versuch 26 mit einem gelben) und jene bekanntlich auf alle Eingriffe empfindlicher reagiren. Zur bessern Controlle wurde nun die Temperatur noch am folgenden Tage gemessen, um die gewöhnliche Abendsteigerung zu bestimmen.

Morgens	8,30	38,9
Abends	6.30	39,3
	8 Uhr	39.4
	9 Uhr	39,4.

Wir sehen also hier fast genau dieselbe Differenz wie gestern, was der Auffassung zur Stütze dient, dass gekochte Diastase nicht fiebererregend wirkt.

30.

Es schien nicht undenkbar, dass das Leberferment, welches im Thierkörper ungekocht eine so deutlich wärmeerhöhende Wirkung darbot, dasselbe auch unter einfachern Verhältnissen zeigen würde. Man muss sich die Sache doch wohl so denken, dass im Organismus durch den Einfluss des Fermentes eine raschere Spaltung hochmolecularer Körper vor sich geht und hierdurch mehr Wärme frei wird.

Von andern Gährungen, z. B. von der des Traubenzuckers, ist die Temperatur der Gährungsflüssigkeit durch den Einfluss der Hefe genügend bekannt. Ich löste deshalb einige Gramme des Leberferments in Wasser und theilte die Lösung in 2 Hälften. Die eine wurde der Siedehitze ausgesetzt und nach dem Erkalten zu 80 Ccm. defibrinirten Kalbsblut gebracht, die andere ungekocht einer gleichen Menge des Blutes zugesetzt. Beide Kolben, mit sehr feinen Thermometern versehen, wurden darauf in ein Wasserbad von 40° C. gestellt.

Am 1., 2. und 3. Tage, wobei die Kolben zwar stets in Wasser, aber nur von Zimmertemperatur gestanden, war die Temperatur beiderseits gleich. Am vierten Tage zeigte sich eine Steigerung bei dem ungekochten Fermente um 0,2° C. (20,0 und 20,2) die am folgenden Tage anhielt.

31.

Derselbe Versuch wurde mit Diastase wiederholt. Jedes der mit den feinen Thermometern versehenen und mit doppeltdurchbohrten Gummistöpseln verschlossenen Glasgefässe erhielt 85 Ccm. frisches defibrinirtes Kalbsblut. Einem derselben wurde dann ein Grm. Diastase mit etwas Blut im Mörser zerrieben zugesetzt; die nämliche Quantität Blut der Controlle wurde für sich im Mörser ebenso zerrieben. Beide Kolben wurden dann von Zeit zu Zeit im Wasserbade auf 40° C. erwärmt und dann der Zimmertemperatur ausgesetzt. Am 2ten Tage war die Temperatur im Diastasekolben 0,1° höher als im andern; doch hielt dieser Unterschied nur einen Tag an. Beide Präparate hatten ferner, abgesehn vom Bodensatze der Diastase, ein verschiedenes Aussehen. Die Controlle war weniger dunkel als das Diastasenpräparat und hatte eine compactere Oberfläche, während dieses viele Glasbläschen zeigte, welche die gleichartig aussehende Oberfläche durchsetzt hatten.

Die beiden Versuche zeigen also, dass diese Fermente

im Blute wirklich einen Spaltungs-Process bewirken, durch den Wärme frei wird. Ich will nun keineswegs behaupten, dass auch im lebenden Thier die genannten Fermente ebenfalls vom Blut aus wirken, dass sie also gerade in dieser Flüssigkeit denjenigen Stoffumsatz erregen, den wir in den Thierversuchen kennen lernten. Im Gegentheil, es sprechen mehrere Gründe dafür, dass auch die erhöhte chemische Thätigkeit des Organismus, die wir als einen Theil des Fiebers bezeichnen, in den compacten Geweben vor sich geht. Aber ich wählte gerade das Blut zu dem letzten Versuch, weil es einestheils leichter wie die Flüssigkeit jedes anderen Gewebes zu beschaffen ist und weil andererseits seine Zusammensetzung ihrem Wesen nach nicht abweicht von der des übrigen Körpers.

32.

Die Parallele des Eucalyptol mit dem Chinin liess sich noch dadurch weiter führen, dass ich die Frage erörterte, ob nicht ersteres auf die Erregung des activen Sauerstoffs durch Protoplasma in gleicher Weise hemmend einwirke, wie es durch die ausgedehnten Untersuchungen von Binz für das Chinin erwiesen ist.

Bei den früheren Untersuchungen über diesen Gegenstand war besonders das Guayakharz und der Indigo als Reagens benutzt worden; bei meinem Versuche wandte ich der weiteren Ausdehnung und Abwechslung halber das angesäuerte Jodkalium und den Stärkekleister an, ein Verfahren, das bekanntlich von Schönbein herrührt. Die chemische Formel, wonach der Vorgang verläuft, ist folgende:

$$2KJ + H_2SO_4 = K_2SO_4 + 2HJ$$
$$2HJ + O_1 = H_2O + 2J$$

d. h. also folgendermassen:

Säuert man eine verdünnte Lösung von Jodkalium und Stärkekleister an, so entwickelt sich in derselben Jodwasserstoff, und dieser wird zersetzt, sobald ein Körper hinzutritt,

der activen Sauerstoff d. h. leicht abzugebendes O_i an sich trägt. Ein solcher Körper kann bekanntlich alles Mögliche sein: Ozon, Wasserstoffsuperoxyd, Bleisuperoxyd, Untersalpetersäure u. s. w. Auch mit Wasser zerriebene indifferente Pflanzentheile thun das Nämliche, und hier ist es das Protoplasma der Pflanzenzellen, welches in Berührung mit der Luft den activen Sauerstoff erzeugt hat [1]).

Die Anordnung des Versuchs, um den Einfluss des Eucalyptols zu prüfen, war folgende:

Ich zerrieb ein kleines Stückchen Schaale von Solanum tuberosum mit Wasser, füllte davon etwa 5 Ccm. in eine Probirröhre und gleiche Menge in eine zweite. Zu dem einen brachte ich 5—15 Tropfen Eucalyptol, zu dem anderen ebenso viel Süssmandelöl. (Die Controlle war deshalb nöthig, um dem Einwand zu begegnen, dass durch das oben schwimmende Eucalyptol der Eintritt der atmosphärischen Luft abgesperrt worden sei, denn im zweiten that das Süssmandelöl genau das Nämliche.) Beide Gläschen wurden nun einige Mal leicht geschüttelt und dann ihr Inhalt mit etwa 15—20 Ccm. des angesäuerten Jodkaliumstärkekleisters versetzt.

Das constante Resultat des Versuches war, dass im Controllpräparate sofort die blaue Färbung der Jodstärke auftrat, dass sie beim Eucalyptol ganz ausblieb oder deutlich geringer war [2]). Ich kann die Sache, wenn ich Alles wohl erwäge, nicht anders erklären, als dass das Eucalyptol in der nämlichen Weise wie das Chinin das Protoplasma hinderte, den Sauerstoff der Luft aufzunehmen, und in seine

1) Vergl. Binz in Virchow's Archiv Bd. 46. S. 145 ff.

2) Hiermit ist also das zu berichtigen, was sich in der Mittheilung von Prof. Binz (Sitzung der Niederrhein. Ges. vom 17. März 1873) bezüglich der negativen Resultate angegeben findet. An ihnen war wohl nur die Verwendung eines bereits mit O_3 beladenen Oeles Schuld (vgl. S. 12).

zwei Atome zu zerlegen, eine pharmakodynamische Auffassung, welche für die Alkoloide durch die schönen Versuche von Rossbach in vollem Umfange bestätigt worden ist [3]).

Man könnte diesen Versuchen noch den Einwand entgegenstellen, dass die Bläuung in dem Eucalyptolpräparate nur deshalb geringer war, weil in der Controlle der Zutritt des Süssmandelöls eine Stärkung der Reaction hervorrufe. Es ist ja möglich, dass das fette Oel gewisse freigewordene Verbindungen enthält, die sich leicht oxydiren und zerfallen und dadurch den Anstoss zu einem stärkeren Austausche des Sauerstoffes geben, welche denn auch die rasche Bildung der Jodstärke erklären würde. Es lässt sich jedoch durch einen weiteren Controllversuch darthun, dass die Anwendung des fetten Oeles den ganzen Vorgang nicht nur nicht beschleunigt, sondern sogar verlangsamt. Jener Einwand trifft also in keiner Weise zu.

Ich habe auch die beiden anderen bekannten Reagentien auf O_i in Verbindung mit Eucalyptol geprüft. Sie eignen sich jedoch weniger gut als der Jodwasserstoff. Während nämlich bei diesem die Hemmung des dem Protoplasma entstammenden activen Sauerstoffs jedesmal eintritt, gleichviel, ob das Eucalyptol frisch destillirt worden ist oder schon lange an der Luft gestanden hat, gelingt der Versuch beim Indigo und Guayakharz nur mit ganz frischem Oele. Bedient man sich eines Eucalyptols, das lange gestanden hat, so erhält man sogar eine entgegengesetzte Wirkung. Der Grund davon ist ganz einfach der, dass ähnlich wie beim Terpenthinöl eine Bildung von Ozon resp. Wasserstoffsuperoxyd stattfindet und der darin enthaltene active Sauerstoff störend einwirkt.

Dass diese störende Einwirkung bei der Probe mit Jodkaliumstärkekleister und Säure nicht zur Geltung gelangt, sondern die oxydirende Kraft des Protoplasmas dennoch

3) Würzburger Verhandlungen 1872. II. u. III. Bd.

niedergehalten wird, spricht natürlich nur um so mehr für die Thatsache, welche in Bezug auf den in Rede stehenden Gegenstand das Eucalyptol dem Chinin ebenfalls wieder gleichstellt.

Wollte ich beim Eucalyptol alle diejenigen Punkte durchführen, welche jetzt wohl allgemein für die neuere Chinintheorie als wesentlich gelten, so bliebe mir vor Allem noch die Erörterung der Frage übrig, wie unser ätherisches Oel sich zu den weissen Blutkörperchen und damit zu der Eiterbildung verhält. Ebenso hätte ich auf die Beziehungen zum Hämoglobin, ferner auf die Beeinflussung von Blutdruck, Puls und Respiration, auf die einzelnen Abschnitte des Nervensystems einzugehen. Ueber alle diese Dinge liegen ausführliche Untersuchungen betreff des Chinins und deren genaue Bestätigungen vor. Ich muss es mir leider aus Mangel an Zeit jetzt versagen, das Eucalyptol in den gegebenen Bahnen weiter zu führen, hoffe aber, dies später noch nachholen zu können. Einstweilen reihe ich hier an, was ich klinisch von dem Mittel gesehen oder auch nicht gesehen habe. Hier einige

Krankheitsgeschichten.

Der erste Fall betraf einen Patienten mit Pyosepticaemie, die sich nach Amputation des Unterschenkels wegen Caries des Fussgelenkes entwickelt hatte, von der ich hier die Temperaturen im Mastdarm gemessen, ohne und mit Eucalyptol, nach den mir von Dr. Mosengeil gütigst überlassenen Daten mittheile.

2. Tag. 8 Uhr Morgens 40,5 5 Tropf. Eucalyptol.
5 Uhr Mittags 39,2 5 Tropf. Eucalyptol, starke Nachblutung.
3. Tag. 6 Uhr Abends 40,3
4. Tag. 8 Uhr Morgens 38,0 } ohne Eucalyptol,
6 » Abends 39,3 }

5. Tag. 4 Uhr Mittags 38,8 11 Tropf. Eucalyptol,
5 » » 38,5
7 » » 37,2
9 « » 36,8.
6. Tag. 8 Uhr Morgens 39,8 5 Tropf. Eucalyptol,
10¾ » » 40,9 11 Tropf. Eucalyptol,
3 » Mittags 37,9
6 » Abends 36,8
7 » » 36,5
8 » » 36.6.

Das Mittel wurde jetzt ausgesetzt, weil man durch dasselbe eine neue Blutung befürchtete. Wir haben hier einen deutlichen Abfall, einmal von 2°, das andere Mal von 4,2°. Zwar kommen bei Septichämie grosse Temperaturabfälle spontan vor, doch sagt Wunderlich in seiner Schrift „Verhalten der Eigenwärme in Krankheiten: — „nach dem Accesse erreicht der Rückgang in der Pyämie nur selten die Norm, wendet sich vielmehr, ehe er 37,5 überschritt, öfters früher, zu erneutem Steigen.“ Auch war dieser Abfall nicht durch den nahen Exitus lethalis bedingt, da die Patientin erst 8 Tage später verstarb. Ich glaube also, wir haben logisch alles Recht, wenigstens einen der hier gesehenen Fieberabfälle auf den Einfluss des Eucalyptols zu beziehen, umsomehr, als wir ja wissen, dass es stark antiseptisch wirkt und im Körper nicht ganz zerstört wird, und ferner dass beim Gesunden der Temperaturabfall über jeden Zweifel feststeht.

Der 2te Fall, mir ebenfalls von Dr. Mosengeil gütigst übermittelt, betraf eine Patientin mit einseitiger purulenter Parotitis mit einer Temperatur von 40°. Sie erhielt dann 11 Tropfen Eucalyptol. Am folgenden Tage war die Temperatur auf 37,5 gesunken und stieg auch nicht höher, als an diesem Tage auch die andere Parotis von der eitrigen Entzündung befallen wurde. Hier lagen ebenfalls alle Verhältnisse so, dass die Beobachtenden den Eindruck

bekamen, der Fieberabfall rühre nur von dem ätherischen Oele her.

Nach mündlicher Mittheilung des Hrn. Dr. v. Mosengeil bewährte sich das Eucalyptol als Antipyreticum noch in 2 weitern Fällen von Septicämie. Leider wurden hierbei keine Aufzeichnungen der Temperaturcurven gemacht. Bemerkenswerth ist noch, dass in diesen beiden Fällen ganz unerwartete Nachblutungen eintraten, die der behandelnde Chirurg dem Eucalyptol zuschrieb. Es wurde deshalb ausgesetzt. Auch Gimbert weist in seiner Schrift auf diese Einwirkung hin und warnt besonders davor, das Eucalyptol in acut verlaufender Lungenphthise zu geben.

Durch gütige Mittheilung seitens Hrn. Dr. Zuntz erfahre ich, dass auch in einem Falle von acutem Gelenkrheumatismus das Eucalyptol ihm einen Abfall des Fiebers darbot, welcher mit höchster Wahrscheinlichkeit nur auf das angewandte Medicament zurückzuführen war. Erwägt man, dass dem Chinin erfahrungsgemäss in dieser Krankheit nur eine sehr geringe antipyretische Kraft zukommt, so fordert jene eine Beobachtung, die von competenter Seite herrührt, wohl zu weitern Versuchen auf.

Der letzte Fall wurde in der hiesigen medicinischen Klinik beobachtet. Es war folgender:

Heinrich Friedrich, Schlosser, 28 Jahr alt;

Patient war bis zu seinem 26. Jahre vollständig gesund. Im April 1870 war er an den Pocken mehrere Wochen erkrankt. Darauf blieb er wieder gesund bis zum August 1872. In Bremerhafen auf einem Dampfschiffe beschäftigt, wurde er am 14. August 1872 von einem fieberhaften Allgemeinleiden befallen, welches von seinem Arzte ihm als Febris intermittens bezeichnet wurde. Zunächst wurde er 10 Tage lang in Bremerhafen behandelt, darauf reiste er nach Barmen, wo er sich sofort wieder in ärztliche Pflege begeben musste und 14 Tage lang hier und dann weitere 21 in Osnabrück verpflegt wurde. Von jetzt an blieb er einige Zeit von den Fieberanfällen verschont, war jedoch sehr schwach und arbeitsunfähig. Aufs Neue erkrankte er in Köln im Oktober 1872, wurde jedoch sehr bald wieder hergestellt.

Im Januar 1873 erkrankte er hier in Bonn abermals. Nachdem er, nach seiner Aussage, bereits zwei Fieberanfälle überstanden hatte, liess er sich in die Klinik aufnehmen. Nach der Angabe des Pat. sollen stets zwischen je zwei Fieberanfällen zwei fieberfreie Tage liegen.

Pat. ist bleich und anämisch, Muskulatur und Fettpolster sind verhältnissmässig schwach entwickelt; im Ganzen ist die Constitution des Patienten für sein Alter und Beschäftigung schwach. Die Milz ist deutlich am Rippenrande fühlbar und steigt mit der Athmung herab. Dieselbe misst $10^1/_2$ Cm. in der Länge und ragt um circa $2^1/_2$—3 Cm. über den Rippenrand. Die Leberdämpfung beträgt rechts 10 Cm., links 4 Cm. Am Herzen und den Lungen sind keine pathologischen Veränderungen zu constatiren. Der Appetit des Pat. ist schlecht. Bei der Aufnahme (einem fieberfreien Tage) war keine Temperaturerhöhung zu constatiren. Das mikroskopisch untersuchte Blut zeigt einen auffallenden Mehrgehalt an weissen Blutzellen, als dies normaliter der Fall ist. Pigmentmassen sind im Blute nicht zu constatiren gewesen.

Unter den obwaltenden Umständen musste die Diagnose in dubio bleiben, bis die demnächst zu erwartenden Fieberanfälle eingetreten waren. Für das Vorhandensein einer Erkrankung an Intermittens sprachen, abgesehen von der Erzählung des Patienten, der Milztumor, die Blutbeschaffenheit und das Allgemeinbefinden. Die folgende Beobachtung des Patienten bestätigte diese Diagnose vollständig und zwar war es eine F. i. quartana.

Prognose: Dieselbe könnte für diese Reihe von Fieberanfallen gemäss der uns zu Gebote stehenden Therapie als fausta bezeichnet werden. Was allerdings eine gänzliche Wiederherstellung des Pat. betrifft, so können wir bei dem Ernährungszustand und der jetzt schon zum dritten Male wiedergekehrten Krankheit des Patienten eine weniger günstige Prognose stellen.

Da Patient seinen letzten (zweiten) Anfall am 13. Jan. gehabt hatte, so mussten wir den nächstfolgenden am 16. Jan. erwarten.

Es wurde anfangs beabsichtigt, die Wirkung des Ol. Eucalypt. globuli auf das Fieber zu beobachten, welches in neuerer Zeit von verschiedenen Seiten eindringlichst gegen vorliegende Krankheit empfohlen worden war. Pat. erhielt daher am 14. Jan. 3× je 10 gtt. Ol. Eucalypt. glob., am 15. Jan. wurde dasselbe jedoch, nachdem Pat. Morgens 1× 15 gtt. zu sich genommen hatte, wieder ausgesetzt, da es

uns zunächst darum zu thun war, den Ablauf eines nicht durch medikamentöse Eingriffe alterirten Fieberanfalles zu beobachten.

Während am 15. Jan. keine besonderen Symptome auftraten, zeigten sich am 16. Jan., dem Tage des zu erwartenden dritten Anfalles, die für eine Febris intermittens charakteristischen Symptome. Schon Morgens zeigten sich Störungen im Allgemeinbefinden wie Appetitmangel, Mattigkeit u. s. w. Circa um 2 Uhr Nachmittags begann der Anfall und zwar mit Gefühllosigkeit und Taubheit in den Fingern und Zehen, schmerzhaftem Recken und Strecken der Extremitäten und des Rückens, häufigem Gähnen, Schmerz und Benommenheit des Kopfes. Gleichzeitig hatte Pat. starkes Frostgefühl, so dass er zitterte und mit den Zähnen klapperte. Dieses wich gegen 4 Uhr einem Gefühl von Hitze; gleichzeitig trat reichlicher Schweiss auf. Bis gegen 8 Uhr liessen sämmtliche Symptome nach und Pat. stand sehr geschwächt und abgemattet aus dem Bette auf. Im Ganzen hatte der Fieberanfall also circa $5^1/_2$ Stunden gedauert. Die Temperaturen während dieser Zeit waren folgende:

2 Uhr = 37,8	6 Uhr = 39,6
3 Uhr = 39,0	8 Uhr = 38,6
4 Uhr = 40,4	10 Uhr = 38,0.

Der Schlaf in der folgenden Nacht war unruhig und gestört durch die Kopf- und Gliederschmerzen.

Am 17. und 18. Jan. war das Befinden des Pat. leidlich gut. Es wurde schon vom 17. Jan. an mit der Behandlung mit Ol. Eucalypti globuli begonnen und erhielt er davon 3 × tägl. 10 gtt.

Am 19. Jan. wurde der vierte Anfall (der erste im 2. Cyklus) erwartet. Es zeigten sich am Nachmittag dieselben Symptome, wie ich sie bereits oben geschildet habe. Der eigentliche Anfall begann um 3 Uhr Nachmittags. Im Ganzen soll derselbe weniger intensiv als die früheren gewesen sein, namentlich waren die Kopf- und Gliederschmerzen geringer. Die Temperaturen an diesem Tage waren folgende:

3 Uhr = 37,6	7 Uhr = 39,6
4 Uhr = 38,4	10 Uhr = 37,6.
5 Uhr = 40,2	

In der folgenden Nacht schlief Pat. verhältnissmässig besser wie sonst in den Nächten nach den Anfällen.

Am 20. Jan. war sein Befinden relativ gut, ebenso am 21. An diesen beiden Tagen nahm Pat. keine Arznei. Am 22., dem

Tage des fünften Anfalles, nahm Pat. Morgens 3 × je 15 gtt. Ol. Eucalypti. Nachmittags traten jedoch das Fieber und sämmtliche anderen Symptome mit derselben Heftigkeit und Stärke auf, wie dies bei den allerersten Anfällen der Fall war, wo gar keine therapeutischen Eingriffe gemacht worden waren.

Die Temperaturen waren folgende:

3 Uhr = 38,0	6 Uhr = 39,4
4 Uhr = 40,0	10 Uhr = 37,2.

Der Schlaf Nachts war schlecht und auch am folgenden Tage ist Pat. noch sehr entkräftet und ohne Appetit.

Da das Ol. Eucalypt. in diesem Falle seine Wirkung zu versagen schien, so wurde, um die Kräfte des ohnehin nicht allzu starken Pat. nicht noch mehr sinken zu lassen, zu der Chininbehandlung übergegangen und zwar wurde in folgender Weise verfahren. Der am 25. zu erwartende Anfall war der 6., also der 3. im 2. Cyklus. Obwohl wir nun wussten, dass die jetzt eingeleitete Chininbehandlung für den kommenden 6. Anfall wirkungslos sein würde, so wurde ihm doch das Medikament gereicht, da bekannter Weise Chinin, zu dieser Zeit gegeben, schon für den nächstfolgenden Cyklus von Anfällen wirkt. Pat. bekam am 24. Jan. Ab. 0,5 Chin. sulph. und am 25. Morgens 2 × je 0,5 Chin. sulph.

Wie wir erwartet hatten, trat auch am 25. noch ein Anfall ein und zwar ein ziemlich heftiger; im Wesentlichen waren die auftretenden Symptome dieselben wie die bei den früheren. Die Temperaturen waren folgende:

3 Uhr = 37,4	7 Uhr = 39,1
4 Uhr = 38,7	10 Uhr = 37,4.
5 Uhr = 40,2	

An den beiden folgenden Tagen, am 26. und 27. Jan., fühlte sich Pat. mässig wohl.

Der 7. Anfall, der 1. im 3. Cyklus sollte am 28. Jan. auftreten; derselbe blieb aber, ohne dass ein weiterer therapeutischer Eingriff seit dem 25. Jan. erfolgt wäre, aus. Ganz frei von abnormen Sensationen war Pat. nun nicht, indem sich etwas Mattigkeit einstellte. Von Kälte, Fieber und Schweiss war jedoch Pat. ganz frei und hatte normale Temperaturen. Nachts schlief Pat. gut und befand sich an den folgenden zwei Tagen wohl. Am 31. blieb auch der 8. Anfall aus, jedoch befielen den Patienten Gähnen, Gliederstrecken, Mattigkeit und etwas Kopfschmerz, während Taubheit in den Gliedern,

Kältegefühl und Temperaturerhöhung nicht auftraten. Fast genau ebenso verlief der Tag des 9. Anfalles. In der Zwischenzeit befand sich Pat. wohl und wurde keine weitere Therapie in Anwendung gebracht. Um nun den 10. Anfall (den 1. des 4. Cyklus) nicht zum Ausbruch kommen zu lassen, nahm Pat. Abends vorher 0,5 und Morgens ebenfalls 0,5 Chin. sulph. In Folge dessen zeigte sich keine Andeutung eines Anfalles, indem dieser Tag für den Pat. ganz ebenso verlief wie jeder andere. Am 9. Februar nahm Pat. nochmals zwei Dosen von 0,5 Chin. sulph. Fieberanfälle oder sonstige Symptome traten nicht mehr auf seit dem 6. Anfalle am 25. Jan.

Bei der am 8. Februar vorgenommenen Untersuchung war noch keine Abnahme des Milztumors zu constatiren. Hierauf sowie auf die Aufbesserung des Kräfte- und Ernährungszustandes wurde von jetzt an Hauptrücksicht genommen. Zu dem Zweck bekam Pat. bis zu seiner Entlassung am 26. Februar 3 × tägl. 1 Theelöffel folgender Arzenei:

℞ Syr. Ferr. jod. 15,0
Aqu. dest.
Syr. spl. aa 30,0
M. D. S.

In der Folge besserte sich das Allgemeinbefinden des Pat. in recht befriedigender Weise. Appetit und Kräftezustand hoben sich augenscheinlich. Die Milz nahm an Grösse ab; dieselbe mass am 20. Februar in der Länge 8 Cm., nur beim tiefen Inspiriren war die Spitze derselben unter dem Rippenbogen noch fühlbar, nicht mehr beim Exspiriren. Am 26. Febr. bei seiner Entlassung wurde die Grösse von $7^1/_2$ Cm. constatirt.

Das Ausbleiben der Wirkung des Eucalyptols in diesem Falle beweist aber nichts. Wie Versuche an mir selbst und in der Poliklinik ergaben, sind dreimal 15 Tropfen im Laufe eines halben Tages eine sehr kleine Gabe, die gegenüber der anerkannten Einwurzelung des vorliegenden Uebels noch kleiner wird. Es ist sicher, dass die Dosirung unseres ätherischen Oels höher zu greifen ist als die des Chinin, denn von letzterem hätte ich wohl schwerlich 5 Grm. auf einmal ohne Beschwerden ertragen können. Und in Fieberzuständen werden noch dazu alle Antipyretica, wie das Chinin und der Alcohol, viel besser ertragen, als im fieberfreien Körper; demgemäss wohl auch das Eucalyptol.

In der hiesigen Poliklinik habe ich in Verbindung mit einigen Commilitonen eine ziemlich grosse Reihe solcher Fälle mit Eucalyptol behandelt, die auch sonst symptomatisch oder direct unter dem Einflusse des Chinins stehen. Dieselben betrafen besonders Kinder. Was ich an allgemeinen Resultatengewann ist folgendes:

Soweit sich aus der poliklinischen Praxis, wo die Controlle bekanntlich eine sehr unsichere ist, ein Urtheil bilden lässt, waren grosse Dosen Eucalyptol sehr nützlich bei den fieberhaften Erkrankungen der Luftwege, speciell auch beim Keuchhusten. In letzterer Krankheit verschwanden die Anfälle einige Mal nach Eucalyptol in prompter Weise.

Mit Bestimmtheit kann ich angeben, dass ein Nachtheil grosser Dosen (z. B. alle zwei Stunden fünfzehn Tropfen bei einem achtjährigen Kinde fast zwei Tage hindurch) nie beobachtet wurde.

Einige Mal gingen unerwartet mehrere Spulwürmer ab. Die Erklärung wird darin zu suchen sein, dass ein Theil des Eucalyptols mit den Faeces fortgeht, und dass ferner auch andere Gliederthiere direct nachweisbar durch die Dämpfe des Oels rasch vergiftet werden.

Zum Schluss mache ich noch darauf aufmerksam, dass die Einführung des Eucalyptols in alcoholischer Lösung für die Praxis sich wohl am meisten empfehlen dürfte. Aus verschiedenen Gründen ist anzunehmen, dass unter dem Einflusse des Alcohols die Aufnahme im Magen am leichtesten vor sich geht, und da der Alcohol in ähnlicher Weise wirkt, wie das Eucalyptol, so wird durch ihn noch eine Verstärkung des therapeutischen Einflusses erzielt werden.

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Vita.

Verfasser wurde geboren in Rolshoven bei Cöln am 8. März 1851. Seine Eltern, Franz Siegen und Elisabeth Siegen geb. Rolshoven, erfreuen sich noch des besten Wohlseins. Den ersten Elementarunterricht erhielt er im elterlichen Hause, und bezog dann im Herbste 1862 das kath. Gymnasium an Marzellen in Cöln, welches er 1869 mit dem Zeugnisse der Reife verliess. Darauf wurde er in Bonn von Prof. Heimsoeth immatriculirt, und von dem zeitigen Decan, Prof. Rindfleisch, in das Album der medicinischen Facultät eingetragen. Während seiner vierjährigen Studienzeit in Bonn war er Schüler der Professoren Doctoren: Binz, Busch, Clausius, Doutrelepont, Finkelnburg, Hanstein, Kekulé, Madelung, v. Mosengeil, Obernier, Orth, Pflüger, Rindfleisch, Rühle, Sämisch, Schaaffhausen, Schultze, de La Valette, Veit, Weber, Zuntz.

Diesen meinen hochverehrten Lehrern meinen herzlichen Dank. Ganz besonders fühle ich mich aber Prof. Binz verpflichtet, der mir mit der grössten Bereitwilligkeit das Thema zu dieser Erstlingsarbeit angab und mir während der Ausarbeitung mit Rath und That helfend zur Seite stand.

Thesen.

1) Die Fermente, welche wir bis jetzt kennen, sind Zellen oder albuminöse Zellenproducte.
2) Im Blute ist der Sauerstoff als gewöhnliches Gas (O_2) enthalten, innerhalb der Drüsengewebe aber in der sogenannten activen Form (O_1).
3) Diphtheritis ist eine Pilzkrankheit.
4) Die Mehrzahl der rein asthmatischen Anfälle beruht auf einer acuten Schwellung der Schleimhaut, nicht auf Krampf.
5) Bei der Behandlung tetanischer Zustände sind die ätherischen Oele in starken Gaben zu versuchen; ihre Anwendung erscheint, theoretisch betrachtet, rationeller als die des Curarin und Chloroform.

ON THE CONNECTION

OF

BRIGHT'S DISEASE

WITH

CHANGES IN THE VASCULAR SYSTEM.

With Illustrations from the Sphygmograph.

BY

A. L. GALABIN, M.A., M.D.,

Fellow of Trinity College, Cambridge.

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ON THE

CONNECTION OF BRIGHT'S DISEASE WITH CHANGES IN THE VASCULAR SYSTEM.

It has long been recognised that under the common name of Bright's disease are included conditions widely differing from each other both in pathology and causation. It may be regarded as generally agreed by all writers on this subject in England that under this name are comprised three principal forms of disease :—

1. Tubal, or Epithelial Nephritis, affecting mainly the epithelium of the tubes, and ending in the production of the large, smooth, white kidney. 2. Cirrhosis of the Kidney, or Interstitial Nephritis, affecting mainly the intertubular stroma, and leading to the granular, contracted kidney. And 3, Lardaceous disease, affecting mainly the arteries.

It is true that there are some who maintain that chronic Bright's disease with contracted kidney commences always by an alteration of the epithelium of the tubes. Thus Dr. Johnson includes it in his class of "Chronic Desquamative Nephritis;" but even he would allow that it differs in an important degree from the more acute disease associated with the large white kidney, and termed by him "Acute Desquamative Nephritis," by the fact that it may be chronic and insidious from its commencement. On the Continent, however, this distinction is not so generally admitted, and both

Frerichs and Niemeyer hold that the contracted kidney is the later stage of the same condition which exists in the large white kidney.

It was observed by Dr. Bright himself, and by all succeeding writers on the subject, that chronic Bright's disease is not a merely local affection, but is frequently associated with important changes throughout the body, of which the most marked are thickening of the arteries, and hypertrophy of the left ventricle of the heart. It has long been taught that these changes are especially characteristic of a granular kidney; but the question has remained undecided, whether they are the consequence of the renal disease, or whether both are the results of some common cause. Probably the majority have hitherto believed that the change in the kidney is the prime cause, though opinions have been various as to what is the mode of causation. Recent discussions, however, have raised the question whether this form of Bright's disease ought not to be removed altogether from the category of diseases of the kidney, and ranked as a general change in the vascular system, of which the renal affection is only a subordinate part, and which may work its most perilous effects in other parts of the body, whilst the kidney remains unchanged.

It has been further argued that this condition cannot fairly be brought within the definition of a disease, but is rather a senile change, and that thus the present conventional mode of treatment of it as a disease is worse than useless.

There are then two questions open to discussion:

1. Are the changes in the vascular system entirely confined to cases of granular kidney; or do they occur also in some degree in association with other forms of Bright's disease?

2. Are they the effects of the renal disease; or are both parts of some general condition?

I have endeavoured to obtain some evidence bearing on these questions—

1. From an analysis of all the cases of Bright's disease

contained in the *post mortem* records of Guy's Hospital during four years, 1869, 1870, 1871, 1872.

2. From microscopic examination of small arteries.

3. From observations on the pulses of some cases of Bright's disease, by means of the Sphygmograph, and on their eyes by means of the Ophthalmoscope.

At the outset I am met by a certain difficulty in classifying the cases. It is allowed that in any given kidney either any two of the three forms of disease, or even all three of them, may be combined in any proportion. But does tubal nephritis in its latest stage lead to wasting of the kidney, so that it comes to resemble in some degree one that has undergone cirrhotic change? I believe most authors hold that this does occur, and a distinction has been made as to the mode in which a granular surface is produced in the two cases; that in a cirrhosed kidney it is caused by contraction of fibrous tissue compressing the tubes, but that in the last stage of tubal nephritis it results from simple wasting of those tracts of tubules which have been rendered incompetent by inflammation. Now it is not uncommon to meet with such a case as the following. A patient's illness commences perhaps two years or more before his death, by symptoms of acute nephritis, with dropsy. It continues throughout to have mainly the character of tubal nephritis, but as it proceeds the urine becomes copious, and of lower specific gravity. After death the kidney is found to show chiefly changes of epithelium, but its surface is somewhat granular, and its cortex rather wasted with excess of fibrous tissue. I will here give very brief notes of a case which may have been of this nature. I have chosen that of a young person, because in such a case an antecedent insidious cirrhotic change is much less probable.

George G———, aged 17, was admitted October 12, 1869, with pleuro-pneumonia. Ten years before he was in hospital with dropsy, coming on after exposure to cold, and his urine was then scanty and dark. He left the hospital with his

dropsy cured, but his urine still contained albumen. He had afterwards occasionally swelling of the feet, and six years later he had pleurisy.

On admission his urine was of sp. gr. 1011, copious, containing much albumen. There was slight œdema of face and feet.

After death the kidneys were found very pale and opaque, their surface somewhat granular, the cortex rather thin in proportion; they weighed 10½ ounces. Microscopic examination showed some wasting of Malpighian bodies, and excess of fibrous stroma. The heart weighed 12½ ounces, the left ventricle was hypertrophied, and the arteries of the kidney were manifestly thick.

Now, if cases of this kind be regarded as tubal nephritis the question is settled that the changes in the vascular system do not belong solely to cirrhosis of the kidney, for in almost all of them the heart is hypertrophied. Thus in the cases enumerated by Dr. Grainger Stewart as fatal in the third stage of tubal nephritis there was in every one hypertrophy of the heart without other cause to account for it. But in all of these the surface was somewhat granular, and the cortex somewhat wasted. It may be argued, therefore, that in all an insidious cirrhotic change had preceded the acute attack; and there can be no means of settling this question, since such a change in its earlier stages is attended by no symptom that would attract notice. It is necessary, therefore, to exclude such cases in ascertaining the associations of tubal nephritis, and I have reckoned all of them among granular kidneys.

But the case is different with a kidney which presents to the eye the usual characters of tubal nephritis, but under the microscope shows some excess of fibrous stroma. There would here be no warrant for assuming that the disease had commenced as an interstitial cirrhosis. So far as my own observation has gone I have found the stroma always somewhat increased in cases of large white kidney in which the

disease had been of long standing. Moreover, it is our experience of chronic inflammation in other organs that it leads to an increase of fibrous tissue in its vicinity, and indeed the same thing results from merely mechanical congestion when long continued.

Referring now to Table I., relating to granular kidneys, I will first notice the very frequent association of atheromatous arteries. This relation agrees with common experience, and has an important bearing. It has been usual in England to speak of atheroma as a degenerative change, but on the Continent it is regarded rather as the result of inflammation, and observations have recently been published in this country, showing that in its early stage there is an active proliferation of cells, such as occurs in inflammation. We have thus associated with granular kidney not merely a thickening or fibroid degeneration of arteries, but an arteritis which may be comparable to the other inflammations common in the course of Bright's disease.

It will be seen that there were ten cases out of sixty-six of decidedly granular kidneys without any hypertrophy of heart. This is not a proof that a granular kidney is not a cause of hypertrophy; for there may have been some failure in nutrition, owing to which the heart did not respond to stimulus. But, on the other hand, we may infer that the absence of hypertrophy in any one given case does not absolutely prove that case to differ in pathology from other granular kidneys.

That condition of heart which is especially characteristic of Bright's disease, namely, simple hypertrophy of the left ventricle, though much the commonest, yet occurred in only half of all the cases in which hypertrophy was recorded. It is easy to understand that if increased pressure in the arteries be the cause of hypertrophy, it is liable also to cause dilatation.

The table of the ages at which granular kidneys were found may throw some light on the question whether they are the

result of disease or senile change. It is a difficult one to answer, since no one has ever yet given a satisfactory definition of the word disease. I should hesitate to give the name of senile to a change which in eight cases out of seventy-nine was found under the age of thirty; and which may occur, as in one of the cases published by Sir W. Gull and Dr. Sutton, in a child of nine. But the important fact remains that it is a change to which the liability rapidly increases with age.

It has been argued that if the disease occurs in the young the hypertrophy of the heart ought to be much greater, since in them the response to stimulus would be more rapid. This conclusion I do not find altogether verified. Hypertrophy occurred in six out of the eight cases under the age of thirty, thus showing that the causes producing hypertrophy were at work; but the average weight of the heart was only 12 ounces as compared with a general average of 15 ounces. I imagine the explanation to be that in the young the disease cannot have run so protracted a course, and that there has not been time for the development of that atheroma of arteries which must largely contribute towards the causation of hypertrophy.

In order to determine the effects of atheroma by itself, I have collected for comparison twenty-three cases in which the arteries were found very atheromatous. These were all cases of aneurism, senile gangrene, cerebral hœmorrhage, or cerebral softening. I have taken only those in which the kidneys were healthy, and also the valves of the heart, and in which there was no other apparent cause for heart hypertrophy. The heart was hypertrophied in thirteen out of the twenty-three, and its average weight was 12½ ounces. Comparing this with the average of 15 ounces for cases of granular kidney, it would seem that, although atheroma of large arteries tends to cause hypertrophy of heart, yet this cause would not be sufficient to account for the whole of the effect produced in Bright's disease, especially when it is remembered that in that case the heart may be hypertrophied and yet no atheroma be found. I have drawn the same conclusion from sphygmographic observations, to be referred to hereafter; for in cases of very rigid arteries where there is no evidence of kidney disease, although the arterial tension is greater than usual, it is yet never so great as in many cases of granular kidney.

Referring next to Table II. relating to cases of tubal nephritis (excluding those of scarlatinal dropsy), of which there were only twenty-five in the four years, it will be seen that there is a distinct association with hypertrophy of the left ventricle, although not so decided as in cases of granular kidney. Hypertrophy occurred in half the cases, and the average weight of the heart was 13 ounces. It will be seen, moreover, that in many of those cases in which no hypertrophy of the left ventricle was found, the disease had only lasted a few weeks.

But turning to the cases of lardaceous disease, the result is very different. In only one case not complicated with granular change was the heart hypertrophied, and its average weight was only 9¾ ounces.

Now, if the hypertrophy of heart were generally due solely to the degeneration of the coats of arterioles, it would be strange that in lardaceous disease, in which such degeneration is much greater and more undoubted, and in which it may be diffused through at least all abdominal organs, no such hypertrophy is found. Indeed, it would seem that no perceptible obstruction is so caused to the blood current, for it is well known that a simply lardaceous liver gives rise in general to no ascites.

The general conclusion is that hypertrophy of the left ventricle belongs especially to a granular kidney, but in some considerable degree also to tubal nephritis of long standing, and this is a strong argument for the view that it results from chronic Bright's disease as a disease of the kidney, and is not a common result of some third condition.

Its absence in cases of lardaceous disease may be explained

on the ground that a lardaceous condition of the kidney does not notably interfere with its power of secretion until quite a late stage, when complicated with catarrhal nephritis.

The most crucial proof of heart hypertrophy being a consequence of renal disease would be to find it occur in cases of kidneys wasted from some cause other than Bright's disease. But in such cases the other kidney generally hypertrophies, and, as might be expected, no effect of renal disease is produced. I have collected, however, in Table III., several cases in which hypertrophy of heart appeared to me to be at least partially due to a condition of this kind.

I believe the old opinion to be the true one, that the heart hypertrophy is due to the altered quality of the blood. It is proved by the case of asphyxia, that the blood may be so changed that it will not pass through the capillaries. Some well-known experiments were made upon animals, by Dr. J. Reid, in which it was found that in the first stage of asphyxia the unaërated blood passed freely through the lungs, but was impeded in the systemic capillaries, so that the pressure in the arteries increased, and that in the veins diminished. In the later stage, after about two minutes, the blood was arrested in the pulmonary capillaries, and the pressure consequently rapidly diminished in the arteries, and increased in the veins.

Dr. Johnson has attributed to the small arteries an action like that of stop-cocks. He supposes that, the blood being noxious to the tissues, the small arteries resist its passage, and thus act in antagonism to the heart, so that both are equally hypertrophied. Of this contraction of the small arteries there is no proof, and the theory appears almost to attribute to heart and arteries an antagonism of intention, as if they were actuated by volition. It seems simpler to suppose that the impediment lies in the capillaries, and is due to a modification of capillary attraction between the blood and the walls of the vessels, and that, the arterial pressure being thus increased, the muscular walls of heart and arteries are both hypertrophied in concert, since both have to act against greater resistance.

Evidence is not wanting that the circulation in living bodies cannot be explained wholly by the mechanical conditions of heart and arteries, but depends also upon chemical or vital forces. In plants and in the lower animals, where there is no heart, the circulation certainly is due to these. Moreover, the phenomenon of stasis in inflammation cannot be explained by mechanical conditions, but must be supposed to depend on a vital relation between the blood and the walls of the vessels. Again, it is common to see persons, in whom there is no deficiency of heart-power, but who show all the signs of a sluggish circulation. Their hands and feet are always cold, and their skin purple, from blood lingering in the capillaries. In them there seems to be a passive congestion, due to a deficiency of tissue change. Many other proofs are collected by Dr. Carpenter to show the existence as a motive force of a "capillary power," that cannot be explained by mechanics. The most important of these are an instance of an acardiac fetus, in which the circulation went on up to the time of birth, and several cases in which the capillary circulation was proved to continue for some little time after death. Since, then, there is so much proof of the existence of this chemical or vital capillary force, it is more natural to suppose it modified by an altered state of blood, than to assume a continuously contracted state of the small arteries, which is entirely hypothetical.

The function of the muscular coats of arteries has been generally supposed to be to regulate the circulation by altering the calibre of the arteries at different times, and it is impossible to doubt that this effect does occur. Some, however, have supposed that they also act as a motive power by a kind of peristaltic action. Thus, a rythmical contraction has been observed by Wharton Jones in the wing of a bat, by Schiff in the ear of a rabbit, and the same has been seen in the retina by the ophthalmoscope. Now a peristaltic wave

in a tube would tend to produce a current in the liquid of its own velocity, and it would, therefore, accelerate a slower current, but retard a quicker one. Therefore no peristaltic wave could accelerate the arterial stream, unless it travelled with the velocity of the pulse wave. It is thus evident that no such slow rythmical motions as have been observed could assist the arterial flow. And it is inconsistent with the usual character of involuntary muscle to suppose it capable of transmitting a very rapid wave of contraction. The arteries themselves, indeed, when made to contract by artificial stimulus, do so slowly and gradually.

But whatever view be taken of the function of this arterial coat, in either case the resistance it has to overcome in contracting is the pressure of the blood in the artery, and if that pressure be increased, the muscular coat is likely to become hypertrophied.

I will notice only one other theory of the causation of hypertrophy, namely, that adopted by Traube. He considers it due to two causes: 1, The increased pressure in the vessels due to the retention of water; 2, The direct resistance caused by the impediment to the circulation through a contracted kidney. It is a fatal objection to the first that the heart is most hypertrophied in those cases in which the excretion of water is increased rather than diminished. His second cause would be a true one as far as it goes; but the quantity of blood passing at any moment through the kidney must bear such a small proportion to the rest of the circulation, that it could scarcely have a very important effect.

It remains next to consider the direct evidence afforded by microscopic examination as to the state of arterioles and capillaries. I have examined only those from the pia mater and arteries in the kidney itself. In the estimation of the hypertrophy of arteries there is a fallacy to be avoided. It is impossible to detect change in the absolute size of a small artery, since there can be no criterion what its natural size at that point ought to be: the only thing, therefore, open to

observation is the relation of its calibre to the thickness of its walls. Now, an artery is subject to *rigor mortis*, and, if placed in a hardening agent while in that state, its contracted condition is made permanent. In order to avoid this difficulty as far as possible, I have used no hardening agent for the pia mater, and in the case of the kidney I have placed it in chromic acid only after an interval of not less than forty-eight hours after death. Another consequence results from this impossibility of detecting by the microscope change in the absolute size of small vessels—namely, that it has not been, and cannot by this means be, proved that any thickening of the walls encroaches on the calibre of the vessel. Indeed we should suppose it is not so, if we judge by analogy from the case of the larger arteries, for as their walls are thickened they generally become dilated. But if the channel is not diminished, it is not clear why any obstruction or hypertrophy of heart should result. It is true that in the case of the larger arteries a mere loss of elasticity tends to cause hypertrophy of the heart, for if they cannot dilate to admit a larger quantity of blood during the time of the heart's contraction its action is made more laborious. But this effect occurs chiefly in the larger vessels, and especially in the aorta; and the minute vessels and capillaries must bear comparatively so small a share in it, that we should not expect that a mere loss of elasticity in them would produce any perceptible hypertrophy of the heart.

The small arteries of the pia mater I have examined first in a little salt and water of sp. gr. 1020, and have afterwards added glycerine at the side of the glass, in order to observe any change produced without disturbing them. I have found, as I believe, that in cases of granular kidney both muscular and external fibroid coats are thickened. On the addition of glycerine, the external fibrous coat and the tunica intima become somewhat swollen after a time, and the former assumes what has been described as the "hyalin-fibroid" appearance. I have never seen this without the use of

glycerine, and I do not think that it indicates the presence of any special material. If glycerine, very slightly acidulated, be used, the external coat becomes at once much swollen and very hyaline, and the tunica intima becomes wavy and its longitudinal fibres more distinct. This appearance is produced almost as much in a perfectly healthy artery.

In healthy arteries the thickness of the external fibrous coat varies much in different places, and some may be found here and there equalling in this respect those associated with granular kidneys. But in the latter the thickening of the external coat is more general. It is equally so, however, in some other cases in which kidneys and heart are both quite healthy—as in old men whose large arteries are atheromatous, and in drunkards and others in whom the membranes of the brain are thickened.

In a case of large white kidney of long duration, where the heart was hypertrophied, I found the arterioles corresponding in all respects to those in cases of granular kidney. In the case of a child aged five, who had had lardaceous disease probably for more than two years, the arteries were quite thin. This corresponds to the absence of heart hypertrophy in such cases.

In the kidney itself, both in cases of granular kidney and in that of large white kidney before referred to, the arteries appeared to be hypertrophied. It is somewhat difficult in a transverse section to distinguish between the circular muscular fibres and the fibrous tissue outside them; but it appeared that in the granular kidneys there was much more of fibrous tissue around the artery, and shading off into the kidney substance near.

As to the state of capillaries or minutest arterioles, I have found a granular, or apparently fatty, condition common in cases of granular kidney, but not unfrequent also in other cases, especially in old persons, where the heart is not hypertrophied.

My general conclusion is that microscopic examination

does not reveal such a change in arterioles or capillaries as to show that the prime cause both of kidney degeneration and heart hypertrophy must be sought for here. I believe that hypertrophy of the muscular coats of arteries, just as much as hypertrophy of the left ventricle, is the natural sequence of increased arterial pressure. On the other hand, a degenerated condition of capillaries or arterioles is comparable rather with the atheromatous condition of large arteries, since both occur in advancing life, under other conditions, but both are more marked in chronic Bright's disease. That a degenerated condition of small vessels does exist, and not merely a universal hypertrophy, is shown by the occurrence of epistaxis and of hæmorrhages in the retina, for these bear the same relation to the condition of minute vessels which cerebral hæmorrhage does to that of the arteries of the brain.

Before proceeding to consider the evidence derived from the sphygmograph, it is necessary to make a short digression as to the interpretation of the secondary waves seen in the pulse tracing. I shall merely state my own view, not having space to discuss the proofs of it, although it differs somewhat from that generally accepted.

In the healthy pulse (vide Plate I.), there is seen in the descent after the first high upstroke the first secondary wave (*b*), which should be but little marked; after this comes the principal secondary wave or dicrotic wave (*c*), and this is sometimes followed by yet another wave (*d*). It is generally said that the first upstroke or "percussion wave" is due to the shock communicated through the blood column from the first impulse of the heart, and some add that this is transmitted instantaneously; while the first secondary wave or "tidal wave" travels more slowly, with a velocity due to the elasticity of the arterial walls, and indicates the distension of the artery by the flow of blood. But experiments with an artificial heart readily show that the wave causing the first upstroke does not travel instantaneously, and that it is coincident with the flow of liquid. Moreover the most violent

impulse so communicated as not to cause any flow of liquid, produces no upstroke, but only a slight quivering of the lever.

The true explanation is that the "percussion" and "tidal" waves form in the artery but one wave, and are only separated by the sphygmograph. Owing to the inertia of the long lever it is carried up a little too high, and when in falling it meets the true arterial wave, it is again tossed up, and thus forms the tidal wave. I have endeavoured to represent diagrammatically the relations of the tracing to the true pulse wave in Plate I. I shall, however, use the terms "percussion" and "tidal" wave as being convenient. Probably the tracings are more useful as clinical indications than if they represented more accurately what occurs in the artery. If the sphygmograph used have a secondary spring to keep down the long lever, the tidal wave may be replaced by two or even by a jagged line. Such a spring is better omitted, because it is apt thus to introduce oscillations of its own.

The dicrotic wave is due to a reflection from the aortic valves at the moment of their closure. If the tension in the arteries is low, the valves close slowly, there is a considerable fall of pressure before their closure, and thus the dicrotic wave is more marked. Hence a dicrotic wave small in proportion, provided the aortic valves are healthy, is an indication of high pressure in the arteries. The wave which sometimes follows the dicrotic bears the same relation to it which the "tidal" does to the "percussion."

In tracings taken in Bright's disease perhaps the most important indication of any is the amount of pressure applied to the artery. In the sphygmograph I have used this is regulated by an invention of Mr. F. A. Mahomed, Resident Medical Officer to the London Fever Hospital—a spiral "eccentric," whereby it is registered much more accurately than by the use of a spring or weight.

Fortunately differences of opinion as to the physical cause of the "tidal" wave do not lead to any difference as to its

practical interpretation. The sphygmograph may give no characteristic sign of slight valvular disease of the heart, and in some cases it may fail to show any indication of thoracic aneurism, but it may be relied upon to reveal the existence of rigid arteries or hypertrophied heart. Indeed I believe that one of its most useful applications would be to estimate the probable duration of life, by showing how far the vascular system has undergone the changes to which it is subject with advancing years, or which may be the only indication of commencing Bright's disease.

Tracings 1, 2, 3, show healthy pulses at various ages. They prove that the mere increase of age does not produce the effects seen in those which follow. The pressure used for healthy pulses generally varies from 1½ to 3 ounces.

Tracings 4 and 5 show the effect of rigid arteries without any Bright's disease. In such cases the pressure used is somewhat greater than normal, and ranges usually from 3 to 4 ounces. But their most prominent character is the occurrence of a large "tidal" wave. This is most manifest when the impulse of the heart is not very strong, as in 4. It indicates that the contraction of the heart is prolonged, and it may occur in even greater degree in cases of obstruction of the aortic orifice.

The tracings 6, 7 and 8, were taken from typical cases of granular kidney, with hypertrophied heart and rigid arteries. In these the high arterial tension is indicated by a pressure varying from 4 to 6 ounces, the latter of which is attained in no other condition, thus showing that in chronic Bright's disease there is something more than the effect of merely atheromatous arteries. The height of upstroke in 7 shows a very extreme degree of heart hypertrophy. In all the large tidal wave shows arterial rigidity, but this is especially marked in 6.

The tracings 9 and 10 were taken from cases in which dropsy had come on recently. They have all the characters common in cases of granular kidney; and other evidence con-

curred with the sphygmograph in making it probable that in them a kidney, previously granular, had suffered a recent catarrhal attack.

I come now to some cases difficult of diagnosis, and in reference to them I have to consider the evidence to be derived from examination of the eyes. There are two conditions chiefly to be observed in Bright's disease—namely, hœmorrhages in the retina and albuminuric retinitis. In the latter the retina becomes dull and at length anæmic; lymph is deposited about the central spot, and the optic disc grows indistinct and its vessels irregular. These two conditions are often found together, but may occur separately. Hœmorrhages may be seen when no complaint is made of failing sight. Not so retinitis. The former indicates the same state of vascular system as does cerebral hœmorrhage—namely, increased pressure and diseased vessels. The retinitis may be regarded either as an interstitial scleriasis analogous to the cirrhotic change in the granular kidney, or as an inflammation, the liability to which arises from the altered state of blood. But I believe that most authorities testify that, though commoner with a granular kidney, it may occur in any form of Bright's disease, and even at an early stage, in which case the latter view would seem more probable.

I will here give brief notes of a case, which show that it may occur as the sequel of acute nephritis from scarlatina of not very long standing:—Joseph T., aged 35, admitted November 2nd, 1870. He had scarlatina three months before, and three weeks later his face and feet swelled, and his urine became scanty and high-coloured. His sight had been getting dim for some weeks. On admission there was much dropsy. Urine scanty (specific gravity 1015), containing fatty and epithelial casts. On ophthalmoscopic examination, the retina was seen to be anæmic, with white spots around the central spot, as they occur in Bright's disease. He remained in the hospital till February 3rd, 1871, when the dropsy was cured, but the urine still albuminous. His eyesight was improved.

Now, I have given the tracings of two cases of renal dropsy, in which the history and symptoms both pointed to a diagnosis of tubal nephritis, but in which the pulse-tracing has nearly all the characters usual in cases of granular kidney. These are 11 and 12. In one of them the occurrence of hœmorrhages in the retina bears the same testimony as to the state of the vascular system as does the pulse-tracing. In both of them the good specific gravity of the urine makes the existence of at least any considerable granular change in the kidney improbable. Having derived from post-mortem evidence the opinion that tubal nephritis of long-standing may lead to hypertrophy of heart and rigidity of arteries, I believe that these are probably cases of that nature.

Next come the tracings of two cases occurring in young persons in whom the condition of the eyes had led to the suspicion of the existence of granular kidney, although the extent of dropsy and some other circumstances pointed rather to tubal nephritis. These are 13 and 14. The tracings show high arterial tension, without any great hypertrophy of heart. That of 13 indicates also great rigidity of arteries. In the case of 14 the state of the kidney was disclosed by post-mortem examination. It corresponded to the ordinary characters of a granular kidney, except that its colour was unusually white, and the microscope showed greater irregularity than usual of epithelium. The history of the case, coupled with the age at which it occurred, appears to afford an instance rather in favour of those who believe that there may result, as a late stage of tubal nephritis, a state of the kidney, and of the body generally, closely resembling that which is generally the result of cirrhotic change.

Next come a series of cases (15, 16, 17) of recent acute nephritis, two of them apparently the result of scarlatina. In all of these the tracings show some characters approximating to those of chronic Bright's disease. The pressure is somewhat greater than is usual in healthy pulses, and the tidal wave is too large in proportion. It has been already

said that this increase in the tidal wave indicates a prolongation of the heart's contraction, such as results from atheromatous arteries or aortic obstruction. It is obvious that a similar effect would be produced if there were an impediment to the capillary circulation. Hence my interpretation of these tracings is, that even in the early stage of acute nephritis such an impediment to the circulation may occur from altered quality of the blood that the arterial pressure is increased, and the heart's contraction made more laborious. If this be true, it is easy to understand that this state of things, if continued long enough, will cause the muscular walls both of heart and arteries to hypertrophy.

The last two tracings, 18 and 19, are introduced to show that the characteristic pulse of Bright's disease may be entirely lost, when complications arise, especially such as are associated with a febrile condition. They show the effect of bronchitis in the first case on the pulse of a granular kidney; in the second on that of tubal nephritis. Bronchitis has a double action on the pulse. The obstruction in the lungs diminishes the pressure in front of the obstructed point, and thus makes the arterial tension less. Also the associated febrile condition has a similar effect from the relaxation of the vessels, and thus on both accounts the pulse tends to become more dicrotic, and its tidal wave to become less marked.

It is in the case of Bright's disease complicated by a febrile state that there is sometimes found a condition which does not otherwise occur, namely a dicrotic pulse which will bear a high pressure. Although the arterial tension in such cases has become low, as evidenced by the dicrotism, yet the heart, which had previously become hypertrophied, gives proof of its power in thus making a strong upstroke under pressure.

I have a few words to say, finally, on the proximate cause of renal dropsy; for since the presence or absence of dropsy is a most marked difference between the various forms of renal disease, a correct interpretation of this is likely to throw

some light upon the difference of their pathology. The first requisite in a satisfactory explanation of renal dropsy is that it should account for that difference in its distribution from that of cardiac dropsy, whereby the one may generally be distinguished at a glance from the other. Now, the amount of transudation and absorption through the walls of the vessels must depend on the resultant of chemical or vital actions, and of the mechanical laws of osmosis, and any disturbance of either conditions may produce an altered result. There are only two mechanical conditions which would tend to increase exosmosis, namely, an increased pressure within, or a diminished specific gravity of the contained fluid. Now, pressure at a lower point of the body exceeds that at one which is higher, but in other respects exactly similarly situated, in proportion to the difference of vertical height. Hence any effect of excessive pressure would be seen most in dependent parts. This is exactly what occurs in cardiac dropsy, which is generally agreed to be the result of increased pressure, and does not occur in renal dropsy. The conclusion is clear that renal dropsy cannot result from increased pressure, and, therefore, if mechanical at all, it can only be the effect of diminished specific gravity. The effect of this would be equally diffused, and would, therefore, be most manifest, as in renal dropsy it is, where the tissues are loosest.

It is an old opinion that renal dropsy is caused by poorness of blood, resulting from the drain of albumen. Poorness of blood is a somewhat vague expression; for the blood is, in one sense, poor in cases of granular kidney, where there is no dropsy, for there is then a scarcity of red corpuscles. But it is only the specific gravity of the liquor sanguinis which can mechanically affect the result, since the corpuscles would behave only as foreign bodies floating in the fluid.

For direct evidence as to the state of the blood,—since bleeding is now never performed in Bright's disease,—I have to refer back, as all writers do, to the observations published by Dr. Christison. It is true that he regarded a granular

kidney as being the later stage of acute Bright's disease; but it is evident from the account of his cases that his instances of early stage were mostly cases of tubal nephritis, those of late stage cases of granular kidney; dropsy being generally present in the former, and not in the latter. His results were as follows:—In the early stages he found the serum to be in specific gravity from 1020 to 1022, instead of from 1029 to 1031 as it was in health. In late stages he found its specific gravity to be normal. Now, conclusions drawn from the state of the serum may be at once applied to that of the liquor sanguinis; since the variations which he found in the amount of fibrin were never sufficient to make a difference of one part in 1,000. We have thus, therefore, direct evidence that the diminished specific gravity, which would be a sufficient cause of the dropsy, does actually exist in acute Bright's disease.

But it is a fatal objection to the opinion which attributes the cause of this to the drain of albumen, that dropsy may appear coincidently with the albuminuria, or sometimes even before it, especially in cases which follow scarlatina. Moreover, Dr. Christison found the deficiency in solids to affect not only the albumen, but the salts equally. But the specific gravity would be equally diminished, and that immediately, by a failure in the power of the kidney to excrete water, and at the outset of acute nephritis the quantity of urine is constantly diminished. This retention of water has often been suggested as a cause of the dropsy, but only as leading to increased pressure in the vessels. For the reasons before mentioned I do not believe that a change of pressure can be the main cause, although it might in some degree contribute to the result.

It has been stated as an objection to the theory attributing renal dropsy to a defective excretion of water, that in cases of tubal nephritis which have reached a chronic stage the amount of urine may be even above normal, and yet dropsy continue. But this generally occurs under the use of diuretics, and, even

if it be otherwise, there is at any rate, so long as dropsy continues, an excess present of that most powerful of all diuretics, namely, water. It may well be, therefore, that the quantity of urine, even though absolutely above normal, is yet relatively too little for the requirements of the individual, and less than a healthy kidney would have excreted under the same conditions. It is also to be considered that in this chronic stage the effect of the drain of albumen may come into play, and contribute in an important degree to keep down the specific gravity of the liquor sanguinis.

I will briefly advert to some other theories of renal dropsy which have been suggested.

1. That it is the result of increased pressure, caused by resistance to the circulation of the altered blood. It is a fatal objection to this that in cases of granular kidney the arterial pressure is most of all increased, and then dropsy is generally absent.

2. That in consequence of the retention of the solids of the urine the specific gravity of the blood is increased, absorption thereby promoted, and, therefore, the pressure in the vessels increased.

If this were true, the amount of dropsy should always be proportional to that of uræmia; but it is found, on the contrary, that in cases of granular kidney uræmia may occur without any dropsy. This theory, as well as the last mentioned, are open to the objections before urged against attributing the result to altered pressure; and, lastly, it is contradicted by the observations of Dr. Christison, in which the specific gravity of the serum was found to be not increased, but diminished, in acute Bright's disease.

3. That renal dropsy is not due to any mechanical cause, but is the result of the irritation of the tissues caused by the altered blood, and thus is an universal inflammation of low degree throughout the areolar tissue. In this view it is compared to the dropsy occasioned by the presence of the trichina spiralis.

In answer to this it may be said that renal œdema may be greater than occurs from even a severe degree of ordinary cellulitis. Moreover, from so universal an inflammation, even if of very low degree, we should expect great constitutional disturbance. But it is not very uncommon to meet with patients in the first stage of renal dropsy, who come for advice simply on account of the dropsy, have not found their health otherwise affected, and have not even noticed anything wrong about their urine. Finally, this theory also is inconsistent with the occurrence of uræmia without any dropsy, for according to it the degree of dropsy ought to be proportional to that of uræmia.

I would draw the conclusion that in different forms of Bright's disease, the excretion of water and the excretion of solids being impaired in very different proportions, and the loss of albumen being again quite independent of either, many of their other characters result from these differences. In tubal nephritis water is retained, and therefore dropsy results; if solids are retained also, various sequelæ in the shape of inflammation may arise, and heart and arteries are often hypertrophied if the disease lasts long enough. In lardaceous disease there is no notable retention either of water or solids until a late stage, and, therefore, generally no hypertrophy of heart. In cases of granular kidney, solids alone are usually retained, and, the change being very gradual and slow, hypertrophy of heart and arteries are the marked result. In the arteries hypertrophy is not the sole change found, but atheroma of large arteries and fibroid or granular change in minute vessels are commonly seen, whether these are purely degenerative, or whether, as I rather believe, they are in part the sequel of inflammation. But such changes are frequent in advanced life, and though they are more common and more marked in chronic Bright's disease, yet the difference is, as I believe, not greater than may be accounted for by the increased strain and the impaired nutrition, which, in such cases, are both present.

TABLE I.

ANALYSIS OF ALL CASES OF GRANULAR KIDNEY

CONTAINED IN THE POST-MORTEM RECORDS OF GUY'S HOSPITAL DURING THE YEARS 1869, 1870, 1871, 1872.

Cases of granular kidney in men	55
" " women	24
Total	79
Out of these cases there was evidence of gout in	20
Cirrhosis of the liver in ...	10
The number of cases of cirrhosis of the liver in which kidneys were healthy were	23
Out of 36 cases occurring during the years 1870, 1871, it was specially recorded that the arteries were atheromatous in ...	19

ASSOCIATION OF GRANULAR KIDNEY WITH HYPERTROPHY OF HEART.

Out of the 79 cases the state of the heart was not noted in ...	8
There was hypertrophy of heart, with other causes to account for it, in	5
Cases remaining for comparison	66
Out of these there was no hypertrophy of heart, kidneys but little diseased, in	3
No hypertrophy of heart, kidneys much diseased, in	10
Hypertrophy of heart in	53
Total	66

In 34 of the cases of hypertrophy the state of the several cavities was specially recorded:

There was simple hypertrophy of left ventricle in	17
Hypertrophy and dilatation of left ventricle	5
General hypertrophy	6
General hypertrophy and dilatation	6
Total	34

TABLE I.—(*continued.*)

	oz.
The average weight of the heart out of all the 66 cases was ...	15½
In 17 out of the 66 cases the kidneys were not smaller than natural. In 13 of these the heart was hypertrophied, and its average weight in the 17 cases was	14½
In 23 cases, in which the arteries were very atheromatous, but the kidneys and valves of the heart healthy, the average weight of the heart was...	12½

These were all cases of aneurism, senile gangrene, cerebral hæmorrhage, or cerebral softening. The heart was hypertrophied in 13 out of the 23.

TABLE OF THE AGES AT WHICH DEATH OCCURRED IN THE CASES OF GRANULAR KIDNEY.

Between 10 and 20	2
„ 20 and 30	6
„ 30 and 40	12
„ 40 and 50	19
„ 50 and 60	21
„ 60 and 70	16
Above 70	3
Total	79

The average weight of the heart in the 8 cases occurring under the age of 30 was 12 ounces. It was hypertrophied in 6 out of the 8.

TABLE II.—TABLE OF ALL CASES OF TUBAL NEPHRITIS, EXCLUDING SCARLATINAL DROPSY, FROM THE POST-MORTEM RECORDS OF GUY'S HOSPITAL, DURING THE YEARS 1869, 1870, 1871, 1872.

Sex.	Age.	State of Heart.	Weight of Heart in ounces.	Period since first attack of Dropsy.	Weight of Kidneys in ounces.	State of Kidneys.	State of Urine during life.
F.	49	Hypertrophy of left ventricle...	11	3 months ...	7½	Pale, smooth, cortex not thin in proportion	Sp. gr. 1017. Highly albuminous
M.	23	Healthy. Left ventricle firmly contracted	.....	4 months ...	15	Large white	
M.	23	Hypertrophy of left ventricle...	15	15 months...	23	Large white	
M.	36	Right side dilated. Embolism of pulmonary artery	18	.....	.....	Large white	
M.	43	.....	11	10 weeks ...	.....	Large, smooth, white	
M.	56	Hypertrophy of left ventricle...	13½	12 months...	.....	Small, smooth, and white. Cortex rather thin. Arteries thick	Sp. gr. 1012. Loaded with albumen
M.	38	Dilatation and hypertrophy. Adherent pericardium	17½	.....	.....	Smooth white; minutely mottled; natural size	
M.	36	.....	11	A few weeks	18	Pale and swollen	Scanty. Sp. gr. 1025
M.	54	Healthy	.....	3 weeks.....	.....	Large, pale, mottled on surface	
M.	26	Dilatation and hypertrophy. Adherent pericardium	20	18 months...	.....	Large white	
F.	64	.....	10	.....	15	Large white	
M.	34	Hypertrophy of left ventricle...	16	.....	11	Large, pale, very smooth	
F.	34	Healthy.....	.....	6 weeks.....	.....	Large, smooth, mottled	

TABLE II.—*continued.*

Sex.	Age.	State of Heart.	Weight of Heart in ounces.	Period since first attack of Dropsy.	Weight of Kidneys in ounces.	State of Kidneys.	State of Urine during life.
M.	48	Hypertrophy and dilatation, especially left ventricle	23	2 years	15	Large white. Arteries thick	Sp. gr. 1010. Loaded with albumen
M.	...	.....	18	3 months ...	12	White. Surface minutely granular	
F.	31	Slight hypertrophy of left ventricle	11	3 months ...	9	Pale, mottled, fatty	
M.	28	No decided hypertrophy.....	11	3 weeks.....	16	Large white	
M.	32	Hypertrophy and dilatation left ventricle	16	.....	20	Mottled, smooth	
M.	29	Healthy	.....	.....	....	Large, mottled, smooth	
M.	57	No decided hypertrophy.....	12	2 years	15	Large white	
F.	17	Hypertrophy. Disease of aortic valves	15½	2 months ...	18	Large white	
M.	52	Hypertrophy of left ventricle...	21	2 years	14	Pale and mottled. Surface very slightly uneven	Sp. gr. 1012. Very albuminous. Epithelial and fatty casts
F.	40	Hypertrophy and dilatation of left ventricle	12	18 months...	13½	Quite smooth. Mottled by opaque white	
M.	32	Healthy	.....	5 months ...	21½	Large white	
F.	55	Healthy	.....	.....	12	Large white	

TABLE II.—(*continued.*)

Number of cases of tubal nephritis, excluding scarlatinal dropsy, in 4 years	25
Of these there was hypertrophy of heart, with other causes to account for it, in	3
Of the rest, hypertrophy in	11
„ no hypertrophy in	11
Total	25
	Oz.
Assuming 10 ounces to have been the average weight of the heart in those cases in which it is only recorded to have been healthy, the average weight for the whole 22 cases is	13

Number of cases of lardaceous disease of kidneys during 4 years	15
Of these there was hypertrophy of heart, the kidneys being also granular, in	2
Of the rest, hypertrophy in	1
„ no hypertrophy in	12
Total	15
	Oz.
The average weight of the heart in 13 cases not complicated by granular change was...	9¾

TABLE III.

CASE OF ATROPHY OF ONE KIDNEY—HEART HYPERTROPHIED.

Charles T., 43, admitted in January, 1870, with hemiplegia.

Post-mortem.—There was found cerebral hæmorrhage. The right kidney was healthy, and weighed 9 ounces. The left kidney was much wasted, and contained several calculi. The heart weighed 20 ounces—the left ventricle was hypertrophied. The arteries of the brain were atheromatous, the aorta not so.

CASE OF CONGENITALLY IMPERFECT KIDNEYS, VERY SLIGHTLY AFFECTED BY BRIGHT'S DISEASE—HEART HYPERTROPHIED.

George L., 26, admitted in May, 1869, with coma, following convulsions. He had had frequent micturition at night for two years. Urine albuminous.

Post-mortem. The kidneys weighed together 3¾ ounces. They were perfect in outline, and the cortex in due proportion. Surface smooth, but with very slight minute granulation. Under the microscope some of the tubes were found dilated; epithelium rather irregular, stroma fibrous in parts. The heart weighed 13½ ounces; the left ventricle was hypertrophied.

CASE OF ATROPHY OF ONE KIDNEY—HEART HYPERTROPHIED.

F. B., 19, admitted into Victoria Park Hospital, February 12th, 1872, with acute phthisis. On March 7th was attacked by right hemiplegia and aphasia.

Post-mortem.—Left ventricle of brain distended by clot from middle cerebral artery. Cerebral arteries and aorta atheromatous. The left kidney was wasted to a cyst, and contained several calculi. The right kidney was but little enlarged, and appeared slightly affected by tubal nephritis. The left ventricle of the heart was much hypertrophied.

APPENDIX.

PLATE I.

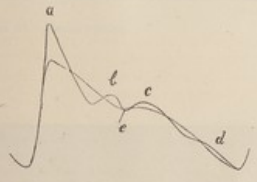


DIAGRAM TO ILLUSTRATE THE RELATION OF THE TRACING OF THE PULSE AS DRAWN BY THE SPHYGMOGRAPH TO THE TRUE PULSE WAVE AS IT OCCURS IN THE ARTERY.

a Primary or "percussion" wave.

b First secondary or "tidal" wave.

c Principal secondary or "dicrotic" wave.

d A nameless wave which, like the "tidal" wave, is the result of oscillation in the instrument.

e "Aortic notch."

The dotted line is intended to represent the tracing which would be drawn if the instrument followed the movements of the artery with perfect accuracy.

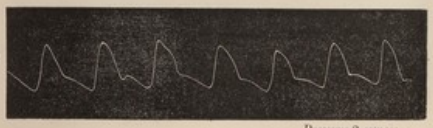
1



Pressure 2½ *ounces.*

Man, aged 30. Healthy pulse.

2



Pressure 2 *ounces.*

Woman, aged 46. Arterial elasticity somewhat impaired.

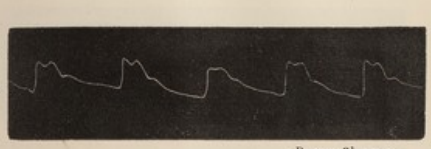
3



Pressure 3 *ounces.*

Healthy man, aged 50. Amplitude greater than normal, probably from dilatation of the radial artery.

4



Pressure 3½ ounces.

Man, aged 50. Radial arteries very rigid and knotty.

5



Pressure 4 ounces.

Man, aged 72. Arteries rigid: heart somewhat hypertrophied.

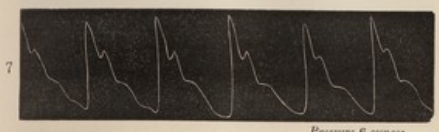
6



Pressure 4½ ounces.

Man, aged 51. Albuminuria. Diagnosis: granular kidney. Arteries very rigid.

7



Pressure 6 ounces.

Man, aged 38. Albuminuria. Diagnosis: granular kidney. Heart much hypertrophied.

8



Pressure 6 ounces.

Woman, aged 59. Albuminuria. Diagnosis: granular kidney. Arteries rigid.

9



Pressure 4½ ounces.

Woman, aged 47. Ten years ago she had dropsy, from which she recovered. Her present illness began with sickness five months ago. For six weeks she has had a cough, with attacks of dyspnœa, and during the last three weeks dropsy has come on.

Urine contains a good deal of albumen: specific gravity 1013. It contains granular and epithelial casts.

Spots of old hæmorrhage are to be seen in both retinæ, but she makes no complaint of her sight.

Diagnosis: granular kidney with catarrhal attack.

10



Pressure 4½ ounces.

Woman, aged 50. Seven weeks ago she had epistaxis which lasted almost continuously for a week. At the end of that time dropsy commenced in her hands and face, and is now considerable. For some years she has been subject to cough.

Urine loaded with albumen: specific gravity 1011; 46 ounces were passed in twenty-four hours. Granular, epithelial, and hyaline casts were found.

Diagnosis: Granular kidney, with catarrhal attack.

11



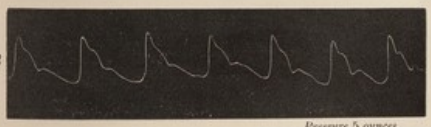
Pressure 5 ounces.

Woman, aged 29. Thirteen years ago she had dropsy, which occurred five weeks after a sore throat. She recovered in three months. She has now extensive general dropsy, which commenced seven weeks ago.

Urine contains much albumen, and epithelial casts are found in it. Its specific gravity varies from 1016 to 1022.

Diagnosis: Chronic tubular nephritis.

12



Pressure 5 ounces.

Woman, aged 44. She is now seven months pregnant, and dropsy came on suddenly for the first time three months ago. Her urine was then scanty and high-coloured. Her dropsy is now very great.

Urine nearly solid with albumen: its specific gravity is 1020; 30 ounces are passed in twenty-four hours.

Her sight has failed during the last month. By the ophthalmoscope are seen patches of old and recent hæmorrhage in both eyes, and lymph in radiating yellow lines around the central spot.

13 

Pressure 4 ounces.

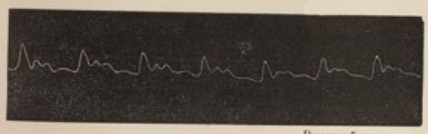
Boy, aged 18. He had scarlatina at eight years old, not followed by dropsy.

He has been ill ten months with dropsy, moderate in degree. He has been subject to vomiting for six months, to occipital headache for four months, and for two or three months his sight and hearing have been failing. He is subject to fits, apparently uræmic, and to epistaxis.

Urine loaded with albumen: specific gravity 1012. About 80 ounces are passed in twenty-four hours.

Both retinæ are mottled all over with old and recent hæmorrhages.

P.M. The kidneys were granular: the left ventricle of the heart was hypertrophied: the arteries were much thickened but not atheromatous.

14 

Pressure 5 ounces.

Girl, aged 18. One year ago she was in hospital with extensive general dropsy, and acute tubal nephritis was then diagnosed. Her urine was then loaded with albumen: its specific gravity 1012; and it contained granular, hyaline, and epithelial casts. Her dropsy was cured, but her urine still contained albumen when she left the hospital.

Two months ago she was again attacked with dropsy after catching cold, but her dropsy was not then so great as on the former occasion.

She became comatose and died. P.M. her kidneys were found smaller than natural, granular on the surface, and differing from the usual appearance of granular kidneys only in the fact that they were very white. On microscopic examination they showed the general characters of a granular kidney, except that the tubes were more than usually choked with epithelium.

There was hypertrophy of the left ventricle of the heart, and its weight was 17 ounces. The arteries were not atheromatous, nor those of the kidney much thickened.

15



Acute Nephritis. *Pressure 4 ounces.*

Man, aged 24. Five weeks ago he had sore throat. His skin is now desquamating, and his urine is albuminous. For one week he has had slight dropsy.

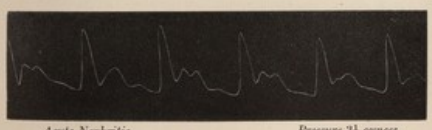
16



Acute Nephritis. *Pressure 4 ounces.*

Boy, aged 17. He was admitted with albuminuria, following a sore throat. His face and hands were desquamating. He had no dropsy, and the albumen disappeared after a few days.

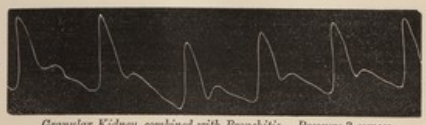
17



Acute Nephritis. *Pressure 3½ ounces.*

Man, aged 24. Three weeks ago he was attacked with dropsy, after taking cold. His urine became scanty and dark, and micturition frequent. Urine loaded with albumen: specific gravity 1020.

18



Granular Kidney, combined with Bronchitis. Pressure 3 *ounces.*

Man, aged 44. For several years he has suffered from bronchitis. For the last five weeks he has been ill with gout. He has now slight œdema of the face.

Urine contains a little albumen: specific gravity 1010.

This tracing shows the variation in the line of arterial tension due to laborious respiration.

19



Tubal Nephritis, combined with Bronchitis. Pressure 2 *ounces.*

Man, aged 32. For five years he has had bronchitis, with fits of dyspnœa. Five weeks ago general dropsy came on.

Urine scanty, containing blood, and much albumen: specific gravity 1018.

With the author's kind regard.

ON THE

Diseases Prevalent among Potters.

BY

J. T. ARLIDGE, M.D. & A.B. (Lond.), F.R.C.P., London,

PHYSICIAN TO THE NORTH STAFFORDSHIRE INFIRMARY, STOKE-ON-TRENT.

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ON THE

DISEASES PREVALENT AMONG POTTERS.

A VERY short acquaintance with the state of health of the inhabitants of the pottery district of North Staffordshire will show that, as in the case also of other seats of manufacture, it is far from satisfactory; and further, that this state of things is due pre-eminently to the prevalence of pulmonary diseases, including consumption. These facts have arrested official notice, and on two occasions a physician has been sent into the locality from the medical department of the Privy Council to investigate them. They were, moreover, influential in determining the inquiry made some years since in the district by the Children's Employment Commission, which ended in the extension of the Factory Acts to the manufacture of earthenware and china.

For very nearly ten years I have been resident within the district; and as physician to the large North Staffordshire infirmary, as well as a factory medical officer for a considerable section of the pottery towns, I have had very large opportunities of observing the diseases most rife among potters, and the conditions of labour and mode of life common among them.

Some of the results of my observation I now propose to state, based upon written records, prepared for the purpose of supplying statistical information on various points connected with the medical history of the population. The records I now use are derived from out-patient medical practice at the infirmary; for I consider that this practice best exhibits the popular maladies of a district. Only cases strictly coming within the scope of a physician's practice are included; but no cases of fever—continued or eruptive, and very few instances of acute disease are among the number catalogued. Further, all the patients were above ten years of age, and of the whole number of 800 only 40 were under fourteen. Very few were above fifty.

The whole 800 were engaged in one or other department of the pottery manufacture. Of these 463 were males and 337 females. It must not be gathered from these figures that male out-door patients preponderated in number. On the contrary, female applicants for medical relief constitute from two-thirds to three-fourths of the entire number of out-patients. There is nothing peculiar in this circumstance, for the experience of the out-door department of hospitals is similar.

In the following tables I have arranged the diseases most prevalent, and stated the rate per cent. of each of them.

TABLE I.

POTTERS—MALES.

Bronchitis	= 36·57
Phthisis	= 20· 9
Rheumatic affections	= 7·79
Stomach disorders	= 8·44
Lead-poisoning	= 8·00
Cerebro-spinal diseases	= 4·32
Cardiac diseases	= 2·81
Epilepsy	= 1·73

TABLE II.

POTTERS—FEMALES.

Bronchitis	= 7·14
Phthisis	= 16·96
Rheumatic affections	= 4·46
Stomach disorders...	= 19·64
Lead-poisoning	= 5·06
Cerebro-spinal diseases	= 2·97
Cardiac diseases	= 2·08
Epilepsy	= 4·46
Chorea	= 2·08
Hysteria	= 1·49
Anæmia, with debility, &c.	= 10·41
Uterine maladies	= 18·15

It will be asked to what extent are these maladies prevalent among potters compared with other artizans. I subjoin the two following tables in elucidation of this inquiry:—

TABLE III.

NON-POTTERS—MALES.

Bronchitis	= 18·00
Phthisis	= 13·00
Rheumatic affections	= 21·00
Stomach disorders	= 19·00
Lead-poisoning	= 0·00
Cerebro-spinal diseases	= 5·00
Cardiac diseases	= 6·00
Epilepsy	= 5·00

TABLE IV.

NON-POTTERS—FEMALES.

Bronchitis	= 16·00
Phthisis	= 11·00
Rheumatic affections	= 1·00
Stomach disorders	= 31·00
Lead-poisoning	= 0·00
Cerebro-spinal diseases	= 2·00
Cardiac diseases	= 3·00
Epilepsy	= 7·00
Hysteria	= 5·00
Anæmia, with debility, &c.	= 10·00
Uterine maladies	= 13·00
Chorea	= 1·00

I must here observe that these figures, for operatives not engaged in the manufacture of pottery, are derived from a much smaller number of cases, namely, from a total of 100 males and of 100 females respectively; consequently the per-centages calculated for the less common diseases cannot be taken as exact representations of the rate of prevalence, although sufficient to exhibit relative proportions. For instance, the table of non-potters—females—shows only 1 per cent. of cases of rheumatism; this we may take, as in some measure, only accidental in the 100 cases that happen to have been collected; in fact, I believe the average to be somewhat higher, although I can, at the same time, admit the resultant figure to be accurately indicative of the comparatively less frequency of rheumatic affections in women not engaged in the manufactories of pottery than in those occupied therein.

There are several facts deducible from the foregoing tables, whose significance cannot be doubted. Foremost among these is the excessive prevalence of bronchitis and pulmonary consumption. This circumstance also appears as vividly from an examination of the tables of mortality in the district. Thus, I have shown in my essay "On the Mortality of Stoke-upon-Trent, and its Causes," that the ratio of deaths to the entire mortality of that parish, arising from diseases of the respiratory organs, is 30·80 per cent., that of England at large being 27·20 per cent., and, excluding the deaths of children under ten years old, the per-centage reached 41·51.

So if we make a calculation from the figures in the tables presented of maladies treated among out-patients, we arrive at a closely similar ratio of the prevalence of diseases of the respiratory organs, namely, 37·39 per cent. A less ratio certainly to that of the mortality from the maladies in question among adults; but, at the same time, this is a circumstance easily accounted for by the fact that the sufferers were out-patients, required to attend at the infirmary, at a more or less considerable distance from their homes, and consequently they represented a class still possessing a moderate share of physical power, the more advanced and weaker individuals being

detained in their homes, or else within the wards as in-patients. In the essay referred to I included under the term "diseases of the respiratory organs" cases of death arising both from diseases of the lungs and from phthisis, on account of the difficulties and uncertainties of diagnosis. It will be convenient generally to pursue the same course in this paper. The so-called potter's bronchitis or potter's consumption is, in by far the larger number of cases, a true consumption, partaking of a tubercular character.

What, it may be now inquired, are the assignable causes of this prevalence of diseases of the respiratory organs within the pottery district? Something may be put down against the climate and soil of the locality. These are not congenial. The climate is damp, and the soil generally a retentive clay, a condition which has been shown to be associated with the production of phthisis. These unfavourable conditions are in turn aggravated by those of man's making, namely, those arising from the processes of manufacture, from unhealthy shops and habitations, from defective sewerage and drainage, and from improper diet and vicious habits. I cannot, on the present occasion, enter into an account of these several causes operating prejudicially on the health of the inhabitants, but may be allowed to refer to two papers I have written, in which they are partially discussed, the one in the *British and Foreign Medico-Chirurgical Review* for July, 1864, the other in the second number of the *Food Journal*, in 1870.

To return to the lessons deducible from the tables already presented, it becomes at once evident how much more rife are diseases of the respiratory organs among male potters than among non-potters. The rate per cent. is 57 to 31 for males. In the case of females no such disparity is evident; indeed, as far as bronchitis itself is concerned, it appears considerably rarer among women employed in the pottery manufacture than among others, although pulmonary consumption is comparatively more common among them.

Some remarks in elucidation of these numerical results are desirable. The prevalence of lung diseases among potters is doubtless primarily due to their occupation, which involves the taking into the lungs more or less dust derived from the clay with which they work. This so-called clay is rather a silicious compound than a true clay. In the various processes it is submitted to, it becomes more or less dried, and particles of it get diffused through the air. The investigations of Dr. Headlam Greenhow have clearly shown how the inhaled dust is productive of lung disease, and from my own experience and examinations I can confirm his conclusions. Male potters are much more the victims of this dust inhalation than females employed in the pottery manufacture; inasmuch as most of the processes in which dust is liable to be thrown off from the clay are carried on by men and lads. A certain proportion of women and girls are engaged in the clay-departments, in turning the potter's wheel, in lathe-treading, in cleaning and scouring the ware. And the last-named process, namely, china-scouring, is particularly destructive of life, by the breathing of dust and the setting up of lung disease.

But the number so engaged represents but a small section of the females, who are for the most part occupied in the finishing departments of the manufacture, namely, in the printing-shops, in "transferring" and in "paper-cutting," in painting and enamelling on the glazed ware, in gilding and burnishing the gold, and in the warehouses. Before these processes are undertaken, the clay has been converted by fire into china or earthenware, as the case may be, and consequently is no longer in a condition to evolve dust. We do not, therefore, meet with the bronchitis or potters' asthma among women as in the case of the men engaged in pot-works. Indeed, the small ratio presented by bronchitis among female potters is remarkable, and the more so, when contrasted with that obtaining among women otherwise occupied. I would not insist much on the greater prevalence among the latter, for much fewer of such women come under observation, and the hundred cases collected would extend over a longer period and probably further into the time of year when bronchitis is more common, nevertheless, the figures are so decided in their teaching that we must allow them their due weight and seek an explanation. This is not a difficult task when we come to consider the surrounding circumstances of female potters as contrasted with those of females of otheroccupations. I have noted the non-exposure generally of the former to the special causes of bronchitis among the male potters; and we have further to take into account conditions of labour favourable to their escape from the ordinary causes of the disease, namely, their protection from cold, their comparatively small exposure to weather and out-door influences, and their generally comfortable state of living and clothing, all these particulars contrasting advantageously for them in comparison with the class of women not occupied in the pottery manufacture, represented by servants of an inferior class, housewives of the poor, laundresses and needy needlewomen, all such being more largely exposed to the ordinary causes of bronchitis, and, at the same time, not so well prepared to resist those causes, by clothing and food, as their better remunerated sister-workers in the pottery manufactories.

On the other hand, the greater prevalence of phthisis among female potters acquires, from the considerations detailed, greater significance. The ratio of the occurrence of this disease ranges within 4 per cent. of that observed among male potters, and 5 per cent. above that met with among female non-potters. Making due estimate of the conditions of labour and of the circumstances of life surrounding them, this fact therefore shows a high rate of the disease among them, and is indicative of the existence of a phthisical diathesis in larger proportion, and the connection of their employment therewith.

I will pass on now to make some observations upon the prevalence of rheumatic affections. The climate of the district is favourable to the production of such affections by its dampness and coldness; but

apart from the influence of this agent there obtain special conditions of manufacture conducive to their prevalence. In respect to these affections the potters present a very decided advantage over non-potters—that is among males. In both classes of labourers rheumatism is a prevalent malady, and experience proves that those most exposed to it are the workers about ovens and furnaces, and in coal-pits. Such operatives suffer by exposure to considerable heat, and to cold or cold currents of air acting simultaneously or very speedily and also forcibly upon an over-heated body.

In this district the proportion of males, non-potters, so exposed to sources of rheumatism is greater than that of the potters. The branches of the potters' art in which the workers are most prone to the alternations of heat and cold, are those connected with the firing of the ware in ovens and kilns. The non-potters subject to rheumatism are the colliers and the workers about iron furnaces and in foundries.

The very usual connection of rheumatism with heart-diseases explains, at the same time, the greater frequency of such diseases among non-potters than among potters.

The next enumerated maladies in the tables are those of the stomach and digestive organs. Under the head of stomach disorders I have included dyspepsia in all its forms and cases of chronic gastritis. Such a group is a very elastic one; and, in the case of females especially, could be greatly expanded by the introduction of many cases which figure as examples of anæmia and hysteria, and of not a few comprehended among uterine maladies; for in all such stomach derangement is more or less a feature.

However, taking the figures as they stand, the deduction is, that both males and females not engaged in the pottery manufacture are more prone to stomach disorders than potters themselves. The explanation of this fact with reference to males is not so obvious as in respect to females. But I may say generally, that the non-potters who apply for infirmary relief are both more over-fed, and, again, more under-fed than the potters. The over-fed are represented especially by the workers in iron, the puddlers and others, whose wages are out of all proportion to their intelligence, and who know no better way of expending it than in gluttony and debauchery. These men appear before the doctor to be relieved from the gastric results of their own evil courses. The colliers partake in the same habits, and keep the "week-end" by extra indulgence in eating and drinking, particularly in the latter; but their means for so doing are not so large as those of iron-workers, and probably the production of stomach disorders among them is not greater than among the potters.

Probably the largest number of dyspeptic males is derived from the under-fed and the ill-fed, represented by broken-down artizans, by dissipated tailors and shoemakers, by ill-paid town and country labourers, and by other unfortunates whose lot is want of work or inability for work, and, as a result, inadequate and unfit food.

That more stomach disorders are encountered among women not

employed in pottery work, is explicable from the fact that the majority of such women are derived from the ill and under-fed class of milliners, seamstresses, and housewives of poor or dissipated men, or widows in great poverty.

The next variety of bodily disorders enumerated in the tables is lead-poisoning. It will be seen that this happens only among potters, and the inference—the true one—at once forces itself upon the mind, that it must be due to some condition connected with their labour. This condition is found in the glaze used in giving the smooth, lasting surface to earthenware and china; and, to a small degree, in some other processes in which lead colours are used, the chief of which is what is termed "ground-laying." The men who use the glaze are called "dippers," because their work is to dip the ware in the fluid glaze. Their hands and wrists are, during their work, constantly wetted with the lead glaze, and their clothes and faces become more or less bespattered with it. The extent to which they suffer is largely dependent on their own care and cleanliness, but to a certain degree also upon personal proclivity to suffer; for it seems certain that the lead enters and affects the system much more in some workers than in others. One or more helpers, women or lads, are engaged by the dipper in taking the ware from him, and in wiping off superfluous glaze. Those likewise into whose hands the glazed ware comes,—the "glost placers"—are also frequently poisoned by the lead.

The poison acts variously in different people and at different times. It occasions colic, troublesome dyspepsia, cerebral derangement, and paralysis in many forms, and to a greater or less extent. This source of sickness must be regarded as, both absolutely and relatively, a preventible one. It is for chemists, in conjunction with practical potters, to discover another mode of glazing pottery, and it is in the power of the workers themselves largely to obviate the peril of their labour.

It would not be correct to represent non-potters as entirely exempt from lead-poisoning, for such among them as use the metal in a form capable of entering the system by absorption must necessarily be liable to it; as are, for instance, house-painters; but probably no members of a craft suffer in so large a proportion from it as do potters, except it be the men engaged in the manufacture of white lead. But few remarks are needed to bring to a conclusion this retrospect of the diseases included in the tables.

Both epilepsy and chorea are unusually prevalent in the population of the Staffordshire potteries. The pathology and etiology of epilepsy are obscure, and the cause of its frequent occurrence in the district in question cannot be satisfactorily accounted for. It is met with in early life, and often is consecutive upon infantile convulsions. These last we can frequently associate with the ill-feeding of infants, and with faulty constitution. Both bad feeding and bad constitutions are common among the inhabitants. Struma, in various forms, affects a considerable proportion; retarded and imperfect

development prevails extensively; and, with physically weak bodies, there are often associated ill-regulated, irritable, ill-furnished minds, craving for exhausting and unhealthy stimuli, too largely found in towns and in town life.

To habits of intoxication, and to other vicious courses on the part of parents, may often be referred the sickly, dwarfed, misshapen, strumous, and phthisical children, so numerous in our manufacturing towns, and from whose ranks their mortality is so largely swollen. To such habits, more, indeed, than to the processes of manufacture, and to such habits, in association with insufficiently nutritive food, particularly on the part of women of the operative classes (often not in consequence of inadequate means, but of the want of common sense and teaching in the selection and preparation of food), do we owe the manifest deterioration of the race in large centres of industry. Such degenerescence is, to a certain extent, counteracted by the constant recruiting of the town population from the dwellers in the country. How considerable need be this immigration into towns from the country around is indicated by the prevalence of so much disease destructive of life, as shown in the tables above, and perhaps even more distinctly by the examination of the registers of the causes of death, and the deductions therefrom, exhibiting the heavy death-rate, and the early period of life at which operatives are cut off,—as exemplified in the statistics of the mortality of the chief pottery towns, published by me in the essay before alluded to.

How far soever the influence of a degraded physical frame and of constitutional taint be effective in the production of epilepsy, there can be no question of its giving origin to chorea. Many physicans, especially those in London, have represented chorea as intimately connected with rheumatism and heart disease; and no doubt these maladies frequently concur as cause and effect; but my experience proves that this relation is not nearly so frequent as supposed; and I reckon that in two-thirds of the cases no rheumatism acquired or inherited and no cardiac disease are to be found. The subjects of chorea are principally ill-nourished, weakly, scrofulous children, with irritable, ill-regulated, and, at times, over-wrought brains, and the surest cure is derived from in-door treatment in the infirmary, which implies good food and air, moral control and discipline, removal from causes of excitement and irritation in scolding or petting parents, and in annoying, quarrelsome brothers and sisters, with the use of baths, but very little physic. In other words the treatment shows that chorea is a result of debility, abnormal cerebral activity, and improper or unfit diet.

The prevalence of uterine disorders among weak women engaged in sedentary work, and to a great extent ill-fed, is a phenomenon common in every town, and not peculiar to the potteries.

Whilst on the head of stomach disorders I referred to their presence in connection with uterine derangements, and I will take this opportunity of remarking upon the lamentable amount of sickness consequent on the abuse of tea by women of the working classes.

Instead of using it simply as an occasional beverage, they make it a principal article of diet, and drink it, usually without milk or sugar, several times a day. At most meals bread and butter is the only solid accompaniment. In many cases, doubtless, poverty imposes on them a meagre diet; but even in such, the one alluded to might be advantageously replaced by other kinds of food not more expensive. But in many instances this defective diet is one of choice, and not of necessity, and the choice is determined often by ignorance and indolence—by ignorance of the simplest cooking processes, and by indolence suggesting a meal that gives the least trouble.

Bitter and strong is the agitation, at the present period, against beer and other intoxicating liquors as the root of all evils; but, in my opinion, there is room for agitation against tea-drinking as carried on in the way spoken of; for I am convinced that a deterioration of health among the working classes, and a lowered vitality in the rising generation, are consequences of the abuse of the beverage in question. Entertaining as I do these views, based on wide observation, I can in no degree sympathize with the politico-social party, who make what they call "a free-breakfast table" the goal of their reforming ambition. The surrender of revenue consequent on their success would be balanced by no increased good, either to the pockets or to the health of the classes for whose special benefit the scheme is urged.

With the Author's compliments

STATISTICS OF STILLBIRTHS.

BY

FRED. W. LOWNDES, M.R.C.S. Eng.,

HONORARY ASSISTANT SURGEON TO THE LIVERPOOL LADIES' CHARITY AND LYING-IN HOSPITAL.

Read October 2nd, 1872.

[*From Volume XIV of the 'Transactions of the Obstetrical Society of London.'*]

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1872.

STATISTICS OF STILLBIRTHS.

The absence in this country of any registration of stillborn infants has often been regretted, but more, as it seems to me, on medico-legal than on any other grounds. Our art concerns the lives of two individuals; we have to a certain and very considerable extent, opportunities afforded to us of ascertaining how far the cultivation of obstetrics as a science and art has affected one of these individuals, and it is gratifying to know that maternal deaths during the puerperal period are much less than they used to be. Doubtless much remains yet to be done in this direction, and it must have struck most of those who read the "Tables of Mortality after Obstetric Operations," published in this year's 'Transactions,' what a considerable number of lives have been lost from want of timely assistance. I propose this evening to draw your attention to the case of the other individual whose life is entrusted to our care—I mean the infant. Statistics are doubtless dry and weary to wade through and write about, and perhaps still more weary to listen to; but as it is from statistics we have learned so much of what has been valuable material in enabling us to follow out the old adage—"Prevention better than cure," I make no further apology for so occupying your time, more especially as these statistics possess many features of interest, and are especially important and likely to be of practical use as the registration of stillbirths in this country is contemplated. But it is as affecting our practice that I wish to consider this subject, and especially as regards the more frequent employment of the forceps. Your late President, in his Annual Address, lays great stress on this point, and says, "Unquestionably this Society has still work to do in urging this point on the attention of the profession."

Let us first examine the statistics of stillbirths in those countries where a systematic registration of such obtains. In the 'Lancet' for 13th July, 1869, page 52, is a medical annotation entitled "Stillborn," giving the following interesting statistics of stillbirths in different Continental States—

Netherlands	5·64 per cent.
Belgium	4·72 „
France	4·63 „
Saxony	4·49 „
Norway	4·46 „
Prussia	4·33 „
Hanover	4· „
Bavaria	3·74 „
Italy	1·94 „
Austria	1·64 „

Some additional details are given respecting the numbers in Italy, the proportion in which, as will be seen, is lower than any other European State, except Austria. The total numbers are, for the year 1864, as follows—

Births	845,454
Mort-nés (stillborn)	14,209

Of these latter the sexes were—

Males	8269
Females	5940

Or 138 males to 100 females.

Legitimate stillbirths	1·85 per cent.
Illegitimate „	6·59 „
Children found exposed	2·61 „
All categories	1·94 „

One cannot help being struck with the remarkable discrepancies in the above figures, taking the highest and lowest average; and at the same time with the great evenness (so to speak) of the figures between these two extremes. Thus, in six different states the figures range from 4 to 4·72

per cent. The greater predominance of males over females is a matter well known to us, and probably due to the larger size of male children. The greater number of illegitimate stillbirths is also a matter of note, and just what we should expect to find when we consider the circumstances that generally attend the births of these unfortunates, a secret delivery, the absence of many measures which would be adopted in the case of a legitimate infant, and if not downright unfair play by wilful omission, certainly no regret on the part of those immediately concerned that the infant has not survived its birth.

It must be remembered, however, that the arrangements prevalent on the Continent for the registration of stillbirths are not uniform. Thus, in 1867, the Paris Statistical Society, taking advantage of the presence in their city of *savants* and statists from all parts of the world, who were visiting the exhibition, proposed that at one of their public séances the following question should be discussed: "What is understood by 'stillborn?' Should the term apply exclusively to infants born before, during, or within a specific interval after the accouchement, or should it include infants presented dead for registration by the civil officer? Does the number of stillborn increase proportionally to the total births? Admitting the increase of stillbirths, is that increase observable in towns and country alike?"[1]

From a paper published by M. Loua on the "Stillborn in Paris," in 1865, the following particulars are extracted:— In France it is the practice to include under the designation of *mort-nés*, not only infants born dead, but also those born alive who die before the legal declaration of birth. The numbers were as follows:—

Total number of births	55,157
Mort-nés	4410

Or 8 per cent.

Number of *conceptions*	59,567

[1] 'Lancet,' Aug. 17, 1867, p. 202.

Or 108 conceptions to 100 births, and 7·4 stillborn to 100 conceptions.

From 1817 to 1863 the proportion of mort-nés to conceptions had increased from 5·53 to 7·09, and in 1865 to 7·40 per cent. Of the two sexes the males furnish by far the greatest number of mort-nés, and the issue of illegitimate connexions more than those of marriage. Thus, to every 100 conceptions, of each sex there were 8·26 males and 6·48 females stillborn; to every 100 legitimate births 6·70, and to 100 illegitimate births 8·53 stillborn. Another way of looking at these facts shows that taking 100 births of each class as the basis, there were 109 male, 107 female, 109 illegitimate and 107 legitimate conceptions. The ratio of female to male mort-né was as 100 to 134. M. Loua notices that the stillborn seem to follow the order of general mortality in respect of their frequency in any season of the year; and he recognises in the coincidence of the maximum of mort-nés with the acmé of cholera the indirect action of that epidemic. He also notices that in nine times out of twelve the barometer is much lower at the dates when the births are most numerous than when they are fewest. He asks, "Faut il voir, dans ce fait, l'indice que les accouchements sont favorisés par les jours pluvieux?"[1]

The following particulars are furnished respecting the returns of mort-nés in Paris so recently as June, 1871 (which, it will be remembered, was soon after the termination of the late war).

Recorded during June, 1871.

Total number of stillbirths .		.	182
Of these 4 were in the .		.	3rd month
4	„	.	4th „
6	„	.	5th „
18	„	.	6th „
27 were in the .		.	7th month
26	„	.	8th „
84	„	.	9th „

13 cases period unknown.

[1] 'Lancet,' Jan. 5, 1867, p. 20.

The large proportion of stillbirths in the ninth month is very remarkable and contrary to what we should expect under ordinary circumstances. Every one who has had any experience will, I am sure, agree with me that premature birth accounts for a larger proportion of stillbirths than any other cause. But considering the state of Paris shortly before these returns were published, and remembering how much the life of the infant is affected by maternal impressions and other circumstances, I think we are justified in considering the above as exceptional.

The following statistics are from the 'British and Foreign Medical Review:'

In Prussia the ratio of stillbirths		.	3·29 per cent.
Sweden	„	.	2·64 „
Saxony	„	.	4·43 „
Hanover	„	.	4·22 „
Mecklenberg-Schwerin	„	.	3·70 „
Schleswig and Holstein	„	.	4·60 „
St. Petersburgh, 1805	„	.	0·20 „
„ 1806	„	.	0·70 „
Russia generally	„	.	0·80 „

These are very remarkable statistics: it would appear that the higher we ascend in the scale of civilisation the greater is the proportion of stillbirths. Appended to the above are the following additional particulars:—"There are more stillbirths of illegitimate than of legitimate children; it has been calculated that where 3·166 per cent. of the legitimate are stillbirths, 4·959 of the illegitimate are stillbirths. According to the calculations of Bickes—

Of legitimate male children 3·559 per cent. are stillborn.
Ditto female „ 2·749 „ „

Of illegitimate male children 5·277 per cent. are stillborn.
Ditto female „ 4·632 „ „

During the twenty-five years—from 1801 to 1825—the proportion of stillbirths to children born alive, was in Leipsic, 1 to 17⅓.

In 1822 it was—

In the district of	Liegritz,	1 to 15
	Arnsberg,	1 to 28
	Coblentz,	1 to 27
	Elberfeld,	1 to 17 (about 16 per cent.)
	Magdeburg,	1 to 18
	Dusseldorf,	1 to 26 (about 4 per cent.)
	Minden,	1 to 37
	Stralsund,	1 to 44½
	Erfurt,	1 to 30
	Merseburg,	1 to 21
	Posen,	1 to 49 (about 2 per cent.)
	Paris,	1 to 30
	Saarlouis,	1 to 16
	Vienna,	1 to 36½
In 1821 in	Berlin,	1 to 19
	Gotha,	1 to 10 (exactly 10 per cent.)

The highest average is thus 1 to 10, and the lowest 1 to 49; the general average is about 1 to 20.

Although from the want of a uniform system the above figures must be considered as open to exception, they suffice to show the necessity of a more uniform system. The great disproportions strongly suggest the question, "Whether stillbirths may not be considered to a considerable extent as preventable?"

Let us now examine the statistics of stillbirths in this country so far as we are in a position to do so.

The first statistics to which I shall refer are those published by the late Dr. F. H. Ramsbotham in the appendix to his 'Obstetric Medicine and Surgery.'

During the period of 1828 to 1850 (both inclusive), being a period of twenty-three years, the numbers were—

Total number of children born . .	49,538
Of these there were born alive . .	47,716
„ stillborn . .	1822

Proportion 1 stillbirth to 26¼th living births, or 3·678 per cent.

Years.	Total births.	Stillbirths.	Proportion.
1828	2422	106	4·376
1829	2230	95	4·26
1830	2247	79	3·5
1831	2179	88	4·04
1832	2377	90	3·79
1833	2641	85	3·218
1834	2473	79	3·19
1835	2327	86	3·7
1836	2292	90	4·
1837	2166	87	4·01
1838	2161	85	4.
1839	2097	85	4·05
1840	2200	80	3·636
1841	2194	87	3·965
1842	2110	69	3·27
1843	2025	72	3·555
1844	1959	60	3·062
1845	1842	64	3·474
1846	1800	58	3·222
1847	1767	62	3·509
1848	1987	69	3·47
1849	2165	88	4·065
1850	1887	59	3·13

Highest average, 4·376; lowest ditto, 3·062.

The proportion of stillbirths is, as will be seen, very low, much lower than the general average of those countries from which the details we have given above have been derived. I have given the numbers for each year of the whole period of

twenty-three years, and also made a calculation to show the varying percentage of the stillbirth ratio. It will be observed that the first year of the series furnishes the highest average (4·376), while the last year furnishes the lowest average with one exception.

Of the stillbirths—

478 were premature
230 „ putrid at full time
253 „ breech presentations
97 „ transverse presentations
50 „ under placental presentations
85 „ „ accidental hæmorrhage
17 „ forceps cases
60 „ craniotomy
18 „ born under convulsions
62 „ „ very lingering labour
9 „ „ ruptured uterus or vagina
128 „ „ prolapsed funis
11 „ „ face presentation
30 „ monstrous

Although I have a very strong opinion that for a fully-matured child to be born dead when the head presents and there is no unnecessary delay is comparatively rare, yet I cannot but consider the above figures as most exceptionally favorable. I look upon a mortality of 4 per cent. as the lowest we can reasonably expect even in these days, where the more frequent and earlier application of the forceps must conduce very much to a reduction of the number of infants born dead. Of the above number the following must be considered as the numbers of unavoidable stillbirths :—

Premature	478
Putrid	230
Placental presentations	50
Accidental hæmorrhage	85
Craniotomy	60
Ruptured uterus, &c.	9

Prolapsed funis	128
Monsters	30
Total	1070

or a percentage of 2·159 on the total births; while a considerable proportion of the following must be considered as partly unavoidable :—

Breech presentations	253
Transverse	97
Convulsions	18
Face presentations	11
Total	379

leaving the following as what may be considered as avoidable cases of stillbirths :—

Forceps cases	17
Very lingering labour	62
Total	79

Of course I am fully aware that the above are open to exception. Thus, premature birth and prolapsed funis are not necessarily fatal to the infant; and it is possible that, had Dr. Ramsbotham been living now, the comparatively small number of stillbirths might have been even smaller still. But I am here dealing with generalities, and I think I am paying a proper tribute to that gentleman's unrivalled experience by presuming that every care and skill were exercised in order to preserve the life of child as well as mother.

I will now give the statistics of the Guy's Hospital Lying-in Charity as detailed by the late Dr. Lever in the 'Reports' of that hospital 1841 and 1849. The first septennial period comprises the years 1833 to 1840, both inclusive, and the first few years show the following results :—

Years.	Total births.	Stillbirths.	Percentage.
1833-4	174	13	7·471
1834-5	478	34	7·113
1835-6	630	34	5·4
1836-7	643	26	4·043

During the whole period of seven years the numbers were—

Children born alive	4432
„ stillborn	263
Total	4695

the proportion being 5·6 of stillbirths to every 100 children born. Dr. Lever contrasts this with Dr. Collins' statistics of the Dublin Rotunda Hospital, which show a percentage of 6·7 in a total of 16,651 births, and with Casper's statistics of foreign states which vary from 11 to 36 births for 1 stillbirth. Dr. Lever proceeds to give the following details respecting the above 263 stillborn children :—

First septennial period.

Vertex presentations	103
Face „	6
Premature labour	40
Breech presentations	30
Foot „	16
Arm and shoulder	8
Arm with funis	2
Funis presentation	4
Placental „	7
„ with perforation	1
Hydrocephalus	1
Delivered by vectis	1
„ forceps	5
„ perforator	24
„ secale	4
Ruptured uterus	3
Flooding	2
Twins	6
Total	263

The statistics of the second septennial period comprise the years 1840-47, both inclusive, with the following results :—

Children born alive	6305
„ stillborn	353
Total	6658

or at the rate of 5·3 stillbirths in a total of 100 births. The cause of stillbirth for the second period is as follows :—

Second septennial period.

Vertex presentations	186
Face „	4
Premature labours	19
Perforation after induction of premature labour	2
Breech presentations	29
Feet	25
Knee	1
Shoulder and arm	5
Arm and funis	3
Funis	7
Placenta	5
Thorax	1
Vectis	1
Forceps	6
Perforator	32
Flooding before delivery	1
Convulsions „	5
Twins	20
Triplets	1
Total	353

There is, as will be seen, a slight improvement in the ratio of stillbirths for the latter period, though only at the rate of ·03 per cent. It seems to me that the rate of infantile mortality must be very considerably affected by the treatment adopted in the second stage of labour; if prolonged, the result must be hazardous to the child.

Let us look at the proportion of cases, where the forceps were applied, in Dr. Ramsbotham and Lever's statistics repectively.

	Total births.	Forceps cases.	Children living.	Children dead.
Royal Maternity Charity.....	49,538	73	56	17
Guy's Lying-in ditto, first septennial period	4,695	9	4	5
Ditto, ditto, second septennial period	6,658	23	17	6
Total.....	60,891	105	77	28

Let us now turn from the statistics of town practice to those of country practitioners. Very candid and full ones are given in the 'Lancet' for 12th November, 1859, by the late Dr. R. Uvedale West, to whose memory so graceful a tribute was paid by your late President in his annual address. The subject we are now considering attracted considerable attention thirteen years ago, and the following passage occurs in a leading article of the 'Lancet' for the 22nd October in that year 1859.

"In the Dublin Lying-in Hospital a record is preserved of stillbirths, and the important distinction is noted between children born dead and those born *dead* and *putrid*. During seven years the proportion of stillbirths in this institution was 1 in 14. Of the total number as nearly as possible one half were both dead and putrid; but hospital practice is but a fallacious test of what obtains in the homes of the population. Thus, in a clinical report of the Royal Maternity

Charity of London, addressed by Dr. Barnes to the Dublin Obstetrical Society, in which this question is examined, it is shown that in the practice of this Charity the stillbirths stood in the proportion of 1 in 34 only." (This it will be seen is a lower rate than that given by Dr. Ramsbotham as existing nine years previously, where the rate was 1 in 27.) The article proceeds, "Taking this as the basis of his calculation he estimated that there were not less than 2658 stillbirths annually in London, no account being taken of abortions. It is an object of the highest importance to test this conclusion by comparison with the experience of other institutions, and still better with the experience of private practitioners. If a certain body of medical men, such as those who constitute the Obstetrical Society of London, would collect their individual results, we might at once be in possession of facts on a scale sufficiently large to be enabled to define with some precision the prevalent rate of stillbirths." In consequence of the above remarks Dr. West published his statistics.

Total births	2998
Stillbirths	111

Or 1 in 27, or 3·7029 per cent.

Of these 111 stillbirths there were:—

Putrid at birth	50
Craniotomy cases	8
Footling cases	7
Funis prolapsed	7
Placenta prævia	3
Protracted labours left to nature . .	6
Forceps cases	2
Monstrosity	2
Accidentally suffocated before arrival . .	2
Imperfectly developed head . . .	2
Anasarcous and ascitic . . .	1
Hæmorrhage during labour . . .	4
Convulsions „ „ . . .	2
Latent compression of funis . . .	1
Arm presentation and turning . .	1

Mothers moribund	3
Placenta putrid or diseased	3
No apparent cause	7
Total	111

In the concluding paragraph Dr. West, with a candour which reflects the greatest honour on his memory, says, "I have thought it important in the study of the causes and frequency of the mortality of children during labour to insert in my table the number of each case. It may thus be seen at a glance how far the inexperience of a young practitioner may contribute to an increase of such mortality. Let us for example examine the number of cases of craniotomy. It will be seen that all the cases that I was exclusively responsible for fall within the first 1000. How far a gradually acquired ability in the use of the vectis and forceps may have enabled me to discard this *wilful murder* from my practice I leave to the judgment of my readers, pointing out at the same time that *pari passu* with what I may call the abolition of craniotomy, or at any rate the greatly diminished frequency of the practice, the fœtal mortality from unassisted protracted labour disappears also from my practice. These are matters for grave consideration. . . . . I have fearlessly and without any extenuation given my experience of stillbirths, as invited by the leading article quoted. It is obvious that nothing but a sense of public duty, and a regard for the interests of absolute truth, can have induced me to parade, as I have here done, the shortcomings of my early career—'*Liberavi animam meam.*'"

A percentage of 4 per cent. is, according to Dr. West, the highest proportion which average county practices should exhibit. Dr. West's example was followed by Dr. Anderson Smith,[1] who gives a percentage of 4·924 stillbirths out of a total of 1320—

[1] 'Lancet,' Nov. 12, 1859, p. 481.

Total births	1320
Stillbirths	65

Or 4·924 per cent.

Forceps cases	25
Craniotomy	3
Version	4

Causes of Stillbirth.

Putrid	24
Malpresentation and position	10
Prolapsed funis	6
Craniotomy	3
Hæmorrhage before delivery	10
Compound presentation	1
Premature	5
Unaccounted for	6
Total	65

The relative proportion of living and dead children delivered by the forceps is not stated.

By Mr. Hadaway, who shows a ratio of 10 per cent. in 732 cases—

Total births in the five years 1855-9, both inclusive	732
Stillborn	72

Or 10 per cent.

Detailed as follows—

Year.	Births.	Stillbirths.
1855 ...	93 ...	6
1856 ...	129 ...	16
1857 ...	171 ...	20
1858 ...	169 ...	18
1859 ...	170 ...	12

Causes of the 12 *stillbirths in* 1859.

Premature (seventh month)	4
Footling and cord	1
Supposed fright to mother	2
Unknown	5
Total	12

Mr. Lambden, who gives a ratio of 3·319 in 482 cases—

Year.	Births.	Stillbirths.
1855	90	4
1856	114	1
1857	88	2
1858	101	3
1859	89	6
	482	16

Or 3·319 per cent.

Details of the above 16 cases of stillbirth:

1855	Putrid	1
	Turning	2
	No cause	1
1856	Putrid	1
1857	Arm and funis	1
	Placenta prævia	1
1858	Placenta prævia	1
	No cause	1
	Turning	1
1859	Putrid	3
	No cause	2
	Version	1

Dr. Smith cites his number of forceps cases, 25 in 1320, as high, but states that no ill effect followed. I think we

are all agreed that a much higher percentage of forceps cases would not only fail to startle us, but would very probably enable us to dispense with unaccounted cases altogether, or considerably to reduce their number.

To sum up the results of the foregoing we find that continental statistics give a general average of rather less than 5 per cent., which must be considered small when we remember that this ratio includes abortions in some countries, and premature children born alive in others; while in the statistics of British practice, which I have been able to procure, the stillbirths, pure and simple, range from 3 up to 10 per cent. I have already alluded to the small number of forceps cases in the statistics given by Dr. Ramsbotham, of the Maternity Charity practice, and by Dr. Lever, of those of the Guy's Lying-in Charity. The former gentleman attributes 62 stillbirths as due to "very lingering labour," and it is lamentable to contemplate the fact that of 49,538 births the forceps cases numbered only 73, while the cases of craniotomy reached 60. Again, Dr. Lever gave us 9 cases of forceps and 24 cases of craniotomy in 4695 births; while in 6658 births he gives 23 cases of forceps and 32 of craniotomy. Out of the first series we have 103, out of the second 186, together 289 stillbirths where the vertex presented, which were neither forceps nor craniotomy cases, and which were, no doubt, cases of lingering labour. From Dr. Churchill's statistics I find that the forceps cases in British practice were as 1 in 171; in French practice, 1 in 140; in German practice, 1 in 106; and the results to the children born are shown as follows:

	Forceps cases.	Children lost.	Percentage.
Dr. Ridler	4223	684	16·197
Dr. Siebold	312	47	15·064
Dr. Harper	302	43	14·238
Dr. ——	232	28	12·07
Drs. Johnston and Sinclair	200	29	14·5
Prof. Schwerer	194	48	24·472

	Forceps cases.	Children lost.	Percentage.
Dr. Beatty, senior	111	0	
Madame Boivin	96	20	20·833
Dr. Pagan	82	19	23·1708
Madame Lachapelle	79	23	29·114
Dr. F. Ramsbotham	73	17	23·287
Dr. Smellie	52	9	17·308
Dr. Roberton	43	7	16·28
Dr. R. Lee	42	31	73·81
Dr. Arneth	45	14	31·111
Mr. Smart	30	13	43·333
Dr. Brunatti	23	6	26·08
Dr. Lawrence	28	4	14·285
Dr. Merriman	21	6	28·571
Dr. Ritgen	20	4	20·
Dr. Boer	19	5	26·316
Mr. Perfect	18	4	22·222
Drs. Hardy and M'Clintock	18	8	44·444
Dr. Hall Davis	15	3	20·
Edinburgh Lying-in Hospital	15	5	33·333
Dr. Churchill	9	0	

It will be seen at a glance that, with few exceptions, the percentage is lowest where the number of forceps cases is the highest, and Mr. Harper has shown this very conclusively in a paper read before this Society thirteen years ago on "The Use of the Forceps as a Means of Lessening Mortality." The results to the children are thus summarised by Mr. Harper, who contrasts his practice with that of Drs. Collins, Hardy, and Johnstone respectively:

Forceps cases.	Fœtal deaths.	Maternal deaths.	Forceps duration.
Collins 1 in 694	1 in 26	1 in 329	38 hours.
Hardy 1 in 355	1 „ 20	1 „ 334	35½ „
Johnston 1 in 60	1 „ 35	1 „ 502	29½ „
Harper 1 in 26	1 „ 47	1 „ 1490	16 „

I have preferred to give the results of practitioners of experience in preference to any of my own, which comprises too small a number of cases and too short a period upon which to form data. But, speaking generally, I may say that I have invariably been able to account for stillbirth after a labour at the full time with a vertex presentation, by delay in seeking medical assistance, and I hope on a future occasion to bring the results of my experience before this Society. But for the present I think enough has been said to show conclusively that a more frequent employment of the forceps must tend to reduce the number of children stillborn.

From inquiries I have made at the local cemeteries and burial grounds I find that the number of stillborn infants annually interred there amounts to at least 6 per cent. on the total number of births occurring annually within the borough, but this must be considered as below the actual number, as it must be impossible for any one but an official inquirer to acquire full and accurate information.

I have already stated that the registration of stillbirths in this country is contemplated and under the consideration of the president of the Local Government Board. I trust that the Fellows of this Society will agree with me that it behoves us to anticipate this proposed measure, and make such suggestions as shall enhance the value of the returns made. The registration of stillbirths is desirable on three grounds—1, medico-legal; 2, obstetrical; 3, statistical; which may be again subdivided as follows:

1. Medico-legal.

(*a*) To place a check upon abortion, fœticide and child murder, by preventing the burial of any stillborn infant unless full particulars of the birth were furnished to the district registrar.

(*b*) To prevent frauds upon the authorities at cemeteries and burial grounds by false certificates of stillbirth.

2. Obstetrical.

(*c*) To enable us to judge of how far stillbirths are preventible by

(*d*) The proportionate ratio in the practice of medical men and midwives, and the qualifications of the latter;

(*e*) The relative ratio in town and country, and in different districts of the same town and country;

(*f*) The proportion of stillbirths due to syphilis;

(*g*) The effects of premature, protracted, complex, and instrumental labour upon the stillbirth rate.

3. Statistical.

(*h*) To obtain more accurate returns than at present is possible of children actually born living or dead. At present children born living are interred as stillborn to an extent quite unknown, and sufficient to make a considerable difference in the returns of birth.

(*i*) To enable us to compare our returns with those of continental and other states.

Many other advantages may be looked for from this proposed amendment of our Registration Act, but the above are sufficient to recommend it to the legislature on the one hand and to ourselves on the other, as calculated to afford us material of great use to us in an obstetric point of view. I hope the subject will be considered of sufficient importance to justify me in asking that a committee may be formed of Fellows of the Society, whose duty it shall be to collect

statistics of stillbirths as at present known; to consider how the proposed registration may be effected so as to ensure all the desired information; to suggest the necessary forms of certificates for medical practitioners and midwives; and to urge upon the President of the Local Government Board that no further delay may take place in securing for the country by compulsory measures the registration of all births whether of living or of dead children.

Dr Parkes
with my kind regards.

ALBUMINURIA

IN CASES OF

VASCULAR BRONCHOCELE AND EXOPHTHALMOS.

BY

J. WARBURTON BEGBIE, M.D.

READ BEFORE THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH, 4TH MARCH.

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ALBUMINURIA

IN CASES OF

VASCULAR BRONCHOCELE AND EXOPHTHALMOS.

THAT albuminuria may exist independently of Bright's disease, and be therefore unconnected with any structural change in the kidneys, is a proposition which few physicians, if any, will be prepared to call in question.

No doubt, the presence of albumen in the urine is the most significant feature in the symptomatology of Bright's disease, otherwise there could have existed no warrant whatever for the employment of the term as a synonym for that disease. The use of the expression in this way is however, faulty, and will become more and more unsuitable as the discovery of albumen in the urine, under other circumstances than those which indicate the existence of a grave renal disease in one or other of its different forms, is made. The presence of albumen in the urine as a notable feature in some cases of vascular bronchocele and exophthalmos has been familiar to me for some time; while a very special interest is attached to the circumstances under which the albuminuria has in a few instances been met with.

I have been unable to find any reference to the association of an albuminous condition of the urine with those remarkable symptoms, the union of which characterizes the disease now well known through the writings of Graves, Stokes, Basedow, Begbie, Trousseau, and other writers. This is, I believe, only to be accounted for by the circumstance that the condition of the urine has hitherto in this disease not been made the subject of any careful examination. It will of course be understood that I exclude from this reference the albuminuria of passive renal congestion, which is so often witnessed in cases of heart-disease, and likewise when the primary

vascular impediment is seated in the lungs, and sometimes also in the liver. From this reference I also exclude those instances of vascular bronchocele and exophthalmos in their advanced stages, in which the heart, and, in their turn, not unfrequently the kidneys, have become involved in organic changes. In the latter, dropsy and albuminuria are constantly present, but under these circumstances they are to be properly regarded as symptoms, not of the disease itself, but of those complications which are prone to occur in its course.

The albuminuria incident to cases of vascular bronchocele and exophthalmos in their earlier stages is essentially a temporary albuminuria, and is evidently unconnected with any form of renal degeneration. But although temporary, disappearing as the other symptoms of the disease are either relieved or removed, the albuminuria has generally been considerable in degree, and sometimes even excessive.

In this respect the coagulability of the urine in cases of vascular bronchocele and exophthalmos has presented a remarkable contrast to the temporary albuminuria which is found in certain other diseases. I have indeed never seen so large an amount of albumen in the urine in any other disease, when the cause of the albuminuria was not inflammatory or organic.

My attention having been called to the association of an albuminous condition of the urine with vascular bronchocele and exophthalmos, in the first instance, owing to the existence, in certain cases, of œdema of the feet and ankles, I became satisfied that the association is by no means of infrequent occurrence. I have found albuminuria in a considerable number of the cases which have lately fallen under observation, and it is more than likely that it may have existed in other instances, although unrecognised from causes to which reference will be made. Albuminuria has existed in persons of both sexes suffering from this disease, and has been more common in cases of female than male patients; but when the much greater frequency of vascular bronchocele and exophthalmos among women than men is kept in view, the symptom has been present in a larger proportion of males than females. In some it has been an evanescent symptom, lasting only for a short time, and when so, only present in limited degree. In others, the albuminuria has been very considerable—it has even been excessive, and it has lasted for weeks, indeed for months—while the other notable symptoms of the complex malady continued, and only disappeared as the latter became relieved or removed. Œdema of the lower limbs, although in the first instance calling attention to the condition of the urine, has not been observed to bear any constant relation to the albuminuria; on the contrary, œdema, and sometimes considerable anasarca of the legs, have been present without any appearance of albumen in the urine; and albumen, when present, has generally existed without any form of dropsical swelling. In the most

notable cases of albuminuria in connexion with vascular bronchocele and exophthalmos, dropsy has not been present.

In prosecuting my inquiry on this subject, a very interesting circumstance became manifest, namely, that the albuminuria was in certain cases limited to the period of digestion—present immediately after a meal, and absent when the person fasted. I had in one case been not a little puzzled by noticing the strange variety presented by the urine within very short periods—the albumen present in considerable quantity one day, and absent the next—present in the urine of the forenoon, and not to be detected in that passed before dinner. By obtaining repeatedly specimens of the urine in this case, and in one or two others, I was able to satisfy myself that in this disease the albuminuria is apt to possess the remarkable character of only occurring during or immediately after the digestion of the food.

This appears to me to be a most interesting feature, and it is also in various ways a very important one. The existence of albumen might readily escape detection if the physician relied on the results of one examination, as he is not unapt to do; and even when taking more than usual care, it is quite possible that in several specimens of urine furnished by the same patient for examination it might be found that no albumen existed. Conceive a patient so affected consulting a medical man before breakfast or luncheon; his urine, carefully examined, found to be non-albuminous; but, from some cause or other—and we know how fickle some patients are—calling for another physician shortly after a meal, when a large precipitate of albumen occurred on applying heat as well as on the addition of a little nitric acid. Remarkable as this character of the albuminuria which occurs in cases of vascular bronchocele and exophthalmos is, there is another feature pertaining to it which is even more striking. The albumen is present in much larger quantity after breakfast than after either luncheon or dinner. In one case of this kind, which I was able to observe at intervals for a considerable time, the urine presented the following characters:—It was passed in average amount, was of healthy colour, reaction, and density. On no occasion was the density observed to fall below 1015, and it never rose above 1025; the average density was that of health, 1020. Very occasionally this urine deposited lithates, and from time to time contained a slight excess of earthy phosphates. Sugar was never present. Albumen existed in this urine daily for upwards of a twelvemonth, but only at certain times of the day, and these times were readily found to be shortly after meals. After breakfast, however, the amount of the albumen was invariably greater than after luncheon, dinner, or an evening meal. Being greatly interested in these peculiar phenomena presented by the urine in the case of a gentleman whom I had recommended to spend the spring months in the south of England, I requested him to take an opportunity of consulting Dr George Johnson of London, whose opinion

in all departments of urinary pathology is so deservedly held in high esteem. In connexion with this case, Dr Johnson wrote to me as follows:—"I saw two days ago a patient of yours, who asked me to write to you. As he told me that the explanation which I gave him of his symptoms is essentially the same as that which you gave him, I need not enter into minute detail in writing to you. I found that the urine passed after his breakfast contained a large amount of albumen; that passed at 3 P.M. contained none; that in the evening, three hours after dinner, contained a small quantity. He has been overworked, and is evidently a nervous, excitable man. I conclude that there is no structural change in the kidney, but that his kidney is irritated and congested at intervals during the process of excreting the products of faulty digestion. I confess that I am at a loss to account for the fact of the urine being more constantly and copiously albuminous after breakfast than after dinner. I repeated to him the usual directions as to avoiding cold and wet and fatigue, and long fasting. I also advised him to make trial of an exclusively milk diet. He told me that you had made the same suggestions to him. I prescribed a mixture of tincture of quinine and tincture of nux vomica, acid hydrochloric diluted, and syrup of ginger, to be taken after food, and advised him to take an occasional dose of chloral hydrate when he is restless or disturbed by dreams.

"It occurs to me to ask whether the breakfast taken after a long interval from the previous meal, and when, consequently, the absorption of materials is likely to be more rapid, gets into his vessels quickly, and in a crude and half-digested state? If this be so, food taken at shorter intervals would seem to be indicated. The urine passed in my room an hour and a half after breakfast became nearly solid with heat and nitric acid. I have often met with cases in which the urine has been albuminous only after food and exercise, but I have not before met with one in which the *breakfast* appeared to be so especially noxious to the kidney."

A brief account of the case to which Dr Johnson alludes in his interesting communication just quoted, will now be given, before offering certain considerations which its observation has suggested.

A gentleman, of about thirty years of age, had long been in a somewhat delicate state of health, although no particular attention had been called to his symptoms till the summer of 1871. He was then suffering from debility, great nervousness, and palpitation of the heart. These had succeeded a condition of looseness of the bowels, which had existed for a considerable time. The prominence and peculiar expression of the eyes in this gentleman were, at the time now referred to, quite characteristic, and suggested the very probable discovery of a goître. This, although the patient had been quite unaware of its existence, was readily detected in the form of a soft pulsating tumour on both sides of the neck. The visible pulsation of arteries in the neck and limbs was present

in a notable degree. On examining the urine of this gentleman, it was found to contain a considerable amount of albumen; but speedily the discovery was made that the albuminuria was not persistent, but, on the contrary, variable in its occurrence. The urine passed after meals was found to be more constantly and highly coagulable than that passed while fasting, and at times the urine was found to be free from albumen. Dr Henderson of Helensburgh, under whose care this gentleman had been, had noted the variable character of the albuminuria, and had distinctly traced its occurrence to the periods of digestion. Both before and after this gentleman's visit to the south of England and residence on its coast, rest from professional duties—which, in his case, were arduous—had been enjoined, and various experiments in diet and regimen had been practised. He had also several remedies in addition to those prescribed by Dr Johnson. From bromide of potassium and belladonna he undoubtedly received benefit, but more especially from digitalis. To the latter, indeed—which, in the form of its tincture, he took persistently for months, in doses of from ten to twenty drops thrice daily, frequently combined with iron, and sometimes alone—he has himself ascribed the chief benefit. Under its use the prominent eyes have retired, the goître has nearly if not entirely disappeared, the cardiac pulsations have fallen from 140 per minute to the normal standard, and the distressing palpitation from which he suffered has been succeeded by calmness in the heart's action. The nervousness and apprehension so characteristic of his malady, and which in his case were often most distressing, have given place to mental calmness and tranquillity. He has become greatly changed for the better in appearance; having been formerly very notably thin, he has now become very fairly nourished. He has returned to his professional duties, and during the past winter has discharged them regularly and comfortably. For months there has been no appearance of albumen in his urine. The last time I had an opportunity of examining the urine, which was passed shortly after breakfast, it contained no trace of albumen. The recovery in this gentleman has been complete.

It is surely a satisfactory consideration that a condition of excessive albuminuria—the urine becoming nearly solid on the application of heat and addition of nitric acid—may, after all, not indicate the existence of any structural change in the kidney. Of course, in connexion with the albuminuria, the presence or absence of certain other important features must, under such circumstances, be taken into account. Apart from the intermittent character of the coagulability, the facts that the quantity and density and colour of the secretion did not deviate from the healthy standard, and still more, that diligent and repeated examination by the microscope failed to detect the vestige of a cast of any kind, were to be regarded as the proofs of the renal derangement being functional and not organic. Still, there is occasion for reiterating the assurance

that albuminuria is not Bright's disease, and for pointing out that, when unconnected with the presence of blood or pus in it, there may be even a highly coagulable condition of the urine, due to causes which are wholly independent of any structural change in the renal substance. Such, I am persuaded, may confidently be affirmed of the albuminuria which is apt to occur in cases of vascular bronchocele and exophthalmos.

Some little time after my attention had been called to the peculiar features of the albuminuria which is incident to cases of vascular bronchocele and exophthalmos, and to which reference has now been briefly made, a very instructive instance of the malady fell under my notice in the person of a medical man, who had been for some years engaged in active practice in the south of England. Calling one day for advice, the gentleman in question told me that he was the subject of Bright's disease, and feared that little or nothing could be done for his relief. He certainly looked ill, was thin and sallow in appearance, and evidently deeply depressed in spirits. Having, however, noticed, as he entered my room for the first time, that he possessed the prominent eyes, with peculiar staring expression, so characteristic of vascular bronchocele and exophthalmos, I ventured—in reliance on my previous observations—to offer the comforting suggestion, that possibly, if not probably, the presence of albumen in his urine might be due to causes which were capable of being removed, and did not indicate the existence of any serious disease. This remark he received with politeness, but with very evident incredulity, mentioning that his condition had already been condemned by medical authority, that he did not expect to be cured, but only felt justified in expecting a little prolongation of life with greatly impaired health. On carefully examining this gentleman, a bronchocele of considerable size, soft, and pulsating, of whose existence he had been unaware, was discovered. His pulse was small, and as frequent as 140 per minute. The urine, on its earliest examination, while the patient was fasting, and between 1 and 2 P.M., was, to his own surprise, found to be free from albumen, its density 1020, and of acid reaction. Subsequent examinations of the urine determined its decided coagulability after meals, and its freedom from albumen while abstinence from food was practised. This gentleman was exceedingly nervous, and very desponding. Under treatment a considerable improvement took place. As he had occasion to pass through London, I begged him to see Dr Johnson, and it is to him Dr Johnson alludes in a letter of date 27th October 1872, from which I now quote:—"There is a striking resemblance between his case and that of the Scotch clergyman whom you were so good as to send to me. Dr —— is extremely feeble and nervous, and I fear that the prognosis is bad. I quite agree with you, that a long sea voyage would be the best course for him, but he seemed unwilling to do anything that would separate him from his family." I have not

seen this gentleman for some time, but his progress can be traced in the correspondence I have had with him. On the 28th of March 1873 he wrote:—"I have gone very comfortably through the winter, considering all things. I am much stouter than when you saw me, and much stronger. I do a good deal of walking in the course of the day. My heart still beats very rapidly, but its action is not so irregular as formerly. Whether albuminuria exists, I cannot say, as I never test for it, and try to banish the thought of it altogether from my mind. The goître is decidedly less in size, but is still visible."

I obtained two specimens of urine passed on the 10th of April 1873. That voided before food was taken, contained deposit of lithates, and was absolutely free from albumen. That passed an hour after breakfast had a density of 1016, and contained a very faint trace of albumen. The letter which accompanied the specimens of urine will exhibit the peculiar nervousness under which my friend still laboured. "I send you," he wrote, "two little bottles with urine which I passed to-day. Please do not send me a bad report, as it will only frighten me. I dread the thought of renal disease so much, that I try to banish the very existence of kidneys from my thoughts. That I shall look with anxiety for your next letter, and yet dread its arrival, your knowledge of my nervous condition will assure you." Happily, I had no occasion to send "a bad report," while, in consideration of his highly sensitive and nervous state, I wrote over the seal of my communication, "good news."

Since the summer of 1873, this gentleman's condition has steadily improved. On the 28th October he wrote:—"I am glad to be able to tell you that my wife was confined on the 27th ultimo, and has done very well indeed. For myself, the only trouble I now have is occasional palpitation." Again, on the 8th December:—"My general health is now very good. I am as fat as ever I was, and my eyes have lost that unnatural stare. There is still a slight enlargement of the thyroid gland, and sometimes—though rarely—my kidneys act very little. Digitalis and iron remedy this. My heart still beats fast." My last communication, of date 17th Jan. 1874, gave a most satisfactory account of his progress, and was written a few days before he embarked as surgeon of a ship sailing with the royal mails for Madeira, Ascension, and the Cape of Good Hope. Before obtaining this appointment, my friend was obliged to go before the medical officer of the General Post-Office for examination as to personal and professional fitness. In this communication he says:—"I find no remedy relieve me so much as bromide of potassium, in 20-grain doses, thrice daily. Aconite and digitalis do not relieve me much."

This case is certainly a very gratifying and encouraging one. Not only has there been an almost entire disappearance of albumen from the urine, but the other symptoms from which the patient

suffered in very notable degree—the palpitation of the heart and throbbing of the arteries, the bronchocele and exophthalmos—have all become very greatly lessened, and will, I fully expect, soon entirely disappear. This hope is justified by the results of experience in other cases. It has occurred to me to witness, within the past two years, three cases of vascular bronchocele and exophthalmos, all in females, in two of which albuminuria existed, gradually improve under the same treatment as that which has been pursued in the instances more fully detailed, and ultimately in these a complete cure has been obtained. In the second patient, whose case has been narrated at some length, renal casts were said to have been seen. This may very possibly have been so, but repeated careful examinations of the urine, made while he remained under my care, failed to detect their presence. I have, indeed, in no instance of the albuminuria occurring in cases of vascular bronchocele and exophthalmos, found renal casts. This remark applies equally to the examples of excessive albuminuria, and the more numerous instances of the disease in which the coagulability of the urine has neither been great nor long-continued. Albuminuria is not a constant symptom of vascular bronchocele and exophthalmos, but it is a frequent one. I am inclined to think that it may hereafter be found a more frequent symptom than my own observations presently entitle me to call it. From the circumstance that I have had no opportunity of carefully watching some instances of vascular bronchocele and exophthalmos which have recently fallen under my notice, having in such been able to make an examination of the urine only on a solitary occasion, it is very probable that albuminuria may have escaped detection. It existed in a patient of Dr Rosa, whom I saw lately, a married woman, æt. 36, the mother of seven children, in whom the malady succeeded the occurrence of lienteric diarrhœa and prolonged lactation. Dr Affleck informs me that he has found albuminuria, of the nature I have been describing, to exist in a sufferer from vascular bronchocele and exophthalmos recently under his care.

Having indicated the nature of the albuminuria which is found in certain cases of vascular bronchocele and exophthalmos, it now becomes necessary to inquire a little more fully into its pathology. Albumen is, in all probability, not a constituent of healthy urine. It has, indeed, been stated by Dr Gigon of Angoulême, that albumen exists in normal urine, and can be thrown down by chloroform. Becquerel, Aran, and Parkes have satisfied themselves that the precipitate which is produced by the addition of chloroform to the urine is not albumen, but a mixture of chloroform, mucus, and organic substances. Albumen, however, is so frequently present in the urine, and occurs under so vast a variety of circumstances, that it becomes a matter of very great importance to determine its clinical significance. The existence of blood, pus, or spermatic fluid in the urine renders it coagulable; but it is scarcely necessary to state

that, in the cases of vascular bronchocele and exophthalmos to which I have referred, there were none of these conditions. Again, the excessive use of a diet composed chiefly or entirely of albuminous matter, such as eggs, has been found by various observers—among others, Barreswil, Hammond, and Brown-Séquard—to produce albumen in the urine. Barreswil, after taking ten eggs, passed albuminous urine for twenty-four hours. There can be little doubt that, in some persons peculiarly constituted, the partaking of certain articles of food difficult of digestion by them, produces albuminuria for a time. Of this nature was the case of the student mentioned by Sir Robert Christison, in whom a large amount of cheese or pastry produced albumen in the urine. Apart, however, from errors of diet, as Dr Parkes has stated, temporary albuminuria will occur in persons with very slight disease. Beneke, when suffering from dyspepsia, noticed albumen in his own urine four times in four weeks. Clemens, Rayer, Martin Solon, and many other physicians, have made similar observations. It may be admitted, then, that albumen, although not an ingredient of healthy urine, may occur in the urine of healthy persons, or of persons whose disorder of health is, at the time of its presence, very slight. Attention has recently been called by Dr George Johnson to the occurrence of albuminuria in healthy persons after bathing in cold water. Again, albuminuria is apt to occur in relation to a great many disorders which are not essentially connected with structural change in the kidneys. Not to dwell upon pregnancy, as a condition of the system with which albuminuria is associated, there is the puerperal state. Again, there is a large number of febrile and inflammatory diseases in the urine of which albumen very often occurs. Among these may be mentioned scarlet fever, measles, erysipelas, smallpox, diphtheria, typhus and typhoid fever, cholera, etc., and, of inflammatory diseases, pneumonia. Over and above these relationships, there are various forms of visceral disease—of disease affecting the heart, liver, and lungs—in the urine of which albumen appears; and the physician, in his observation of such cases, is on the outlook for its occurrence, and ascribes it, when it does come, to the general impediment to the circulation which the following diseases—(I name them as illustrative examples only) dilatation of the heart, cirrhosis of the liver, emphysema of the lungs—produce; for, owing to these, the renal circulation necessarily suffers. In such diseases, the albuminuria is almost invariably associated with a diminished secretion of urine. It is not necessarily so in the albuminuria of vascular bronchocele and exophthalmos,—the quantity is generally unaffected, and so are the other characters of the urine.

Dr Roberts of Manchester has very clearly pointed out, that, in endeavouring to determine whether the presence of albumen in the urine be dependent upon the existence of organic disease of the kidneys or not, the question in each individual case must be considered in connexion with the three following points:—1. The temporary

or persistent duration of the albuminuria; 2. The quantity of the albumen present, and the occurrence and character of a deposit of renal derivatives; 3. The presence or absence of any disease outside the kidneys which will account for the albuminuria.[1]

Now, viewing the albuminuria of vascular bronchocele and exophthalmos under these aspects, the following observations may be made:—1. The albuminuria is temporary; for, according to Dr Parkes's definition of that condition, it has totally disappeared while the patient is under observation; but, instead of lasting a few days or weeks, as holds true of most instances of temporary albuminuria, properly so called, it has lasted for many months, indeed for a year. Intermittent or remittent albuminuria would be a better signification than temporary, for the albuminuria of vascular bronchocele and exophthalmos, but, better still, because more definite, albuminuria occurring during or after digestion. 2*d*, The amount of albumen which is present in the urine when the cause of its manifestation is independent of organic or inflammatory disease is usually small; very often it is not more than a mere trace. The amount of albumen in the urine when passive congestion of the kidneys results from cardiac or other visceral disease may indeed be considerable, but I do not remember ever to have seen the urine under such circumstances very highly coagulable; very highly or excessively albuminous has, however, been the character of the urine in at least one example of vascular bronchocele and exophthalmos, the recovery in which has been complete. Again, in the temporary albuminuria of other maladies, there is generally some deviation, often notable, from the normal condition of the urine in other respects; the quantity, density, and reaction of the urine are often affected; and there is the presence of lithates in excess, or an undue amount of earthy phosphates. Not so, generally at least, in the cases of vascular bronchocele and exophthalmos which I have seen. With the exception of its containing albumen in considerable or large amount, the urine has been healthy. Neither has there been in the latter any deposit derived from the kidneys. In this respect, indeed, there is the interesting fact of a copious presence of albumen in the urine, without any trace of casts of one kind or another, and without any renal epithelium or blood. Further, the form of albuminuria which we are now considering, differs from any other form hitherto described, in being limited to the period of digestion of the food. 3*d*, The presence of disease apart from the kidneys is of course conspicuous in vascular bronchocele and exophthalmos, while the peculiar morbid condition of the nervous system and of the bloodvessels in that disease, as well as the spanæmia which exists, must, I think, be taken into consideration in our endeavour to determine the pathology of the albuminuria, which we now know to be in some way or other associated with it.

In vascular bronchocele and exophthalmos, there is always present

[1] On Urinary and Renal Diseases, 2d edition, page 172.

much disturbance of the nervous system—the sufferers from this disease are invariably highly nervous—they are often hysterical. The primary disorder of the circulation, both cardiac and vascular, is of the nature which we associate with derangement of the nervous system. The organs and parts of the body in which the local manifestations of disturbance are seated, are organs and parts freely supplied with bloodvessels and blood,—the thyroid gland, the spleen, which, although not invariably, is often affected, and the deep ocular tissues. To these must be added the kidney. From the failure of due nervous influence, the small vessels, and, it may be presumed, the capillaries, in the thyroid gland, and the deep-seated orbital vessels, become dilated, and the circulation through them in consequence interfered with. We can infer from the consideration of the essential nature of the renal circulation that if an obstruction to the return of blood through the inter-tubular capillaries and veins exists, either from an obstruction in the heart or lungs, or from a disordered state of the vessels themselves—a condition which I believe to exist in vascular bronchocele and exophthalmos—favoured by the more or less watery state of the blood itself, there will occur a transudation of serum, carrying with it albumen, through the walls of the Malpighian capillaries into the tubes, and thus the urine will be rendered coagulable.

But in order to explain the limitation of the albuminuria to the period during and after digestion of the food, it is necessary to regard the increased afflux of blood which then takes place, as leading to an altered physical relation between the blood and the walls of the vessels, and likewise determining an engorgement of the Malpighian capillaries, while the loss of tonicity in the efferent vessels is thus rendered temporarily more injurious. In other words, the renal circulation, in its comparatively tranquil condition, is unaffected by the disordered state of the capillaries and small vessels; but, when excited by the stimulus of a recent meal, it is unequal to the task, and the resulting interference determines the albuminuria.

But, further, there is something in the character of this albuminuria to ally it with the albuminuria of indigestion, to the occurrence of which a brief reference has been made. Sufferers from vascular bronchocele and exophthalmos have frequently an inordinate appetite and craving for food. They have bulimia; and, in the cases I have shortly recorded, this symptom was notably present. A large meal taken hurriedly is not unlikely to influence, as a remote cause, the production of the albuminuria. I think Dr Johnson's explanation of the greater amount of albumen in the urine after breakfast than dinner, may be correct—the same explanation had occurred to my own mind—that the earlier meal taken after a long fast gets into the bloodvessels quickly, and in consequence leads to a greater disturbance of the renal circulation. Besides, hot tea and coffee, with eggs, consumed at breakfast, may

be presumed to be articles more likely than others to furnish to the blood the offending material.

I have not attempted to exhaust this interesting subject, but have for the present limited myself to pointing out the occurrence of a form of albuminuria which, so far as I am aware, has not in its details been previously described by any observer, namely, albuminuria occurring during and after digestion in cases of vascular bronchocele and exophthalmos.

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ON THE ABOLITION OF

PRIVATE SLAUGHTER-HOUSES

IN TOWNS,

WITH SUGGESTIONS FOR FUTURE LEGISLATION ON NOXIOUS TRADES CONNECTED THEREWITH.

BY

WILLIAM HARDWICKE, M.D., M.R.C.S., &c.,

Medical Officer of Health for Paddington, Deputy-Coroner for Central Middlesex, and one of the Honorary Secretaries of the Public Health Section of the Social Science Association.

A Paper Read at a Meeting of the Health Department of the Social Science Association, on Monday, March 30th, 1874.

F. S. POWELL, Esq., in the chair.

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ON THE ABOLITION OF

PRIVATE SLAUGHTER-HOUSES IN TOWNS,

AND ON

FUTURE LEGISLATION FOR NOXIOUS TRADES.

THE present time is one peculiarly propitious for inviting discussion upon slaughter-house reform in the metropolis, and for urging upon Parliament the consideration of legislative measures affecting the slaughtering of cattle, as well as of several other noxious trades in close relation to the same. It is one of those departments of public health legislation in which the interests of many private persons are, more or less, affected on the one hand, and the health of people congregated in cities on the other. It would be a waste of time to enter minutely into all the legislative Acts bearing upon the regulation of offensive or noxious trades, for I may assume that most persons who are here present and disposed to take part in the discussion this evening are already informed upon the subject. The Metropolis Building Act of 1844, as far as the metropolis is concerned, has been very generally understood by the metropolitan vestries and the butchers themselves, to virtually abolish a greater part of the private slaughter-houses of the metropolis after the 9th of August this year, that is, thirty years after the passing of the Act. Although there may be some legal quibble as to the interpretation of clause 55, it enacts it will become illegal to erect or carry on any of the offensive or obnoxious trades specified in the Act in premises that are not beyond fifty feet of any first-class dwelling-house or forty feet from any open way.

The following is the clause:—

"LV.—For the purpose of making provision concerning businesses, offensive or noxious, be it enacted, with regard to the following businesses, that is to say, blood-boiler, bone-boiler, fellmonger, slaughterer of cattle, sheep, or horses, soap-boiler, tallow-melter, tripe-boiler, and any other like business offensive or noxious, so far as relates to the erection of buildings

in the neighbourhood of any such business, and so far as relates to the carrying on of any such business in the neighbourhood of any public way or of other buildings of the first or dwelling-house class. That it shall not be lawful hereafter to erect any buildings of the first or dwelling-house class nearer to than fifty feet from any building which shall be in use for any such offensive or noxious business; but if a building already existing within fifty feet be hereafter burnt, pulled down, or destroyed by tempest, such building may be rebuilt; and that it shall not be lawful for any person to establish or newly carry on any such business, either in any building or vault, or in the open air, at a less distance than forty feet from any public way, or than fifty feet from any other such buildings of the first or dwelling-house class, and that if any such business be now carried on in any situation within such distances, then from the expiration of the period of thirty years next, after the passing of this Act, it shall cease to be lawful to continue to carry on such business in such situation, save as is hereinafter provided; and that if any person erect any building in the neighbourhood of any such business contrary to this Act, then, on conviction thereof before two justices, he shall forfeit a sum not exceeding fifty pounds, for every day during which such building shall remain near to such offensive or noxious business; or if any person establish anew any such business, or carry on any such business contrary to this Act, then, on conviction thereof before two justices, such person is hereby made liable to forfeit, for every day during which such business shall be carried on, a sum not exceeding fifty pounds, as the justices shall determine, and that it shall be lawful for the justices also to award to the prosecutor such costs as shall be deemed reasonable."

There are, however, provisions in the following clause of the Act for an appeal to justices at Special Sessions, for showing reasons to suspend any order for abolition, as well as to suspend orders for parties to adopt within reasonable time practicable means for preventing nuisances from arising in future from these businesses.

The 55th clause, which would abolish about four-fifths of the private slaughter places now in existence, has been looked for by most of the butchers for some years past, who have had warning of the change impending. And it is more than probable that their private interests would have been stimulated to provide for the new state of things by establishing well-regulated abattoirs in lieu of the old places, had not an attempt at unwise legislation last year tended to frustrate their inclinations to use public abattoirs.

The more intelligent citizens of several of the largest towns of the kingdom have seen fit to condemn the present system, after the most deliberate consideration, and have adopted the more modern system of killing and storing cattle in abattoirs, in preference to the old slovenly and obnoxious methods in vogue.

Hitherto annual licenses have been granted by magistrates under the Metropolitan Local Management Amendment Act,

but this can no longer be done without acting in direct contravention of the aforesaid building Act, nor will the attempt be made to do so, warning having been given in all the licences issued last October to this effect.

Under ordinary circumstances it was rather singular that Dr. Brewer should have advised the Metropolitan Board of Works (who, by-the-bye, if they had had an officer of health to advise, would not have been persuaded to endorse such a Bill in the last Session of Parliament, in spite of the opinions of nearly all the metropolitan and provincial medical officers of health who have experience, and given attention to the slaughter-house question) to undertake to bring in a Bill to repeal the clause of the Metropolitan Building Act, prohibiting the carrying on of offensive and noxious trades, and thus endeavour to perpetuate the evils of private slaughter-houses, which in my opinion, are practically irremediable by any kind of regulation and inspection by local officials.*

When the Bill was brought forward for second reading last Session, the House of Commons referred it to a Select Committee to inquire into the regulations of noxious trades, and the following recommendations were reported:—

1. Sec. 55 should be repealed; additional slaughter-houses be erected at Copenhagen Cattle Market.
2. Private slaughter-houses should be regulated by bye-laws.
3. That slaughter-houses should be made the means of detecting and preventing contagious disease of cattle.

With due deference to the members of the Select Committee, I must be allowed here to say, as one competent to speak on the subject, that the second recommendation is utterly impracticable. The Committee, as amateurs, visiting these places, were grossly deceived as to the facts and circumstances under their observation. They remarked almost one fact only, namely, that of cleanliness at the time of their visit. The officers of public health visiting these places at all hours, and seeing them when kept in the most perfect condition attainable by sanitary control and inspection, state most emphatically that they cannot be otherwise regarded than as nuisances at least, in all built up parts of town. In words used by medical officers of health, and in reports of inspectors

* Dr. Brewer's short Bill consists of the following clause:—"From and after the passing of this Act, the slaughtering of cattle or sheep by a butcher, in his slaughter-house, duly licensed, shall not be held to be carrying on an offensive or noxious business within the meaning of the said Act, 7 and 8 Vict. c. 84."

of nuisances, these places are described in strong language, as "atrocious nuisances, "abominable nuisances," "intolerable nuisances," and by myself as "virulent nuisances"—words that can be fully substantiated by anyone who frankly describes what he finds and sees in its true light. The following is a summary of defects found upon inspection of twenty-eight slaughter-houses visited by the Sanitary and Public Health Committee of Paddington in 1872, when previous notice had been given of their visit, and it might naturally be expected every effort was made to show the slaughter-houses as decent as possible, in order to have a renewal of the license.*

What are these defects and objections? They may be classed under three distinct heads.

1. *As to Site.*—Nearly all the private slaughtering places have no air space surrounding them; they are close to, instead of being detached and away from inhabited dwellings; they are mostly old premises converted into a makeshift place. Stables in a mews, a cart shed, coachhouse, washhouse, kitchen, back yards, or vaults may be seen; often the entrance is through a shop, or passage of the house, or down steps into an area below.

2. *As to the condition.* — In hot weather slaughter-house nuisances are the worst, when the polluted air attracts swarms of bluebottle flies, which infest larders of adjoining houses; rats come up from untrapped drains, seeking blood and offal; a saturated subsoil reeks from leaking of joints in the paving; traps of drains are left up, or taken up to wash down as much refuse as possible, including blood, which contaminates the sluggish current of a sewer for miles in its course; the loading or carrying of blood and manure for removal in badly constructed uncovered carts; then come tallow melters' carts to collect fat which, by being kept several days, stinks, and so makes the

* Twenty-four had defective paving, either in the floor of the slaughter-house, or the cattle-pens. Scarcely any were defective in lime whiting or washing down, because the visit of inspection was daily looked for. Thirteen had untrapped drains, the cover being left up, or thrown aside Twenty were situate within twenty or thirty feet of inhabited dwellings. Fourteen were in mewses, or used as stables, or in a court. Three were approached through a public shop. In fourteen the killing and removal of offal could be witnessed from adjoining or opposite houses. Five had living rooms above formerly but not now occupied. Eight were in very confined positions, most of them a covered yard or premises at the back. One had a blood-well composed of porous bricks in an offensive state—since abolished. One was found where pigs were kept in the cattle-pens. One had a dust-bin in the slaughter-house itself. One had the sheep-pen roof only three feet from the floor.

tallow melting offensive, which it need not be if done properly. I say little about the improper layerage of cattle standing in their dung, and the meat, how it must be damaged by being exposed to this organic effluvia and that of their respiration. Here also I must not forget the cruel and degrading practice of killing the animals in each others presence, which is revolting, and cannot be sanctioned. What also mean the extravasations of blood observed frequently on parts of the carcase, but the blows and violence used in getting them into their place, or tumbling them down area steps? Who does not object to the dangers of driving cattle through the crowded streets, their loud bellowing, and the heavy blows of the drovers, conducted mostly now at night to escape observation?

3. *As to construction.*—None of the private slaughter-houses have been erected with a view to efficiency. It is nonsense saying, as one witness talked about before the Noxious Trades Committee, that capital had been expended upon the erection of these places. They are, as before stated, old premises converted into makeshifts. The pens for the cattle are close, ill-ventilated, dark, without water supply; the drainage is generally good, but the opening into the main sewer too often untrapped. No proper catchpit, no covered receptacle for dung, or barrels for blood, fat, and offal; floors instead of being sloped, or of impermeable asphalte, or well paved, are generally leaky at the joints from the cement giving way to frequent slushing.

If I am permitted to enlarge upon the objectionable practice of private slaughtering in towns, the strongest reason I would urge against it, and in favour of a scheme of slaughter-house reform, is the absence of that guarantee or security for a proper quality of meat which the public ought to demand. The amount of meat sold in markets, and at third-rate shops, and made into sausages of a very inferior nutritive value, is something enormous, and that which is absolutely diseased and condemned is an almost fabulous quantity. This I might readily place before you in figures; the amount made known weekly in the press cannot be but a small part of what finds its way into the market. There is too much of lean meat generally used as food. Some cattle are mere scarecrows, to say nothing of being unhealthy when killed *to save their lives*, or prevent death, from starvation. Efficient inspection of the slaughter-houses would greatly improve the trade in the 3000 butchers' shops of the metropolis, and as I am ready to prove without detriment to the poor, as has been alleged so strongly

The evidence of the medical officer of health of Whitechapel ought to be regarded on this point.*

Close inquiry, by means of efficient methods of inspection, will discover that, at present, animals are killed when in a dying state, some having been exposed to overcrowding in railway trains, and get suffocated; others suffering from febrile changes from want of air, water, and food whilst in the cattle-trucks, or from bad layerage in *transitu*. The loss in this way is considerable, estimated by some butchers at eight shillings a day in the best bullocks. The damage to the meat from all these sources is well known to many butchers, who prefer country-killed meat for keeping longer and better than that from animals kept and killed in foul places, the flesh of which sooner commences a putrefractive change.

The remedy for all this points in the direction of efficient inspection by officers competent to judge of the quality of meat, and constantly occupied in this duty, which is expert work, and cannot be well entrusted to numerous local sanitary inspectors. For instance, in the northern districts there are 54 slaughter-houses in Kensington, 28 Paddington, 57 Marylebone, 90 St. Pancras, 110 Islington; and to inspect all these the cattle would have to be seen alive, what are daily going in, and all that goes out of the private slaughter-houses, night and day, at all hours—a matter of impossibility where the places are, as at present, isolated, and it must be remembered, are on private premises, where it would be too much to grant power of entry at all times. My own belief is that it is not possible in all cases to judge of the quality of the meat in the dead carcase; at any rate, it would be easier to judge from the living animal. These arguments in favour of special inspection apply with equal force to measures needful for detecting and preventing the spreading of cattle diseases—a matter of paramount importance not lost sight of by the Committee on Noxious Trades.

Should it be thought advisable to condemn the old system of killing cattle in private slaughter-houses, and should a recommendation go forth from this Association in favour of establishing abattoirs in lieu thereof, the question worth consideration would then arise, in what manner and by what authority, the work could be best accomplished. The following is the resolution of the Medical Officers of Health Association, passed last year, and again re-affirmed during this month after a most dispassionate discussion, when the subject was again brought forward by Dr. T. Orme Dudfield, of Kensington:—

* "Mr. Liddle is of opinion that if private slaughter-houses were abolished, and only licensed public slaughter-houses were used, competent officers appointed to inspect the animals before and after killing, and with power to condemn all meat that was unfit for human food, then it is probable that much of the bad meat, which now finds its way into some of the butcher's shops in London, would be prevented, and the sanitary officers of all local boards would be saved the trouble and responsibility of inspecting and condemning the bad meat exposed for sale."—*Quarterly Report*, January, 1870.

Moved and seconded—

"That, in the opinion of this meeting, the slaughtering of cattle in the private slaughter-houses of London should be abolished, and that a certain number of public slaughter-houses should be created, either under the supervision of the Corporation of London, or the Metropolitan Board of Works; and that those corporations should be urged to undertake the erection of such buildings, and also the management of them, forthwith."

The following amendment was moved and seconded—

"That it is expedient that the 55th Section of the Metropolitan Buildings Act, 1844, be repealed, but that the number of slaughter-houses be gradually diminished by the abolition of many which are not in a thoroughly fit condition, and which have been in a measure tolerated on account of the uncertainty of the law; and that general, precise, stringent, and uniform regulations should be laid down to be adopted by the vestries; and that greater power be given to the local authority to take proceedings at any time of the year in cases where untrapped drains, want of care and cleanliness, cruelty, and general deficient arrangements for layerage are found to exist, penalties being recoverable in any police-court; and that no slaughter-house be licensed until the owner has signed a copy of bye-laws pledging himself to carry them out fully and faithfully."

The amendment was supported only by the mover and seconder, and the original motion was carried.

The only two members supporting the amendment have districts more of a suburban character than the rest of London. My own opinion is in favour of the Metropolitan Board of Works in the metropolis taking the entire charge of supervising the trade in cattle-killing, and noxious trades connected therewith; and in country places, for the Council of the corporation of a borough town, and the Local authority, whether urban or rural, taking charge of all such places, granting licences or permits for killing cattle for sale, and for the carrying on of other noxious trades. The framing of such regulations or bye-laws as may be needful for their efficient management, is not difficult, while, at the same time, it will be proper to leave to private speculation an opportunity of carrying out the work, provided it was under inspection. In the former case a Bill would have to be introduced by the Metropolitan Board of Works.

For the latter amended clauses in the promised Public Health Bill, to be considered in Parliament, would readily meet the case without special legislation. What clauses should be introduced to regulate all noxious trades I need not here occupy your time to consider more in detail, than remark that they must be based upon the known facts and experience now before Parliament and the public, and must afford a better guarantee for the prevention and removal of nuisances than we now possess.

Another of the most objectionable features of private slaughtering of cattle not to be forgotten, and the cause of serious nuisances to a neighbourhood, arises from inefficient means for the disposal of blood and other kinds of offal, of which the quantity is very large.

Blood is not only a valuable commodity wasted. It is worse than wasted, inasmuch as it is washed down into drains and sewers, creating and keeping up dangerous nuisances at a distance along the whole line of sewerage. Even when it is saved in barrels or tubs, and carted away with the fat in uncovered carts, it is in such a decomposed state as to be unfit for passing through public thoroughfares. The absence of any proper means at hand for utilizing this substance for its albuminous properties, or for making artificial guano, brings about another evil, namely, the blood and offal from many private slaughtering places is often in a partially decomposed state given to pigs. This rough and disgusting kind of pig-feeding brings into the market a low class of bacon, tempting to poor people for its low price, although far from being cheap at any price. The butcher's offal-fed pig would soon disappear from the market if facilities were placed at the disposal of butchers to use the blood for its albumen value or for artificial manure. As soon as economical methods of storage and proper apparatus are provided in the neighbourhood, of a well-regulated public abattoir, a large number of noxious trades will cease to be public nuisances. The products of slaughter-houses can not only with facility be converted into artificial manure by methods free from smells, if properly carried out, but have a high commercial value, and a ready sale as agricultural fertilizers in every place where they have been tried.

Taking into consideration the blood and offal converted into artificial manure, the tallow and fat rendering-down, before it became putrid, and what are called the scraps, which can be sold for food for fowls and pigs, the butchers will find, upon due inquiry into the result attained in other large cities, that their own interests are not so much, if at all, endangered, as

many of them allege who are opponents to a change in the plan recommended for the metropolis.

Public health authorities ought not to be ignorant of the fact, that a good interest, as well as profits may be obtained for any capital expended upon works of this kind. There is scarcely any town of considerable size, where a slaughtering place, cattle-pens, a boiling-down shed, and a knacker's yard, are not needful receptacles for the deposit of offal and refuse, and their conversion into a saleable product, in the form of manure. It would be a profitable undertaking, either to the municipal authorities, or taken up by a private association. I say this from personal inquiry into the modes of dealing with the refuse of towns, and from reports in my possession, to which I have had occasion to refer.*

Having denounced in rather strong language what I conscientiously believe to be the eradicable evils of the present cattle-killing sheds, my paper would be incomplete if I were to omit to mention what are the essen-

* A report from a visit to the Slaughtering, Fat-Melting, and Refuse-Rendering establishment of New York, states :—"A hog-killing place, where 700 hogs had been killed the previous night, was found odourless, scrupulously clean, and not a trace of blood or offal seen. The blood is led by a simple contrivance to the cocks for removal; it is all caught by a fertilizing company, without trouble to the slaughterers, and they pay for it over 2000 dollars a year. Every provision is made for the comfort of the animals, excellent ventilation, &c. The tanks of the melting vats for condemned animals burn up offensive gases, and give out no bad smells during the process. In a Jewish slaughter-house the proprietor told the inspectors that the blood was sold for 6000 dollars a year. At the Butchers, Hide, and Melting Company the cattle were in pens as fresh and sweet as a model farm-yard—food and water in abundance. Every member of the party seemed convinced that the profits made through the utilization of blood were well worth having. This new and valuable way of treating scraps and blood is, as far as we are able to learn, a great improvement upon any method heretofore used, either in this country or in Europe. By this new process, the success of which seems to be proved, a similar result is reached without offensive odour, and a fertilizing produce worth forty to fifty dollars a ton, from 'tankings,' and also from blood, without the addition of chemicals, in the space of one or two hours. This method will be used at the Brighton (Mass.) abattoirs, and by means of it the blood and offal of all the animals slaughtered each day will, in the course of the same day, be reduced to the form of a simple and inodorous powder. If it can be shown at Brighton (Mass.), as we believe it will soon be done, that foul odours, which have been thought to be inseparable from slaughtering, bone-boiling, and fat-melting, are avoidable, and that, in the final treatment of refuse animal matter, the ammoniacal products of decomposition, instead of being wasted to pollute the air, may be saved to enrich the earth, it will be a sanitary and economical demonstration, whose good effects will not be limited to the State of Massachusetts."

tial features of an establishment to cope with them, and capable of reducing to a minimum all nuisances that at present beset, and will in future embarrass, the sanitary authorities of cities that neglect to legislate for civic purification. I am satisfied that all noxious trades must sooner or later be regulated under stringent municipal bye-laws, and that they cannot be left entirely to private speculation.

The site most convenient for the establishing of public abattoirs should be an open space or field, not farther distant in the suburbs of a town or city than a short drive or walk; it should be suitable for one or more blocks of building; it should be near a railway, water communication, or the cattle market, and the following buildings should be contiguous, but distinct from each other:—

1. Slaughter-house for bullocks, sheep, and pigs, with good water supply at high pressure; open ventilation, good drainage, asphalte floor, with convenience for the removal of blood, hides, offal, and carcases of animals.

2. A set of cattle pens, or layerage, with store for fodder, water trough, facility for cleansing, and the removal of manure, &c.

3. An engine and boiler house, with tall chimney for carrying off gases, according to the necessity of trade—with sufficient steam power for the tallow-melting tanks, greaseworks, bone-boiling, glue-making, &c.; hot water for washing and cleansing.

4. A store-house for hides, for bones, wool, hair, &c.; and the blood and offal, and all offensive materials, to be kept in covered tanks. The making of animal charcoal may be desirable, and other processes which I need not mention here.

5. In part of the same premises, or an adjoining field, should be a tanpit, the knacker's yard, with suitable appliances.

6. Dwellings for men and stabling for horses may have to be provided for those who have to be employed on the premises, or for carrying out the work.

The main points I endeavour to prove are these:—

1. The impracticability of attempting to reconstruct the old private slaughter-houses in built up parts of towns.

2. The impossibility of securing, in the present private slaughter-houses, sufficient air space, means of cleansing, and drainage to prevent them from becoming nuisances injurious to the health of people living in their vicinity—nuisances arising from the pollution of the air, drains, and subsoil.

3. The remedy I propose is the erection of abattoirs in convenient suburban localities, or where the objectionable practice of driving cattle in streets and thoroughfares of towns can be avoided; where cattle can be kept sweet and clean in well-kept

pens, duly provided with pure water, fresh air, shade in hot and shelter in cold weather; where they are less exposed to violence and brutality of drovers; and where the transport of blood, offal, hides, and tallow can be effected, without nuisance or complaint of neighbours, to places where they can either be sold, or preserved by methods free from objection, or utilized at once for the owners thereof.

4. And lastly, this is the most important recommendation, where, as I maintain, there is no difficulty in carrying out, viz., in the inspection, by competent judges, of the cattle before they are killed, or of the meat supplied, either in carcase or otherwise, to the public markets, or to retail dealers.

In conclusion, permit me to remark, that it will neither be creditable to Parliament, nor to the local governing bodies of the metropolis and large towns, to endure the present state of things a day longer than is necessary. The Act, as I have said, will shortly come into operation, and a great achievement will be performed by simply remaining quiet and doing nothing at all. Should any efforts be made by persons interested in the prolongation of the Act, it will then, I urge, become the duty of this Association to make some representation to Government with a view of effectually putting a stop to a system so offensive, so injurious, and so repugnant to the feelings of all right-minded persons.

Among those present at the discussion were the following:—Mr. R. T. Arnold, Dr. Bateson, Mr. J. J. Brown, Mr. Sneade Brown, Mr. R. Calvert, Dr. Carpenter, Mr. W. J. Cooper, Dr. Thomas Dudfield, Professor Gamgee, Mr. C. V. Game, Mr. P. C. Greatorex, Mr. C. J. Grece, Dr. Hardwicke, Mr. E. L. Jacob, Mr. D. Jarvis, Mr. H. N. Mozley, Mr. C. A. Ratcliffe, Dr. George Ross, Dr. Sarvis, Mr. G. W. Shaw, Mr. Joseph Smout, Mr. J. Smout, jun., Mr. Thomas Webber.

Dr. Sarvis (Medical Officer of Health for Bethnal Green), said he should give his vote against the total abolition of private slaughter-houses. In summer he thought it was an advantage to have them near where the meat was to be sold, for nothing was so injurious to the meat as its transit. We might keep the best of the slaughter-houses.

Professor Gamgee was much astonished to hear a medical officer of health advocate the continuance for one day of a single private slaughter-house in any city of the world. It was also astonishing to him to find how hard it was for the public interest to be protected in opposition to that of a trade, which always would have advocates looking out for, and seeking in every possible way, to add to its

pecuniary advantages, and counting as nothing the sanitary well-being of the public at large. He was stating a fact, when he said that private slaughter-houses rendered it impossible to inspect animals, and prevent the sale of diseased meat. He was sorry to have to acknowledge the fact that even the government virtually sanctioned the trade in diseased animals, which was attended with such bad consequences. The private slaughter-houses offered facilities for keeping up this trade of supplying meat which was often poisonous. It was wrong to bring forward medical men to give evidence in regard to diseased meat. There was but one safe rule, and that was the one which had been given by the great law-giver, Moses, namely, that such meat should not be eaten. If we had had a law to suppress the trade in this kind of meat, we should probably have meat twenty-five or thirty per cent. cheaper than it is now. Any rational method to put a stop to disease would, if successful, add largely to the food of the country, but it was very difficult to impress this fact on the public mind. The ignorance in the minds of those who were able to introduce a better state of things was, he thought, disgraceful, and it was intolerable that there should be no system of watchfulness, the want of which condemned our people, in many instances, virtually to starvation. Simple precautions, such as were mentioned in the paper, would obviate these evils. He would develope, as much as possible, the free-trade in meat, but stop that in disease, but its abolition was impossible so long as the private slaughter-houses existed. He had come to the conclusion, from what he had seen of the use made of diseased meat in sausages, and by the third-rate butchers, that it was with them impossible to have any system adequate to protect the public.

Mr. Sneade Brown called attention to the evidence which had been given in a former number of the "Transactions" of the Society, in regard to Leith and other towns where private slaughter-houses were not allowed, and in which it was shown that there had been an advantage to everyone. In a Report of the Medical Officer of the Privy Council in 1865, where the slaughter-houses had been examined, in the course of an inquiry instituted for the purpose of devising measures for the prevention of a disease which had appeared on the continent, there were some terrible pictures of the condition of slaughter-houses. All of them were found to have objectionable features.

Dr. Alfred Carpenter said the question ought to be taken up in reference to every large town in the kingdom. In the district with which he was connected new slaughter-houses had been forbidden and public abattoirs erected. But he was sorry to say they had not been much used, the butchers preferred to continue using their private places; perhaps because they could slaughter their diseased cattle without being observed. The adoption of the public abattoirs would be an advantage for the poor. In the private slaughter-houses even good meat became tainted by contact with the

bad conditions which prevailed. As for the objection taken against their abolition, on the ground of the difficulty of transit in hot weather, he (Dr. Carpenter) thought there would be no harm done if at such a time meat were not eaten at all. Much of the disease that prevailed then was produced by eating animal food in a state of change, and people would be much better without it. The law required to be altered to meet the requirements of the case in many ways. He hoped this Society would make a strong effort against the continuance of such places. The evidence of the Parliamentary Committee which sat upon the subject was entirely one-sided—no one was heard adverse to them.

Dr. George Ross (Medical Officer of Health for St. Giles') supported the views contained in the paper. Nothing was more abominable than the condition of the slaughter-houses, and there could be no doubt that if they were abolished it would be a great boon to the public. He was opposed to any compromise, because he could see no way to improvement short of entire abolition. There was a much greater difficulty in dealing with the subject in this metropolis than in small towns. There would be a great difficulty in providing for four abattoirs for the metropolis as proposed, and in his opinion it would be better that Copenhagen Market should be supplied with accommodation adequate to the wants of the whole metropolis. Under proper regulations for the prevention of nuisances, if this were the case, there would be no more then than there are now connected with the live cattle.

Mr. Charles V. Game looked upon the paper as entirely theoretical, and denied that private slaughter-houses were necessarily injurious, he having lived for twenty years, and brought up a family, over one, and they were as healthy a family as any in London. The present slaughter-houses ought to bept in a cleanly and creditable manner.

Mr. R. C. Greatorex said that immense supervision was necessary, and that was one reason why private slaughter-houses should be abolished. Their position was bad, and all sanitary science agreed that they were injurious to the health of the people. On economical and other grounds he advocated the abolition of the private slaughter-houses.

The Chairman said he had taken much interest in the subject, and a year ago had felt it his duty to take such parliamentary action as led to the defeat of Dr. Brewer's bill. Something more precise in the definition, and more stringent in the remedies, was required to meet the condition of the slaughter-houses in our large towns. The matter, as it concerned them, should not be left till found to be a nuisance, but taken as being, in a degree, a nuisance *per se*. And the only question was whether these places should be under strict regulations in the heart of towns, or entirely forbidden. He had taken note of the evidence offered before the Select Committee of the House of Commons of 1873, on this subject, and of the

character of the witnesses, and had found that many frivolous and irrelevant questions were asked. A great experiment had been carried on at Edinburgh, which had resulted in a great saving financially, and an improvement in the public health, yet no one from that city was examined as to these matters. A great discussion had likewise been carried on at Liverpool on the subject, and definite results arrived at there; but no gentleman from that town was consulted. Most of the witnesses examined were interested persons carrying on the very trades, the nature and effect of which were under discussion. A medical man or two was among them, and examined; but he (the Chairman) feared their evidence was not respected. It was a misfortune that this inquiry should have led to so disappointing an issue. He was quite convinced, in his own mind, that we should have in our great cities slaughter-houses under the control of the public authority. He felt doubt, however, whether they should be open to any one to slaughter in them when he chose. He would have them let to individuals. If they were placed entirely and exclusively in the hands of the public authority, that authority would go to sleep, but if let to individuals under suitable conditions, it would take care that the tenants observed these conditions. If we were to have private places we must have a license on the one hand, and bye-laws on the other. The license must be granted by the authority which supervised, with rigid conditions and rigid bye-laws regulating the manner in which the business should be conducted. In this matter it was our duty to stimulate public opinion, and watch its operation. It was impossible to enforce such things greatly in advance of public sentiments, and before we could have what we desired in a really efficient manner, we must have a strong municipal authority—an authority possessing the public confidence. To the spread of scientific education he looked for good results, and he was convinced that one of the results of the public education of the country would eventually be far more effective than laws relating to the public health.

Dr. Carpenter moved—

"That the subject be referred to the Standing Committee of the Health Department, in order that the Council may be recommended to take action in the matter if necessary."

Dr. T. O. Dudfield seconded the motion, and in doing so, called attention to the fact that a great many private slaughter-houses had been tolerated by the medical inspectors, which would never have been allowed to continue had it not been known that they were, as a whole, in consequence of the law, non-permanent.

Mr. Sneade Brown proposed, and Dr. Carpenter seconded, a vote of thanks to the Chairman, which was carried unanimously.

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NOTES ON ENTOZOA.

BY

T. SPENCER COBBOLD, M.D., F.R.S., F.L.S.,

PROFESSOR IN THE ROYAL VETERINARY COLLEGE.

Part I.

[*From the* Proceedings of the Zoological Society of London, November 18, 1873.]

(Plate LXIV.)

The present communication is the first of a series of papers in which I propose to bring together many scattered observations made from time to time in the intervals of more sustained scientific work. Of necessity these notes will exhibit an incomplete and fragmentary

character; but I have felt that it were better they should be presented in this form rather than remain altogether unemployed. The Society is aware that I am indebted to several of its Fellows, as well as to many other persons, for contributions of Entozoa; and I therefore embrace this opportunity of explaining that it has hitherto been quite impossible for me to do justice to these various gifts. In individual instances, as, for example, in the case of the Nematodes obtained by Prof. Flower from the Aard Wolf, I have made a single species of parasite the subject of lengthened comment (Zool. Soc. Proc. 1870); but to do so in all future and similar cases would require more undivided time and attention than is again likely to be at my disposal; moreover, in the majority of single and separate contributions the parasite rarely demands more than a passing remark serving to confirm, to verify, or it may be to refute statements made in some previous record.

The classification of the Entozoa is confessedly in a very unsatisfactory state; and I regret to observe that the more extended one's examinations become, the greater are the difficulties which one has to encounter in this respect. The geographical distribution of the Entozoa is a subject of increasing importance; and it is one towards the elucidation of which I conceive that such notes as these may eventually help to contribute their quota. It would be a work of supererogation to express the sum of my indebtedness by mentioning *seriatim* the names of all my friendly contributors, most of whom have transmitted solitary specimens. However, in the case of sets of specimens or collections, small and great, my acknowledgments are especially due to Mr. Charles Darwin, F.R.S., to Mr. Charles W. Devis, to Mr. Robert Swinhoe, H.M.B. Consul at Chefoo, to the late Dr. William C. Peebey, who brought me an interesting series of Helminths from India, and to Prof. Murie, M.D., formerly Prosector to this Society. Lastly, in these prefatory remarks, it is only necessary to add further that in this serial record no particular order of description will be observed beyond that which a mere general group-affinity may occasionally suggest as natural, convenient, or suitable.

1. Filaria horrida (Diesing).

The first number and private reference on the list of a series of parasites which I received from Mr. Darwin in August 1869 refers to a set of worms obtained by him "from the stomach of an American Ostrich at Bahia Blanca, North Patagonia, in 1832." Having already identified a similar series of Nematodes that had long lain concealed in the Museum stores of the Hunterian Collection, I had no difficulty in recognizing the species. There were, in all, ten specimens—seven females and three males. The latter were in good condition, and varied in length from 5 to 5½ inches. Five of the seven females were perfect, the shortest measuring 20 inches, and the longest 29 inches, this latter worm having a breadth of exactly ⅛ inch. I may mention that one of the Hunterian specimens measured 35 inches; but examples have been referred to as extending

beyond 3 feet. I believe this entozoon was first described anatomically by Prof. Owen, from the College specimens above mentioned; but the only original remarks on the subject that I have seen from his pen are some few contained in the article "Entozoa" in Dr. Todd's 'Cyclopædia of Anatomy,' and others given in his 'Lectures on Comparative Anatomy.' From the published dates of these contributions (1839 and 1843 respectively), I am led to conclude that the first discovery of this worm actually rests with Mr. Darwin, since the late M. Diesing's specific description and nomenclature was evidently based on an examination of specimens obtained by Natterer in Brazil, the date of which would probably be about 1833, or perhaps a year later. However, as Natterer was many years previously resident in Brazil, and no special date of his particular "find" is given by Diesing, it is impossible for me to speak with certainty on this point. It is perhaps of little moment; and under any circumstances the systematist's specific title must be allowed to stand.

My examination of the females confirms Prof. Owen's statement respecting the simple character of the uterine organ and the forward position of the reproductive outlet. To be precise, I find that the vulva is placed $\frac{1}{70}$ inch below the points of the labial papillæ. Of these papillæ I think there are ten or twelve, two of which project conspicuously beyond the rest, forming, as Diesing well observed, conical spines. Their length from base to apex is scarcely more than $\frac{1}{200}$ inch; but their appearance suggests a structural affinity with the similar oral spines occurring in the Guinea-worm. In *Dracunculus*, however, according to Bastian, the two large spines are placed before and behind the mouth, not laterally.

This parasite from the Ostrich does not reproduce viviparously; at least there were no free embryos in the uterine duct. The eggs were chiefly of two sizes, those in which yolk-segmentation was going on measuring $\frac{1}{140}$ inch in length, whilst the perfectly mature ova, containing coiled embryos, gave an average of $\frac{1}{150}$ inch, being at the same time proportionally broader than the smaller eggs, whose transverse diameter was not more than $\frac{1}{1000}$ inch.

As this worm has probably never been figured, I append a representation of the male (Plate LXIV. fig. 1), also an outline of the head and tail of the female (fig. 2), of the natural size, with a separate outline of the oral spines of the latter magnified sixty diameters (fig. 3). I likewise represent the egg in two stages of growth (fig. 4). Lastly, I may remark that this worm has not hitherto been recorded from the cavity of the stomach. That on this particular point there is no error in Mr. Darwin's MS., I think highly probable, not only from the distinguished collector's known accuracy, but from the circumstance that the parasites were so much coiled round one another that it took me nearly half an hour to unravel and separate them. I further presume that the tangled state was the condition in which they were originally found in the bird's stomach.

2. Filaria immitis, Leidy.

During the autumn of 1869 I received from Mr. Swinhoe the heart

of a dog, the animal, according to the statements of the donor, having died at Shanghai in the spring of the year, "after three days of great suffering."

During the month of February 1872 I also received, through Mr. Walsh, a second preparation of the heart of a dog which had died at Yokohama, Japan, under similar circumstances. This second specimen was transmitted by Mr. Dare, together with a communication which was published in the pages of the 'The Field' for February 24, 1872.

In both cases the *Filaria immitis* appears to have been the sole cause of the death of the dogs in question, the ventricles and auricles being completely blocked by the presence of a large number of these worms.

To Prof. Bennett of Edinburgh I am also indebted for a single specimen taken from another Chinese dog, the original preparation of the heart, as I saw it in 1850, resembling in all respects the specimens which I have now in my possession.

As, in my recently published 'Manual of the internal Parasites of our domesticated Animals,' I have already enlarged upon the symptoms produced by this parasite, and upon other practical points gathered from the statements of Mr. Dare, Dr. Lamprey, and others, I am here only concerned to add such scientific details as could not be published at any length in that small treatise.

In the interval which elapsed since the transmission of Mr. Swinhoe's contributions similar specimens of the worm were received at Netley and at the British Museum from Dr. Jones Lamprey. In the first instance they were briefly and very inadequately noticed by the late Dr. Baird in a paper communicated to the Linnean Society (May 2nd, 1867); but they have since supplied materials for the publication of an admirable paper by Assistant Prof. Welch of Netley ('Lancet,' March 8, 1873).

Although Mr. Welch's paper leaves little to be desired in reference to the facts of embryonal development, as far as can be gathered by an examination of the uterine contents of the female worm, yet there are some points well worth verifying in this matter; and there are others in connexion with the structure of the adult worm which appear to have escaped Mr. Welch's attention as well as Dr. Baird's. Unfortunately, I have had no opportunity of consulting the original paper by Dr. Joseph Leidy. As regards the specific name, for which, I believe, Dr. Leidy is responsible, nothing could be more to the point, as the term *immitis* expresses, metaphorically, the truly cruel character of the Entozoon.

Most of the following data have been gathered from an examination of the specimens supplied by Mr. Swinhoe, to whom I owe an apology for not having made an earlier record of the results. However, the dog's heart was exhibited at the Liverpool Meeting of the British Association in 1870; and a brief notice of it subsequently appeared in the published Reports of the Meeting for that year. It is the more incumbent upon me not to delay the publication of these few microscopic details, since Mr. Swinhoe, in a recent letter, informs

me that Dr. Little, of Shanghai, is diligently engaged in working out the structure and development of the parasite.

Dr. Krabbe, of Copenhagen, has likewise supplied some interesting particulars; but, in the absence of any references, I am led to conclude that the Danish author's observations are based on the previously published statements of Prof. Leidy ("Husdyrenes Indvoldsorme," Tidsskrift for Vet. 2den Række, ii. 1872).

On the the 15th of April last I examined a number of these worms, two of which, male and female, are represented in the accompanying Plate. To the naked eye the sexual differences are readily discernible. The female (fig. 5) maintains almost throughout a uniform calibre of about $\frac{1}{60}$ inch; but at the head it diminishes to $\frac{1}{120}$ inch, and at the tail to about $\frac{1}{140}$ inch, the caudal point being bluntly convex. The oviducts of all the females examined were crowded with eggs, and in certain situations the eggs were collected together in the form of large ovoid masses. The largest eggs had a long diameter of $\frac{1}{250}$ inch, with an average breadth of $\frac{1}{1500}$ inch. These contained coiled embryos; the diameter of their bodies varying from $\frac{1}{3000}$ inch to $\frac{1}{2250}$ inch.

The male parasite is readily recognized by its comparatively slender body, having a diameter of about $\frac{1}{90}$ inch, and also by its elegant spirally curved tail, which is three or four times twisted upon itself with the regularity of a corkscrew (fig. 6). The coiled portion is much narrower than the body of the worm, and it finally dwindles down to a breadth of $\frac{1}{300}$ inch, its extreme point being blunt, as in the female. Within a short distance of the extremity the two spicules, of unequal length, may often be seen projecting from the cloacal outlet, this part of the worm being also furnished with a well-marked horseshoe-shaped bursa (fig. 7). This organ may be described as consisting of two transparent folds or extensions of the cuticle, each lateral division of the hood being supported by four oval glandular rays. The rays are apparently eight in number, and arranged in pairs. The uppermost pair is the largest, the other pairs gradually decreasing in size from above downwards. When viewed laterally, these oval rays present a beaded appearance, collectively forming a rather striking microscopic object. The diameter of the largest ray is only about the $\frac{1}{750}$ inch, whilst that of the smallest is not more than $\frac{1}{1800}$ inch from side to side*.

As seen in the drawing, the epidermal layer of the skin was generally found projecting more or less beyond the limit of the dermis. I regarded this as a *post mortem* production. In one case

* Since this paper was read to the Society my attention has been called to a much more detailed description of the worm by Mr. Welch, of Netley, communicated to the 'Monthly Microscopical Journal' for October 1873, p. 157. The author gives many particulars that have escaped my notice, and he also interprets some of the facts observed by us both in a very different sense from that I have adopted. He recognizes but one *spiculum*, and regards the oval rays as generative appendages, of the nature of *vesiculæ seminales*, communicating with the seminal duct. He observed twelve of these oval appendages, and, amongst other things, describes the intestinal tube as terminating in a blind cæcal extremity.—T. S. C.

I obtained very satisfactory evidence of the existence of a minute terminal papilla, whose breadth scarcely exceeded the $\frac{1}{8000}$ inch. I have no doubt that this papilla is connected with a caudal gland; but I could not obtain a clear definition of the parts beneath the true skin. Lastly, I may add that immediately above the bursa I noticed a series of lines obliquely directed upwards, representing as many parallel foldings of the integument. The general character of these markings in all respects corresponded with the similar appearances figured and described by Eberth in *Enoplus*, *Enchelidium*, and other nematode genera.

3. Filaria hebetata, sp. nov.

In the month of July of the present year I received from Mr. Millen Coughtrey (then acting as Demonstrator of Anatomy in the Liverpool School of Medicine) the heart of a Seal, together with a communication, from which I further learn that Mr. Coughtrey was himself indebted to Mr. T. J. Moore, the well-known Curator of Sir William Brown's Museum, for the opportunity of dissecting the animal in question. Mr. Coughtrey remarks that the Seal was an adult male example of *Stemmatopus cristatus*, or Hoodcap, the occurrence of which is very rare on the British coasts. The animal was captured in February at Frodsham, on the Cheshire side of the Mersey, and died at Widnes in the beginning of June, having been exhibited in the county during the interval. Mr. Coughtrey adds:—"I made a hurried, but careful *post mortem* examination, since it was suspected that it had died of poison. The dissection embraced every organ except the brain and spinal cord. The abdominal viscera were free from cysts or any appearance of worms. Within the stomach were three mackerel, in different stages of digestion. On removing the tongue and pharynx, with the larynx and trachea, I perceived a nematoid worm grasped by the *rima glottidis*, one half of it being within the posterior buccal orifice, and the other half within the larynx. I am sorry to say it was afterwards lost, through the carelessness of an attendant. The worm was about four inches in length, much thicker than those in the heart, and of a dusky hue. The thoracic viscera were removed *en masse*, when the lungs were found studded throughout with miliary tubercle. The worms were chiefly in the right ventricle of the heart, entwined and laced amongst the *columnæ carneæ*, being embedded and surrounded by blood-clot. The left side of the heart was quite free from worms; and there were none in the pulmonary artery or other large vessels. When fresh the worms were extremely elastic, so that they might be stretched to half their length beyond the natural size and yet would not break. The muscular system generally was healthy. I have sent you all the worms except two, which I preserved, and afterwards gave to a friend."

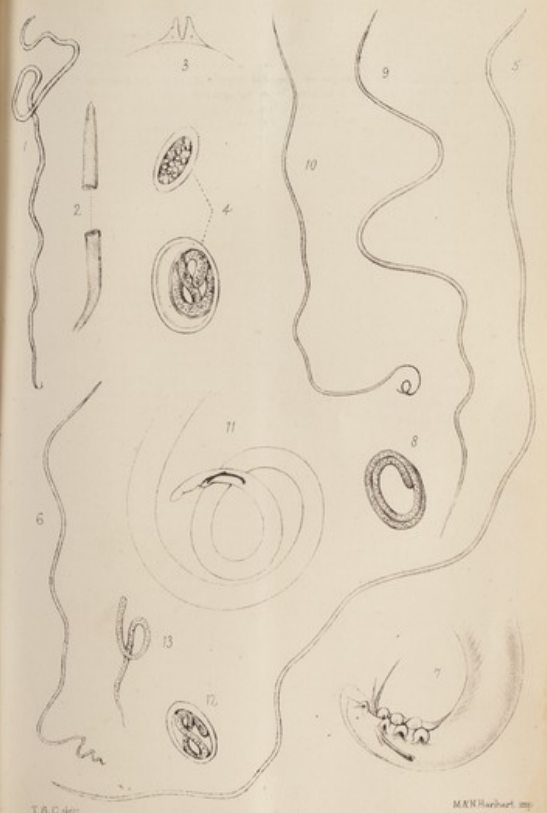
In offering this abstract of Mr. Coughtrey's lengthened and interesting account of his examination, I have purposely selected only such remarks as seemed to bear more or less upon the question of parasitism; and in doing so I have adhered very closely to the actual

wording of his communication. Mr. Coughtrey has since left Liverpool for New Zealand, whence, in due time, I hope to receive from him further entozoal contributions. The nematode from the larynx was probably an example of *Ascaris osculata*; but the Hæmatozoa from the heart are, I believe, new to science. I have named the species as above, the specific title chosen having reference to the blunted extremity of the tail. In brief, the characters may therefore stand as follows:—Body of nearly uniform thickness, narrowing slowly in front and behind; head simple in both sexes, semicircular in profile; tail of the female straight, rather suddenly narrowing to a fine rounded point; tail of the male spirally curved, furnished with two unequal spicules, blunt at the tip, where it forms a small knob. Length of the female 6 inches; male, up to 4 inches.

In addition to the above diagnosis, I may add that the head of the female suddenly curves forward from a point where it measures about $\frac{1}{70}$ inch transversely, whilst the tail, at a corresponding distance from the extremity, does not exceed the $\frac{1}{175}$ inch in diameter. The knob-like projection at the end of the tail of the male measures only $\frac{1}{250}$ inch in breadth. As usual, in similar cases, I found the uterine ducts crowded with ova in various degrees of development, all of the most advanced embryos being included in a delicate chorional envelope. The position of the vulva could not be made out; but the embryos (figs. 12 & 13) were taken from the oviduct at a distance of one inch from the head. The long diameter of the ova gave an average of $\frac{1}{175}$ inch, the embryos having a length of $\frac{1}{500}$ inch, or thereabout, the greatest breadth scarcely exceeding that of the human red blood-corpuscle.

EXPLANATION OF PLATE LXIV.

Fig. 1. *Filaria horrida*, male: nat. size.
2. The same, female: head and tail, nat. size.
3. The same: outline of the oral spines, enlarged.
4. Ova of the same, highly magnified.
5. *Filaria tenuilis*: female, nat. size.
6. The same: male, nat. size.
7. The same: tail of the male, enlarged.
8. Ovum of the same: mag. about 500 diam.
9. *Filaria hebetata*: female, nat. size.
10. The same: male, nat. size.
11. Tail of the same: male, enlarged.
12. Ovum of the same: mag. about 350 diam.
13. Embryo of the same, set free.



NEW OR RARE NEMATODE ENTOZOA.

NOTES ON ENTOZOA.

PART II.

BY

T. SPENCER COBBOLD, M.D., F.R.S., F.L.S.,

LECTURER ON PARASITES AT THE MIDDLESEX HOSPITAL MEDICAL COLLEGE.

[*From the* PROCEEDINGS OF THE ZOOLOGICAL SOCIETY OF LONDON, February 3, 1874.]

(Plate XVIII.)

Whilst engaged in writing the concluding portion of my first set of "notes," I received for examination a nematode parasite, some brief account of which will appropriately commence the present series.

4. Filaria gracilis, Rud. (Plate XVIII. figs. 1–4.)

On the 20th of August, 1873, Mr. Samuel Smith, M.R.C.S., of Clifton, transmitted an example of this entozoon, with a request that I would identify it. Finding that the specimen was a male, and unaware that the males of this species ever attained a length of 20 inches, I at first supposed that we had to deal with a new form. However, on subsequently analyzing its characters, I became satisfied that the worm was really only an unusually fine male *F. gracilis*. The frequency of the occurrence of this nematode in the abdominal cavity and other parts of the trunk of Monkeys is a matter of common observation. I remounted no less than four preparations, representing numerous examples of this Worm, for the Museum of the Royal College of Surgeons. Some of the Hunterian specimens were originally obtained by Professor Owen from the cavity of the pleura of a Capuchin Monkey, others having been removed by him from the thorax of an Orang-outang. From Mr. Smith I have learnt that the present example formed one of a group of five Worms, all of which were found lying between the folds of the omentum of a Spider Monkey (*Ateles*). My informant also remarks that one of the Worms was enclosed in a "false sac, formed by a twisting of that portion of the serous membrane which is immediately connected with the inferior curvature of the stomach." It further appears that the example in question was the smallest of the five, although I found it to measure upwards of twenty inches without any stretching. Whilst the Vienna helminthologist, Diesing, only allowed an extreme length of four inches for the male worm, the French authority, Dujardin, stated that specimens had been reported up to a length of 12½ inches. Females have been recorded as reaching a length of 5 feet.

Not being acquainted with any satisfactory representation of this Entozoon, I have thrown the parasite into a series of folds so as to enable me to display its full length and general appearance (Plate XVIII. fig. 1). I have also added an enlarged and accurate outline representation of the head and neck (fig. 2). The description of the Worm by Dujardin leaves little or nothing to be desired. Speaking of the tail, he observes that the extremity is furnished with two or three papillæ, serially disposed in front of or above the point. I examined

these minute prominences carefully (fig. 3). There were two short conical papillæ placed within about the $\frac{1}{800}$ of an inch from the actual extremity, the point itself being furnished with an excessively minute prominence, whose base scarcely exceeded the $\frac{1}{1000}$ of an inch in diameter. There was a very distinct appearance of a centrally placed duct (which I regarded as the tubular extension of a large caudal gland), the end of which had apparently become detached from the interior of the minute terminal papilla. As already remarked by Dujardin, the lateral lines of the body are brown-coloured and very conspicuous. I was particularly struck with the remarkable distinctness of the contents of the seminal tubes, whose separate particles could readily be seen through the thick integuments. Unwilling to injure the specimen, which I afterwards returned to Mr. Smith, I merely inserted the point of a fine needle into the main channel, and thus obtained a large quantity of the spermatozoa. These small particles, notwithstanding their long immersion in strong spirit, presented a tolerably characteristic appearance—the larger and fully formed corpuscles giving a long diameter of $\frac{1}{1400}$ of an inch (fig. 4). I may add that several of the corpuscles displayed, more or less perfectly, the well-known flask-shaped envelopes so often described in connexion with this group of parasites.

5. Spiroptera turgida, Duj. (Plate XVIII. fig. 5.)

On the 15th of April, 1873, I examined the contents of a small phial in which were two Worms that I had long previously received from Dr. Murie. They were sent to me during the time of his official connexion with the Society's Menagerie. The smaller parasite, as was stated on a label, came from the stomach of an Opossum (*Didelphys azaræ*). The worm was evidently a female, but, unfortunately, not in a satisfactory state of preservation. It measured more than an inch in length by $\frac{1}{10}$" in breadth. The accompanying figure may be useful (fig. 5); but the minute characters were mostly either lost or obscured. The mouth was round, and certainly furnished with several minute teeth, the number of which could not be accurately ascertained.

6. Ascaris cuspidata, T. S. C. (Plate XVIII. fig. 6.)

The larger of the two Worms above mentioned appears to be new to science. Dr. Murie labels it as having been obtained from the stomach of a Green Monkey. I have little doubt that his record refers to one of the Monas (*Cercopithecus*). The Worm is a true Ascaris, and, although a male, measures fully 3¼ inches from head to tail. Owing to imperfect preservation, the spicules have been lost. The three oral lobes are particularly prominent. The caudal extremity is furnished with a very fine spine, or cusp, formed by an extension of the epidermis (fig. 6). This minute cusp curves backwards, and measures only $\frac{1}{1000}$ inch in length by the $\frac{1}{4000}$ inch in breadth at the narrowest part. The anal aperture is placed at a distance of $\frac{1}{25}$ inch from the extremity of the tail. The eggs have a long diameter of about the $\frac{1}{150}$ of an inch.

7. Ascaris maculosa (Rud.). (Plate XVIII. figs. 7–10.)

On the 9th of October, 1873, I received a letter from Dr. J. Alexander Macdonald, of Woburn, Bedfordshire, stating that he had forwarded to me a pigeon which had been found dead on the previous morning. It seems that the owner of the bird had erected a large pigeon-house, and had imported a number of Antwerp Smerles, these birds all continuing in a perfect state of health until about a week before the above-mentioned date, when, to use Dr. Macdonald's words, "first one and then another was attacked, and so on, until four or five of the pigeons had died after a few hours' illness." The suddenness of these attacks not unnaturally suggested poisoning; and, accordingly, says my informant, the owner "had the curiosity to open one of the birds, when, to his astonishment, he found the intestines stuffed with worms."

Under these circumstances I was invited to make an accurate inspection of the pigeon forwarded to me, likewise to report the results of my examination, and to suggest any remedial or prophylactic measures which might be likely to prove useful.

Two days later I received a letter from Dr. Macdonald stating that several others of the flock had died, and it further appeared to him probable that the daily list of sick and dying would continue to increase. On the 14th of the month my informant reported that three more of the birds were dead; but this mortality still left twenty-five birds in the owner's possessson, some of which were affected. Under these circumstances I lost no time in forwarding a full report of the facts observed, together with the recommendation that a few grains of santonine should be mixed with the food. Speaking of the birds seen on the 14th October, Dr. Macdonald says that "one which appeared in a hopeless state was at once treated (by the owner) to two grains of santonine;" and when my informant saw the bird in the afternoon of the same day "it had so far recovered as to be hopping about and picking up food." On the 4th of the following November the same correspondent obligingly informed me that the epidemic had been "at last mastered." It seems that altogether twelve birds had perished, the remainder now appearing perfectly healthy.

It is not stated whether the final and satisfactory result appeared to be due to the administration of the remedies I had recommended; but, in any case, the cessation of the disorder following so close upon the employment of santonine is worthy of being noticed. I had almost hoped that my report, in the interests of science, would be published; but, so far as I am aware, such has not been the case. In regard, however, to the dissection (upon which that report was mainly based), I have felt sure that the scientific and practical data it supplied were of sufficient interest to be placed on permanent record. The necessary dissections and microscopic examinations were made on the 9th and 10th of October, whilst the bird was perfectly fresh. The blood, muscles, and cellular tissues, and every organ of the body, apart from the digestive apparatus, were found to be thoroughly healthy; and it was only when the alimentary canal

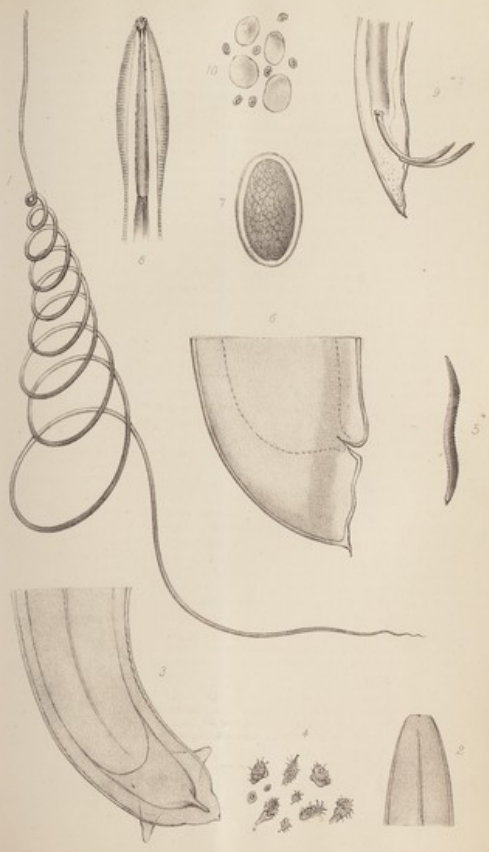
had been laid open that I found any visible traces of parasitic disease. From the lower opening of the crop downwards to the termination of the small intestine the canal was more or less crowded with nematode entozoa, all of them being referable to the above-named species. In spite of this state of things, and notwithstanding, also, that the small intestine was inflamed throughout (showing several large ulcerated patches, which here and there measured fully an inch in length), the body of the pigeon exhibited no traces of emaciation. From this it was evident that the parasites had developed rapidly, and that the malady had a correspondingly rapid formation. The local distribution of the parasites themselves was especially noteworthy. One specimen, two inches long, extended from the crop to the proventriculus. The cavity of this latter organ and also that of the gizzard were crammed with worms, which completely blocked the passage between the two. Three of the worms had also placed themselves within the pyloric opening, their bodies partly lodging within the upper part of the duodenum. The duodenum itself was crowded with these Ascarides; but their numbers somewhat decreased towards the lower folds of the small intestine. I removed 36 worms from the œsophagus, proventriculus, and stomach, besides 166 others from the intestinal canal, thus obtaining a total of no less than 202 nematodes from this solitary avian bearer. Considering the comparatively large size of these entozoa, this high degree of infection must be pronounced remarkable. The largest female worms measured $2\frac{1}{2}$ inches in length. One of the most interesting facts—serving to exemplify a well-known habit of lumbricoid worms generally—consisted in the circumstance that two of the parasites had succeeded in perforating the horny lining membrane of the gizzard. The injuries had evidently been accomplished during the life of the host, since the walls of the gizzard were inflamed opposite the perforations made by the heads of the parasites. There was a little half-digested and green food within the stomach, the débris of which, when placed under the microscope, showed several characteristic nematode ova. There were no free embryos anywhere discernible; neither had the development of the freed eggs proceeded beyond the coarsely granular stage of yolk-segmentation. Free eggs were also found both in the small and large intestine. The eggs measured about $\frac{1}{144}$ inch by $\frac{1}{216}$ inch in diameter (fig. 7). An admirable description of the adult parasite has been given by Dujardin; but since the published figures of Rudolphi, Goeze, and Bremser are incomplete, I have thought it desirable to supplement them with others. Thus fig. 8 gives a magnified view of the head of a small female *Ascaris maculosa*, and shows especially the crenulations or transverse striæ on the *alæ* or lateral membranous appendages. These are veritable annulations, or fine circular striæ, and not merely contractions of the integument as some have supposed. Dujardin speaks of the alæ as scarcely visible; but I always found them more or less well marked and semielliptic in shape, as Rudolphi originally described them. Another sketch (fig. 9) shows an enlarged view of the tail of the male; and, on the whole, this representation agrees

very well with Bremser's figure of the same part. The arcuate spicules, however, are not so sharp at their tips as his illustration implies, and they are certainly more uniform in thickness. Dujardin remarks that Rudolphi has represented the spicules as being straight, whereas he himself always found them curved. Rudolphi, however, was scarcely in error, since I have repeatedly noticed that these arcuate organs are very nearly straight in their perfectly retracted condition. Like Dujardin (and without having previously consulted his description), I was particularly struck with the appearances presented by certain large perivisceral corpuscles, the presence of which originally suggested the specific name of the worm. Dujardin very appropriately calls them *corpuscules orbiculaires diaphanes*, but compares them, somewhat unfortunately, to little acephalocysts. These bodies, as he says, are many times larger than the ova. For my own part, I believe they are nutritive in character, and, like the fluid in which they float, are, I suspect, chemically comparable to the juice of flesh. At all events, Dr. Marcet has proved that the perivisceral fluid of the large lumbricoid worm of the horse (*Ascaris megalocephala*) partakes of this character; and it is no uncommon thing to notice similar corpuscles in the bodies of other nematode worms. Dujardin himself refers to similar bodies in an Ascaris from the Perroquet. I have purposely represented a few of the eggs along with the nutritive corpuscles, side by side, in order to show their relative sizes (fig. 10).

Notwithstanding the facts thus set forth in connexion with the parasitic epidemic affecting the Antwerp Smerles, the entozoa in question do not appear to be very common. Dujardin has remarked that Heister, at Rostok, and Gebauer, at Breslau, found this parasite abundant at the beginning of the 18th century; but, according to examinations conducted at Vienna, the worm was found in the Common Pigeon in only 11 instances out of 245, and thrice only in 38 examples of the Ring-Dove; moreover the examination of 87 other pigeons and doves of different species yielded entirely negative results. These data are of high practical interest, and they serve to throw light upon questions of epidemiology. I may add that the Dublin helminthologist, Bellingham, long ago noticed the occurrence of this parasite in Ireland.

EXPLANATION OF PLATE XVIII.

Fig. 1. *Filaria gracilis*: male, nat. size.
2. The same: head and neck, enlarged 40 diam.
3. The same: tail, mag. 70 diam.
4. The same: spermatozoa, mag. 350 diam.
5. *Spiroptera turgida*: nat. size.
6. *Ascaris cuspidata*: tail, enlarged.
7. *Ascaris maculosa*: egg, mag. 330 diam.
8. The same: head of female, mag. 20 diam.
9. The same: tail of male, mag. 35 diam.
10. The same: eggs and nutritive corpuscles, enlarged.



T.S.C. del. et lith. M.& N. Hanhart, imp.

NEW OR RARE NEMATODE ENTOZOA.

[*From the* Proceedings of the Royal Society, No. 150, 1874.]

ON THE

ANATOMY AND HABITS

OF THE

GENUS *PHRONIMA* (Latr.).

BY

JOHN DENIS MACDONALD, M.D., F.R.S.,

STAFF SURGEON ROYAL NAVY, ASSISTANT PROFESSOR OF NAVAL HYGIENE, NETLEY MEDICAL SCHOOL.

Of all groups of Crustacea the Amphipoda would appear to exhibit the widest range, in the modification of their parts or organs, without obliterating the delicate lines of natural affinity running through them as a whole. This is well exemplified in the interesting paper of Dr. R. Willemöes-Suhm, Naturalist to the 'Challenger' Exploring-Expedition, "On a new Genus of Amphipod Crustaceans" founded by him, and named *Thaumops* (Phil. Trans. vol. clxiii. p. 629). This genus, although exhibiting many characters in common with *Phronima*, presents some striking points of difference traceable in the external jaw-feet, caudal appendages, the position of the generative pore, and certain particulars in its internal anatomy.

Of the several characters given of *Thaumops* the presence of only four caudal appendages is perhaps the most exceptional; for in the generality of the Uropteра there are six besides the telson, which is obviously the equivalent of the seventh abdominal segment of Macrura. Thus the six pairs of abdominal appendages, including the tail-fins of the prawn or shrimp, for example, are represented in *Phronima* and its allies, with the exception of *Thaumops*. The manner in which the fourth and fifth pair of swimmerets and the caudal fins of Macrura are modified in the Uropteра is well seen in the accompanying figures; fig. 2 in *Phronima* and fig. 3 in a neighbouring group of Hyperians, which also serve to show why, at least in the former genus, only five segments have hitherto been recognized in the abdomen.

During the exploratory voyage of H.M.S. 'Herald,' in the S.W. Pacific, numerous species which I have always been in the habit of referring to the genus *Phronima*, were taken in the towing-net; and I may remark that the assumed parasitic habit of these creatures was never, at least, a prominent fact to me; they were so often taken either perfectly free, or tenanting a nidamental case, such as that subsequently to be described. Those who, like Dr. Suhm, are acquainted with deep-sea dredging are usually cautious how they refer the doubtful products to their proper habitat; whether it be the bottom that has been reached, or some zone of the watery space above. Indeed it is quite possible for

the narrow area of the tallow-arming of the deep-sea lead to include fortuitously, and carry down *Phronima* or any other little crustacean naturally living near the surface; and contact with the bottom would finally press it into the tallow, so as to mislead the observer as to its true habitat. Conversely, in bringing up the dredge from a given depth, it may finally carry with it any more superficial objects casually lying in the track which it takes.

Fig. 1 of the accompanying drawings from nature represents a species of *Phronima* captured in lat. 30° 16′ S., long. 176° 27′ W., of which I find the following description in my notes taken at the time:—

Head exceedingly elongated from above downwards, with considerable enlargement of the back part, which contains two masses of transparent, rounded ?, and tapering columns, whose bases occupy the fulness of the poll, exhibiting a regularly facetted appearance externally on two rounded protuberances. The apices of these organs, on the other hand, converge and blend with the rays of the compound eye, of which, indeed, they may be said to form part, probably assisting nocturnal vision by the production of luminosity. The long axis of the head is therefore at right angles with that of the body, its form being full above and gradually tapering downwards. The aperture of the mouth is situated at the smaller extremity, inferiorly guarded by its mandibulæ, maxillæ, and one pair of foot-jaws, appertaining to the cephalic segments. The latter organs are more distinctly recognizable than the others, on account of their more superficial position. They are short, apparently united at the base, and curved forwards, terminating in two ovate and acute appendages lying side by side, serrated on the borders and beset with short hairs.

The eyes are small where the facets are lateral, and the apices are invested with black pigment, but large where they swell out the back of the head, the points of the same cones meeting in a red spot, quite distinct from, and internal to the black one. This condition is also observable in the Hyperians, and is worthy of further study in a physiological point of view.

Two minute two-jointed antennæ arise from the head, just above and in front of the eyes. The posterior or upper surface of the second joint of these antennæ is clothed with short stiff hairs.

There are seven pairs of thoracic limbs; but the first two are separated from the others, to some little extent, by arising on a plane anterior and inferior to them, the first two tergal pieces being somewhat wider than those of the succeeding segments. They are, moreover, distinguished from the other limbs by possessing a minute spine-like movable claw, bounded, on either side, by a short styliform process. There is also a rudimentary manus developed upon the posterior part of the second segment above this claw, that of the first pair being the stoutest, although the limbs themselves are the smaller of the two. Both pairs of members now described, while they very distinctly belong to the thorax, act the

part of foot-jaws as in the higher Crustacea. The five succeeding pairs are more especially restricted to the thorax, the third or middle one being stoutly chelate, normally directed backwards, with the pollex superior, and enjoying a very considerable range of motion.

The first pair is usually thrown forwards over the head, and the last backwards over the abdomen, the first flexure corresponding with the first swimmeret; the second pair is the longest.

The fourth, fifth, and sixth thoracic segments are each furnished with a pair of elongated and laterally compressed respiratory vesicles, connected with the posterior and inferior part of the epimeral pieces behind the articulation of the corresponding limbs. These vesicles increase in size from before backwards; and indeed a very rudimentary one may be seen behind the third pair of limbs. The last, or seventh, thoracic segment is of unusual length, tapering posteriorly, to correspond with the narrowness of the abdomen, in which also the segments are of greater length than those of the thorax. It has been already stated that in *Phronima*, as in the Macrura, there are seven segments in the posterior division of the body; functionally, however, three of these may be said to belong to the abdomen and four to the tail. The three abdominal segments bear each a pair of swimmerets, arising near their posterior border, and consisting of a stout or inflated foot-stalk and two narrow, acuminate, annulated and setaceous terminal pieces.

The three anterior of the four caudal segments bear a narrow fan, consisting of three pairs of slender appendages furnished with two short styliform tips. The first and third of these caudal members are much longer than the second.

The oral organs (with the exception of the jaw-feet), the large chelæ, and the foot-stalks of the swimmerets are tinted with a rich purple pigment. All the other parts are hyaline and transparent.

EXPLANATION OF THE PLATE.

Fig. 1. Lateral view of *Phronima* (species ——?). With the exception of the antennæ, the limbs and appendages of one side only are represented, magnified about 9 times.

Fig. 2. The abdomen and tail of the same, further enlarged, to show the several segments, numbered (from before backwards) 1, 2, and 3, with swimmerets, and 4, 5, and 6 bearing caudal appendages, while 7 is the terminal segment or "telson."

Fig. 3. The abdomen and tail of a Hyperian for comparison, all the numbers having the same signification *.

The evidence of Dr. Willemöes-Suhm supports my own experience, that there is no metamorphosis in this group; and as it is very probable that the history of the development of *Thaumops* would resemble that of *Phronima*, the following observations may be of some importance, as

* 1', 2', 3', 4', 5', and 6' being the appendages of the corresponding segments.

carrying the process a little further than it has perhaps yet been traced by him:—

In lat. 21° 0′ S. and long. 17° 45′ W. off the island of Ono, Fiji Group, apparently the same species of *Phronima* as that above described was taken in the towing-net, but with the addition of a numerous progeny of young in a large gelatinous but tough nidamental case. This interesting nest was shaped like a barrel, but with both ends open, and the external surface was somewhat tuberculated and uneven. The wall of the tube presented numerous round and puckered openings, observing no very definite arrangement, but through which entering currents were observed to pass. These openings generally, though not invariably, pierced the tuberculations.

An external membrane, with an internal lining, were distinctly visible, both seeming to be continuous at the rims of the tube. The space between these layers was filled up with a pulpy substance, in which scattered nucleiform bodies were detected with a higher power of the microscope.

I have been particular in the description of the case, as some far-fetched guesses were made as to its real nature. The cutting, piercing, and tearing implements of *Phronima* would very soon alter and reduce a bell-shaped Medusa, a Salpian, or a Pyrosoma tube to the required pattern; for there is usually a great uniformity in the character and appearance of this case.

"With regard to *Phronima*," says Mr. Spence Bate*, "our knowledge is small: its habit is that of an inhabitant of the gill-cavities of some one or more species of *Medusa*; but in the Collection of the British Museum, entrusted to my care for examination, is a very curious case that was sent home from Naples by S. P. Pratt, Esq., as being the one in which the animal was taken. The structure is thick, fleshy, semitransparent, and studded over the surface and round the orifices—one of which is smaller than the other—with numerous white excrescences. Examination with the microscope shows the substance to be pervaded by bundles of fibres; each fasciculus is twisted together near its centre; these, some of them being larger than others, star the structure thickly, and still more plentifully where the white excrescences appear."

However problematical the nature of the case, that its use is for nidification there can be no further doubt.

Though I have already given figures of the specimen above noticed to my friend Major Holland, R.M.L.I., for a paper on *Phronima*, published in 'Science Gossip' (April 1869), I trust that allusion to it here may not be out of place.

In a subsequent commission on the North-American and West-Indian Station in H.M.S. 'Icarus,' I have frequently captured "*Phronima* in its bag," as my messmates would say. In order to bring the swimmerets into full play, the animal protrudes its body tail foremost from the case,

* Annals and Magazine of Natural History (Third Series), March 1858.

only calling into use the fine tips of the third and fourth pairs of thoracic limbs to hold fast its charge. When it fully retires into the case, the claws of the two posterior pairs of legs are pressed backwards against the lining membrane, so as still more effectually to secure its hold on the approach of danger.



(For private circulation only.)

LECTURE.

Friday, February 13, 1874.

ADMIRAL SIR R. SPENCER ROBINSON, K.C.B., in the Chair.

ON THE VENTILATION OF SHIPS, ESPECIALLY OF LOW FREEBOARD, AND HOSPITAL SHIPS.

By JOHN DENIS MACDONALD, M.D., F.R.S., Staff-Surgeon, R.N., Assistant-Professor of Naval Hygiene, Netley Medical School.

On the nomination of the Director-General of the Medical Department of the Navy, and with the sanction of the Admiralty, I have undertaken to deliver a lecture, to the members of this Institution, on the subject of "Ship Ventilation"—a subject which has always been an important one to our Navy, but is more particularly so at the present time, arising from the rapid and extraordinary march of the science of naval architecture of late years, in keeping with the requirements and exigencies of modern warfare.

I presume then it will be quite unnecessary to take up much of your time with preliminary remarks on the hygienic value of wholesome, fresh air, or recall to your minds the catalogue of evils incident, either to its inefficient supply or to its vitiation from any cause. This being conceded, I shall proceed at once to the review of principles, and of such systems as may deserve special notice, referring to any historical matters of interest connected with them; and finally, make some few observations, suggested by the study of the subject, which, I venture to hope, may be of practical utility.

It is usual to divide the sources or means of ventilation into (1) Natural and (2) Artificial.

To the natural category belong, first and especially, the prevailing winds and other aerial currents, which should be aided by wind-sails and air-shafts; and, secondly, those local differences occurring in the specific gravity of neighbouring columns of air, arising from the operation of existing or ordinary conditions, which should be favoured by a suitable arrangement of conduits and uptake cowls.

The artificial means being quite analogous to the natural, may be divided in a similar way, viz.:—first, mechanical propulsion, by rotatory fans, &c., establishing in-, or out-going currents, as the case may be; and, secondly, extraction by heat, applied artificially, and acting through special channels. In addition to these leading principles, we must pay due attention to the ceaseless action of the law of the diffusion of

gases, which is too frequently overlooked by those whose acquaintance with the resources of mechanics may be competent enough in the abstract.

The expansibility and elastic property of common air may be regarded as its intrinsic cause of movement, so as to afford in it an interesting contrast with the blood of animals. Thus the blood, as an incompressible fluid, is circulated through the animal economy by means of the combined contractility and elastic force of the heart and vessels in which it is contained; but the air which it is our object to renew and circulate in circumscribed localities is, in itself, quite elastic and compressible, while the walls of the channels by which it enters or finds its escape, are usually rigid and unyielding. Its elasticity and expansion, moreover, increasing as they do with the temperature, the tension thus induced will naturally render it mobile, so to speak, and determine its movement in that direction in which it finds the least resistance. Here, then, we have an explanation of the ascensional tendency of air contained in the openings between the timbers of wooden ships, or in laterally confined spaces, with an elevation of temperature. Thus, when no aspiratory or propulsive force is in operation, an updraught will be produced by an increase of temperature with a concomitant increase of volume in one portion of air as compared with another in its immediate vicinity. This ascensional force, resulting in ordinary as a spontaneous effect, may be assisted or directed by artificial means, such as the systems proposed by McKinnell and Watson, or the cause itself may be further developed by the direct application of heat, after the system of Sutton, who, availing himself of the galley fire, turned an existing source of caloric to the most economical account.

McKinnell's tube within tube, and Watson's tube divided by a longitudinal septum, as in the shafts of mines, are but slightly different applications of the same principle, the currents passing in opposite directions being concentric in one case, and side by side in the other. They are both intended to favour natural ventilation.

The system of Sutton, on the other hand, is more definitely artificial, heat being specially employed to effect an extractive movement of the air requiring to be withdrawn. It may be well here to compare the relative merits of what has been denominated the plenum principle, and that of extraction just referred to.

When Mr. Sutton was advocating his method of extracting foul air from ships by the agency of heat, Sir Jacob Ackworth the then Surveyor of the Navy put the question, "Do you know how far you are to draw it out?" The answer was, "Only six inches, for, if I can extract it never so small a distance, the incumbent air will press forward of course, and cause a constant change." From that period to the present, the plan of extraction, and the plenum principle so-called, have each had their own supporters, and good authorities are still divided in opinion on the subject. The safest position would seem to be that circumstances alone must suggest the applicability of the one or the other in any particular case. I think, however, it will be admitted that from the very configuration and structure of a ship, the natural mode of

ingress of pure air is through the central part of the body, while the egress of foul air should be lateral. Now this proposition is not at all incompatible with independent lateral modes of ingress, and central channels of egress, which, although holding a secondary place, are nevertheless quite indispensable. The effect of a spring on the cable, with open ports to the wind, and of an axial air tube in a steamer's funnel, would respectively illustrate such cases. We know also that gases diffuse or commingle with a rapidity which is inversely as the square root of their density. Active egress would therefore favour the law of diffusion *without* or outside the body of the vessel, while by active ingress, or plenum ventilation, the exercise of that law is fostered *within*. Moreover, in the former case, the air would be insensibly renewed, while in the latter, cold draught is usually a concomitant. In a moral point of view, it is quite proverbial, that an attractive or drawing force is more effective than a propulsive one, and the same would appear to be the case in the physical world. Thus, for example, it would be easier to suck or draw up a given quantity of water from a depth of 20 feet, than to impel it to a corresponding height. So also it would be easier to extract 1,000 cubic feet of air from the body of a vessel, than to force the same bulk of air into it. The pressure on the contained air would be diminished in one case, so as to induce the ingress of denser air at every opening, while the pressure though increased in the other, would not only more tardily produce the opposite effect, but favour stagnation in the very localities, that would require the most efficient ventilation.

If there is no fallacy in this reasoning, we might conclude that extraction would be preferable to inward propulsion, were we restricted to either. My own feeling, however, is, that no restriction of this kind need exist, and that both may be employed simultaneously when it would be feasible to do so.

The ventilation of circumscribed or inclosed spaces, should be made to imitate mild open air conditions as nearly as possible, and its motto should be "*bonâ fide* perpetual and insensible change."

This is the great desideratum, and I shall now pass on to notice some of the more important systems, proposed or adopted with the view of accomplishing it, commencing with natural ventilation, or, rather the means intended to make it more efficient.

Ventilation by windsails, appears to have been the earliest system in use on board ship, and I am not aware that the precise period of their introduction is known.

They were probably suggested by the observed deflecting power of ordinary sails, more especially when a ship is "on a wind" as nautical men express it. A great volume of air is often sent down through the waist of a ship, from the body of the mainsail, or main trysail, and it is common enough to spread small sheets of canvas, even between decks, to deflect a current of air through a hatch or other convenient opening to ventilate the space below. This principle was known to the ancient Egyptians, from whom the so-called *mulguf*, or wind conductor, has been handed down to the present inhabitants of Cairo, and other neighbouring towns. The mouths of the "mulguf" were so arranged

as to open, at the top of the house in the direction of the prevalent winds.

The windsail in its simplest form consists of a small expansion of canvas spread out to the wind, and connected below with a cylindrical portion, of sufficient length to reach the floor of the space which it is intended to ventilate.

On some unhealthy stations as on the West Coast of Africa, very great length is given to the windsails, with the view of taking the air from a presumably purer stratum than that immediately resting on the water. Figs. 1 and 2, exhibit the two more usual forms of the windsail, viz., the shark's mouth, which requires to be trimmed to the wind; and the fixed form which is so constructed as to receive the wind from whatever direction it may come.

With the high pretensions of this latter, it is scarcely possible that it could be so effective, at any particular time, as the shark's mouth. I have, however, attempted an improvement in it, as is shown in Fig. 3, in which the throat is collapsible, so as to interfere as little as possible with the passage of the air into the shaft. It may be necessary to give the shaft of a windsail, a cunning curvature, when it would otherwise play into the hammocks, and chill the bodies of the sleepers in its vicinity. In some instances, instead of the lower extremity being simply open, it is rounded off, and furnished with numerous perforations, by which means the air is more equably diffused.

The principle of the windsail is also applied in the construction of a receiving cowl and conduit of more rigid material, namely, sheet-iron or copper, which has the advantage of being permanent, and may be used for up, or down draught, as it may be found necessary.

Cowls may be simply *movable*, requiring adjustment by hand, or automatic, needing no interference, being freely versatile on a pivot, and offering the least resistance to the current of air when in the required position.

The known aspiratory effect of ordinary cowls when turned away from the wind, has given rise to numerous modifications of the apparatus, with the object of suiting it to the more efficient discharge of the uptake function alone. In order to comprehend the *rationale* of this function, some little knowledge of the physical properties of common air is necessary. It must be remembered that air is a mobile and elastic fluid, susceptible of very considerable changes in its state and condition by the operation of ordinary and often apparently trifling causes. Its molecules exert an attraction for each other, so that one moving particle would tend to carry other neighbouring particles with it; and those laws by which any disturbance in the equilibrium is restored, exhibit a proportionate power in the reaction. It is thus subject to mechanical movement, friction, stagnation, compression, and rarefaction more or less approaching a vacuum. Now when this latter condition is induced by artificial means, which is only another way of expressing natural means wielded by art, we avail ourselves of the movement, originating in the effort to restore equilibrium, in connection with the mutual attraction of contiguous particles already mentioned.

Figs. 4, 5, 6, and 7, exhibit several forms of uptake cowls. Fig. 4 is apparently a very excellent one, invented by Mr. Boyle a few years ago. It was supplied to Her Majesty's ships "Lord Warden," "Monarch," and others for trial. In the former vessel, it was fitted to one of the air-shafts opening upon the poop, and communicating with the bread-room, shaft-alley, and the limbers. This apparatus consists of a short horizontal tube or cylinder, resting and admitting of rotation upon the upper end of the air-shaft, which opens into it below. But the most ingenious part of the arrangement is an infolded conical curtain, with an axial cone of metal so placed within it, as to allow the air to pass from apex to base, under a certain degree of compression, thus eliciting the air from the shaft, which is carried off at the opposite end of the cylinder, with a force proportionate to that of the wind rushing through it. A small model of this contrivance, exhibited by the inventor, acted in a very satisfactory manner. The shaft was made of glass, so that by gently blowing through the horizontal tube, the rapid ascent of a piece of cotton wool placed loosely on the hand was rendered visible. I may remark, however, that I have seen the same effect with other models constructed for a similar purpose, and must agree with my distinguished colleague Professor Parkes, who is of opinion, that, nothing but a critical series of comparative experiments tested by the anemometer, can enable us to form a just estimate of the relative superiority of one form over another. Fig. 5 acts by simply turning its expanded mouth away from the wind, and Fig. 6 resembles it, but there are several bell mouths placed one within the other. Lastly, in Fig. 7 the aspiratory effect is produced by axial and circumferential currents.

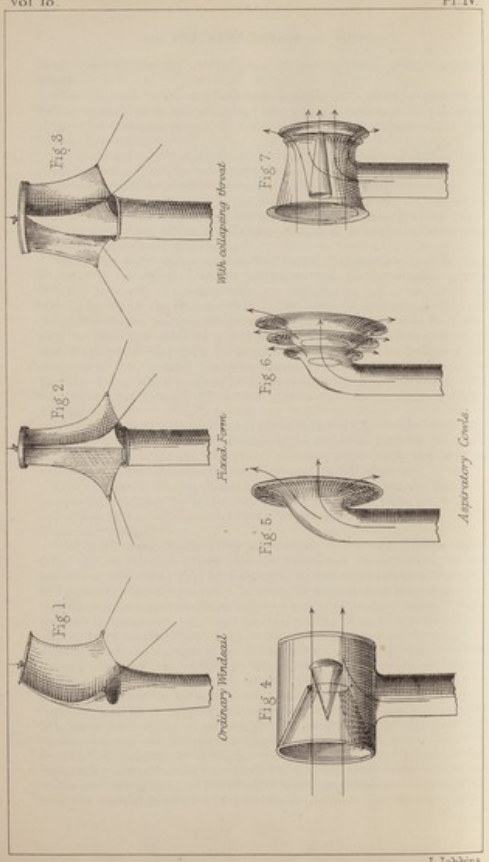
I shall now briefly describe the ventilation of the several decks and compartments of Her Majesty's ship "Lord Warden," as a fair example of an ordinary ironclad. As in too many other cases, the more important features of the ventilation of this ship formed no part of her original design, but were chiefly carried out by Captain Brandreth, R.N., and the dockyard authorities at Malta, subsequently to her arrival on the Mediterranean Station.

Little need be said of the ventilation of the main deck, which is sufficiently insured by the ample size of the numerous hatches, fore and aft in the middle line, and the lateral inlets of the ports, so long as they may safely remain open at sea.

A considerable ascensional force is generated by the heat of the galleys, of which there are two, viz., one for the Officers and one for the ship's company. A large volume of air within the funnel-casings is also constantly moving upwards while under steam. It is important to remember that the plates or sashes of these casings, where they communicate with the decks of ships, usually move on transverse pinions, so as to be capable of taking two oblique positions, but with a difference in the course of the air-currents in each case. When the upper half of the sash is pressed inwards towards the funnel the ascent of the heated air from the deck is secured, whereas an opposite result will follow when it is trimmed in the contrary way.

The excellent scheme of ventilation devised by Arnot for the Field

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J. Jobbins

Lane Ragged Schools has been adopted to some extent in the "Lord Warden," "Royal Oak," and several other ironclads. Thus, a number of stout copper tubes, with curved necks and bell mouths, are distributed at stated intervals along either side of the upper and poop decks. These tubes open flush between the beams of the lower deck over the mess and sleeping places of the men. But, where cabins are built, they extend lower down, and give off lateral branches, which pierce the contiguous bulkheads, so as to supply two or more compartments, either by their free ends or suitable openings made in their sides. To obviate the draught-like current of air naturally resulting from this arrangement, the extremities of the tubes are turned slightly upwards. By this means the air simply overflows into the surrounding space.

Since the arrival of the "Lord Warden" at Malta, ample and well-protected openings were made through the decks with good effect, wherever it was feasible to do so. Two of these ventilate the after part of the ward-room, which was formerly a *cul de sac*, and was frequently very close when the mess servants were employed there. Others, of similar size, bring the engineer's berth and the gun-room into communication with the main deck; these compartments being also ventilated from the upper deck by cowls and air shafts. Nevertheless, it is obvious that, in the arrangements spoken of, one deck must be benefited at the expense of another, which I maintain need never be the case, if the provision of proper channels for the escape of foul air is made in the original design of the ship.

Captain Brandreth, taking advantage of the strong currents of air usually entering the bow-ports on the main deck, placed a large cowl on either side, so as to deflect it downwards to the lower deck, or even to the fore store-room, a locality much requiring ventilation in most ships.

The removal of foul air from the lower deck is assisted by side-shafts or channels running beneath the beams, and opening into the three hollow masts, and into the fore and after funnel-casings, all of which materially favour the up-draught from below.

A single cowl just in front of the foremast, surmounts a tube which descends to the fore store-room, where it divides into two branches to supply the prison cells, which are thus rather better ventilated than the surrounding space.

The carpenter's and gunner's store-rooms receive a good supply of air from two ample cowls on the top-gallant forecastle.

The magazines (fore and after), are ventilated on Captain Jerningham's plan with rotatory fans and tubular down-takes opening upon the floors from beneath; and there can be no doubt that extraction would, in this case, be preferable to the plenum principle. Dust is raised in clouds, and the pressure must be very considerable under existing circumstances. The magazines in most ships are now ventilated in this way, and the system obviously demands a radical change.

Fresh air is conveyed to the bread-room through a valve-door opening in one of the two square air-shafts descending from the poop;

the equivalent valve in the other shaft acting as an outlet for impure air. The starboard shaft, moreover, passes down with the necessary curvature to the limber on the corresponding side of the screw-bed, while the port one terminates simply at the roof of the alley. By throwing back both valves in the bread-room, it will be seen that all communication with the lower parts of the ship will be cut off, and while this is the case, the smell of biscuit imbuing the hot air is often offensively strong at the mouth of the up-take cowl.

The bilge air finds access to the furnaces, and the two large tubes by which the ash-buckets are brought up from the stokehold act most efficiently as ventilators—one usually acting as an uptake, while the other discharges an opposite function.

By an ingenious contrivance, also, the foul air of the stokehold is carried up through a tube in the axis of each funnel, a powerful draught being generated by the surrounding heat. Since this improvement was effected, the temperature of the stokehold under full steam is known to have fallen as many as 40° in certain places.

Finally, by the judicious arrangement of septa or curtains of sheet iron, so as to determine up and down currents on the opposite sides, the ventilation of both engine-room and stokehold has been much improved. I have been thus particular in describing the ordinary means of ship ventilation, that you may be better able to comprehend what a little further exercise of art can do.

Sir Gilbert Blane notices briefly a ventilating appliance, which was fitted to the French frigate "La Nymphe." This consisted of "a "square wooden pipe, of about 9 inches in the side, coming from "between decks, running along the side of the ship, and opening over "the gunwale of the forecastle." The idea appears to have been copied in some of our ships at the time, and we read of the substitution of metal for wooden tubes subsequently. A comparative estimate of its utility may be formed from some further remarks of the same author. Thus, a "better contrivance than this," he says, "has lately been "adopted on board some hospital and prison ships. It consists in an "aperture made in the middle of the deck overhead, 3 feet long by 1½ "wide, from whence a tube ascends, tapering into the open air about "6 feet above the upper deck; and to prevent strong currents of air "from descending, a screen is made to traverse with the wind by means "of a vane, so as to keep the opening to leeward of it."

In the further history of this interesting subject, we have to notice some of the more important schemes or means of artificial ventilation.

During the greater part of the last century the hygienic requirements of the Houses of Lords and Commons seem to have given a considerable impulse, though of a remitting kind, to the whole subject of ventilation, whether applied to public buildings, private dwellings, or ships.

From the ventilation of the House of Commons, and originating with a single individual, the celebrated Dr. Desaguliers, we may be said to have derived two of our present leading agencies in ship-ventilation, namely, the extracting power of heat, and the rotatory fan. True, indeed, Mr. Sutton, apparently without any knowledge of Desaguliers previous

succeeding decks would be carried off by eight longitudinal air-shafts, engaging with the intercostal openings, an area equivalent to about one-sixth of that of the solid measurement of the ship's walls or shell. The advantages that may be fairly expected to accrue from the adoption of this scheme may be stated as follows:—

1. The admixture of bilge-air with that of the orlop deck is rendered nearly impossible by its continual removal through the shelf-tube of the lower deck, leaving but little scope for the operation of the law of diffusion.

2. The bilge air being carried away by the shelf-tube of the lower deck is effectually cut off from intrusion upon any other deck.

3. The air ascending from each of the succeeding decks is separately eliminated, without any possibility of commingling by reversal of the currents or otherwise.

4. As a corollary, the pouring forth of foul air from the lower parts of the ship into the living and sleeping places of the Officers and men would be entirely prevented.

5. The extracting power would be exerted upon the mass of air just beneath, or between the beams, where the products of respiration and other impurities naturally accumulate, while its place would be taken up by purer air.

6. No stagnation of air could happen in any part of the shell, so as to favour the tenacity of infection; under which circumstances, also, fumigation would be always more effective than it is usually found to be.

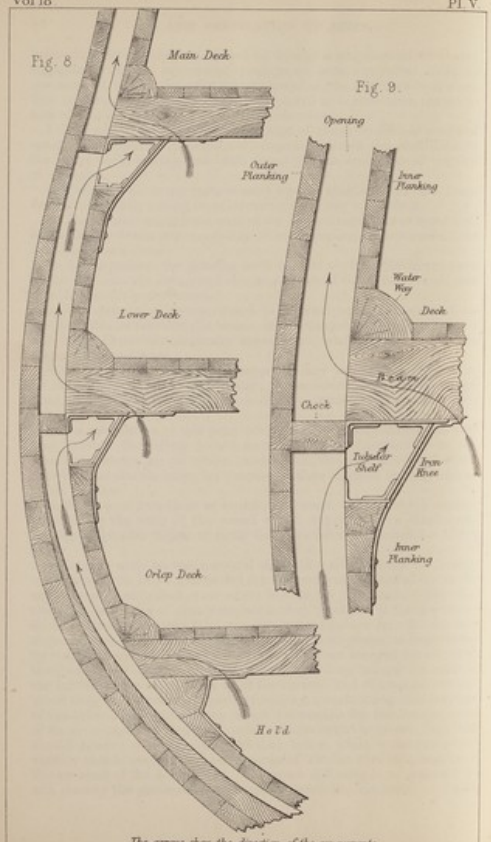
In the application of this principle to iron ships, any difficulties that may prevent themselves would certainly not be insuperable to the practical constructor in whose hands, even the most stubborn materials become plastic, when his purpose is once inspired to carry out an object in view.

In the case of iron ships as in those of wood, the extensive surface afforded by the walls of the vessel would suggest that a similar advantage might be taken of it for the more efficient ventilation of the hull.

Whatever may be the material of which a ship is built, no locality would appear to be so well suited for the removal of vitiated air as the angle beneath each deck, just where it joins the ship's side. This fact simplifies our idea of ship ventilation very much, for if there were 50 decks, vitiated air would take up the same position in all. In iron ships, however, the office of iron knees and shelf-pieces is superseded by the buttress-like ends of the beams, which are secured by rivets to the frames, so that the difficulty of running a longitudinal air-tube in the locality referred to, meets us at the outset. But it must be remembered that, in the ordinary mode of forging and forming the buttress of the beams, an angular space is left favourable for the transmission of an air-tube, to which a corresponding form might be given. This tube should receive conduits from the deck below, while in the immediate vicinity should be planned the openings of similar conduits, leading to the air-shaft of the deck above. A second difficulty here presents itself, namely the perforation of stringer plates, the integrity of which

Journal R.U.S Institution.
Vol 18

Pl. V.



The arrows shew the direction of the air currents.

J. Jobbins

is regarded as of so much importance to the longitudinal strength of the ship. A little ingenuity of construction, however, would, doubtless, effect the purpose in question, with no material sacrifice of strength.

The system I have just detailed together with the adoption of the deck air-channels of Dr. Edmonds, and the intelligent use of all other available means would be well suited for hospital or prison ships. It will also be obvious that whether the air-shafts are made to communicate with the funnel-casing or the hollow masts, or furnished with extracting fans, the principle must be in effect the same.

Should vessels of the Monitor class be multipled in our Navy, the question of ventilation as applied to the circumstances of closed ports and hatches, necessitated either in action or in heavy weather, must become one of the greatest importance. To this class belong such vessels as Her Majesty's ships "Glatton" and "Devastation," a good general account of the ventilation of which, on the plenum principle, is given by a writer in the April number of "Naval Science" for last year. Of the former vessel it is stated that "a shaft 5 feet 6 inches "by 6 feet 4 inches, is sent from the top of the upper deck to the level of "the main deck just abaft the smoke stack, and is continued above "the upper deck to a height of 12 feet, the walls being rifle proof, and "furnished with doors leading to the upper and first decks, both of "which must, however, always be closed in rough weather.

"At the bottom of the shaft are four fans, connected with two "transverse tubes, the upper of which is 16 inches by 12 inches, and "the lower 16 inches square. The fans, propelled by the engine, "collect fresh air from the shaft and send it into the tubes, from "which latter it is conveyed by means of pipes to every cabin and "compartment of the ship, fore and aft, by goose-necked funnel ends, "that open a few inches from the floor of the deck, and are each "supplied with a screw valve. There are in the 'Glatton' 133 of "these outlets.

"The principle adopted in the 'Devastation,'" says this writer, "is pretty much the same, but the arrangements are different in "many respects. Thus, there are in this ship three shafts and five "fans. One shaft is fixed just abaft the mainmast, and has two fans. "The other shafts are placed just abaft the foremast, and are close to "each other, but distinct, the smaller enclosing one, and the larger, "two, fans. All these fans are provided with distinct sets of engines, "and work independently; but it is an important arrangement, and "one that contrasts favourably with the system adopted in the "'Glatton,' that if one or two shafts are blocked, or otherwise ren-"dered useless, the third can, by intercommunication, supply air to "all the compartments." It only now remains to be stated that, as no special provision is made for the removal of vitiated air in either of the ships referred to, after its more or less complete diffusion, it must find its escape as best it may. It will be seen that the constructors have been at much pains to isolate the function of the different fans, and provide against the contingency of injury to a part interrupting the action of the whole. It is, however, quite a question whether a

very effective ventilation could not be maintained, even without the rotatory fans, if only a fair channel of escape could be made for the impure and expanded air. The whole subject is, indeed, so new, and involving, as it does, considerations upon which good authorities are yet at issue, that I would approach with caution any criticism as to the detail of the system carried out in the ventilation of our low freeboard ships. What I have further to say, therefore, must deal with general principles.

In devising the arrangement of tubes for the supply of air on the plenum principle by means of propulsion, at least five essential particulars must be taken into account:—

1. The manner in which the external air is conveyed to the axis of the fans.
2. The size of the original shaft in relation to the required supply.
3. The number of divisions and subdivisions of that shaft.
4. The angle at which these divisions and subdivisions are given off.
5. The calibre of these latter as compared with that of the primary trunk.

The full consideration of these several points would take up more time than I have at my disposal at present, but they would form good subject matter for discussion. In conclusion, therefore, I shall only repeat my conviction, that the system of extraction would be found to be much more satisfactory than the plenum principle, as applied to the ventilation of our low freeboard ships.

I WILL now, with your permission, call attention to these diagrams. In this one, representing an athwart ship vertical section of the "Hercules," we have the double bottom laid open, the ventilation of which is a desideratum. This is the "Warrior," the first of our iron vessels made with large perforations in the frames to reduce the weight, a purpose which is carried out in a much more satisfactory way by the bracket-plate system introduced by Mr. Reed. Here we have a mixture *of* the longitudinal and transverse systems of framing, the frames and longitudinals dividing the bottom into compartments. Some use may, perhaps, be made of the wing passages for the purpose of ventilation, but in what way this may be done, has yet to be considered. A very considerable space may be used for that purpose. The "Lord Warden" is a wood-built ironclad, so called. Four and a half-inch plate was considered to be quite sufficient to repel shell at the time the "Warrior" was built, but as improvements in gunnery went on, additions to the thickness of the plate were found necessary, and an inch and a half plate was placed between the outer planking and the timbers in the "Lord Warden." This vessel was built solid, and is literally three times as strong as a ship with channels between the timbers. But still several of these were kept open and in communication with the bilge. This, a radical defect, the oldest and most persistent evil connected with wooden ships, actually exists in the "Lord Warden." Though in many instances the channels alluded to are preserved for the purpose of ventilating the walls of the vessel, they can only do so by pouring mephitic air into the officers' cabins, and into the sleeping space of the men.

In tracing out the different systems of ventilation already followed, I shall direct your attention to these plans on the wall. The upper plan represents the system of Desaguller. The rotatory wheel originally placed by him over the House of Commons in 1736 was rather defective. In the first place, its axis was perfectly central; it had about twelve radiating blades, and the discharge pipe gradually contracted instead of expanding in its course. Now there is great reason to believe that as air tubes are distributed through different parts of a vessel instead of becoming smaller and smaller they should expand in a certain ratio; extraction is also more satisfactorily

effected when the tubes gradually expand, as they diverge from the fan to the body of the vessel. *Here* (pointing) in the modern fan, we have several improvements introduced, by which, with a very much smaller machine, a relatively greater effect is produced, the arms of the fan are less numerous, and the axis is eccentric. *This* is a section seen end on, showing the position of the axis and the hollowing out of the fan blades on each side, near their attachment. The centrifugal force of the fan drives the air with very considerable velocity in the direction of the discharge pipe, and at the same time an in-going current takes place at the axis. It is absolutely necessary that in plenum ventilation this in-going current should be cut off from all communication with the body of the vessel, and pass down from the open air through a tube having direct communication therewith. Subsequently to this, other schemes of ventilation were proposed, viz., Mr. Sutton's and Dr. Hale's, which I have already mentioned.

In the early part of the present century an ingenious contrivance for the ventilation of ships was invented by Mr. Perkins. You will find an excellent account of it in a little work by Dr. Finlayson, R.N., upon the baneful effects of too frequently washing ships' decks, in which a plate of this ventilator is inserted as a frontispiece. You will perceive a tank on each side half filled with water, and communicating with each other across the ship by means of a horizontal pipe. With the upper part of these tanks other tubes are connected, viz., a discharge pipe, and one leading from the bilge, so that as the ship rolls from side to side by the pumping power produced, the bilge air is drawn up into one tank while it is expelled from the other. This principle has lately been carried out in a more satisfactory way by an American invention, under the name of "Thiers' Automatic Bilge Pump and Foghorn." The system has been adopted in the "Thetis," and in several other ships, and a commission appointed to report upon it gave a favourable account of its operation. The principle is the same as that of Perkins, but with this difference, that Perkins seems to have taken advantage of the pitch as well as the roll. In "Thiers'" plan, the motion is purely lateral, the rolling being taken into consideration. Mr Perkins, on the other hand, placed his tanks obliquely, so that a partial effect might attend the pitch as well as the roll. In Thiers' invention, a tube leads down to the bilge from the upper part of a cylinder placed on each side of the ship, and containing a portion of mercury, both cylinders being connected by a transverse tube. As the ship rolls to port, the mercury ejects the water accumulated on the corresponding cylinder, and conversely when it rolls to starboard the mercury will run along the connecting tube to the other side, and eject the water from the starboard cylinder. By a similar contrivance the bilge air is expelled, water being the moving piston, so to speak.

I have on the table several forms of cowls. The great point giving effect to *this* simple cowl (Fig. 5) is, that the expansion of the mouth is very considerable in proportion to the volume of the tube, seeing that one atom of air in motion will attract other atoms in its vicinity. *This* cowl (Fig. 6) was planned by myself on that principle by invaginating, as it were, a number of conical pieces or lips in this way, and it acts very effectively. In *this* instance (Fig. 7) the outer cylinder confines a portion of the air passing between it and the bell-like expansion af the air shaft, through the back of which another conical portion passes. The aspiratory effect of the circumferential current is such, that the passing air, by its homogeneous cohesion, attracts the air around the circumference of the shaft-mouth, and by the same law, provision is made for the central part of the current to be acted upon.

Mr. Boyle's cowl (Fig. 4) I have already given a description of. There seems much mechanism in it. I have here a model of the scheme proposed in diagrams 8 and 9. Here we have a lower-deck cabin with glass slides, so that you may see the effect of a taper burning in it, and over the shelf-piece of this cabin openings exist communicating with the channels between the timbers. *This* would be an open space, corresponding with the deck above; *here* are the ends of the beams cut off; here we have the tubular shelf-piece, and *here* a number of chocks which cut off these channels from all communication with the deck above. By lighting the taper, and placing it inside, we see the ill-effects of a circumscribed space. The light is now dim, owing to the foul air, but by putting an uptake cowl in communication with the tubular shelf, and imitating the motion of the wind by a pair

of bellows, we brighten the light. If you were to substitute a human being for this light, which would be perhaps a very fair thing, because one individual is equivalent to one candle, you will see that he could not live long under such circumstances.

Mr. Edwin Chadwick, C.B.: I will preface the questions which I am going to ask by a short statement. There was in Paris a gentleman who, of all other men of science, handled the air, if I may so speak, more boldly and successfully than anybody else I have met with; and he contracted to remove vitiated air from hospitals, to introduce fresh air at a given rate of temperature, and to remove it from any part at a given rate of speed. He did whatever was required of him on the principle of aspiration. I would not rely on my own observation, but I would greatly rely on the observation of General Morin, who has thoroughly studied this question. I was informed that in consequence of the great losses that had occurred in the transport of horses to the Crimea, they applied to the Professor, Leon Duvoir Le Blanc, who did this work for hospitals and other edifices, to ventilate ships; and I was told that he had accomplished the same result that was clearly accomplished in the hospitals—introducing air at a given warmth, and removing it at a given speed,—and that it was done with perfect success as to the horses. One cannot follow a lecture like this without plans in hand and a little consideration, but I should like to know from the lecturer what results he conceives are clearly attainable by the present state of knowledge or practice upon this question. Is it such that we can now do what was said to have been done, and indeed actually was done, with respect to the horses, namely, introduce air into any room, and at any temperature you liked, and remove it at a given speed, such as you would prescribe. I should like to know from him if we have attained the same results, and whether we can send down air at a given temperature into the lower parts of the ship, or any part where the ship may be crowded, and remove it at a given speed?

Dr. Macdonald: It can be done if only an order be given to do it. It may be expensive, no doubt. Instead of injecting air into a vessel, I should use extraction in preference, and certainly the amount of horse-power required to throw off, or to extract a certain number of cubic feet per minute, is a matter of simple calculation. You take, first of all, the effective velocity of the fan, which would be, perhaps, three-fourths of the entire velocity of the extremities of the fan. That, multiplied by the sectional area of the opening through which the air passes, will give you the number of cubic feet per minute. Then you have to take the other questions connected with it. Say the effective velocity is 70 feet per minute, then what would be the number of feet through which a heavy body must fall in order to attain the velocity of 70 feet. For 70 feet it comes to about 76·5. You have to multiply this again by the number of pounds. You can reduce the number of cubic feet of air into pounds by dividing by 13, because 13 cubic feet of air is equivalent to a pound. Then the whole result must be divided by 33,000, which is the number of pounds a horse will lift one foot per minute. That calculation could be easily done. Then, of course, if you wish to get warm air in, you must have combustion to effect that, and pass the air over the heating surface.

Mr. Chadwick: This result, it is to be borne in mind, was obtained in Paris, by the principle which General Morin approved, by the principle of suction direct from the chief source of vitiation, and not by fans, simply by means of heating the air of the ascending vitiated flues, at the exit of the tubes, and regulating that heat with skill. That, certainly, was done in the hospital La Riboisière, on a contract to change the air of the hospital, 56 cubic metres per bed, I think, for two sous a day, to keep it at 60°, to change it every hour, which is a rate of warming that we do not attain, with our cheap coal, in England. The Society of Arts have sent to the French naval department for particulars as to what was done with the horses. I may say, about the care of the sister service there, that, although they thought of ventilation, in consequence of the great loss of horses, it did not appear to have occurred to them to be necessary on account of the loss of men.

Dr. de Chaumont: I am inclined to think the question of Mr. Chadwick, which has been answered by Dr. Macdonald, is one entirely of expense, and, whether we take up the question of the ventilation of ships, barracks, or hospitals, there can be no doubt the air can be changed sufficiently often, and be warmed sufficiently, if the expense is sanctioned for the purpose. Hitherto, this has been the great

difficulty, and in no case, either in ships, barracks, or hospitals, has the air been changed in anything like the degree necessary to keep up perfect health, either for men or animals. With regard to the method of applying the principles of ventilation, I am quite of the same opinion as those who have already spoken, that extraction is very much superior to propulsion, in every case, and in the case of the Hospital La Riboisière in France, it was shown that the artificial means of supplying air supplied only 15 per cent. of the whole air, and the remaining 85 was supplied by the natural methods of extraction, which are applicable in almost every case. There are two or three points which we must bear in mind. In paying a visit to the "Devastation," with Dr. Macdonald, I saw the method of ventilating the Officers' cabins, and the method was, that a tube was carried along the upper part of the cabin, with holes perforated in the sides of the tube. The number of holes made was only equal to the sectional area of the tube. This was perfectly insufficient to supply the proper quantity of air in the cabin, because the loss in dividing an air tube into a number of small openings is in the ratio of the square root of the opening. It would be quite easy, therefore, to calculate the quantity of space that would have to be given. If you divide your air tubes into four parts, you lose one half of your available air-space by this process; therefore, whenever a division takes place, there must be a proportionate expansion of the tube. This is a point continually lost sight of, though insisted upon by all the more recent writers on the subject. Then, there is another point that I think has not received proper attention, and that is the very serious effect of angles in tubes. I do not know any work where anything like a formula is laid down on this subject, but, taking a glance at the different diagrams, it seems quite evident that, where you have, in so many cases, a right angle, the air passing down or up must suffer an immense amount of impact on coming to that right angle; that at least one half of it must tend to come to a standstill, and there must be a proportionate loss. It struck me, one might deal with this according to the laws of hydrodynamics, and calculate out the loss that would be found in something the same ratio as the resistance of bodies in water. I think the formula would be met pretty well by a fraction, the numerator of which would be unity and the denominator would be one, plus the square of the sine of the angle the tube would make with the original axis. Where we have a right angle, the amount would be reduced one half, and this table would show the proportionate loss at successive angles.* (Dr. Macdonald: The table referred to by Dr. de Chaumont, representing the formula, was left behind, by accident, but that certainly is an approximation to the truth, if not the thing itself.) This point ought to be taken into consideration that, wherever we have an angle, we must use a formula like this, and where we have a subsequent angle, the amount will be the product of the two available results. And if, again, we are to proceed to minor tubes, in addition to these angles, we must allow very much more for the loss, unless we expand the tubes proportionately. These points are well worthy of consideration, as forming some of the most essential points on the subject of ventilation.

Dr. Macdonald: I can illustrate the bad effect of angles by a very simple diagram. Suppose the tube is bent at an angle of 90°, the air, though compressible, would follow the ordinary laws of light and heat in its motion, thus passing down at the rate of 1,000 feet a minute. The deflection back again would be very con-

* Assuming the available air to be represented by VA, when V is the velocity and A the sectional area, the coefficient of friction due to angles may be taken as $\frac{1}{1 + \sin^2\theta}$, so that the net available delivery of air will be: $VA \cdot \frac{1}{1 + \sin^2\theta}$, which will equal $VA \times 1$ when θ is 0°. At other angles it will be as follows:—

$\theta = 15°$	$\frac{1}{1 + \sin^2\theta}$	$= 0.937$
$= 45°$	,,	$= 0.667$
$= 70°$	,,	$= 0.531$
$= 90°$	,,	$= 0.500$

So that a right angle reduces the amount to *one-half*.

siderable in that case (an angle of 90°). A certain portion of air, however, would pass on. The angle of deflection is equal to the angle of incidence; that law would be carried out very perfectly, and the air in the tube would suffer a remarkable series of deflections thus. There must be a very great loss in this way—a loss which is calculated to be equivalent to one half—so that we should have only 500 feet passing out instead of 1,000.

Now, we will draw a diagram in which the tubes gradually increase in size from their origin. The course of the air is direct, in some instances, after the first deflection. There is, comparatively, little loss in the transit of air by this expansion, which permits of the delivery of the air without the enormous friction which must take place by the continued contraction of the tubes. I have now only to return you my thanks for the kind attention you have paid to me.

Mr. Chadwick: I would commend to the attention of the naval hygiènists a very good book, published by M. de Fonssagrives, the professor of Hygiène at Montpellier, and for a long time a Naval Surgeon. It certainly contains a great deal of fact, worthy of attention, on this, amongst other subjects. Amongst other things, I may mention one that as a statist I was very much interested in. He shows the degree of mortality amongst different classes of seamen—those down below and those higher up in the ship—that the degree of mortality ranges with the degree of vitiated air to which they are habitually exposed.

The Chairman: We must all admit that this subject is an exceedingly interesting one, and hitherto, I think, it has not received the attention which its importance demands. I say, as a sailor of experience and knowledge, there is no question that the system of ventilation in our ships has been of the rudest kind, not at all corresponding with the progress of science in the present day. I have listened, with great interest, to the observations which have been made by the lecturer, as well as to the very interesting statements of Mr. Chadwick and others. For my own part, I believe the true principle of ventilating ships, as well as buildings, is the principle of extraction, and you will really do very little good, unless you thoroughly and completely adopt the system of getting rid of the foul air, by artificial means, if you please, and leaving the supply of pure and fresh air to natural means, which will be easily accomplished. I think the meeting will join with me in expressing our thanks to Dr. Macdonald, for the very interesting lecture he has given us.

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ON THE

STRUCTURAL COMPOSITION OF URINARY CALCULI.

BY

H. VANDYKE CARTER, M.D. Lond.

(Abstract.)

Having occasion during his late residence in Western India to remove numerous urinary calculi from persons belonging to the indigenous population, the author was enabled to pursue an inquiry into the character of these concretions which he had begun in the year 1859 by the chemical analysis of a large number of such calculi; it remained then to make use of the microscope as a means of investigation; and since no record of a similar systematic inquiry to that now undertaken has been published, so far as known to the author, it was thought desirable that this *hiatus* in medical literature should, however imperfectly, be forthwith filled up.

The plan adopted was to submit minute fragments taken from the real or apparent nuclei, and from succeeding layers and crust, of the calculi examined to the scrutiny of average optical powers, the highest available magnifying about 300 diameters. Distilled water was the ordinary medium employed; and in all cases chemical tests were conjointly used for the purposes of detection or confirmation. After sufficient practice, it became apparent that the microscopic analysis of calculi, thus carried

out, is not only valuable, but that it is even more delicate than the simply chemical method; by it will be seen more clearly than before that no urinary deposit long retains an unmixed character, and besides will be gained a more accurate conception of the true character and structure of these hurtful concretions.

Particular attention has been given to the nature of their nuclei or first-formed ingredients, for it was obviously desirable to ascertain the material and mode of commencement of calculi; and the author regards it as not the least interesting of his results to have determined the very frequent presence of urates in a globular form at their very earliest beginnings. Stones removed from adults as well as from children may possess a nucleus thus formed, and to such origin may succeed all varieties of calculous deposit. The next most common ingredient of the nucleus is the oxalate of lime, and this, too, in a form not generally recognized, viz. as large rhombic crystals; frequently, however, octahedral, oval, and dumb-bell forms are seen, as well as spheroidal bodies originating from the latter; these last-named structures are very characteristic. The nucleus of a calculus was comparatively seldom found to consist of uric-acid crystals.

Next in importance to the observation that globular and granular urates very frequently compose the nucleus of all varieties of calculi, and have commonly associated with them the oxalate of lime, is the evidence elicited of the operation of known physical influences in determining the form which these salts assume in connexion with stone. Reference is made to the modifying influence of a colloid medium upon the process of crystallization (as so admirably illustrated in Mr. Rainey's well-known researches); and it is here found that both urates and oxalates may present the various stages of globular particles, complex globules, or spheroids, either separate or blended, and laminæ of varying thickness: there are evident signs, too, that these structures undergo, at times, more or less disintegration, which may be followed by a rearrangement of their molecules; and more especially does the calcic oxalate seem disposed to undergo these changes, the resulting forms being highly characteristic. Neither uric acid nor the phosphates were found otherwise than in a simpler crystalline form; the former, however, when liquor potassæ is added, may in combination be noticed to pass into minute globular particles, the urate of potash being seen in the field of the microscope to assume some of the characters of ordinary urates; and phosphate of lime artificially produced was also observed in the shape of dumb-bells and spheres.

The author refers to the structure of Raphides, agates, some shells, &c., and concludes that urinary calculi should not be classed with ordinary concretions or mere mineral masses; it is not, however, evident that the animal basis of calculous matter, essential ingredient though it be, presents or retains a strictly cellular character.

Some details respecting the source and other characters of the calculi

submitted to examination are added, with a Table specifying the sex, age, &c. of patients, weight and composition of the specimens, &c.

A second Table shows an arrangement of the ascertained ingredients in a form which, if not complete, may yet be useful; in it the rarer substances belonging to urinary calculi are not included.

Next follows a description of each well-known ingredient as it was observed to occur:—*Uric acid* is here crystalline, but not in the shapes prevalent in urinary deposits; free crystals (1) are comparatively rare, and open columns (2), or else compact laminæ (3), are most frequent; amidst these the very general occurrence of simple oxalate-of-lime forms is mentioned. The *Urates* are present in calculi, as (1) granules, (2) acicular crystals, (3) globules, (4) laminæ: the relation of the two latter forms is described, and the apparent character of their animal basis. *Oxalate of lime* would seem to be the most prevalent of all ingredients, and it exists as (1) granules, if the particles were really shapeless, as they appeared to be; (2) crystals—octahedral (often of enormous size), ovoid, and dumb-bell shaped; (3) spheroids, or large composite globular particles of very characteristic appearance, and originating from the last-named form; (4) laminæ, also characteristic. Some remarks are added on crystals of the oxalate artificially produced. Certain *crystals*, styled *peculiar*, are described; they are frequently admixed with the urates, and, upon grounds specified in detail, are judged to be a form of the calcic oxalate. *Phosphate of lime*: free crystals of this salt have not been seen in calculi; and compact layers, authoritatively recognized, have varied in appearance either as the result of disintegration, or possibly in consequence of some difference in chemical composition—a point amongst others needing further investigation. Respecting the *triple phosphate* and the *fusible calculus*, details of their microscopic appearance are submitted in the communication under notice.

In conclusion, figures arranged in four Plates, with descriptions, are appended.

VII. ON THE CALCULOUS DISEASE IN BOMBAY AND SOME OTHER PARTS OF INDIA.

THE origin and chemical composition of urinary calculi form a topic of never-failing interest to both physician and surgeon; and I have found the structural composition of these concretions, as revealed by the microscope, to be not unworthy of notice.

Yet numerous as are the facts bearing on the subject of the calculous disease which are now on record, it is unquestionable that for its complete elucidation much information of various kinds is still needed; and since it may be useful to study the characters of this disease as it occurs in different quarters of the globe, the instance is here proposed for consideration of India and England, being one respecting which data of comparison are now available.

Several years ago, I ascertained that in the Presidency of Bombay—or, more briefly, Western India—the calculous disease is both frequent and peculiar in some of its features; for in several districts cases of stone in the bladder are numerous, and the calculi which are extracted are found, on chemical analysis, to contain the two characteristic ingredients, namely uric acid with its compounds and the oxalate of lime, in proportions strikingly different to those existing among calculi removed in England.

The following table shows the results of analysis, and of numerous calculations I had made, in a summary form:

TABLE I.

Percentage of Calculi in Western India and in England, having for their nucleus (A) *Uric Acid*, (B) *Urate of Ammonia*, (C) *Oxalate of Lime*, (E) *Earthy Phosphates; or composed of* (D) *Cystic Oxide.*

Series.	Grant College, Bombay.	College of Surgeons, London.	Guy's Hospital.	Norwich Hospital.
A	11·76	43·16	52·40	48·87
B	44·54	31·21	9·14	38·61
	56·30		71·79	
C	38·65	14·75	22·59	13·27
E	3·36	10·40	10·57	7·24
D	0·84	0·46	5·28	none

Farther, amongst Indian specimens, oxalate of lime occurs in 50 per cent of calculi wholly composed of one ingredient, while amongst those contained in the museum of the London College of Surgeons, it does not occur in more than about 14 per cent. From these and other data the following conclusions were drawn: '1. That in the Bombay Presidency the proportion of calculi having oxalate of lime for their nucleus, or being wholly composed of it, is about twice as great as in England, taking for comparison certain standard collections there. 2. That the proportion of calculi having uric acid or a urate for their nucleus, or entire substance, is considerably less in India than in England; in the former, urate-of-ammonia calculi are somewhat more frequent than uric-acid calculi; the opposite is the case in England. 3. That the number of calculi wholly composed of earthy phosphates, or having them for a nucleus, is proportionately much fewer in India than in England; the difference being chiefly owing to the rarity of mixed phosphates in the former.'*

* Quoted from an article entitled 'An Account of the Calculi contained in the Museum of the Grant Medical College, Bombay, with some General Remarks on Calculi in India. By H. V. Carter, M.D., &c.;' published in the *Transactions of the Medical and Physical Society of Bombay for the Year* 1859. One hundred and fifty calculi were examined and arranged on the same plan as that adopted in the Catalogue of the Royal College of Surgeons, London.

Now although these statements have been tacitly accepted in some influential quarters, no facts calculated to ratify or correct them have, so far as is known, until lately been made public; I would therefore invite attention to the following authoritative evidence, and since there is not in the volume quoted below any reference to my earlier analyses, it follows that this confirmative testimony is as independent as it is decided. 'In calculi which have been received from India the proportion having a nucleus of oxalate of lime far exceeds that which prevails in this country. Thus of 170 calculi taken from natives in India, the cases of which are recorded in this volume, 100 had a nucleus of oxalate of lime, 38 of urate of ammonia, 25 of uric acid, and 7 consisted of the mixed phosphates; so that in these cases the proportion of nuclei consisting of uric acid and of the urates amounts to little more than 37 per cent.'*

These comparative results may be usefully expressed in a tabular form, thus:

TABLE II.

Percentage of Calculi having a nucleus composed as in Table I.

Series.	Grant College,	College of Surgeons,	
	Bombay.	India.	England.
A	11·76	14·7	43·16
B	44·54	22·3	31·21
C	38·65	58·8	14·75
E	3·36	4·1	10·40

On comparison of my results with those published in the Catalogue of the London College, there appears in the latter a predominance of the calcic oxalate amongst Indian calculi exceeding even that indicated in the Bom-

* Quoted from *A Descriptive Catalogue of the Calculi, &c. contained in the Museum of the Royal College of Surgeons of England*, by Thomas Taylor, F.R.C.S., &c. Supplement I., 1871. The Preface, p. vii. The author had previously stated that in the former Catalogue uric acid and the urates were found 'to form the first step towards the formation of eleven-fifteenths of all calculi from the human subject.'

bay researches; and, as well, fewer urates and more of uric acid than were found in my analyses. Yet no great stress need be laid on minor points, because the urates themselves contain upwards of 90 per cent of uric acid, and therefore very closely resemble it in important characters; and it will be found that they are very seldom indeed free from the oxalate, which may in fact so abound that it becomes a question, on some occasions, which class of salts should determine the name of the deposit under examination.

Another way of viewing these results would be as follows: although A, B, and C predominate in the nuclei of calculi as above stated, yet these ratios do not indicate their respective frequency as a constituent of calculi at *all* periods of the formation of the latter; for I find, on taking either of two series of analyses (Bombay) at disposal, that oxalate of lime (C) is decidedly the most frequent of all calculous ingredients, next the urates, and least often the uric acid: thus in 113 calculi among my earliest analyses, these ingredients were found respectively 78, 66, and 30 times; and in 101 calculi recently examined it again appears that the urates seldom predominate except in the nucleus, that the oxalate occurs equally often in the nucleus and at some subsequent stage, and the uric acid is twice as frequent as a subsequent deposit as it is when nuclear. The observations now offered are thought to be of interest, and when more is known of the sources of these various materials, such comparative inquiries may be of great utility in unravelling, as it were, the history of urinary calculi. It is worth while showing these features of my Indian calculi in a tabular form; thus, in a collection of upwards of 200 specimens, it is found as follows:

	Percentage.		Times.
Uric acid forms nucleus . .	12	Exists in all parts . .	33
Urates „ „ . .	51	„ „ . .	64
Oxalate of lime „ . .	37	„ „ . .	74

Respecting the variations noticed in the respective analyses above detailed, it should next be observed that the calculi contained in the museum of the College of Surgeons were forwarded from the north-west provinces

of India (where stone has long been known to be common), and from Bengal; while the collection in the Bombay College and my own specimens were gathered within the limits of Western India, inclusive of Scinde. With reference to the wide districts of Southern India included under the Madras Presidency, it happens that no data suitable for comparison with the above have come to hand: it would seem indeed that 'stone' is a disease less frequent here than elsewhere; and, in fact, that a local distribution of the affection may be observed in India as well as in other countries.

The sum of the present remarks is, that over a very large part of India, as compared with home districts, there is found amongst urinary calculi a decided predominance of oxalate-of-lime ingredient.

Having regard to his own results, Mr. Taylor next proceeds, in the place cited above, as follows: 'This fact is of extreme significance, as it illustrates in a very striking manner the influence which the use of a vegetable diet, and the almost total abstinence from alcoholic drinks and from animal food, has upon the nature of the calculus; a circumstance which has been doubted by some writers on the subject. The natives of India from whom these calculi were taken were either soldiers or belonged to the labouring class, for the most part of a low caste, living chiefly on rice and vegetables, or the poorest kind of animal food, and drinking water.'

Such observations are of great interest as bearing on dietetic influences; another factor is, however, the climatic conditions under which Hindoos live, as thus referred to in the article quoted from the *Bombay Transactions:* 'A partial explanation of this circumstance may be found in the facts, that rich animal food is much less used here than in England; that the waste of the albuminous and gelatinous tissues is also less active; and lastly, that by the copious perspirations inevitable in this climate, a considerable amount of organic matter is got rid of, which under other conditions would pass off in the urine as urates.' My attention having been directed to the apparent *substitution* of oxalic for the uric acid, as

explained in the earlier part of the above article, I was disposed to regard the instance in question as one upholding the view, that in the Tropics there is a constant sub-oxidation of products within the body, such as tends to the imperfect combustion of hydrocarbons, whether ingesta or formed internally (as the animal sugars, or certain effete products), and consequently to the elimination of oxalic rather than carbonic acid. Physiological data are, however, yet wanting; and it only remains to observe that oxalic acid is probably hurtful within the frame, chiefly from its strong tendency to form an intractable salt in combination with lime: the presence of this base may even directly favour the increase of the acid; and as it is likely the local prevalence of oxalate-of-lime calculi may be often explained by an excess in drinking water, or vegetable food, of calcareous salts, so here appears a means, in avoiding the use of such ingesta in an unprepared form, whereby these calculi might be rendered less frequent.

H. V. CARTER, M.D.

The Parasitic Fungus of Mycetoma.

By H. Vandyke Carter, M.D.

Dr. Vandyke Carter showed a specimen and drawing (including upwards of thirty original figures) in illustration of the appearance, structure, and growth of 'mycetoma' or the fungus-disease of India. (Plate V, fig. 1.)

As it is probable that a full account of this affection may separately appear, the author would, at present, offer a few remarks respecting the development of the red mould which is to be regarded as the parent of the sclerotioid and other more obscure forms peculiar to mycetoma (being, in fact, the essential cause of this disease), and as representing the species some time since defined by Berkeley as follows:—"*Chionyphe Carteri:* hyphasmate ex albo flavo-rubroque: sporangiis demum coccineis: sporis breviter fusiformibus."

The circumstances which have been observed are these, upon specimens of both varieties of the fungus disease, placed under different conditions, there has appeared, on several occasions, a kind of mould not seen elsewhere. The first time it was noticed was in the month of May, 1861, upon part of a diseased foot which had been placed in water for maceration; its duration previous to accidental discovery could not have been long, and a few weeks afterwards it ceased to spread. The next occasion of its occurrence was during the following year, in the month of April, in connection with a specimen of mycetoma preserved in spirits; and again, also about the same date, the mould was seen on some rice paste in which some fresh black fungus particles had been placed, in order to ascertain if they could be made to grow artificially. On a subsequent occasion the same red mould made its appearance in connection with some of the more common pale, soft fungus particles, which had also been sown in rice; and, further, on examination with the microscope, it then seemed as if its mycelium did not arise from, or in relation

with, the crystalline fringe which envelopes these particles, but rather as if the branching mycelium fibres emerged directly from the interior of the finely granular substance composing the bulk of these roe-like particles.

It may here be remarked with reference to the various opinions which have been expressed as to the true nature of these pale, soft, subglobular bodies, characteristic of the second variety of mycetoma, that while admitting the comparative failure of microscopic analysis to detect ordinary cell-structure in these, such negative evidence cannot be regarded as of paramount weight in the instance of these low organisms; and more especially so when compared with the affirmative proof embodied in the last observation just mentioned, which would have all the force of a synthetic demonstration of the originally, or essentially, mycoid nature of the bodies in question.

This red mould has always presented the same character; it has not been seen except in connection with the fungus disease; and it makes its appearance at one time of the year, namely during the hot season (April to June in Bombay), having there a brief life. On all occasions named above, it occurred in patches, scattered or continuous, and of size no larger at first than a pin's head; the superficial aspect of the patches, which corresponds to the outer layer bearing the organs of fructification, is of a deep maroon or crimson colour, somewhat lobulated, and when examined with a lens presenting numerous scattered lighter coloured granules, distended spore-capsules, in appearance not unlike the fungiform papillæ of the tongue as seen in a healthy person. The patches have been found to vary in thickness from $\frac{1}{70}$ to $\frac{1}{8}$ inch; in the former case being friable, in the latter acquiring a toughish consistence, with a tendency to tear vertically, and then the part below the surface, *i. e.* the thick mycelium, displays a lighter buff-coloured tint. The margins of the growing patches were generally indistinct, but a crenated free edge might be seen when a certain thickness was attained.

The mould commenced upon, and did not extend beneath, the free surface of its matrix, whether morbid specimens or rice-paste; it spread in the form of a layer, and it showed no tendency either to penetrate below the surface, or to assume the form of globular masses. The colouring matter was in the morbid specimens, where growth had been very luxuriant, very intense in tint; it stained the fluids and tissues beneath the surface, *e. g.* the cutis as well as

cuticle, but on examination no spores or fibres were found in either papillæ or corium, although plentiful enough in the epithelial layers; once the general appearance was as if fresh blood had been freely sprinkled over the foot and sides of the bottle.

It may therefore be inferred from the facts stated, and from known similar instances in the history of simple vegetable parasites, that this peculiar mould assumes the characters found in 'mycetoma' in consequence of the new conditions under which its growth then proceeds, namely, away from light and air, and in contact with the warm living tissues and fluids of the human foot and hand; but that when permitted or induced to grow outside the body, these its errant forms may revert to the more normal type, previously in great part obscured.

In the light of these observations certain rarer features and varieties of mycetoma, already on record, become intelligible.

The occurrence of *Chionyphe Carteri* as a parasitic entophyte amongst the lower animals and plants is as yet unknown; nor has the mould been detected in its natural habitat. Whether or not it be a distinct species, or a mere variety of one of the mildews known to attack grain in India, its occurrence in what may be termed its infecting form may be regarded as widespread and commonly sparse, yet sometimes common; since mycetoma is a disease happening, though rarely, in distant parts of the country, and even in adjacent countries, as Persia, but as a frequent affection it is rather limited to a few well-known districts. Under any circumstances a confluence of circumstances attends the development of the disease, as is apparent from the fact that, so far as the author is aware, several members of the same family are not affected at the same time.

The habit of the people, it may be added, to go with uncovered feet is doubtless the explanation of those parts of the body being (as is the case with the analogous disease of Guinea-worm) much the oftenest affected; occasionally however, the hands are attacked, and such might be anticipated if it be allowed that germinal matter (as spores, &c.) from the fungus finds an entrance *ab extra* either through natural apertures, the sweet-ducts, &c., which would admit spores, or through the thorn-pricks or punctures, to which patients very frequently attribute the commencement of the swelling.

DESCRIPTION OF PLATE V.

Fig. 1 illustrates the Structure of the Red Mould found in connection with Mycetoma (*Chionyphe Carteri*). (Page 3.) From a drawing by Dr. Vandyke Carter.

A fragment of the new growth as this appeared upon a specimen of the foot-disease placed in water to macerate. Taken from the deeper tinted superficial layer containing the organs of fructification. × 300 diam.

a, a. Ovoid spores, nearly colourless, very abundant; size, $\frac{1}{12}\frac{1}{00}$ by $\frac{1}{12}\frac{1}{00}$ in.

b, b. Spore-capsules, spherical, numerous; dimensions coloured $\frac{1}{25}$ in., colourless $\frac{1}{25}$ in.

b'. A few large vesicles; size, $\frac{1}{12}\frac{1}{00}$ to $\frac{1}{12}\frac{1}{00}$ in.

c. Round cells, coloured interior, frequent; size, $\frac{1}{12}\frac{1}{00}$ by $\frac{1}{12}\frac{1}{00}$ in.

d, d. Round clear cells, frequent; dimensions, $\frac{1}{25}\frac{1}{00}$ to $\frac{1}{12}\frac{1}{00}$ in.

e, e. Long-jointed tubular filaments, some coloured, others clear; diams. from $\frac{1}{12}\frac{1}{00}$ to $\frac{1}{12}\frac{1}{00}$ in.

f. Pigment-masses, occasional, vary in size from $\frac{1}{25}$ to $\frac{1}{25}$ in. some appear to be capsulated, $\frac{1}{12}\frac{1}{5}$ in. diam. Besides there are numerous minute granules.

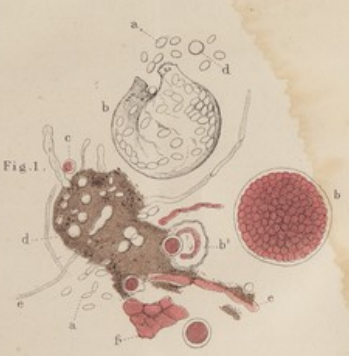


Fig. 1.

MEDICO-TOPOGRAPHICAL REPORT

OF

THE STATION OF FUTTEHGURH, N.W.P. INDIA.[1]

By Wm. CURRAN, L.R.C.P. Edin., M.R.C.S. Lond., etc.,
Assistant-Surgeon Army Staff.

Reprinted from the Edinburgh Medical Journal for January and February 1873.

Introduction.—I have been induced to select this station for the subject of my report, in consequence of having served there on two different occasions, more continuously and for longer periods than elsewhere in this country. Having been, during the greater part of my residence, the senior medical officer in charge of troops, I had an opportunity of assisting at the deliberations and of co-operating with the efforts of the local authorities in the direction of sanitary improvements; and when I add that I was, besides, for several months engaged in discharging the duties of civil surgeon, I may, I trust, be permitted to say, that I am more intimately acquainted with this locality than I am with any other place of equal extent in Upper India. The district acquired a very unenviable notoriety in 1857, and some of the minor engagements that restored to us the command of the North-West took place in its vicinity. For these reasons, and also because I believe that no attempt at an exhaustive report has been made up to date by a member of the department, I venture to occupy the ground, and I shall be more than repaid for my trouble if the following sketch, however imperfectly executed, will be found in any way to supply the deficiency. It is based on observations made by myself in person, for the years 1863 and 1866, during the greater portion of which I resided there;

[1] The report summarized above is the substance of that which was submitted by me on the occasion of my examination, "previous to promotion," in January 1869. All details of a purely local character are omitted, but though confined to the description of one station only, it need not be taken as applicable to that alone; on the contrary, it may be regarded as equally pertinent to other stations in the plains, and hence such interest as may be supposed to attach to it—"Mutato nomine, de te fabula narratur,"—and what is said of Futtehgurh will, *ceteris paribus*, apply with equal force to other military cantonments in the North-West. It is drawn up in accordance with the "Instructions to Medical Officers in making up Annual Reports," prescribed by "Regulations," and will, it is hoped, convey a better idea of the surroundings of a station in the tropics than is usually presented for the perusal of non-military professional readers.

but it is equally applicable in the present day, as no substantial change has taken place; and I am glad to hear that the intention, which was formerly entertained, of removing the gun-carriage agency to Allahabad,[1] and with it the European troops to Cawnpore, has been virtually given up by the Government, and that the station is to be maintained as heretofore on its old basis, under perhaps better provision for exercise and amusement, and with more commodious barrack arrangements than existed in my time.

Geological Formation.—The soil underlying the station itself and its immediate vicinity is sandy, containing much mica, and in parts also traces of saltpetre; and it is so little retentive of moisture as to become perfectly dry within a few hours of being drenched with rain. Its natural aridity is, however, relieved by the presence of numerous neem and peepul trees;[2] and the high winds of summer are in some measure, though but very partially, stayed by these. Yet it is not favourable to the growth of grass, and the absence of vegetation gives its surface a dry and parched appearance from November till June. In the district[3] it partakes of the general characteristics of the Gangetic valley, in which sand and clay alternate without leaving behind any appreciable marks of a well-defined separation; but there appears to be no subsoil stratum of impermeable clay, and the composition of the water, which can be easily reached, is free from an excess of saline impregnation, and pretty uniform throughout. The ground, in fact, forms, like most of that in its vicinity, part of the older alluvial deposit, which contains beds of "kunkur" here and there, and is probably of marine origin, while that which prevails in the immediate neighbourhood of the barracks exhibits a more recent form of alluvium, contains black mould, and is evidently of fresh-water origin. Strata of gravel and clay occur in both forms, and the surface sand does not contain ferruginous material anywhere. What there is of vegetable admixture is soon neutralized by cultivation, and the great fertility of the district is universally recognised.

The general outlook is, however, characterized by considerable sameness and monotony, but a well-ordered system of irrigation

[1] This transfer has, I am informed, taken place since this report was written.

[2] Elsewhere in this report, and under a heading which, as being too local, I suppress, I thus describe the influence of trees, and I have everywhere, and during the whole of my service in India, encouraged as far as I could their cultivation and growth:—"They serve to diminish the force or arrest the passage of malaria; they cool the surface by preventing too rapid evaporation; and there is good reason to believe that epidemic cholera is more prevalent and fatal in districts that have been deprived of trees than in those that are protected by them. There is no doubt that their absence lessens the fall of rain, and I could bring evidence to prove, were this the proper place for doing so, that some districts which are now little better than barren wastes, or icy and inhospitable deserts, were formerly blessed with a teeming soil and a happy climate, and were deprived of both by being denuded of trees."

[3] By the term "district" is everywhere meant the country, as distinguished from the station and its immediate surroundings.

has done much to counteract the effects of these; and the proximity of a large river, and its numerous tributaries, the frequent occurrence of shady clumps of mango-trees near the roads, and the industrious habits of the cultivators, have sufficed to change the face of the country, and endow it with a surface soil, which, while it yields heavier crops readily to cultivation, is also capable of producing fruit and vegetables in abundance.

Topographical Description.—The military station of Futtehgurh, lat. 27° 22′ N., long. 79° 41′ E., situate about 80 miles from Cawnpore, 112 from Agra, and 184 S.E. of Delhi, is healthily situated on the right bank of the river Ganges, which separates it from the province of Oude, and is, according to Thornton, 550 feet above the level of the sea. It occupies the centre of a highly cultivated, well watered and fertile country, whose vegetable productions have obtained for it a more than local celebrity, and secured for themselves an export trade that might bear favourable comparison with that of any settlement of similar extent in Northern India.

The district, covering in area a space of 2122 square miles, and containing, with the large outlying city of Furruckabad, a population of upwards of a million souls, is very productive and fertile, yielding indigo, cotton, Indian corn, wheat, barley, tobacco, sugar-cane, opium, rice, dhall, and other cereals of good quality.

There are no very striking features of natural scenery, and no mountains or valleys, unless we include in this designation the valley of the Ganges, which extends indefinitely on either side; but when the river is swollen to its utmost in the rains, and rolling along in its might, when vegetation is everywhere tropical, and all nature seems to revive and shake off its former forlorn aspect, there are combinations of sound and scene in the clouds above, and in their produce the waters below, which it would not be easy to describe, and which could scarcely be paralleled anywhere out of India. Its surface clay is, in many places near the station, turned to good account in the construction of tiles and earthenware pots; and a manufactory for making vessels of this kind, soda-water bottles and other similar articles, which existed there before the mutiny, and which was under the management of a skilled European potter, turned out articles of earthenware of which numerous broken specimens still remain of a very superior character, and fully equal to those made in England. Unfortunately, however, the establishment was destroyed by the mutineers in 1857, the potter himself being killed at the same time; and though stool-pans and wash-hand basins of good quality, and of which I freely availed myself for the use of my hospital, are still made in the jail, they can in no way compare with those referred to above; and every attempt to revive the trade on its old footing has signally failed, and will continue to do so until some European tradesman of equal capacity appears on the field. Saltpetre is largely obtained in the district, and the sale

of it affords the Government a considerable revenue. Several indigo firms ply a more or less profitable trade in that dye; and Futtehgurh is known all over India as an emporium for the construction and sale of tents.

Productions.—The potatoes, pumpkins, rice, and corn that grow in the environs of the cantonment, are specially worthy of commendation, and are, I was credibly informed, eagerly sought for, not only in neighbouring stations but all over the upper provinces, and in more distant parts of the interior.

Of the meat and bread supplied to the troops, I can myself speak in terms of unqualified praise. I find, by a memorandum made on the spot, that "a portion of the meat ration was condemned on one occasion only during the year 1866;" and taking these facts into consideration, I more than once, during my stay there, took the liberty of bespeaking the attention of the authorities of my department to the peculiar advantages of the place, and of recommending it as a centre whence supplies of the above useful articles might be easily and abundantly procured. It is only necessary to add, that the land is everywhere too valuable to be allowed to run into waste or jungle; and there are no "jheels" or marshy ground in the vicinity. The holy Ganges and its lesser sister stream the Ramgunga, rolling ever but with variable force in their muddy beds, have effectually drained the district, and constituted themselves the scavengers, and at the same time the factors of whatever filth or malaria it produces.

Water.—The water was of good quality, and could at all times be easily procured from numerous "pucca" (stone) wells in barracks and elsewhere. Filters were always had recourse to, and I could never trace any occurrence of disease to the influence of this fluid. There was no analysis available for reference at the time of my incumbency, but I believe one has been made since.

Special Characteristics and prevailing Diseases.—The station itself, commandingly situated on an eminence which is about forty feet above the bed of the Ganges, and intersected by "nullahs" (ravines) which converge at right angles towards that stream (and which, were they not too frequently allowed to become the receptacles of native filth, would be excellent drains), has been very judiciously selected, and the good roads by which it is surrounded considerably enhance its other attractions.

It enjoys the repute of being a healthy station, and though but in a measure an outpost for detachments of regiments (whose headquarters are elsewhere), it possesses a large Government gun-carriage factory, which supplies the greater part of Upper India with these conveyances, and affords a residence to numerous European families and tradesmen.

It has the further advantage of cooling breezes from the river, of capital levels for drainage purposes, and of being at some distance from the noise and associations of a large and densely crowded native bazaar.

The hot season is supposed to commence earlier there than at other places in its neighbourhood, and the heat is said to be unusually persistent and oppressive during the earlier months of the summer. I have not, however, found this to be the case within my own experience. The thermometer rarely exceeds 88° in a well-ordered house, and the general absence of cholera, coupled with the fact of its local repute, and comparative freedom from other grave visitations, incline me to regard it as unfairly saddled with these peculiarities. Fever of a mild and yielding type, and presenting the ordinary features of continued, intermittent, etc., prevails during, but especially about the decline and after the cessation of, the rains, and this to such an extent as at times to prostrate large numbers of the inhabitants and render Coolie labour costly and hard to procure.

That this fever is in some measure dependent on influences emanating from the river cannot, I think, be easily disproved; and when we consider the extent of surface and the amount of decaying matter in the shape of human and other remains, etc., laid out to fester under a sweltering sun, by the recedence of its waters, our surprise will be that so little mischief is done, and that the fever, instead of degenerating into typhus, passes off in a few days under the usual restraints of a military hospital. As this is a result which does not quite correspond with what one might be led to anticipate, I think it right to allude here to the influence of rivers on the healthiness or otherwise of cantonments situated near their banks, as compared with those in the interior, and at the same time to glance, as briefly as I can, at the circumstances under which the former condition is realized.

River.—The river itself may, in reference to the point under review, be considered as presenting two aspects worthy of notice, and which depend for their development on the season of the year and the amount of rainfall. From July to October it presents a vast rolling stream some three or more miles in width, which overflows its banks in various directions, and carries away the accumulated filth of previous months. For the greater part of this time, its yellow turbid waters rush along with a continual noise, at a pace of eight miles an hour, and gathering strength as they advance, sweep onward with them whatever of animal or vegetable debris they can lay hold of. At this season, everything likely to prejudice health is carried away, frequent showers renew the drooping vegetation, and flush to saturation all "nullahs" and depressions in cantonments. Everything looks bright and cheerful, Nature has arrayed herself in her gaudiest robes and put forth the choicest specimens of her workmanship, and this continues till about the middle of October, when the waters begin to retire, and leave behind them as they recede a glistening surface of white sand of variable extent, and a few streams easily fordable, that meander slowly to the sea. A few melon-beds and patches of rice

cultivation, which yield a scanty but remunerative crop, to the industry of some enterprising cultivator, and which are of course washed away by the first fall of rain, sparsely dot its banks, and grave in some measure to diversify the monotony of the void around. From this immense tract of uncovered surface drying in a hot sun, marsh miasm may reasonably be expected to escape and abound along the banks of the Ganges; and so indeed it does; but the scanty supply of water in the river, its slow and equable flow, the condensing action of the sun on the surface of the sand, and the almost entire absence of decaying vegetable or animal refuse, serve to counteract its influence and neutralize its power. Indeed, we may strike the former out of the calculation altogether, for every loose scrap of it has been long since removed; and as to the human remains, so great is the demand for this filthy supply, and so numerous are the scavenger beasts and birds that thrive by it, that all animal matter subject to decay is speedily devoured and rendered inert. Their appetite appears to be almost insatiable; and it is well it should be so, for the authorities, instead of discouraging this horrible practice, positively lend it their aid; and the remains of prisoners who die in the jail are carted off, almost as soon as the breath is out of them, to the river. The same happens to those who die unclaimed in the dispensary, or who are brought in for examination from the district; and though well accustomed to such scenes, I was almost startled out of my propriety one morning, by meeting the body of a malefactor, who had just been executed, and which was almost as warm as my own, as it was being borne along on an open bench through a public highway to the same destination. And yet such are the salutary provisions of Nature, and so abundant is the demand for animal flesh of any kind, that little or no mischief comes of all this. I have myself frequently observed that one night sufficed these useful creatures for the entire removal of the eatable portions of a stranded corpse; and it is sometimes almost ludicrous, were it not so disgusting, to see a parish dog at one end of it and a jackal at the other, while carrion kites and crows flitted about and pecked at one another, out of sheer desperation at being deprived of what they considered a lawful perquisite.[1]

[1] How often have the terrible words of Byron occurred to me as I contemplated these and other scenes of like import; and the proximity of a snake or crocodile seems to invest them with greater force and propriety:—

> "It is as if the dead could feel
> The fey worm around them steal
> And shudder as the reptiles creep
> To revel over their rotting sleep,
> Without the power to scare away
> The cold consumers of their clay."

But this river has been for centuries the receptacle of the "slovenly unhandsome corses" of the millions who dwell by its banks, and the witness of scenes which the pens of a Dante or a Defoe, of a Baxter or a Bunyan, would fail to adequately realize or portray. So disagreeable was the impression produced on my own mind by the constant recurrence of such scenes, that in a moment of unusual depression—perhaps I ought to add, of desperation—I

Nothing remains but a disjointed skeleton, and there it will probably remain for months, harmless, no doubt, but still very ghastly, every bone in its perfection, bearing evidence of its Maker's fashioning, and the polished skull with empty sockets turned up to heaven—almost provoking, by its grotesque hideousness and shining incongruity, a sneer at the vanity of human aspiration, and calling for censure of a practice which is as much opposed to the requirements of decency and health as it is to the common instincts of our nature and our civilisation. In addition to the fever mentioned above, bowel disorder, congestive derangement of the liver, and rheumatic affections are frequent among the troops. Ophthalmia also obtains, and is doubtless due to the glare caused by the absence of trees and to the dusty condition of the barrack compounds; and the bulk of my patients consisted of persons seeking relief from one or other of these ailments. Leprosy is not uncommon in the more unfrequented parts, and calculus is very prevalent among the village and agricultural portion of the community. It is supposed to be in great part due—for I am not aware that there is any limestone or magnesian limestone formation in the district—to the vegetable or coarsely-powdered material of their farinaceous food, and to the absence of that admixture and combination of nitrogenous elements to which Europeans are accustomed. This hypothesis has the evidence of analogy in its favour, inasmuch as a similar, though much less frequent, tendency exists in parts of the Highlands of Scotland, in Norfolkshire in England, and near Malaga in Spain; in the first and last of which places it is well known the poorer people live, in great part, on barley-meal and on cakes made of the dried husks of the grape, while the use of pears and cider is regarded as tending to its causation and development in the other. I had the personal assurance of the English medical gentleman resident at Malaga, on my visit to that place in 1858, that such was certainly the cause of calculus in the persons who consulted him,

gave vent to my feelings in song, of which the following may be taken as a fair index and refrain:—After recapitulating the associations of my youth, beside the Bride and Avonmore, and contrasting them, of course unfavourably, with the dreary solitude before me, I wind up as follows, in imitation of poor Griffin:—

> Then flow thou on, thou filthy river;
> Flow thou on, thou stinking river;
> The time will come—God send it soon—
> When I will quit thy banks for ever.
> And then, while on the briny deep,
> Or beside the streamlet's rippling billow,
> I while away the vacant hour
> On greasy loam, or 'neath shady willow,
> I'll sing of thee in accents wild,
> With quivering lips and in tones of horror,
> And wail the day when I first beheld
> Thy sweltering flood, O filthy river!
>
> O, know ye not, that slimy river,
> Know ye not, that steamy river,
> Whose stinking flood by cliff and wood,
> Goes sweltering on for ages ever?

and I can assign no other than that given above for its prevalence in the district of Futtehgurh. By the courtesy of the civil surgeon I was afforded an opportunity of extracting several calculi, and succeeded in every case that came under my care. They varied but little in composition, being chiefly made up of triple phosphate and of oxalate of lime. It will only be necessary to add, that the Ganges has considerably deviated from its original course even within my recollection, and that it appears more disposed to recede from than approach to the station.

With regard to the more important question of the comparative healthiness of cantonments placed on or near rivers, and those situated inland, I have left myself no room to discuss it here, and I may find some more convenient opportunity of doing so elsewhere hereafter. I may say, however, that my own experience is in favour of the location of troops in stations bordering on rivers rather than in those which have no such proximity; and I believe that were sufficient statistics available for comparison, it would be found that the former are on the whole healthier than the latter. I have lived myself for months together on or near the banks of the Ganges; I have slept in the open air within hearing of the ripple of its waters; I have boated on its reeking surface, and bathed in its dirty stream; and the result has been always to my own satisfaction: and I venture to think that Benares and Allahabad, Cawnpore and Agra—bad as they are in many respects—would compare favourably, in regard to situation and salubrity, with Peshawur and Gwalior on the one hand, and with Jhansie and Secunderabad on the other. But, as I said before, the subject would require more time than I can now command, and occupy more space than the dimensions of this paper would allow; and I hope that the above will suffice at present for the purpose I had in view in drawing attention to it.[1]

Hospital.—The hospital, formerly the residence of one of the courtiers of the late King of Oude, and as such, of course, not originally destined for the reception of sick soldiers, is a large airy quadrangular building, within easy reach of barracks, and consists of two principal wards, the outer of which is 105 feet long, and 24 feet broad, while the inner is also 105 feet long, and only 20 feet

[1] Col. Sleeman, alluding to the unhealthiness of deserted fortresses as residences for troops, says, "There is no doubt that the most healthy stations in this and every other hot climate are those situated upon dry, open sandy plains with neither shrubberies nor basins" (*Rambles and Recollections of a Bengal Civilian*, by W. H. Sleeman, vol. i. p. 314); and though instances to the contrary might be easily produced, there is little doubt that he is mainly in the right. To the same effect speaks old Burton, in that strange book of his, wherein he treats "de omnibus rebus et quibusdam aliis," when he says, "The best soil commonly yields the worst air; a dry sandy plat is fittest to build upon, and such as is rather hilly than plain, full of downs, a Cotswold country, as being most commodious for hawking, hunting, wood, waters, and all manner of pleasure." —*The Anatomy of Melancholy*, p. 532.

broad. It is agreeably situated in a large enclosure, which overlooks the station-club, and ornamental park, prepared by the Government for the residence of His Highness Maharajah Duleep Singh, when removed from the Punjab in 1846, and is well protected by intervening trees against emanations from the river. It is capable of accommodating without any pressure a much larger number of sick than our demands ever imposed upon it, and through the medium of three other smaller compartments, of which it consists, affords better facilities for the segregation of infected sick, and the treatment of ophthalmia, smallpox, or other contagious disease, than any similar institution within the compass of my acquaintance in India. Ventilation is fully secured by the presence of several large closable roof and side windows, and its effects are further enhanced by the lofty roof, deep overhanging verandahs, perforated walls, and numerous other outlets with which it was originally fitted. A large detached building of smaller construction and in the same enclosure—though sufficiently apart to ensure privacy—was set aside for the use of the sick women and children of the detachment. The water-supply was good and abundant; there was no overcrowding at any time in either; and the latrines and lavatories were fully adequate to our requirements.

Composition of Rations.—With regard to the composition and the quality of the food supplied to the troops in India, the soldier's ration consists of 1 lb. of bread, and 1 lb. beef and mutton; the former is given six days in the week, and the latter generally, if not always, on Sundays. The vegetables available for use varied, of course, according to season, but the following were, at one time or other, within the reach of all: potatoes, pumpkins, yams, onions, and the ordinary cabbage of the country. Biscuit did not form a part of the ration, but bread of excellent quality was always given, and tea and coffee were the only articles of the kind supplied by the public. These necessaries were, of course, supplied by the commissariat, but the villagers were encouraged to bring oranges, apples, grapes, pease, cauliflower, and other similar products, and the men were allowed to purchase these *ad libitum*. The soldiers, as a rule, prefer their meat boiled, as they thereby secure a certain amount of soup, but variety and flavour can never be satisfactorily obtained unless skilled cooks in sufficient numbers, and kitchen-ranges of better construction are more generally provided. As to the kitchen utensils, they consisted of the ordinary copper pans and pots supplied to troops in India, which were tinned twice a month, and which were always found adequate to our wants. From what I have already said, it will be easily inferred that the ration was usually of good quality, but there is too much sameness in it; and I most fully agree with Dr Parkes in recommending greater variety, by the addition of some fatty element in the shape of cheese or bacon. This, however, is a matter for Government interference, and variety

can only be secured by the introduction of some such skilled cooking as that advocated above.

Hospital Diets.—There are eleven different diets in the Indian as against the English scale, and their variety is so great and composition so diversified, that while retaining all the essential principles of a uniform dietary, they serve to meet most of the requirements of the sick and fulfil the reasonable expectations of the surgeon. Yet, though so very comprehensive, they have to be sometimes supplemented to meet the ever-changing demands of tropical disease, and the usual extras of beer, wine, effervescing beverages, etc., are frequently required for the treatment of European sick. The English scale appears to contain more solid material than the Indian, while the latter is somewhat richer in starchy and saccharine elements, and, as may be expected in a country where thirst is so common a concomitant of illness, contains more fluid also. I never had occasion to depart from the authorized scale, through any local deficiency or failure of supplies, etc., and I can freely say, that while undue expenditure was properly discouraged by a rigid supervision on the part of the local deputy inspectors, I had no reason to complain of the manner in which my requisitions were dealt with by those officials.

General Summary.—I regret to say, that in consequence of the deficiency of the materials now at hand, I am unable to give as much information under this head as I could wish; but I hope that the subjoined tables, which are based on authentic regimental records, will suffice for the general purpose in view, and lend support to the favourable estimate I have been induced to form of the station here referred to. It will be noted, on perusal of these tables, that two of the deaths that occurred in 1863 were due to very rare and exceptional causes, and the mortality in 1866 was increased by cholera and sunstroke. The former rarely visits Futtehgurh, and the latter was more fatal there during the hot months of 1866 than it was known to have been for several years previously. Though no great admirer of the doctrine of averages, I yet believe they are more likely to guide us in our conclusions respecting the salubrity or otherwise of a particular locality, than any instances drawn from individual observation or limited experience; and I submit that due allowance should be made for local repute, and for the general estimation in which a station is held by those most concerned or who know it best. Bearing this in mind, and subject to the above qualification, the following summary will, I think, bear out what I have said, and enable any one to see at a glance that Futtehgurh is a healthy station.

Return showing the number of Admissions, Deaths, and causes of Mortality, in the Left Wing 88th Regiment, from January to September 1863.

Months.	Strength.	Total Admissions	Percentage of Admissions to Strength.	Percentage of Deaths to Strength.	Diseases which caused the Mortality.
January . .	406	46	11·33	...	
February .	407	32	7·86	...	
March . .	396	31	7·82	...	
April . . .	393	32	8·14	0·25	Syph. sec.
May . . .	392	45	11·45	...	
June . . .	392	70	17·85	...	
July . . .	393	56	14·24	0·25	Feb. typhoid.
August . .	393	75	19·08	0·25	Syph. second., Ascites, and Dysent. ch.
September .	394	50	12·69	...	

Remarks.—The causes of the mortality here referred to, though somewhat exceptional, are yet such as may, and often do, transpire in more favoured climes; and it is, I believe, generally allowed that the sequelæ of syphilis occasionally assume a worse type and more progressive character in the tropics than they are wont to do in colder latitudes.

And from January to October 1866.

Months.	Strength.	Total Admissions.	Percentage of Admissions to Strength.	Percentage of Deaths to Strength.	Diseases which caused the Mortality.
January . .	345	12	3·44	...	
February .	177	9	5·08	...	
March . . .	173	15	8·67	0·57	Hepat. ch.
April . . .	262	10	3·31	...	
May . . .	238	22	9·24	0·41	Insolatio.
June . . .	238	26	10·92	0·41	Hernia.
July . . .	240	39	16·25	0·41	Insolatio.
August . .	240	38	15·83	1·25	1 Chol., 1 Feb. cont., 1 Tetan.
September .	238	38	15·96	...	
October . .	238	26	10·82	0·41	Feb. cont.

Remarks.—It will be seen from this table that such diseases as hernia, tetanus, and continued fever—which are certainly not peculiar to Futtehgurh—carried off each his victim in 1866, while the visits of cholera and sunstroke are like those of angels, "few and far between." They are, indeed, so few as not to modify materially the conclusions already arrived at.

Characteristics of Prevailing Diseases, and General Conclusion.—As previously stated, fevers, diarrhœa, and rheumatism, were the complaints most frequently brought under notice, but they rarely presented very striking or peculiar features, and the former readily yielded to the combined influence of rest, regimen, and antiperiodic remedies. Its access was frequently accompanied by insomnia, more or less sensorial disturbance, and some troublesome frontal pain; and its principal sequela was a certain paroxysmal form of cephalæa, which had a tendency to recur again and again, and which was only re-

lieved by cold douching and local depletion. The bowel disorder manifested itself chiefly, and especially so during the colder months, in the form of a sub-acute or chronic diarrhœa, which rarely degenerated into dysentery or became complicated with hæmorrhoidal relaxation, and which, after the seasonable employment of a dose of oil, generally succumbed to the use of stupes over the belly and infusion of logwood.

Dysentery is not common, and there was only one case of cholera among Europeans during the two years that I resided there. As to the rheumatism, it occurred chiefly among the older soldiers, and was rarely productive of dangerous consequences. It was probably due to the agency of night air, and I experienced no difficulty in removing its more troublesome symptoms by ordinary remedies. The ophthalmia that obtained was, as I ventured to surmise above, most probably due to the glare and dust of a sandy soil, unrelieved by the ever-grateful shade of trees or other obstacle, and enhanced by the hot winds of summer, which carried it along at a racing speed, and made it penetrate every crevice with a force that the shrivelled woodwork of an Indian barrack-room is incapable of resisting. Hepatic complications prevailed, of course, at Futtehgurh as elsewhere, and especially during that stagnant condition of the atmosphere which is so graphically called "muggy" by the older residents, and which is very common in the intervals between falls of rain. They were attended in several instances with the usual phenomena of congestion and abscess; the latter, however, was comparatively rare, and I believe there are fewer occurrences of this kind in Futtehgurh than in others of the military stations in the North-West. Sunstroke carried off some Europeans during my second hot season residence; but no inference can be drawn from its occurrence in favour of the insalubrity or otherwise of a station in the plains, for the liability to it is pretty equally divided throughout the peninsula, and Futtehgurh was by no means in advance of its neighbours in this respect. Chest affections were comparatively rare, and by no means difficult to manage; and the few cases of phthisis that came under notice were more the result of hereditary predisposition, night exposure, and the ordinary wear and tear incidental to the life of a soldier, than to any local agency, atmospheric or otherwise. There certainly was no overcrowding or want of ventilation; there was no marshy ground, jungle, or stagnant water in the vicinity, and what came in the shape of rain was either quickly absorbed by the parched and porous soil or removed by the numerous natural outlets provided by the river fall. Every attention was paid to the cleanliness of barracks and to the clothing of the troops. The admissions to hospital were at no time disproportionately large; and as the disease that prevailed appeared to be in the main due to causes over which we had no control, I did not think it necessary to suggest any deviation from the usual practice and usage of the regiment; and I have now no reason to regret my silence under the circumstances. Speaking generally, then, in view of the facts

disclosed within, and with every desire to avoid overloading the subject with a mass of detail—whose relevancy in this place might be reasonably questioned—I would at once say, that Futtehgurh combines advantages for military occupation and residence which do not exist collectively elsewhere, and I have already intimated that the sanitary condition of the station is satisfactory. The barracks are, however, badly constructed, and require renovating, or rather replacement, by structures of greater capacity and more in accord with the experience of the day than the present ones can claim to be. A fives or racquet court is required; it is now generally admitted that there should be some kind of gymnasium near all barracks; and the addition of a club and reading room would greatly relieve the monotony of a soldier's life everywhere in India.[1] Were these improvements fully carried out, and were trees more extensively cultivated in the station and near barracks, Futtehgurh would be a decidedly suitable place for the lodgement of troops, and there is no place in the North-West in which I would more willingly serve again. Its generally low average of mortality, its comparative freedom from epidemic disease, the superior quality of its vegetable productions, and ready accessibility by land or water, point it out as a centre of supply and a medium of communication with the

[1] The late Mr Lang, for many years editor of the *Mofussilite* newspaper, and otherwise a conspicuous figure in India, expresses himself in this direction to the following effect:—

"I hope to see in every large station a library, to which every soldier at stated hours shall have access. I hope to see soldiers' gardens—such as the late Sir Henry Lawrence recommended—in which the men may, when they feel disposed, work or amuse themselves in the cold season. I hope to see a theatre in every large station, built and kept in repair, not by subscriptions from the poor men, but at the cost of the State. I hope, in fact, to see the British soldier in the East, not petted, pampered, and made a fuss of, but made as sensibly comfortable as the climate in which he serves will admit of his being made."—*Wanderings in India*, by J. Lang, p. 248.

In like manner, Mr Dunlop, of the Bengal Civil Service, alluding to the listless, enervating, and monotonous stagnation of military life in the plains of India, says very truly, that "spasmodic gaiety subsides before the hot weather begins, and the soldier is driven to drink, in the delusive hope of escaping the effects of depressing atmosphere, and maddening vacancy of mind: Government would be amply repaid, in the health and happiness of its troops, and the saving of some portion of the expenses incidental to sickness or intemperance, if it subscribed by wholesale to every illustrated periodical published in England, as also a selection from the non-illustrated press, for six months in the year, and sent a copy of each paper to every European regiment or depôt in India, from April to October."—*Hunting in the Himalayas*, p. 140.

"Madness itself," as the *Saturday Review* once observed, "might spring from the inexorable dulness of a life of unvarying sameness, unrelieved by occasional flashes of enjoyment;" and "what our soldiers want in India is less the removal of certain temptations than the ability and the knowledge to satisfy their nature in a healthy way." These latter words were used by Mr Olmsted in another place, and under very different circumstances, but they are equally applicable in the present contingency; and he very properly adds, that "little good will be done by an attempt to remove the sustenance of disease, if the food of health is not provided."—*A Journey in the Slave States*, by F. L. Olmsted, p. 627.

larger stations in the North-West. If, in addition to these advantages, we take into account the comparative immunity we enjoyed there from that plague of military hospitals—venereal—we will, I think, have no difficulty in recognising its superior claims to a separate occupancy and support as a military cantonment.

How far our comparative freedom from this last-named scourge may be ascribed to our isolation, to the small number of troops in garrison, or to the distance of the bazaar from the barracks, I cannot undertake to decide, but I am satisfied that the system of keeping up a self-supporting establishment of native women near barracks is a good one, and the oftener these are changed, within of course reasonable periods, provided suitable substitutes can be procured, the better. The system of bazaar provision which, aided by the resident magistrate, we were able to make for their establishment, answered admirably.[1] We rarely had as many as half-a-dozen men under treatment at any one time, and their complaints were of so mild a character as to require but simple remedies and short confinement. I am not speaking here as a moralist, but as a medical man, and in that capacity I conceive I have a right to avail myself of the results of my experience, and adopt such measures of mitigating disease as my judgment may approve. Both of these concur in recommending the scheme here contemplated; to render it effectual, however, change is occasionally necessary, as satiety often begets disgust, and I believe with the late Mr Guthrie, that however pure a small number of women to whom a large body of men have access may be at first, they ultimately become diseased, even though there was no disease on either side in the beginning. This accords with reason, and with what we know of the secretions of the vagina even

[1] *Apropos* of the above, I have just read a passage in Mr Motley's great work—*The Rise of the Dutch Republic*, page 340—which has almost taken my breath away, and which shows that the practice here so fully advocated can boast of a much higher antiquity than any I could claim for it. While describing the preparations made by Philip for the purpose of reducing the Netherlanders to submission, and putting down Protestantism amongst them, he adds, "With an army thus perfect on a small scale, in all its departments, and furnished, in addition, *with a force of two thousand prostitutes, as regularly enrolled, disciplined, and distributed as the cavalry or the artillery*, the Duke embarked upon his momentous enterprise on the 10th of May 1567, at Carthagena;" and thus affords additional proof of the wisdom and sagacity of Alva! Verily there is some advantage in Mr Carlyle's ideal of a fostering despotism; and that Philip the Second of Spain, the confidant of popes and cardinals, the upholder of the Inquisition, and the virtual slayer in cold blood of Counts Egmont and Horn, and of hundreds of others, for holy Mother Church and conscience' sake, should allow prostitutes to march and be mustered with his troops, is a fact of peculiar significance in these days of mawkish prudery and unhealthy political squeamishness. I was at one time greatly shocked at a charge preferred, if I mistake not, by Bishop Bull, in his *Apologia* against the Papacy, of increasing its revenues by levying a tax upon brothels in Rome, and thereby encouraging the practice of prostitution; but I am shocked at nothing now—my motto is "Nil admirari;" and what I have quoted above, supplemented as it is by the experience of the world, confirms anew, were such required, the truth of the remark advanced by Horace some nineteen hundred years ago—

"Expellas naturam furca, tamenetsi recurret."

in the healthy female, and it is almost needless to add that the liability to it is enhanced by privation and uncleanliness. Wherever syphilis originated—whether it was imported from the East by the Crusaders, or brought from the West by the followers of Columbus, or whether it was generated *de novo* in the French army before Naples—matters little to our present inquiry; it is now confessed to be the same everywhere, and its history is written in characters too legible to be misunderstood. A dispassionate perusal of this would, I think, supply facts enough and to spare in support of the views I have just enumerated; and there is no country in the world in which the practice of prostitution is more amenable to restraint, or more in want of it, than India.[1] Yet, strange to say, it has been

[1] This will appear to more advantage from the subjoined article on Prostitution in India, which I contributed to the pages of the *British Medical Journal* for 15th June 1867, and which has been regarded by all the medical officers to whom I showed it as a fair and truthful, though perhaps imperfect, representation of the whole subject:—

"*The Social Evil in India.*—In a country presenting so many social anomalies as India, and in which every domestic requirement is provided for by a proper gradation of caste, it is scarcely to be wondered at that the social evil should be a recognised institution, and that little or no reproach should attach to the practice of prostitution. Yet such is really the case. The Hindoo religion gives a certain amount of sanction to its existence by requiring the presence of common women at some of its ceremonials; and the sensuous apathy and indifference of the Mohammedans, added to their love of pleasure and morbid craving after excitement, supply ample materials for its propagation and encouragement. It enjoys, indeed, an immunity which places it in a position of vantage as regards comparison with other offices; and such of its fair votaries as combine with their ordinary *rôle* a knowledge of the Terpsichorean art, occupy positions and emoluments to which their less favoured, though chaster, sisters may aspire in vain. They generally occupy good houses in the best streets, are carried about in flashy conveyances without any appearance of compunction or shame; and though the older, more respectable, and fastidious, may affect to regard them with suspicion and place them on a level with those degraded pariahs of the *sweeper* caste, for whom their birth constitutes a disqualification they can never overcome, they are, nevertheless, very content with their position, and rarely wish to resign it. Certain it is that, whatever may be the social inconveniences incident to their life in private, their conduct is not subject to that surveillance or restraint in public, or provocative of that censure from the general community, which is so often and so unsparingly dealt to their sisters in the cooler West. There is no country in the world in which the pursuit of filthy lucre is more keenly prosecuted for its own sake. The *auri sacra fames* pervades all ranks of the native community; and Mammon is worshipped with a vigour and perseverance which put his more recognised, though not more popular, fellows completely into the shade. It will, therefore, scarcely be wondered at, that girls are frequently brought up from their cradles, or purchased surreptitiously, with the avowed object of being turned to profitable account, as the private paramours of wealthy lechers or public votaries at the shrine of Venus. A system of kidnapping for this purpose has existed from time immemorial in native states. Well-informed persons say that it still exists to a large extent even in our own remoter territories; and it was only the other day that some procuresses in the service of the King of Oude were criminally indicted before the High Court of Calcutta for purchasing some of the famine-stricken female children of Cuttack and Orissa for incorporation with the inmates of his harem at Garden Reach. Disclosures of like import, but rarely of such repulsive magnitude, still crop up betimes in some of our Mofussil courts. A certain number of women of this class were formerly

allowed to run riot up to a very recent period, and even now it entails no reproach or inferiority in the eyes of the public. The result I need not stop to consider here; the subject is obviously too extensive for discussion in this place; and it is time I brought this already too lengthy paper to a close. I am glad to find that recent legislation has done something in the direction of interference, and I hope to see the day when the whole question will be dealt with on its own merits alone, by the strong arm of the law, and not be shelved as heretofore with a peevish shrug of non-recognition, or tolerated under the instigation of a mistaken philanthropy, and in a weak spirit of connivance and compromise.

recognised as a constituent part of regimental establishments. They used to be mustered as such at the usual monthly parade; and the advantages of their recognition were so apparent that their presence even now is regarded with favour by the medical and military authorities of sundry stations and regiments. The Government of India, in the North-West, knowing these facts, knowing also that opposition on their part would be not only fruitless, but positively injurious, by putting a penalty on a practice which is productive of profit, which can be cultivated without effort or labour, and which entails no reproach, wisely determined to deal with the matter on its own merits; and we recognise with satisfaction, that they have issued a series of regulations relating to the subject, and which would, we think, if carried out in a proper spirit, tend to mitigate its horrors, and divest it of its worst sting. Unfortunately, however, the apathy so common in India has stepped in to blunt the edge of good intention. The old civilian and military jealousy has cropped up in some places, and there is wanting everywhere that co-operation and unanimity without which no adequate result can be secured. The regulations are only partially carried out, through private medical agency, in some stations; they are not enforced in others at all; and we have heard of a large central station in which the Cyprians can, by simply changing from one side of a street to another, evade and jeer at those sent in quest of them by the local surgeons. This should not be so; indeed, it seems extraordinary, in this nineteenth century, with the exigencies of the present and the experience of the past staring them in the face, and loudly proclaiming the necessity of interference, that the authorities do not take more urgent steps in the matter, and rather put down by the high hand of official injunction a growing nuisance that stalks uncontrolled in their streets, than allow it to fester by the wayside, and corrupt unseen, and to the very core, the persons of soldiers, and, through them, of their partners and innocent offspring. Surely, there need be no such excessive squeamishness about the liberty of the subject in a country like India, whose people, or rather the portion of them here referred to, know nothing of, and care less for, the provisions of the Habeas Corpus, in which prostitution is a recognised calling, and in whose climate there are, besides, unavoidable elements of mischief enough to try the strength and endanger the efficiency of the very strongest. Medical officers have exhausted to no purpose the armoury of argument, suggestion, and remonstrance; but they can do little of themselves, and nothing without the aid or sanction of the civil power. We are glad, however, to find that, in this, as in many other instances, our brethren have proved themselves the pioneers of progress; and we hope they will not be deterred by indifference, or even opposition, from grappling in its own stronghold with an evil which has been, and is likely to continue, one of the worst of those with which their science or humanity has to deal."

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ON THE

HOURS OF MAXIMUM MORTALITY

IN ACUTE AND CHRONIC DISEASES.

BY

JAMES FINLAYSON, M.D., L.R.C.S.E.,
FELLOW OF THE FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW

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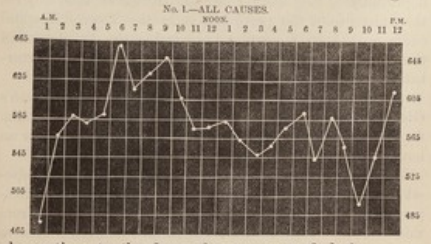
ON THE HOURS OF MAXIMUM MORTALITY

IN ACUTE AND CHRONIC DISEASES.

"BURDACH states that the greatest mortality of diseases "occurs after midnight. My results for fever are decidedly "opposed to this, as they give for the greatest number of "deaths the hours from noon to 6 P.M." Such was the statement of the late Dr And. Anderson, of Glasgow, in a paper on Fever, published in 1840. The object of the present communication is (1) to show that the results from *fever* statistics do, in point of fact, differ from those obtained from the *general* mortality returns, or from those of *chronic* diseases; (2) to indicate reasons why such a difference should exist; and (3) to illustrate the subject by a comparison of the statistics drawn from Glasgow sources with those obtained elsewhere.

Mr West Watson, the City Chamberlain, tabulated the whole deaths (some 13,000) occurring in Glasgow in 1865, according to the hours at which they were reported to the registrars as having taken place. On reducing his

No. 1.—ALL CAUSES.



observations to the form of a curve, we find three noteworthy facts. (*a*) A marked difference between the terminal hours of the day, viz., between 11 to 12 P.M., and 12 P.M. to

1 A.M.; (*b*) a maximum gradually reached and consistently maintained, from 4 to 10 A.M.; and (*c*) a very slight tendency to elevation in the afternoon hours.

(*a*) The immense difference between the number of deaths from 11 to 12 P.M., as compared with those of the succeeding hour, from 12 P.M. to 1 A.M., can scarcely represent any actual fact in nature: as I suggested in a paper recently read before the Glasgow Philosophical Society, the difference seems to arise from the peculiarity of these two hours forming the boundary line which separates one day from another. Curiously enough, an exactly similar perturbation occurs in Schneider's larger tabulation of 57,000 deaths in Berlin. (See Appendix.) But in the curve No. 2, in the present paper, representing the deaths from chronic disease as obtained from the Town's Hospital records, while there is a great disparity between the two hours in question, the excess in this case is on the different side. The only explanation which occurs to me is, that the public might, perhaps, incline to put cases occurring at this doubtful time within the day on which they had been watching for the fatal event, while the officials at a workhouse might be supposed to prefer to return such ambiguous cases for the fresh day on which, as night nurses, they were entering. However this may be, the existence of the perturbation in the Town's Hospital curve, with such a complete reversal of the distribution of the figures, seems to leave no doubt of the terminal perturbations being due to extraneous causes.

(*b*) The maximum mortality from 4 to 10 A.M. in Watson's Glasgow table, agrees essentially with Schneider's maximum for Berlin, although by the latter it is made to fall about an hour earlier.

The beginning of this period corresponds, as I have elsewhere shown, with the minimum attained by the pulse, temperature, and urinary excretion in their daily range. From this hour the range of these important functions begins to increase, so as to reach a maximum in the course of the day. "Hence, we may either say that the period of minimum vital energy, which exists during the first few hours after

midnight, being deepened and perhaps prolonged, coincides with the summit of the death curve; or, phrasing it otherwise, that the time having arrived for a fresh rallying of the vital energies for a new day, the dying are found to be unable to respond to the call, and so they perish in greatest numbers at the very hours in which the living are manifesting, in every way, a renewed vigour."*

(*c*) In addition to the great maximum in the morning, there is, perhaps, a slight tendency to an increase in the afternoon or evening hours—4 to 8 P.M.—shown both by Watson's and Schneider's figures. This feature may be due to the disturbing influence of acute diseases on the general mortality as will be explained hereafter.

PROPORTION PER 1000.

	A.M. 12-4	A.M. 4-8	A.M. 8-12	NOON. 12-4	P.M. 4-8	P.M. 8-12
Watson——13,854 Deaths,.....	159	180	174	162	166	159
Schneider—57,984 Deaths,.....	169	191	169	152	163	157

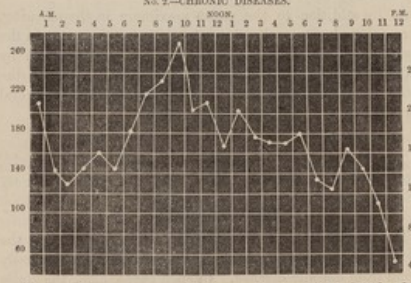
CHRONIC DISEASES.

The mortality from chronic disorders must follow the law of the general mortality, inasmuch as death usually occurs from chronic disease. In such cases, also, the daily period of depression in the vital functions, already referred to, must have a less complicated sphere for its operation. To study such cases, I obtained the records of the Town's Hospital for the poor of the city. If children under 10 years be excluded, nearly all the deaths in this institution are due to chronic disease. In the Appendix there is a list of the diseases in 100 deaths impartially selected over the whole records. This list shows that 95 per cent. of the deaths were due to chronic diseases; there was, indeed, an unusually large number of cases of very old people dying from senile decay, so that (children under 10 being excluded) the average age at death was 51 years. An excellent selection of chronic cases was thus found ready made; all the deaths, except those under 10 years of age,

* On some indications of a daily periodicity in the vital functions of man. By James Finlayson, M.D.—*Proceedings of the Glasgow Philosophical Society, 1873-74.*

were included in the table. The curve shows in addition to the perturbation in the first and the last hours of the day already discussed, a single maximum of a very marked character occurring in the forenoon, and falling, perhaps, somewhat later than in the case of the deaths from all causes.

No. 2.—CHRONIC DISEASES.



To test the accuracy of this conclusion, 1077 deaths from phthisis pulmonalis were tabulated separately. This affection was selected as a typical chronic disease. 814 of these cases were from the Town's Hospital, and 263 from the Royal Infirmary records. The correspondence of the results is perfectly satisfactory.

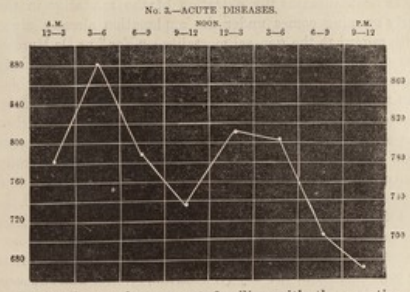
	PROPORTION PER 1000.					
	A.M.		NOON.			P.M.
	12–4	4–8	8–12	12–4	4–8	8–12
4045 Deaths in Town's Hospital representing Chronic Diseases,	156	174	226	176	151	117
1077 Deaths from Phthisis Pulmonalis,	162	182	223	176	152	105

ACUTE DISEASES.

Acute diseases are well represented by that great class of cases treated in the fever wards of our Infirmary and in the Fever Hospitals of the city. The disorders thus included consist chiefly of typhus, enteric, and relapsing fevers, smallpox, scarlatina, and measles, with a few cases

of other infectious diseases, and a small proportion of disorders admitted by mistake—chiefly pneumonia and pleuropneumonia, acute cerebral affections, and obscure suppurations. A tabulation of 6183 deaths in such cases shows the mortality to present a double elevation in the course of the twenty-four hours—one in the early morning, and another in the afternoon or evening. When reduced to periods of three hours, so as to abolish minor fluctuations, the curve brings this double maximum clearly into view.

No. 3.—ACUTE DISEASES.



But to those who are not familiar with the practice of our fever wards and hospitals in Glasgow, it may be more satisfactory to have smaller numbers made up of definitely ascertained and well-known diseases. 1487 deaths from typhus, enteric, and relapsing fevers, smallpox, scarlatina, and measles, show likewise a double maximum for the twenty-four hours.

	PROPORTION PER 1000.					
	A.M.		NOON			P.M.
	12–4	4–8	8–12	12–4	4–8	8–12
1487 Deaths from Typhus, Enteric and Relapsing Fevers, Small-pox, Measles, Scarlatina (see Appendix),	162	180	170	164	177	147

But, further, in the case of Glasgow, typhus (exanthematic) was selected as the typical acute disease, and as the

diagnosis in the deaths thus tabulated was made at the City Fever Hospitals, it may be relied on with confidence. In 828 deaths from this disease there is not merely a second maximum, but the proportion of deaths from 4 to 8 P.M. exceeds that of any other period of 4 hours.

	PROPORTION PER 1000.					
	A.M.		NOON			P.M.
	12–4	4–8	8–12	12–4	4–8	8–12
828 Deaths from Typhus,	156	**176**	169	150	**188**	161

In acute diseases the elevation of the mortality in the early morning comes under the explanation already given with regard to the general mortality; "For," as Dr Murchison says in his *Treatise on Fever*, "it is usually in the early morning that the vital processes are at the lowest ebb." But the second elevation of the mortality in acute diseases, traceable in the afternoon or evening, is no doubt related to the exacerbations of the fever which so generally occur after mid-day. Dr J. B. Russell regarded 8 P.M. as usually concurring with the maximum of the daily exacerbation of the temperature in typhus (*Glasgow Medical Journal*, vol. I., p. 495.) But in severe cases, and *a fortiori* in those which prove fatal, the tendency is for the maximum to occur sooner, and, what is more important, for the exacerbations to begin earlier in the day (*Wunderlich, Medical Thermometry*, X. 7.) Any time in the afternoon or evening would thus accord well with thermometrical investigations, and the figures represent the second maximum of the mortality in febrile diseases as occurring sometime after noon and before 8 P.M.

Thus far a study of my own statistics led me, when I found that the researches of others, especially those of Berlinski and Casper and of Schneider, had conducted them to substantially the same results as regards the main facts here adduced, and to somewhat similar opinions as regards the explanation. The former writers seem to have refined to the extent of separating the inflammations of important organs from the class of fevers. They thought the maximum mortality from inflammations occurred earlier in the day than in the case of fevers—the direct damage to vital

organs terminating life in the one class, exhaustion after the exacerbation determining the death in the other class. The figures hitherto compiled do not seem to warrant such an opinion, but those founded on by Berlinski and Casper, and the returns of Schneider, as well as my own, will be found in the Appendix.

With the exception of the figures supplied by Dr Steele at Guy's Hospital, but few observations have been published on the hour of death in this country. Dr Steele's conclusion was rather against any influence being exercised by the time of day in this respect; but in Guy's Hospital the large proportion of acute cases might tend to mask the law of the general mortality; indeed, his proportions for periods of six hours, as given in the Appendix, seem to agree with the double elevation already described as found in acute diseases.

So far as this inquiry can be said to have a practical application, it lies on the surface. Not unfrequently our sole indication for treatment is to try to avert impending death; a knowledge of the time when the tendency to death is greatest may serve to direct special attention to these critical hours. In particular, it is worthy of note that death from exhaustion seems specially apt to occur in the early morning—a time when the tired attendants, unless forewarned, are not unlikely to be less alert. The question of life or death—affecting it may be a case where recovery is perfectly possible—sometimes turns upon attention to the timely supply of food or stimulants in accordance with the necessities of the patient, and the statistics submitted furnish an indication and warning intelligible alike to the popular and the professional mind.

APPENDIX.

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SOURCES OF PRESENT STATISTICS.

My friend, Dr J. B. Russell, Medical Officer of Health for Glasgow, kindly placed at my disposal the books containing the hours of death in the two fever hospitals of the city (Parliamentary Road and Belvidere).

He likewise obtained for me, through the kindness of the authorities, similar records from the Town's Hospital for the poor of the city.

Dr Thomas, the superintendent of the Glasgow Royal Infirmary, placed the records of mortality in that institution at my command.

Dr Samson Gemmell and Dr James Barr gave much time in co-operating with me for the tabulation of these extensive materials. Deaths entered as occurring at 12 P.M. were tabulated as between 11 and 12. All indistinct records were passed over.

CAUSES OF DEATH AT THE TOWN'S HOSPITAL.

Although assured from personal observation and general repute that the deaths at this institution were almost wholly due to chronic disease—provided children were excluded—it was thought desirable to select the first 12 or 13 deaths out of each of the 8 volumes dealing with the period in question. Of 100 deaths above 10 years of age thus selected, 5 were due to acute diseases (2 acute bronchitis, 1 erysipelas, 2 pleuro-pneumonia); 95 per cent. came fairly under the designation of chronic disorders. The mean of the ages in the 100 cases was 51 years.

Phthisis Pulmonalis, sometimes with Bronchitis or Diarrhœa as complications,	21 per cent.
Chronic Bronchitis, Cardiac, Renal and Liver Disease, and Dropsy,	34 "
Senile Debility, Cerebral and Paralytic Disease,	34 "
Syphilis, Cancer, Chronic Rheumatism, Gastric Ulcer, Sloughs,	6 "
	95 "

MORTALITY FROM ALL CAUSES.

	Total.	A.M. 12-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	A.M. 11-12	P.M. 12-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	P.M. 11-12
Deaths in Glasgow in 1865, according to Watson,	15,554	474	388	380	380	331	361	413	429	446	497	576	679	682	660	546	553	478	391	342	307	332	494	543	611
Deaths in Glasgow, 1855-57, according to Schneider,	37,994	1467	1060	1042	994	939	931	1039	1060	1039	1071	2393	2405	2072	2056	2205	2103	2083	2284	2295	2144	2138	2102	2029

CHRONIC DISEASES.

	Total.	A.M. 12-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	A.M. 11-12	P.M. 12-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	P.M. 11-12
Deaths in Town's Hospital, Glasgow (Males), 1855-57, excepting Chronic Diseases,	4045	213	141	129	147	150	143	182	219	232	273	293	298	265	272	174	169	172	179	155	135	165	146	112	59
Deaths from Phthisis Pulmonalis,	1077	64	32	34	35	39	45	54	58	68	57	59	43	44	58	45	42	46	40	26	34	37	35	17	

ACUTE DISEASES.

	TOTAL.	A.M. 12-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	11-12	NOON.	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	P.M. 11-12
Deaths in Fever Wards and Small-pox Wards of the Glasgow Royal Infirmary, 1845-1864, ...		120	137	114	147	141	133	137	109	130	101	105	84	96	132	125	126	123	109	116	104	73	92	107	94
Deaths in Fever Wards (apart from Smallpox Wards), 1864-1875, ...		83	73	90	78	85	84	82	69	92	72	84	67	84	73	83	73	73	71	81	71	73	71	74	56
Deaths in Smallpox Wards 1864-75, ...		10	6	3	7	7	7	7	7	1	1	2	3	1	1	2	1	1	3	1	2	2	3	1	3
Deaths in Parliamentary Road Fever and Smallpox Hospital since founded, ...		45	41	43	37	54	51	43	41	41	37	52	55	34	43	39	30	34	38	47	65	43	39	40	46
Deaths in Belvidere Fever Hospital since founded, ...		10	11	10	12	6	13	9	12	5	6	19	16	11	7	12	15	18	15	8	12	8	8	11	4
	6183	256	268	262	301	291	286	278	238	273	237	263	227	292	247	294	293	265	256	233	254	220	223	253	203
Deaths from Typhus Fever, ...	828	37	30	42	47	37	37	31	31	30	38	27	33	31	33	37	33	42	36	34	44	29	43	27	34
Deaths from Enteric Fever, ...	79	3	2	4	4	3	1	4	3	2	2	5	2	4	1	4	6	7	4	3	1	3	7	3	2
Deaths from Relapsing Fever, ...	69	2	3	1	5	0	5	2	3	4	1	3	4	5	3	3	3	2	4	0	5	2	2	3	3
Deaths from Smallpox, ...	286	18	11	10	15	15	17	14	19	9	11	11	15	15	9	11	15	11	15	9	15	12	8	5	10
Deaths from Scarlatina, ...	41	3	2	1	1	1	3	1	0	1	1	1	3	5	0	2	2	1	2	3	1	5	2	0	0
Deaths from Measles, ...	182	7	4	7	9	6	11	7	6	15	14	6	14	7	10	9	6	6	6	10	1	6	3	3	1
	1467	69	52	53	76	72	74	59	62	61	67	65	69	58	62	67	67	71	67	59	67	58	67	43	55
Deaths from Pleurisy, Pneumonia, Empyema, Pericarditis, ...	265	10	6	8	16	12	15	10	17	11	19	10	13	8	12	15	18	9	11	14	7	10	8	13	6

GENERAL MORTALITY.

	Total Observations.	Proportion per 1000.			
		A.M. 12-6	NOON. 6-12	12-6	P.M. 6-12
Virey: quoted from Oesterlen, ...	304	237	273	250	240
Buck: quoted from Oesterlen, ...	1,958	306	242	211	241
Berlinski and Casper, ...	5,591	252	291	243	214
Schneider, ...	114,183	268	261	233	238
Steele, ...	2,452	268	223	285	224
Smoler, ...	1,000	334	276	189	201
Watson, ...	13,854	250	264	246	240
Quetelet, ...	5,250	266	252	278	204
CHRONIC DISEASES.					
Berlinski and Casper, ...	4,463	255	301	239	205
Schneider,*	4,266	287	255	239	219
Finlayson, ...	4,045	231	326	262	181

ACUTE DISEASES.

	Total Observations.	Proportion per 1000.			
		A.M. 12-6	NOON. 6-12	12-6	P.M. 6-12
Anderson: Fever, ...	500	256	242	280	222
Berlinski and Casper—					
Fevers and Exanthems, ...	462	234	260	236	270
Inflammations, ...	666	240	246	274	240
Schneider: Fevers and Exanthems,†	708	239	234	260	267
" Inflammations,‡ ...	1,430	275	211	245	269
Finlayson: Fevers and Exanthems,	1,487	260	252	257	231
" Acute Diseases, ...	6,183	270	247	261	222

* Made up from his entries against the following: Bauch—und allgem. Wassersucht, Stickfluss, Lungenlähmung, Scropheln, Lungenschwindsucht, Abzehrung, organische Krankheiten des Gehirns, Herzens, &c., Krebs, Entkräftung.

† Made up from his entries against Fieber, Scharlachfieber, Masern, Pocken.

‡ Viz.: Gehirnentzündung, Luftröhrenentzündung und Bräune, Entzund. der Brustorgane, Entzund. der Unterleibsorgane, hitzige Gehirnhöhlen-Wassersucht.

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NOTE

SUR L'ESPACE CUBIQUE ET SUR LE VOLUME D'AIR

NÉCESSAIRES

pour assurer la salubrité des lieux habités

PAR M. LE GÉNÉRAL MORIN.

Extrait des Annales du Conservatoire des Arts et Métiers.

L'étude que je présente aujourd'hui m'a été inspirée par la lecture d'un Mémoire publié à Édimbourg, en 1867, par M. le docteur F. de Chaumont, chirurgien militaire, professeur adjoint d'hygiène à l'école médicale de l'armée anglaise, Mémoire intitulé : *De la ventilation et de l'espace cubique*.

En reliant les résultats des observations du savant chirurgien anglais avec les belles recherches de M. F. Le Blanc sur la composition de l'air confiné, il m'a paru que l'on pouvait en déduire des indications utiles pour les progrès de l'hygiène publique, à laquelle nos diverses administrations civiles ou militaires continuent d'accorder trop peu d'importance.

Quoique l'acide carbonique soit le gaz qui, dans tous les lieux habités, détermine inévitablement l'une des altérations les plus graves de l'air, l'action des vapeurs et des autres gaz produits par les émanations cutanées contribue aussi, bien souvent, à augmenter l'insalubrité et le malaise qu'il occasionne. Les expériences des chimistes nous permettent d'apprécier approximativement le volume d'acide carbonique qu'un individu sain, à l'état normal, peut développer par heure; mais elles ne nous apprennent malheureusement rien quant aux autres émanations, et la science physiologique est encore muette sur l'action que ces matières indéfinissables exercent sur l'organisme.

Tout ce que nous savons, c'est que ces émanations semblent

croître, sinon dans la même proportion, au moins dans le même sens que le volume du gaz carbonique, et que l'odeur désagréable et parfois nauséabonde qu'elles produisent augmente de même.

M. F. Le Blanc, dans son remarquable Mémoire de 1842, intitulé : *Recherches sur la composition de l'air confiné*, s'exprime dans les termes suivants :

« Sans vouloir nier que diverses causes puissent concourir à « rendre insalubre une atmosphère limitée, il faut reconnaître « comme un fait d'expérience que la proportion d'acide carbo- « nique dans les lieux habités et fermés, presque toujours ap- « préciable, croît avec le degré d'insalubrité et peut en fournir, « pour ainsi dire, la mesure. »

Mais la détermination de la proportion d'acide carbonique contenue dans l'air exige des expériences spéciales, tandis que la présence des matières organiques, qui altèrent la pureté de l'air, nous est directement révélée par nos sens.

Sous ce rapport, les impressions de l'organe de l'odorat peuvent fournir, dans certaines limites, un indice probable de la salubrité.

Cette opinion, qui a été émise il y a déjà quelques années, par M. Boussingault, au sein de l'Académie des sciences, paraît avoir reçu une confirmation assez remarquable par les observations suivantes dues à M. le docteur F. de Chaumont, chirurgien de l'armée anglaise, qui les a consignées dans sa notice sur *la ventilation et l'espace cubique*[1] imprimée en 1869. L'auteur montre, dans le tableau suivant, la corrélation qui existe entre la proportion d'acide carbonique contenu dans l'air et les sensations de l'organe de l'odorat.

1. On ventilation and cubic space, by staaf surgeon, F. de Chaumont, assistant professor of hygiene, army medical school, Netly. Édinburg, 1869.

PROPORTION d'acide carbonique.	IMPRESSION DE L'AIR SUR L'ODORAT.	OBSERVATIONS.
0.001408	Excessivement vicié et nauséabond.	L'auteur anglais se sert pour exprimer le degré d'altération de l'air du mot *closeness*, qui n'a en français d'équivalent que celui d'air renfermé, également impropre. Le vrai sens qu'il m'a paru convenable de lui attribuer est celui d'air vicié.
0.001200	Très-vicié.	
0.001154	Excessivement vicié et nauséabond.	
0.001090	Excessivement vicié.	
0.001072	Très-vicié.	
0.000962	Très-vicié.	
0.000921	Vicié.	
0.000840	Un peu vicié (observation de jour).	
0.000843	Un peu vicié.	
0.000820	Un peu vicié.	
0.000804	Très-vicié.	
0.000804	Vicié.	
0.000759	Peu désagréable, légère odeur.	
0.000697	Un peu vicié.	
0.000658	Peu vicié.	
0.000642	Peu vicié.	
0.000508	Sans altération.	

L'auteur de ces observations fait remarquer que les impressions signalées, malgré quelques divergences entre elles, semblent suivre une marche régulière, et qu'elles ont d'ailleurs été consignées à différents moments, mais toujours avant que la proportion de l'acide carbonique contenu dans l'air ait été connue. Il ajoute que quelques-unes de ces différences peuvent être attribuées à ce que l'observateur n'entrait pas toujours directement dans le local, en venant de l'extérieur, ce qui eût été désirable, parce qu'alors le sens de l'odorat est bien plus impressionnable.

Il pense, d'après ces expériences, qu'il est permis de conclure que, quand l'air des salles ne contient pas plus de 0,0006 de son volume d'acide carbonique, l'odeur causée par la présence des matières organiques est imperceptible dans beaucoup de cas, et que cette proportion doit être regardée comme correspondant au minimum de pureté acceptable de l'air.

Des proportions d'acide carbonique contenues dans l'air des lieux habités.

Les chimistes admettent en général qu'à l'état normal l'air, regardé comme pur, contient une proportion d'acide carbonique

comprise entre 0,0004 et 0,0006[1]. Pour les calculs nécessairement approximatifs que nous nous proposons de faire, nous supposerons que cette proportion soit $\frac{1}{n} = 0{,}0005$.

D'une autre part, les expériences les plus récentes conduisent à évaluer à 38 grammes le poids de l'acide carbonique qu'un homme ordinaire expire par heure. La pesanteur spécifique de ce gaz étant 1,524 fois celle de l'air, qui, à zéro, pèse 1^k,298 le mètre cube, le volume d'acide carbonique expiré par heure et ramené à zéro peut être évalué à $\frac{0^k{,}038}{1{,}298 \times 1{,}524} = 0^{mc}{,}020$. C'est la valeur généralement admise.

Mais, outre le gaz acide carbonique que l'acte de la respiration introduit dans l'air, et qui tend à en altérer la salubrité, il s'y développe aussi incessamment de la vapeur d'eau qu'il importe de n'y pas laisser accumuler.

Les observations exécutées dans plusieurs casernes occupées par des soldats jeunes et en bonne santé, par M. F. Le Blanc, pour une Commission[2] formée par M. le Ministre de la guerre dans le but de constater l'état de salubrité de ces locaux, ont fourni les résultats résumés dans le tableau suivant :

DÉSIGNATION de la CASERNE.	Capacité de la chambre.	Nombre d'hommes.	Espace cubique par homme.	Durée du séjour.	Proportion d'acide carbonique dans l'air.	Proportion de vapeur d'eau calculée par à 15°.	Volume de vapeur correspondant à la proportion par heure de séjour.
	mc.		mc.	h. m.		gr.	mc.
De l'Assomption.	341	25	15.6	10 15	0.0032	7.67	0,0132
Rue de Babylone.	600	52	11.54	10 45	0 0034	7.08	0,0092
Quai d'Orsay...	94	11	8.54	10 00	0.0088	7.60	0 0146
							0,0123

On remarquera que, par suite de la clôture des chambres et de l'absence du renouvellement de l'air, la proportion d'acide

1. *Traité de Chimie* de M. Cahours, 1er volume, page 129 ; M. Leblanc admet la proportion de 0.0006 (*Annales de Chimie*, 3e série, t. V, page 231.

2. Cette Commission était composée de MM. le général Schramm, président ; Genty de Bussy, intendant militaire, Cathala, colonel du génie, Boussingault (de l'Institut), Brault et Moizin, médecins militaires, et F. Leblanc, rapporteur. (*Annales de Chimie et de Physique*, t. XXV.)

carbonique dans ces chambres a dépassé de beaucoup celle de l'air normal, ce qui met en relief le défaut de proportion de ces locaux où le nombre d'hommes devrait être réduit du tiers, au moins, afin d'allouer à chacun environ 16 à 20 mètres d'espace.

Quant au volume de vapeur dû à la présence de chaque homme et rapporté à une heure de séjour, et qui est en moyenne de $0^{mc}{,}0123$ supposé à 15°, s'il est notablement inférieur à celui de $0^{mc}{,}0433$, qui résulte des expériences de M. Dumas, la différence doit être attribuée à ce qu'une partie de la vapeur dégagée se condensait sur les murs, et le volume des $0^{mc}{,}0123$ peut être regardé comme celui qu'il faudrait évacuer par heure au moyen d'une ventilation continue.

D'après les données précédentes, les volumes des gaz et des vapeurs nuisibles à la salubrité, exhalés par heure et par individu sain, seraient :

Acide carbonique	$0^{mc}{,}0200$
Vapeur d'eau entraînant les autres émanations cutanées	0 ,0123
Total . . . . . $m =$	$0^{mc}{,}0323$

Nous prendrons en moyenne $m = 0^{mc}{,}030$.

En partant de ces données, on peut se proposer de résoudre le problème suivant :

Quel est le volume d'air qu'il faut introduire dans un local habité par un homme pour y entretenir un état de salubrité suffisamment voisin de celui de l'air extérieur?

Appelons E l'espace cubique occupé par l'homme.

$\frac{1}{n} = 0{,}0005$ la proportion normale moyenne d'acide carbonique contenu dans l'air qu'on regarde comme pur ;

$m = 0^{mc}{,}030$ le volume moyen du mélange d'acide carbonique ($0^{mc}{,}020$) et de vapeur ($0^{mc}{,}010$) qu'il faut, dans les cas ordinaires, extraire par heure et par individu ;

Pour les hôpitaux, il conviendra de faire $m = 0^{mc}{,}040$ au moins, et $m = 0^{mc}{,}060$ pour ceux des femmes en couches et des blessés ;

x le volume d'air à extraire et à introduire par heure et par individu pour que la proportion d'air vicié ou d'acide carbonique ne dépasse pas une valeur $\frac{1}{n'}$, déterminée par l'observation, et que nous prendrons égale à 0,0008 au plus, limite à laquelle se manifeste déjà, dans les lieux habités, une certaine odeur, d'après les observations de M. de Chaumont;

Le volume d'acide carbonique contenu dans l'espace E sera :

$$\frac{1}{n} E = 0{,}0005\ E.$$

Le volume de gaz ou de vapeur développé par la respiration :

$$m = 0^{mc},030.$$

Le volume x d'air neuf à admettre fournira, en acide carbonique : $\frac{1}{n} x = 0{,}0005\ x$.

Le volume x d'air vicié à extraire en soutirera :

$$\frac{1}{n'} x = 0{,}0008\ x.$$

Le volume total d'acide carbonique contenu dans l'espace E, sous l'action d'une ventilation et d'émanations continues, sera :

$$\frac{1}{n} E + m - x \left(\frac{1}{n'} - \frac{1}{n}\right);$$

et si son rapport au volume E de l'espace occupé doit être constant et égal à $\frac{1}{n'}$, on aura :

$$\frac{1}{n} E + m - x \left(\frac{1}{n'} - \frac{1}{n}\right) = \frac{1}{n'} E,$$

ou :

$$m - E \left(\frac{1}{n'} - \frac{1}{n}\right) = x \left(\frac{1}{n'} - \frac{1}{n}\right);$$

d'où

$$x = \frac{m - E \left(\frac{1}{n'} - \frac{1}{n}\right)}{\frac{1}{n'} - \frac{1}{n}}.$$

En introduisant dans cette formule les données précédentes, on trouve pour

E =	10^{mc}	12^{mc}	16^{mc}	20^{mc}	30^{mc}	40^{mc}	50^{mc}	60^{mc}
x =	90	88	84	80	70	60	60	40

L'on voit que, plus le volume des lieux habités augmente, plus celui de l'air à renouveler pour y entretenir un degré déterminé de salubrité diminue, mais qu'il croît à l'inverse à mesure que l'espace cubique alloué par personne est moindre.

Casernes. Ainsi, l'espace alloué, dans nos casernes, au soldat, et qui, d'après les proportions normales réglementaires, n'est que de 10 à 12 mètres cubes par homme, exigerait, pour le maintien de la pureté de l'air à 0,0008 d'acide carbonique, un renouvellement d'air de 88 mètres cubes par heure et par individu, ou de 8 à 9 fois par heure.

Il n'y a donc pas lieu de s'étonner de l'impression désagréable que l'on éprouve quand on entre le matin dans les chambres de caserne de nos soldats, où il n'existe aucune ventilation régulière autre que celle qui se produit par les cheminées ordinairement sans feu.

Dans les casernes anglaises, l'espace cubique alloué à chaque homme est de $16^{mc},98$, et le volume d'air renouvelé est fixé à 85 mètres cubes par heure et par homme. C'est, comme on le voit, le chiffre déduit de la formule précédente.

Chambres à coucher. Une pièce, qui a cette destination, pour une seule personne, et qui a 4 mètres de largeur sur 5 mètres de longueur, et 3 mètres de hauteur, ou 60 mètres cubes de capacité, est généralement considérée comme suffisamment grande, et cependant, pour y entretenir le degré de salubrité indiqué plus haut et désirable, il faudrait y faire circuler 40 mètres cubes d'air par heure. Or, il n'est aucune personne en bonne santé qui, en rentrant le matin dans sa chambre, après en être un instant sortie, ne soit impressionnée par l'odeur plus ou moins sensible qui s'y est répandue s'il n'y a pas eu de ventilation.

Mais si, au lieu de n'être occupés que par une seule personne, les locaux le sont, au contraire, par plusieurs, les causes d'in-

fection augmentent dans une proportion rapide, même quand il s'y produit un renouvellement d'air sensible.

Pour que la proportion d'acide carbonique et de vapeur ne dépassât jamais alors 0,0008, il faudrait, comme on peut le faire voir aisément à l'aide de la formule précédente, que le volume d'air renouvelé fût de 540 mètres cubes, ce qui correspondrait à un renouvellement complet produit neuf fois par heure.

En proposant de régler les proportions des cheminées, de manière qu'elles puissent produire, avec un feu modéré, un renouvellement de cinq fois par heure, je suis donc resté au-dessous de ce qu'exigerait, pour le cas précédent, les conditions d'une salubrité convenable, si le séjour de six personnes devait être prolongé.

A l'appui de cette opinion, je puis citer l'une des plus importantes expériences de M. Le Blanc.

Expérience sur une chambre à coucher occupée la nuit par deux personnes. Cette chambre avait une capacité de 81 mètres cubes, ce qui n'allouait, par personne, que $40^{mc},50$, proportion très-faible. Mais elle était pourvue d'une cheminée dans laquelle le feu avait été en activité dans la soirée. Or, il résulte des observations faites au Conservatoire[1] qu'une cheminée sans feu, lorsque la température extérieure est de 10°, et celle de l'intérieur de la pièce et même celle de la cheminée de 22° seulement, détermine l'évacuation d'au moins 400 mètres cubes d'air par heure. L'air était donc renouvelé dans la chambre en expérience environ cinq fois par heure.

Pour appliquer la formule précédente, nous avons donc :

$$x = 200^{mc},\ m = 0^{mc},030,\ E = 40^{mc},50.$$

On en déduit :

$$\frac{1}{n'} - \frac{1}{n} = \frac{0^{mc},030}{200 + 40^{mc},5} = 0,000124,$$

et $\frac{1}{n'} = 0,000624$, c'est-à-dire à très-peu près la proportion

1. *Études sur la Ventilation*, 1er volume, page 295.

normale, comme M. Leblanc l'a trouvé. Aussi le savant chimiste a-t-il constaté que la salubrité de cette pièce était très-satisfaisante, mais à la condition que la cheminée y produirait une ventilation active.

Influence de la grandeur des locaux. Pour un espace cubique de 100 mètres cubes par personne, on trouverait, par la formule donnée, que le volume d'air nouveau à introduire, pour maintenir la salubrité dans la limite fixée de $\frac{1}{n'} = 0,0008$, après une heure, serait nul, ce qui veut dire seulement que la proportion d'acide carbonique exhalée par un individu, pendant une heure, serait tout juste suffisante pour amener l'état de l'air à cette proportion. Mais, après cet intervalle, la production d'acide carbonique et des autres gaz se continuant, l'air s'altérerait de plus en plus.

Ainsi, dans l'exemple que nous avons pris plus haut d'une chambre à coucher de 60 mètres cubes de capacité, en supposant qu'il n'y ait aucun renouvellement de l'air et que, par conséquent, $x = o$, on trouverait qu'après dix heures de séjour de nuit dans une chambre complétement close, la quantité de gaz et de vapeur développée serait :

$$m \times 10 = 0^{mc},300,$$

et la formule deviendrait :

$$0^{mc},300 = 60^{mc} \left\{ \frac{1}{n'} - 0,0005 \right\};$$

d'où l'on tirerait :

$$\frac{1}{n'} = \frac{0^{mc},30 + 60 \times 0,0005}{60} = 0,00550,$$

c'est-à-dire dix fois plus que la proportion normale dans l'air.

L'hypothèse d'une chambre complétement close est évidemment exagérée, attendu que le refroidissement de l'air, pendant la nuit, détermine toujours un certain renouvellement par les joints des portes et des fenêtres ; mais l'application précédente suffit bien pour expliquer l'infection partielle des chambres à coucher des appartements, même les plus élégants.

Elle montre en même temps les inconvénients graves qu'offrent, pour la salubrité, les locaux habités, dans lesquels il n'existe aucun conduit d'évacuation, aucune cheminée qui puisse permettre au moins une ventilation naturelle, même quand il n'y serait jamais allumé de feu.

Sous ce rapport, la plupart des salles d'école, celles de catéchisme dans les églises, les classes et surtout les chambres sans cheminées des casernes, laissent beaucoup à désirer, et il serait facile de les améliorer en prenant des précautions, simples à exécuter, pour éviter l'inconvénient des rentrées d'air froid près des personnes, inconvénients sur lesquels les rapports des officiers du génie me semblent avoir insisté plus peut-être qu'il n'était juste de le faire.

Application de la formule aux expériences de M. Le Blanc sur l'amphithéâtre de physique et de chimie de la Sorbonne.

Cet amphithéâtre avait, en 1842, une capacité de 1 000 mètres cubes, et pouvait contenir 900 auditeurs, ce qui n'allouait à chacun que $1^{mc},111$ d'espace cubique. Il est difficile d'imaginer une proportion plus défavorable. Il était dépourvu de tout moyen de ventilation. La seule ressource, pour y prévenir l'asphyxie des auditeurs, était d'en tenir la porte ouverte.

M. Le Blanc y a constaté dans l'air les proportions suivantes d'acide carbonique :

	Proportion d'acide carbonique dans l'air.
Un moment après l'ouverture du cours de M. Dumas, 400 auditeurs environ étant présents. . . .	$0^{mc},0065$
A la fin de la leçon, 900 auditeurs étant présents.	0 ,0103

La formule précédente, appliquée, en supposant le renouvellement de l'air tout à fait nul, donnerait des proportions d'acide carbonique notablement plus fortes; mais la différence tient évidemment en grande partie à l'influence favorable, quoique insuffisante, de l'ouverture des portes.

Le résultat de ces expériences, exécutées en 1842 et publiées à cette époque, était tellement frappant, et l'état qu'elles constataient si déplorable et si peu flatteur pour un établissement de haut enseignement, confié aux plus illustres organes de la

science, qu'on aurait dû s'attendre à voir l'administration de l'instruction publique s'empresser d'y porter remède. Il n'en a rien été, et, après trente années écoulées, l'état des choses est encore le même.

L'on pourrait, il est vrai, appliquer semblable observation à la salle des séances de l'Académie des sciences elle-même qui, malgré les réclamations et les plaintes si souvent formulées par ses membres, laisse tant à désirer sous le rapport de la salubrité.

Application aux hôpitaux. Dans les hôpitaux ventilés, on alloue un espace $E = 50$ mètres cubes par lit et un renouvellement d'air fixé à un minimum de 60 mètres cubes par heure; mais l'on doit admettre que, tant par la respiration que par les émanations cutanées, le volume de gaz vicié, développé par heure et par individu, ne peut être, comme nous l'avons indiqué, inférieur à $m = 0^{mc},040$.

En introduisant ces données dans l'équation

$$x = \frac{m - E\left(\frac{1}{n'} - \frac{1}{n}\right)}{\frac{1}{n'} - \frac{1}{n}},$$

on en tire :

$$\left(\frac{1}{n'} - \frac{1}{n}\right)(60 + 50) = 0^{mc},040,$$

d'où

$$\frac{1}{n'} - \frac{1}{n} = 0,00036;$$

et comme $\frac{1}{n} = 0,0005$, il s'ensuit que

$$\frac{1}{n'} = 0,00086,$$

valeur qui, d'après les observations de M. le docteur de Chaumont, correspond à un air peu désagréable, mais ayant une légère odeur.

Or c'est ce que l'on observe dans les hôpitaux, où le degré de ventilation supposé est régulièrement obtenu; mais on voit par

là que le chiffre de 60 mètres cubes de renouvellement de l'air dans les salles ordinaires des hôpitaux, qu'on a si longtemps hésité à adopter, est loin d'être exagéré.

J'ai cru utile de faire connaître les indications fournies par les observations directes du savant chirurgien anglais, M. le docteur de Chaumont, et d'en comparer les résultats avec ceux des belles expériences exécutées, il y a longues années déjà, par F. Le Blanc, parce que leur ensemble confirme l'exactitude des volumes d'air que, depuis longtemps, je regarde comme nécessaires pour assurer la salubrité des lieux habités.

J'ajouterai qu'en ce qui concerne les hôpitaux, ces proportions ont été adoptées par le Comité consultatif d'hygiène et du service médical des hôpitaux, créé en 1864, sous la présidence de nos confrères, MM. Dumas et Rayer, et qu'elles sont considérées comme normales par l'Administration de l'assistance publique.

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PAMPHLETS

53

53