Report on the chemical pathology of the malignant cholera. Containing analyses of the blood, dejections, &c.; of patients labouring under that disease in Newcastle and London, &c.; ... / [W. B O'Shaughnessy].

PUBLISHED BY AUTHORITY OF

THE CENTRAL BOARD OF HEALTH.

REPORT

ON

THE CHEMICAL PATHOLOGY

OF THE

Malignant Cholera:

CONTAINING

ANALYSES OF THE BLOOD, DEJECTIONS, &c. OF PATIENTS LABOURING UNDER THAT DISEASE IN NEWCASTLE AND LONDON;

&c. &c. &c.

By W. B. O'SHAUGHNESSY, M.D. &c. &c.

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PREFATORY NOTICE.

THE subsequent Report on the chemical pathology of the Malignant Cholera, as it prevailed in Newcastle-upon-Tyne, was presented to the Central Board of Health on the 7th of January. Its publication has been delayed to the present time, for the purpose of allowing the experiments therein detailed to be more extensively repeated before their results should be admitted as universal facts in the history of this disease.

The results of the analyses performed by the celebrated ROSE of Berlin having just reached this country,—the occurrence of the disease in London having given extensive opportunities for the additional investigation of the subject, and lastly, the identity of the disease in all places being thus experimentally ascertained, it is not thought desirable to postpone any longer the publication of the original Report; to which is now ap-

pended a statement of the results obtained in the analytic inquiries into the nature of the London epidemic.

The materials for this Report were principally collected at Newcastle-upon-Tyne during a visit undertaken in December last, at the urgent request of one of the Vice Presidents of the Royal College of Surgeons of London. Deeply indebted as I am to this distinguished gentleman for the many facilities afforded, through his exertions, to the prosecution of my inquiries, I regret that I am not at liberty to offer him exclusively and openly the tribute of the following pages. I must content myself by expressing my sorrow that his benevolent anxiety for the probable improvement of our modes of treating this disease, by the chemical investigation of its nature and effects, has not met a more gratifying return than my experiments have hitherto afforded.

ON THE CHEMICAL PATHOLOGY

OF THE

MALIGNANT CHOLERA.

TO THE PRESIDENT AND MEMBERS OF THE CENTRAL BOARD OF HEALTH, LONDON.

GENTLEMEN,

I HAVE the honour to lay before you an account of some chemical inquiries conducted recently at Newcastle-upon-Tyne, into the composition of the blood and alvine dejections, in the malignant cholera now prevailing in that town and its vicinity.

In doing so I have considered it advisable, in order to make my statement more generally intelligible, to separate the strict details of chemical manipulation from the observations by which the results of my experiments must be prefixed and associated. I have therefore drawn up the description of the analytic processes employed, in

the form of an Appendix to the general Report; to which I now proceed to solicit your attention, and which I shall divide into three principal Sections.

In the *first*, I will lay before you a concise, but careful, sketch of the *exact state* of *our present knowledge* of the chemical composition of the *blood* in the *normal* or *healthy* condition.

In the *second*, I will present you with an account of all the analytic inquiries yet instituted on the chemical pathology of the malignant Cholera; noting the discrepancies between the several experimentalists, and stating the results of my own investigations.

In the third and last division, I will inquire into the extent to which these investigations entitle us to form pathological or therapeutic conclusions; and I will endeavour to point out and explain the indications of treatment which they apparently afford.

SECTION I.

ON THE EXACT STATE OF OUR KNOWLEDGE OF THE CHEMICAL COMPOSITION, &c. OF THE HUMAN BLOOD, IN THE NORMAL OR STANDARD CONDITION.

It is obvious, that in any inquiry into the chemical pathology of a disease in which a presumed alteration of the blood, from the state in which it exists in the healthy individual, becomes a subject of especial investigation, it is essentially necessary to start with well-defined ideas the standard composition of that fluid.

And here I feel it incumbent on me to declare, that in addressing *the Central Board of Health* I would not venture to dwell, even for a moment, on the normal constitution of the blood, had not the most recent, and perhaps the best writer on this subject, M. Lecanu, of Paris, given his results to the public, since the Board commenced its arduous labours,—labours which, I am entitled to presume, have been sufficient to monopolize the studies of its members.

The reputed ingredients of blood drawn from the venous system in a state of health may be conveniently arranged and considered in three leading groups. *First*, those invariably present, in a proportion little varying from a certain standard, and universally recognised by all chemists and physiologists. *Secondly*, those usually present, but occasionally absent, and perpetually liable to alteration in their quantity. *Thirdly*, those substances, the

presence or absence of which in standard blood is asserted by some authorities and denied, or not recognised, by others.

Under the *first* head, may be included *water*, *albumen*, *fibrine*, *colouring matter*, *extractive matter*, and various *saline substances;* viz. the carbonate of soda, muriates, sulphates and phosphates of soda and potassa, carbonates of lime or magnesia: phosphates of these earths, and minute quantities of *iron* in an unknown state of combination.

Under the *second* head may be specified *a fatty compound*, consisting of an oily and crystallizable matter; also *urea*, or the peculiar animal principle of urine.

The *third* embraces a considerable number of substances: namely, the FREE *acetic acid*, *carbonic acid*, *cholesterine*, FREE *carbon*, and traces of *manganese*, *silica*, and *copper*.

With respect to the functions of most of the substances enumerated in the first class of ingredients, it would be manifestly improper and superfluous to enter here into any detailed observations. There are some, however, which imperatively require more particular notice. Of these the *colouring matter* deserves our principal attention.

The relations of this substance have ever been noticed with curious interest, excited doubtless, in the first place, by the intimate changes it seems to experience during the passage of the blood to the arterial from the venous system. The change of the blood from dark to scarlet taking place thus coincidentally with the alteration in its anatomical position—viewed also in connexion with the simultaneous withdrawal of oxygen from the air, and the expiration of carbonic acid from the lungs, have, to many physiologists (especially those of the strictly anatomical

school), constituted sufficient evidence that *excess* of *carbon* was the cause of the dark colour, and consequently that "rubefaction," "arterialization," and "oxygenation" of the blood were absolutely synonimous terms; a doctrine, which, I feel bound in candour to admit, till very recently I believed to be correct.

But to the cautious reasoner, who requires firm premises to be established, before he derives a single confident conclusion, it will appear strange, that as yet, the proximate and ultimate composition of the colouring matter of the blood remain open to strict investigation. That it is exclusively animal in its constitution all chemists are, it is true, at the present day agreed, and the most recent analyist, M. Lecanu, describes processes by which it may be separated from the other ingredients of the blood, and its quantity estimated with sufficient accuracy. Nevertheless, the cause of the original coloration and even the precise constitution of the substance itself—especially the mode in which its elements are combined, still remain perfectly unknown. We are, consequently, not entitled to reason absolutely on the secondary phenomena presented by the air on this colouring substance. The consideration of the probable functions of the *saline matters*, will fully exemplify this position.

The attempt to demonstrate the uses of the saline ingredients of the blood, is a circumstance of very recent occurrence, and almost exclusively attributable to the extraordinary clinical statements of Dr. Stevens of *Santa Cruz*, whose experiments have riveted the attention of all the scientific physiologists and practitioners of Europe and America; and the probable importance of which may be estimated from the fact, of their having been characterised by Dr. Prout, as apparently unfolding "the germs of immense benefit to mankind."

Dr. STEVENS' experiments may be thus briefly described. He found that dark blood extracted from a vein, could not be made to assume the scarlet tint, by exposure to, or admixture with, the air, *except saline matter was present*, but that the addition of the minutest possible quantity of a salt, even destitute of oxygen (such as the *chloride of sodium*), immediately restored the red colour. Proceeding on this indication, Dr. STEVENS used a combination of saline remedies in his treatment of the secondary period of yellow fever, a disease, in which, in this stage, blackness of blood is a most prominent symptom. The results of this practice are described to have been of the most gratifying character.

Dr. STEVENS however, as far as I have been able to understand, has not *theorized* very widely on this foundation, and he seems to rest satisfied with the evidence thus afforded, of the essential value of the salts to the blood in the living body, in preserving it in a normal condition, and enabling it in some unknown manner to discharge its functions in the respiratory and nutritive actions.

Dr. PROUT coinciding to a certain extent with the views of Dr. STEVENS, has gone a step further in assigning the particular use of one of the individual salts, namely, the muriate of soda, in the digestive process, during which he thinks it probable that this salt undergoes decomposition, muriatic acid being secreted into the intestinal canal, the free soda remaining in the serum of the blood, which thus becomes endowed with alkaline properties. Dr. PROUT also brings forward some happy illustrations of the great and extraordinary effects which small quantities of matter can accomplish; in order to show that the minute proportion of the salts to the mass of the circulating fluid, does not invalidate the hypothesis

which invests them with important functions, in many of the most remarkable processes discharged by the machinery of organized beings.

From the consideration of the preceding facts, it appears highly probable,

1. That the saline matters exercise an important, though certainly not defined or ascertained, control over the change which takes place in blood, during its passage from the venous to the arterial system.

2. That their absence or diminished proportion is connected in some unknown manner with the production of various diseased conditions.

3. That the colouring matter of the blood is not the sole ingredient affected by the respiratory process.

The second group in the broad classification I made of the constituent parts of the blood, embraced the *fatty matter* and *urea*. The *usual* existence of the former in healthy blood is now a universally acknowledged fact. The latter occurs more frequently in diseased conditions, but may still be regarded as an occasional but rare ingredient of the healthy fluid.

The third and last class of substances asserted to exist in normal blood, is composed of those, the presence of which is either disputed altogether, or supported on too limited testimony. I include in this division, *cholesterine*, *ozmazome*, *cruorine*, FREE *carbon*, *acetic acid*, *carbonic acid*, *silica*, *manganese*, and *copper*.

The three first of these substances have been described by M. DENIS of Commercy (MEUSE), in his elaborate work "*Sur le Sang Humain*," as occurring in many of his numerous analyses. In a Report, however, addressed to the *Société de Pharmacie*, by M. LECANU (Paris, Septembre 1831), it is distinctly shown, that with respect to these substances, the experiments of M. DENIS are extremely

fallacious. I therefore pass these over and proceed to the next in the catalogue, viz. FREE *acetic acid.*

The alleged existence of this acid in the normal blood constitutes an important topic for consideration in this report, since an analyst of some reputation, M. HERMANN of Moscow, has recently at the same time announced the discovery of this substance in the healthy blood, and has also endeavoured to establish on this supposed fact some very peculiar opinions, professedly founded on experiment, on the chemical pathology of the malignant Cholera.

The paper from which I derive my knowledge of M. HERMANN'S experiments and conclusions, occurs in the German *Annalen der Chym. und Physic*, tome xii. p. 161, and is rendered into a more accessible form in the *Journal de Chimie Medicale, for November* 1831. It is entitled in the French version, "*Changemens qu'eprouvent les secretions de l'organization humaine par l'effet du Cholera.*" The essay commences with an analysis of blood, supposed to be healthy and drawn from the experimentalist himself, and taken as the standard in his future researches. As it is of the highest consequence in this inquiry to have clear ideas on this particular point, I subjoin a translation of M. HERMANN'S very brief account of this analysis.

M. HERMANN'S ANALYSIS OF HEALTHY BLOOD.

"The blood after 24 hours had separated into serum
" and clot, the latter was then allowed to remain on a
" paper filter, until no more serum was absorbed. The
" proportion of serum to crassamentum was thus de-
" termined to be as 57 to 43, and the specific gravity of

" the former compared to water was 1=27. *Tincture* of " *turnsol* was reddened by the serum, and when poured on " the clot, was still more deeply coloured. In order to " prove that the red colour was not produced by admix- " ture with the colouring matter of the blood itself, the " clot was divided into two portions, and each placed in " separate vessels; pure water was poured on one, and " tincture of turnsol added to the other. Before the water " took the slightest tint from the colouring matter of the " blood, the *turnsol* was permanently reddened.

" The serum and clot were next separately mixed with " the carbonate of baryta, and the mixtures boiled in an " apparatus communicating with the mercurial bath. " Carbonic acid was disengaged, which at 28 barometer " and 10° thermometer of REAUMUR, gave to every 100 " volumes of serum, 18. 1 of gas, and to 100 volumes of " crassamentum, gas, 21. 2.

" The blood therefore" (says M. HERMANN) " con- " tains some free acid, but it remained to be determined, " whether this free acid was not the *carbonic acid* itself. " Equal quantities of the same clot being boiled in the same " apparatus, *without* carbonate of baryta, only 10. 4 of " carbonic acid was disengaged, consequently the acid " existing in the clot is equivalent to 10. 8 carbonic acid.

" To determine the nature of this supposed acid exist- " ing in the blood, the clot was mixed with water and " sulphuric acid, and the filtered liquor distilled. This " experiment would prove that the blood contained com- " binations of acetic acid. In the product of the distilla- " tion, no other acids could be detected than the *carbonic*, " *acetic, muriatic, and phosphoric.* It is therefore evident, " that the free acids are probably the *carbonic* and *acetic.* " Besides, the phenomena afforded by the analysis of the " blood drawn in *Cholera* render the evidence absolute,

" that it is the *acetic* acid which exists in the *free* state in " standard or healthy blood."

On the preceding assertions and experiments I deem it necessary to offer a few observations, the principal object of which is, to disprove M. HERMANN's statements, by the consideration of his own analysis, and by direct experiments instituted by myself, in order to decide the question at issue.

In the first place, according to the concurrent testimony of all the organic chemists who have examined the blood, either prior or subsequently to M. HERMANN's analysis, free alkali or an alkaline carbonate, is recognised as an ingredient of standard serum. BERZELIUS, PROUT, THACKRAH, MARCET, DAVY, &c., all allow this fact. It is consequently evident that *serum* cannot contain any *free* acid, unless perhaps an excess of the *carbonic*, which scarcely interferes with the alkaline reaction of the carbonate of soda. *In a state of disease*, for example, in rheumatism, it is not improbable that an access of *acetic*, or more probably of *lactic* acid, may occur; as is shown to a certain extent by the acid odour of the cutaneous secretion. The same remark applies also to hectic fever, and to some forms of the calculous diathesis.

2. It must have been remarked, that M. HERMANN's proofs are almost exclusively of an indirect description, and relate to the quantity of *carbonic* acid obtained from the blood, *when boiled with the carbonate of baryta*. The subsequent facts will, I presume, demonstrate the fallacies to which such a mode of investigation is exposed. The avidity with which blood attracts carbonic acid from the atmospheric air, is well known to all animal chemists—the rapidity also with which blood generates the same gas, when in a state of incipient putrefaction, is equally notorious: yet with these facts before him, M. HERMANN

ventures to advance an opinion on the quantity of this gas obtained by *boiling* undiluted blood, which had previously been exposed to the air for twenty-four hours. Again, let any one attempt to *boil undiluted* blood, and what will he obtain? At least a dozen of new products, *carbonic acid, carbonate of ammonia, prussic acid, &c.*, the results of destructive distillation. As well, therefore, might M. HERMANN include the prussic acid among the ingredients of healthy blood, as the acetic, at least as far as the experiments hitherto examined denote.

3. M. HERMANN, to avoid all sources of fallacy, adds carbonate of baryta to one specimen of blood, boils this, and compares the resulting quantity of carbonic acid, with that obtained from a specimen boiled without any carbonate of baryta. The quantity of gas procured in the former case being greater than in the latter. M. HERMANN receives the fact as strong testimony in support of his opinion. Both the experiments are, however, altogether inconclusive, inasmuch as the enormous quantity of carbonic acid obtained where no carbonate of baryta was employed, affords abundant and most convincing proof, either that putrefaction had set in in the blood examined, or that the temperature at which the distillation was conducted, had given rise to the generation of new products; or finally, that the absorption of carbonic acid from the air had proceeded to an unusual extent. Even Dr. REID CLANNY, who theorizes especially on the presence of carbonic acid in venous blood, and on the functions he supposes it therein to fulfil, does not believe that more than 1 cubic inch of carbonic acid exists in 16 $\frac{1}{3}$ of healthy blood. M. HERMANN's experiments, if admitted, would prove the presence of one tenth of its volume of carbonic acid in a given specimen of blood.

M. HERMANN, in order to determine directly the

nature of the supposed free acid, boils the diluted coagulum with dilute sulphuric acid, and obtains *carbonic*, *acetic*, *muriatic*, and *phosphoric* acids. With respect to this alleged result, he could not have obtained *phosphoric* acid, as it is not volatile when thus distilled; secondly, he necessarily should have procured *sulphurous* acid, of which he makes no mention; thirdly, he does not inform us how he proved the presence of the *acetic* acid in the mixture; and lastly, even though it were obtained, the existence of *free* acetic acid in the blood examined, would not thus be demonstrated, inasmuch as the blood not unfrequently contains *saline combinations* of the acetic acid (or of the *lactic*, an acid of closely similar properties), which the sulphuric acid must have decomposed, and thus artificially set free the acetic acid obtained.

But to satisfy my mind directly on the matter, I submitted to examination five specimens of blood, drawn from perfectly healthy individuals. I allowed the blood to separate into serum and coagulum in *close* vessels; I diluted the serum largely with distilled water, and brought the mixture to the boiling point in a distilling apparatus. The product in the receiver was perfectly neutral, did not affect test papers of any description, was rendered alkaline by the addition of a single drop of a dilute solution of soda, and when subsequently evaporated to dryness, left a minute saline residuum, containing not one particle of any combination of acetic acid.

The clot was similarly treated, and with the same results.

Again, a paper having been published about three months since, by M. ORFILA, in the *Annales d'Hygeine publique*, on "*poisoning by* ACETIC *acid*," I instituted several experiments to ascertain the minutest quantity of that acid which I could detect in the blood, and I found

that a quantity so small as to do little more than neutralize the natural alkali of the serum, could be satisfactorily recovered from any proportion of blood by the processes I have just described, and which I would follow exactly in any medico-legal inquiry.

Taking the preceding facts deliberately into consideration, I believe I am fully justified in stating, that M. HERMANN has failed to establish the existence of *free* ACETIC acid, as an ingredient of healthy blood; and consequently that he is not entitled to assume his analysis as a standard for the estimation of the alterations in that fluid, effected by, or producing, diseased actions in the general system.

The remaining ingredients in the questionable class of the component parts of the blood, are, *carbonic acid*, FREE *carbon*, *manganese*, *silica*, and *copper*.

I need not remind the medical members of the *Central Board of Health*, of the controversies so long maintained regarding the presence or absence of *carbonic acid* in the blood; or that upon the decision of the question apparently hinged the fate of many of the brilliant speculations devised to account for the phenomena of respiration. The credit of deciding the dispute *by actual experiments*, and proving the presence of the acid, should be divided between Dr. CLANNY of *Sunderland*, Dr. STEVENS of *Santa Cruz*, and Mr. MITCHELL of *Philadelphia;* but I feel bound also to state, that the experiments of Dr. CLANNY do not appear to me completely free from sources of error. —I have, notwithstanding, satisfied myself thoroughly of the presence of the carbonic acid in venous blood, by an experiment founded on the actions discovered by DUTROCHET, to which he has applied the designation of "*endosmose*" and "*exosmose*"—actions which GRAHAM, FAUST, and MITCHELL, have demonstrated to take place between aërial as well as inelastic fluids. But to this

experiment, unexceptionable as I am inclined to regard it, I do not consider it necessary to allude more minutely in the present report.

It is perfectly impossible that we can investigate correctly many of the phenomena of respiration or circulation in the normal state, or that we can correctly study those diseases like *the malignant Cholera*, in which these functions are evidently influenced, until we have clear and definite conceptions of the *extent* of our *certain knowledge* of the origin or mode of generation of this *carbonic acid* of the *venous blood*. Neglecting this inquiry we must inevitably be led astray by the plausible and probably fallacious hypotheses with which we are so copiously supplied by the premature speculations of our physiological chemists.

Opinions on the source and function of this carbonic acid may be conveniently referred to three leading varieties. According to the first it is considered to originate in the lungs by the combination of *free* carbon with oxygen, and to be thence absorbed into the system; into this hypothesis, however, I will not enter upon any discussion, taking it for granted that the medical members of the Central Board, and the rest of my professional brethren who may subsequently peruse this Report, are too well acquainted with the researches of Dr. EDWARDS and other standard physiologists, to need my repetition of the arguments, or re-statement of the facts, by which they overturned this opinion.

The second hypothesis, devised to account for the source of this acid, is novel as well as ingenious in its nature. It is that advanced by Dr. CLANNY, who supposes the carbonic acid to be derived from active "*assimilation*" alone.*

* See Lecture on Typhus Fever, by Dr. CLANNY, "*Lancet*," Vol. ii. An. 1828-29, pages 450 and 773; and Letter from Dr. CLANNY, "*Lancet*," Vol. i. An. 1829-30, page 59.

With this view I regret I cannot coincide, for, before it can be admitted, Dr. CLANNY is bound to show the presence of carbonic acid in the thoracic duct* or in the chyle. The proof required is, I admit, difficult, if not impossible, to be obtained, in an experimental point of view; but the rules of strict ratiocination will not be satisfied with less connected evidence.

The third opinion regarding the carbonic acid of the venous blood is one which I myself would venture to throw out, and which constitutes a kind of patch-work, composed of the ideas of EDWARDS, STEVENS, CRAWFORD, HUSSENBRATZ, and LA GRANGE. I believe it to arise principally from the process by which the *detritus* of the system is removed. The carbon and hydrogen of the renovated or changing parts, thus combine with oxygen, supplied them through the intermediate agency of the colouring matter of the blood, which, perhaps with the co-operation of the saline matter, acts as the vehicle in which the oxygen is conveyed from the air cells of the lungs. Carbonic acid and water are thus formed,—the arterial blood merges into the venous kind, and is returned to the pulmonary circulation. Here, by an action of aërial "*endosmose*" and "*exosmose*," the carbonic acid is exchanged for oxygen,† and the same circle of actions is renewed.

Before this opinion can be admitted, I frankly allow

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* Much, of course, depends upon the sense in which Dr. CLANNY employs the term "assimilation." I take it for granted that he means digestion or chylification.

† Dr. CLANNY, in his lecture, states that oxygen never can be taken into the circulation, *because a horse is killed when atmospheric air is blown into his veins!* Dr. CLANNY forgets that atmospheric air is not oxygen, and that while the blood in respiration absorbs and dissolves minute quantities of the latter alone, inflation of the veins introduces an enormously large quantity of an insoluble gas, which must prove instantaneously fatal by its mechanical effects.

that the proximate nature and ultimate constitution of the sanguineous colouring matter must be much more minutely ascertained. It is not necessary, as some have imagined, that arterial blood should be found to contain more oxygen than venous blood; for the oxygen is not absent in the latter, it has only changed from the colouring matter, to the condition of carbonic acid or water. It would, however, be essential to show that the arterial colouring matter contained more oxygen than that of the venous blood. That such is actually the case is rendered highly probable by the experiments of M. F. MICHAELIS, recorded in his *Inaugural Dissertation*,* presented last year to the *University of Berlin*. According to his ultimate analysis, the colouring matter of venous blood contains much less oxygen than that of arterial blood.

Reviewing, then, the whole of the preceding statements and observations, it becomes manifest that while we must admit the existence of carbonic acid in venous blood, we are not entitled to speak of its origin or function in any positive terms; consequently that its absence in a state of disease cannot be assumed as a cause of the deranged condition, and at most can only be regarded as an effect of very minor importance.

I now arrive at the consideration of the next asserted element; namely, FREE *carbon*; the substance to which so many theorists have ascribed, and still attribute, the black colour of venous blood.

Fondly as this idea has been cherished by anatomical physiologists, no satisfactory experimental proof has hitherto been afforded of the existence of carbon in the blood, in any other form than that of a binary, ternary, or even quaternary combination. No mention of any such

* In 12mo. Parthenopolitanis, Formis Burshkianis.

ingredient occurs in the tables of analytic results given by BERZELIUS, MARCET, DENIS, or LECANU; but within the past week I find, to my surprise, that Dr. CLANNY has, in a table of the results of a comparative analysis of a specimen of blood drawn from a healthy person, and of another taken from an individual labouring under malignant cholera, not only enumerated FREE *carbon* as an ingredient in both cases, but even declares the *quantity* to have been 32° in the healthy and 66° in the morbid example. I am exceedingly anxious to learn by what direct mode of analysis this extraordinary result was obtained, but until the experiments are published in full and satisfactory detail, I am not warranted in recognising the existence of the principle in question, and I am compelled to receive with caution all reasoning founded on its alleged presence in healthy or its preponderance in morbid blood.

Of the remaining substances, described by some writers as constituents of the blood, namely, *copper*, *manganese*, and *silica*, a very cursory notice will be sufficient. The existence of infinitessimally small traces of copper in various organic matters, was first pointed out by MEISNER. A few months since, a French chemist, M. SARZEAU, in repeating and extending MEISNER'S experiments, conceived that he detected it in blood. Being engaged at the time in a series of medico-legal analysis, I also repeated the experiments; and notwithstanding every precaution to avoid any possible fallacy, I did obtain traces of copper from 12 ounces of the blood of a gentleman labouring under pulmonary apoplexy. I have not since had leisure to extend the inquiry, but in the mean time I thought it right to publish the results,* as

* On the Existence of Traces of Copper in organic Matters; by W. B. O'SHAUGHNESSY, M. D.—*Lancet*.

a warning to medico-legal chemists analysing organic matters supposed to contain cupreous poisons.

I do not therefore believe that copper should be for the present inserted in the tables of our analysis of the blood. I trust the readiness with which I make this concession will exonerate me from the imputation of entertaining any illiberal feeling towards the experimentalists, whose analyses I have had, or shall have, occasion to criticise in this Report.

The remarks I have made are equally applicable to the only remaining substances, viz. *silica* and *manganese*, both of which, as well as *alumina*, can be separated in traces, from very large quantities of blood.

I have thus enumerated and commented on the various substances asserted, or proved to exist, in standard human blood; and I have canvassed such points in the origin and supposed functions of some of the ingredients, as I found to be concerned in the chemical pathology of the disease I shall presently inquire into. I now proceed to the important subject of the comparative quantities of the constituent parts of the blood contained in a given portion of that fluid. To this I would take the liberty of especially soliciting the attention of the "Central Board," inasmuch as it will be seen, in my analysis of *blood* drawn from individuals labouring under Cholera, that an absence, or deficiency of some of the ingredients, and a remarkable deviation from the normal proportion in others, constitute *(as far as my observations extend)* the most remarkable features in the chemical phenomena of the disease—thus fully verifying the prediction of M. Lecanu. "Il peut fort bien arriver que le sang "dans les cas pathologiques différe plutôt par un change- "ment dans la proportion relative de ses principes que "par la présence de quelque principe accidentel."

It is from the analysis of this distinguished chemist that I shall take my standard for the comparison between the healthy and morbid conditions. I accord M. LECANU this preference, because his processes are perfectly free from fallacy, and incomparably the best ever devised for the quantitative and qualitative analysis of the blood. They have moreover been repeated on a multitude of individuals of both sexes, of every age, stature, temperament, &c. and have been rewarded by the gold medal of the *Académie de Médecine* of Paris, conferred at the public sitting of the 12th July 1831.*

STANDARD ANALYSIS OF THE BLOOD.

BY MONSIEUR L. R. LECANU.

Journal de Pharmacie, No. IX. September 1831, p. 502.		
Water	780.145	785.590
Fibrine	2.100	3.565
Albumen	65.090	69.415
Fatty matter:		
a. crysalline	2.430	4.300
b. oily	1.310	2.270
Colouring matter	133.00	119.626
Extractive *soluble in alcohol water*	1.790	1.920
Albuminate of soda	1.265	2.010
Muriate of soda, Muriate of potassa, Carbonate, Phosphate, Sulphate } Alkaline	8.370	7.304
Carbonate of lime, Carbonate of magnesia, Phosphate of lime, Phosphate of magnesia, Phosphate of iron	2.100	1.414
Loss	2.400	2.586
Total	1000.000	1000.000

* *Nouvelles Recherches sur le sang par* M. L. R. LECANU.—*Journal de Pharmacie*, &c. ix et x. *Septembre* et *Octobre*, 1831.

With the preceding table, I feel it necessary to compare that published a few days since, by Dr. CLANNY, as *his* standard of healthy blood, and according to which he estimates the supposed abnormal condition of that fluid in the malignant Cholera.

STANDARD ANALYSIS BY DR. CLANNY.

(*Lancet*), *January*, 1831.

Water	756
Coagulated albumen	121
Colouring matter	59
Free carbon	32
Fibrine pressed and dried	18
Muriates of soda and potassa, Carbonates of soda and potassa, animal extract	14
Total	1000

I subjoin the mode of analysis recommended by Dr. CLANNY, and which I presume he employed in this instance, and the perusal of which will, I believe, afford an explanation to a certain extent of the discrepancies between his table and that by M. LECANU.

DR. CLANNY'S MODE OF ANALYSIS.

"After the separation of the blood into serum and "crassamentum, the serum was coagulated at a well-"regulated temperature, and being cut into small pieces, "was placed upon a perforated Wedgewood funnel, and "the serosity drained off; besides which, the coagulated "albumen was washed most carefully with warm water,

" and the washing added to the serosity. The coagulated " albumen was weighed; the serosity and the washings " of the coagulated albumen were placed in a Wedgewood " capsule and evaporated, and the salts which were left " were evaporated and weighed. The crassamentum " from which the serum was poured was weighed, and " the fibrine separated from the colouring matter was col- " lected in a fine linen bag, through which a current of " distilled water was passed. The fibrine was pressed in " a press of my own construction, and when all the water " was pressed out it was weighed. The solution of the " colouring matter which passed through the linen bag " was evaporated, and the colouring matter weighed."

A mere glance at the preceding mode of analysis, will show that the quantity of water cannot be correctly stated, for, in the first place, the serum was only coagulated, not perfectly dried as it should be, until all its hygrometric moisture had been expelled. Again, the quantity of albumen is exaggerated proportionately to the deficiency of water. The fatty matter being altogether overlooked by Dr. CLANNY, tends also to increase the apparent proportion of albumen. Again, the 32 parts of FREE *carbon* spoken of, should, according to our present state of knowledge, be added to the colouring matter; and unless the fibrine was dried in some other and more perfect manner than that recommended, and I presume practised, by Dr. CLANNY, at least three fourths of the apparent weight should be added to the amount of water. Again, no note is made of the comparative amount of the extractive and saline matters, no incineration having been performed.

The discrepancies in the amount of water, colouring matter, fibrine, albumen, and saline matter, in the standard analyses of M. LECANU and Dr. CLANNY, thus receive an obvious explanation.

But two more points remain for consideration in this department of the report. 1st. The amount of the normal proportion of serum to crassamentum. 2nd. The normal specific gravity of the serum. Of these, the former, though extremely liable to variation, and though ever appreciable with much difficulty, may be safely reckoned at 57 serum to 43 crassamentum, and the specific gravity of the serum may be estimated at an average of 1—28.

SECTION II.

ON THE CHEMICAL PATHOLOGY OF THE MALIGNANT CHOLERA.

Review of the Analyses performed by Dr. Turnbul Christie, *Professor* Hermann, M. Foy, *and* Dr. Clanny.—*Results obtained by the Reporter, &c. &c.*

As far as I am aware, but four analyses of the blood, dejected and vomited matters, &c. &c., obtained from patients labouring under malignant cholera, have yet been performed. I give them in the order in which they have been published, and I shall comment upon them in the same rotation.

The first I have met with relates to the dejected matters alone, and is published in the work the title of which I subjoin. * I shall not, however, dwell minutely on the experiments Dr. Christie has narrated, as they merely indicate the existence of albumen in a state of solution in the fluid parts, and of fibrine in the solid flaky matter of these peculiar evacuations. As far as Dr. Christie's experiments extend, (and they were conducted on too limited a scale to be of much importance,) the results coincide, in some respects, with those obtained in my own investigations.

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* Observations on the Nature and Treatment of Cholera, and on the Pathology of Mucous Membranes, by Alexander Turnbul Christie, M. D.

The next analysis in order of publication is in greater detail, and extends to the blood, bile, vomited and dejected matters, and the urine. I allude to that by Professor HERMANN of Moscow, being the sequel of the paper containing the peculiar statements regarding the presence of the acetic acid in standard blood, on which I have already animadverted. M. HERMANN's investigation of the chemical pathology of cholera, having acquired much notoriety, I am obliged, in this report, to examine into his conclusions with some minuteness.

M. HERMANN commences with the analysis of the blood, which fluid he describes as of extraordinary viscid consistence and dark colour. In one analysis, he states that the blood examined, was drawn from a man who died of the disease in a few hours, after having suffered from violent vomiting. The blood afforded the proportion of 40 serum to 60 of crassamentum, thereby indicating a considerable deficiency in the amount of water it should naturally contain. The serum was highly *alkaline*, its sp. gr. 1.36. The crassamentum on the other hand is described as having been sensibly *acid*, and duly impregnated with *carbonic* acid, ascertained by ebullition with carbonate of baryta.

This alkaline re-action of the serum, M. HERMANN describes as a constant phenomenon in cholera, after vomiting has occurred, and he further states, that it disappears as the patient approaches convalescence. The blood, when microscopically examined, was found to have lost its globular structure, and to contain numerous membranous shreds, the apparent *debris* of its former healthy arrangement.

I am truly unwilling, while addressing the Central Board of Health, to depart in the least degree from the utmost courtesy of expression towards those writers and

experimentalists whose statements I am necessitated to allude to; but candour and strict justice compel me here to express my conviction, that the majority of the observations now quoted are not altogether correct. The evidence in support of this charge, rests principally on the following data.

In the first place, the author asserts an impossibility, when he describes an alkaline serum and acid crassamentum to have been obtained from the same blood. Secondly, he views the reported alkalescence of the serum as an *aberration* from the normal state, whereas the concurrent testimony of all other observers, fully proves it to be the invariable accompaniment of health. Thirdly, M. HERMANN'S microscopical observations are contradicted (as will subsequently be seen) by those made in *Newcastle* by Dr. WHITE and myself. Lastly, M. H. makes no allusion whatever to the absence or presence of saline substances in the blood he examined. It follows from these remarks, that the only unquestionable part of his analysis of this fluid, is that which relates to its deficiency in *water*.

The next branch of M. HERMANN'S analysis treats of the vomited matter, which he states to contain a small quantity of FREE *acetic acid*, *mucus*, *water*, *ozmazome*, *acetate of soda*, *muriates* and *phosphates of lime*, and *magnesia;* no bile, albumen, or caseum, was present, but in subsequent analyses, he states that he detected traces of *butyric acid*.

I shall not offer any lengthened comment on the preceding statement, because, according to the version in the *Journal de Chimie Medicale*, the matters analysed were allowed to stand for some days before they were examined; decomposition must consequently have commenced, and the results rendered altogether fallacious, especially with respect to the presence of *acetic* acid.

M. HERMANN next proceeds to the examination of the *aqueous* dejections, and describes them to be possessed of the characteristic fœtid odour of alvine evacuations. In this respect I believe that M. H. is at variance with the majority of observers, at least in the specimens I have myself examined, they were perfectly free from smell before putrefaction set in. This event was, besides, of very rapid occurrence, and productive of very disagreeable odour.

The constituent parts of the dejected matters, M. HERMANN states to be *free acetic acid*, and traces of *butyric acid*, a small quantity of the peculiar principles of the *bile*, traces of *albumen*, *mucus*, *ozmazome*, *acetates*, *butyrates*, and lastly, *water*. He further describes the acidity of the dejections as being a universal fact, and sometimes being so great as to corrode the steel instruments used in dissection.

All these statements are, I regret to add, still more at variance with my own observations than were those to which I have already adverted. It will be seen in the details of my experiments, that not only were the dejections *not acid*, *but* that they were *strongly alkaline*, and contained a considerable quantity of carbonate of soda. Again I must say, that I scarcely think it possible to pronounce with certainty on the presence of minute quantities of butyric acid, or the butyrates in a mixture like that passed by the cholera patient. In the next place, I feel satisfied that the principles of bile are seldom or never present in the *true* dejection. Lastly, M. HERMANN entirely omits the analysis of the curdy or flaky matter which occurs so remarkably in these evacuations.

On his analyses of the bile and urine I shall offer no remark, because they contain no important fact, and I have had no opportunity of myself examining these secretions.

The preceding experiments, however, afford their ingenious author a frame-work for the construction of some peculiar hypotheses relative to the therapeutic indications dependent on the chemical pathology of this disorder. He condemns venesection in cases where evacuations have already occurred, "*because the blood has already lost too much of its fluid parts.*" He regrets that the proposal of M. JŒNICHEN, for the injection of warm water into the veins, was not put into practice; and he seems to think this the last source of hope in the violent forms of the malady. M. HERMANN also derives some physiological and pathological deductions from his experiments. Having shown, as he imagines, that healthy blood contains acetic acid, and having further, as he believes, demonstrated the absence of a great portion of that free acid in blood drawn in cholera, he next reminds us that acetic acid is a solvent of fibrine, and he then at once arrives at the positive conclusion that "*the thickening of the blood in cholera is produced by the absence of* ACETIC ACID, fibrine and albumen being insoluble in water, but soluble in that acid." As additional proof of this opinion he dwells with emphasis on the pathological observations of M. JŒNICHEN, who, in numerous dissections in this malady, found the internal surfaces of the heart, its valves, and muscular columns, interwoven with strings of solid fibrine. Connecting all these supposed facts with the strongly acetous character which he assigns to the intestinal excretion, he finally concludes that this acid originally entered into the composition of the blood, that through its deficiency the blood has become preternaturally dense, and that the "*immediate cause of death is the thickening of the blood and the suspension of the circulation thus occasioned.*"

The only remark I shall at present offer on the fore-

going deduction, is the expression of my gratification that M. HERMANN should have arrived at so rational a conclusion, from the erroneous premises pursued through the entire inquiry.

The next analysis to which I have to invite the attention of the Board, is that recently instituted by the reporter in the districts afflicted with cholera in the North of England. The broad results of these inquiries have been communicated to the public in *The Lancet* and *Medical Gazette* of the 31st December, 1831, in a brief letter, copies of which I have the honour to lay before the Board.

These results were based on the examination, first, of three excellent specimens of blood drawn in the malignant cholera; secondly, of one drawn in a case of ordinary though violent feculent and bilious diarrhœa; and thirdly, of the dejected matters of eminently characteristic appearance (a portion of which is already before the Board), passed by one of the patients who died of malignant cholera, and whose blood was also examined. After fruitless attempts to obtain *materiel* for analysis in Sunderland, I repaired to Newcastle-upon-Tyne, where the disinterested kindness and scientific zeal of the medical gentlemen of that town at once supplied me with abundant specimens. The first opportunity occurred in the case an outline of which I subjoin, and which was witnessed by Dr. MACWHIRTER, of Newcastle; Messrs. THACKRAH, NAILOR, and BREARY, of Leeds; Mr. NESHAM, surgeon, of Newcastle; several other gentlemen, and myself.

CASE AND ANALYSIS.

No. 1.—*Malignant Cholera.*

Mrs. BARRAS, æt. 39, widow, of excellent habits, good general health, in rather comfortable circumstances, and residing in a lane adjoining the river, Sandgate, Newcastle, was seized with cramps, epigastric pain, and giddiness, at about 10 P. M., on the night of the 17th December. According to the statement of her female friends she soon after became deadly cold, her countenance altered to the expression of death, she lost all voluntary power, and her eyes became deeply sunk in their orbits. In this state she is reported to have spent the night, having vomited and been purged about six times. A more precise history could not be obtained.

At 9 A. M. on the 18th, she was seen by Mr. NESHAM, by whose direction a vein was opened in the arm. The blood issued difficultly, was at first viscid and very dark, but it subsequently assumed a more lively colour. The blood was placed aside, in a small basin, and at 11 A. M. (when I arrived) had separated into a loose, bulky, crassamentum, and transparent, but unusually viscid, serum. The crassamentum having been disturbed and broken up by some of the gentlemen present, the serum only was removed for analysis. The patient passed no urine from the commencement of the attack until its fatal termination on the night of the 18th December. For the details of the mode in which this specimen of serum was analysed, I beg leave to refer the Board to the Appendix. I shall here only insert the results, giving at the same time, in one column of the table, LECANU's analysis of healthy serum as a standard for comparison.

SPECIFIC GRAVITIES.

Cholera Serum from Mrs. BARRAS.	Healthy Serum (LECANU).
1.041	1.028

No. I.

COMPARATIVE ANALYSIS OF STANDARD AND CHOLERA SERUM.

INGREDIENTS.	Standard of Lecanu.	Serum from Mrs. Barras.	REMARKS.
Water	906.00	854.00	
Albumen	78.00	133.00	
Urea	0. 0	1.40	
Organic matter:			
Soluble in alcohol and water	1.69	*4.80	* Number in 2nd column embraces both the *organic matter* and *albumenate of soda*.
Albumen combined with soda	2.10		
Fatty matter:			
a. crystalline.....	1.20	†1.40	† 2nd column includes both the *oily* and *crystalline* principles.
b. oily	1. 0		
Muriate of soda....	6. 0	4.00	
Muriate of potassa..			
Sub. carb. of potassa	2.10	‡0.00	‡ The 0 in 2nd column refers to the carbonate of soda alone.
Phosphate of potassa			
Sulphate of potassa			
Carbonate of lime	0.91	0.00	
Carb. of magnesia			
Phosphate of lime		§1.60	§ The 2nd column here embraces
Phos. of magnesia			*Phosphates of soda*
Phosphate of iron			*Sulphates of soda*
			Phosphates of lime } 1,60
Loss.....	1.00	0.80	Phosphates of magnesia and iron }
Total	1000.00	1000.00	

The tabular view thus afforded of the results in this analysis, is interesting in several points of consideration. It shows in the first place, the absence of a large proportion of water; secondly, the corresponding preponderance of albumen; thirdly, the presence of urea; fourth, the absence of the alkaline carbonate; fifth, a great *deficiency* of saline materials. I should also add, that the experiments which gave origin to this table, were witnessed by Dr. WHITE of Newcastle; Mr. HAWTHORN, the nephew of Dr. MACWHIRTER, was present when the urea was obtained, and Dr. MACWHIRTER himself on entering the room a few minutes after, having been requested to smell the watch glass on which the nitrate of urea was deposited, without any previous knowledge of its nature, compared the odour to that of a "*pot de chambre.*"

CASE AND ANALYSIS.

No. 2.—*Violent Bilious Diarrhœa.*

The second case which afforded me materials for continuing the inquiry, occurred in the practice of Mr. HAWTHORN: it was one of very severe bilious and feculent diarrhœa with vomiting. I gladly embraced the opportunity of examining this case, as I was exceedingly anxious to ascertain whether the loss of water in the blood was an invariable sequel of profuse intestinal evacuations. The result shows the singular fact, that the fluid parts of the blood were, on the other hand, *increased* in this instance. The consideration of this phenomenon, and of some other peculiarities of the dejections in the malignant cholera, to which I shall advert in the sequel, will probably furnish at no distant period a diagnostic

test of chemical accuracy, by which the indigenous cholera of Great Britain to which we have ever been accustomed, can be distinguished from the disease of the same name with which the present winter has, for the first time, made us too familiar.

In the annexed table, M. LECANU's standard, the analysis of cholera serum No. 1, and of the serum from Mr. HAWTHORN's patient, are all placed in juxta-position. The crassamentum was analysed as well as the serum, but was found perfectly normal in its ingredients and their proportions.—See *Appendix*.

No. II.

COMPARATIVE ANALYSIS OF SERUM IN HEALTH, MALIGNANT CHOLERA, AND BILIOUS DIARRHŒA.

INGREDIENTS.	Healthy Standard of Lecanu.	Malignant Cholera. Mrs. Barras.	Bilious Diarrhœa. Mr. Hawthorn's.	REMARKS.
Water	906.00	854.00	921.75	
Albumen	78.00	133.00	61.85	
Urea	0.00	1.40	0.00	
Organic matter:				
Soluble in alcohol and water	1.69	4.80	*5.20	* The 5.20, third column, includes organic matter, and albumenate of soda.
Albumen combined with soda..	2.10			
Fatty matter:				† The 2d and 3rd column include both *oily* and *crystalline* principles.
a. crystalline ...	1.20	†1.40	†1.90	
b. oily	1.00			
Muriate of soda.. Muriate of potassa	6.00	4.00	‡5.00	‡ The 5.00, 3rd col. embrace the muriate of soda and potassa, the carbonate, phosphate, & sulphate of soda.
Carbonate of soda	.....	0.00	§2.30	
Phosphate of soda	2.10			
Sulphate of soda				
Carbonate of lime				§ The 2.30, 3rd col. include the carbonate, phosphate, and sulphate of soda.
Carb. of magnesia				
Phosphate of lime	0.91	1.60	‖1.10	
Phos. of magnesia				
Phosphate of iron				
Loss.....	1.00	0.80	0.90	‖ The 1.10, 3rd column, embraces all the earthy salts.
Total	1000.00	1000.00	1000.00	

Sp. gr. of the serum 1.28. *Reddened turmeric paper.*

In the preceding case, therefore, the quantity of water *exceeded* the normal standard; that of the albumen was below the average proportion; the salts were normal in quantity, and the serum preserved its alkaline character.

CASE AND ANALYSIS.

No. 3.—*Intensely Malignant Cholera.*

The case I now proceed to describe is in several respects the most important of the entire. It occurred in the Cholera Hospital, Sandgate, Newcastle, on the 21st December; and was witnessed by Drs. Gibson, White, Morries, several other medical gentlemen, and myself.

The patient, James Dewar, aged 39, a sailor, of good habits and colossal frame, was attacked at 6 A. M., on board the smack *Nimble*, of Leith, with spasms, cramps, purging, and vomiting of the peculiar fluid, which I need not describe. At 9 A. M. he was brought to the Cholera Hospital. Soon after his arrival he passed a copious characteristic dejection, which was preserved for analysis. He was then given a little ammonia by the House Surgeon, Mr. Glenton. Another evacuation followed in about ten minutes, and was also set apart.

When I saw this patient at 11 A. M. he was perfectly pulseless and cold, his face contracted and of a tarnished silvery or fishy aspect; he suffered horribly from cramps, and uttered cries like one shouting through a barrel. It was on the whole the worst case but one that I witnessed during my stay in the infected districts.

A little after 11 A. M. some blood was taken from an

orifice in both arms, and about 8 ounces, dark in colour and viscid in consistence, were with some difficulty obtained; the patient writhing about his bed so constantly that the blood could not be preserved from contact with the atmosphere. This blood was also set aside for analysis.

Before leaving the ward, and in presence of Drs. Gibson, White, Morries, and others, I tested the dejections with yellow turmeric paper, and that passed before the ammonia was given, changed the colour of the paper to a deep permanent brown. I should add, it had been ascertained that he had taken no medicines previous to his admission into the Hospital.

Notwithstanding the most assiduous attention and active treatment Dewar died the same day at 4 P. M.

The blood was allowed to rest for three hours, and in the interval I proceeded with the analysis of the dejected matters. Dr. White, a gentleman highly skilled in natural philosophy, and to whom I feel much indebted for the patient attention which he bestowed on my investigations, was present at almost the entire of the experiments instituted in the case I now describe.

The serum and coagulum, when carefully separated and weighed, were in the proportion of 43 serum and 57 crassamentum, by which an extraordinary loss in the aqueous portion of the blood was pointed out. The crassamentum was then examined in the manner described in the Appendix, and found normal in the proportion of its ingredients, so that the addition of a certain quantity of water would have restored it to its original density, proportions, &c. For this reason I have not included the crassamentum in the tabular statement of the analysis.

The serum was of the sp. gr. of 1 . 45, and was devoid of the least action on litmus or turmeric papers. I need

not dwell on the other steps in the examination, as they did not differ in the least degree from the proceeding adopted in the case of Mrs. BARRAS, fully detailed in the Appendix No. 1. The whole analysis in DEWAR's case was completed the day after his death. The general results can scarcely fail to prove interesting, especially when collated with all the preceding Tables, as in the adjoining form.

No. III.

COMPARATIVE ANALYSIS OF SERUM IN HEALTH, MALIGNANT CHOLERA, AND BILIOUS DIARRHŒA.

INGREDIENTS.	Healthy Standard of Lecanu.	Malignant Cholera. Mrs. Barras	Bilious Diarrhœa. Mr. Hawthorn's.	Malignant Cholera. Dewar.	REMARKS.
	0.	1.	2.	3.	
Water	906.00	854.00	921.00	866.80	
Albumen	78.00	133.00	61.85	124.0	
Urea	0.00	1.40	0.00	0.00	
Organic Matter, soluble in Alcohol and Water	1.69	*4.80	*5.20	*4.00	* Embrace the organic matter and albumen of soda.
Albumen combined with Soda	2.20				
Fatty matter:					
Crystalline	1.20	1.40	1.90	1.23	
Oily	1.0				
Muriat. Soda	6.00	4.00	5.00	2.17	
Muriat. Potassa .					
Carbonate of Soda	.....	0.00			
Phosphate of Soda	2.10		2.30	0.5	
Sulphate of Soda					
Carb. Lime					
Carb. Magnesia.					
Phosp. Lime ...	0.91	1.60	1.10	0.70	
Phosp. Magnesia					
Phosp. Iron					
Loss	1.00	0.60	0.90	1.5	
Total	1000.00	1000.00	1000.00	1000.00	

I will not trespass on the attention of the Board by detailing the results obtained in the fourth case, because, in the first place, they in every essential particular corresponded with those in the case of DEWAR; and, secondly, the specimen was furnished me by a gentleman who forgot to forward me the history of the patient.

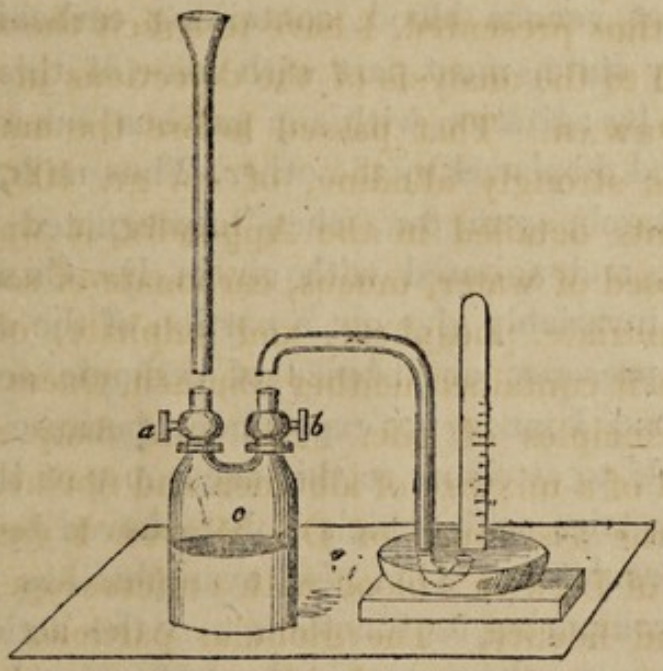
Again, in a fifth specimen, for which I am indebted to Dr. MOLISON of Edinburgh, I found the aqueous materials diminished, and the saline intermediate, as in the case of Mrs. BARRAS. I have also been informed by a gentleman who witnessed Dr. MOLISON's case, that, like that of Mrs. BARRAS, it was of a protracted type.

Before I proceed to offer any further commentary on the facts thus presented, I have to solicit the attention of the Board to the analysis of the dejections in the case of JAMES DEWAR. That passed before the ammonia was given was strongly alkaline, of sp. gr. 1007. By the experiments detailed in the Appendix, it was found to be composed of water, mucus, carbonate of soda, and the acetate, muriate, phosphate, and sulphate, of the same base. But it contained neither albumen, traces of caseum, or the principles of bile. The solid flaky matter was composed of a mixture of albumen and fibrine.

With the assistance of Dr. WHITE I examined the structure of DEWAR's blood with a microscope of singular power and fidelity. The globular particles were quite distinct, no membranous shreds were perceptible, and the fluid did not differ in any apparent degree, as to its physical organisation, from that drawn from an individual in robust health. When freely exposed to the air, it in all cases rapidly assumed a fine scarlet colour; and the same effect was produced with equal celerity by the addition of the usual salts deficient in the abnormal blood.

That this blood had a strong affinity for oxygen, or, I

should rather say, a great power for decomposing atmospheric air, I found by the repetition of the experiment devised by Dr. CHRISTISON, and described in his paper "*On the mutual action of blood and air*" (*Ed. Med. and Surg. Journal*, Jan. 1831). Four ounces of blood, previously deprived of its fibrine by agitation with pieces of lead, were introduced into a double-necked bottle, the tubulures of which were armed with stop-cocks, in the manner represented in the drawing. From the tubulure *b* a bent tube proceeded, which opened in the mercurial bath, under a small graduated jar. A funnel tube was adapted to the tubulure *a*, and both these tubes were only adjusted to the apparatus as occasion required.



The stop-cocks being shut, and the tubes removed, the blood was agitated with the portion of atmospheric air in *c*; the tubes were then arranged, and the stop-cock *b* opened. The mercury rose immediately into the bent tube, inducting an absorption of the air *c*. The end of the bent tube was then placed under the graduated jar, and the air *c* expelled into the jar by pouring water into

the bottle through the funnel *a*. The residual air was thus found to contain a large proportion of carbonic acid.

Satisfactory proof was thus obtained that the blood was competent to discharge the respiratory processes connected with the absorption of oxygen and discharge of carbonic acid. What these actions are, and what is the precise interchange of elements which takes place, I have shown in the first part of this Report that it would be premature to speculate, our knowledge being yet imperfect on several essential links in the chain of inquiry.

I did not attempt to ascertain the *exact* amount of the absorption of air or evolution of carbonic acid in the preceding experiment, because it is open to one source of fallacy—that venous blood, containing carbonic acid, in the healthy state, must part with some of this acid mechanically, by agitation with any gaseous substance, one being indeed displaced by the other. Thus milk, solutions of sugar, starch, gum, &c., when impregnated with carbonic acid, and agitated with air in Dr. CHRISTISON'S apparatus, invariably give out a portion of the acid.

On the presence or absence of carbonic acid in the cholera blood I made no experiment, because I cannot bring myself to attribute to this agent any of the almost magic properties with which it is endowed by some ingenious speculators. One, for example, has gone the length of comparing its functions to "the action of the spiral spring on the balance-wheel of a watch." I, on the other, regard the presence of carbonic acid as a *sign* of the perfect accomplishment of our respiratory and circulatory functions; its absence as an *effect* low down in the scale of causation, proceeding from derangement of these essential actions. Indeed, instead of adopting the metaphor just quoted, I would rather select for my illustration another part of the time-piece, and compare the

carbonic acid to the hands by whose motions is estimated the regularity or aberration of the internal machinery.

In short, wherever *protracted asphyxia* occurs, carbonic acid must be deficient in venous blood, no matter what theory of sanguification we embrace. That such a state of protracted asphyxia forms a primary feature in the *cold blue* cholera, every competent observer must admit, and all the symptoms of the disease unanimously proclaim.

The summary of my experiments may therefore be described as denoting a great but variable deficiency of water in the blood in four malignant cholera cases—a total absence of carbonate of soda in two—its occurrence in an almost infinitesimally small proportion in one; and a remarkable diminution of the other saline ingredients. Again, in the dejections passed by one of the patients whose blood was analysed, we find preponderance of alkali, and we recover the other saline matters deficient in the blood. Lastly, the microscopic structure of the blood, and its capability of aëration, are shown to be preserved.

What light any of these conclusions may cast upon the chemical pathology of cholera (taken as a *universal fact*) I shall not stop to investigate, until I lay before the Board an outline of the last analysis, the results of which have been published; namely, that by Dr. CLANNY of *Sunderland*, to whose opinions I have already more than once alluded. For the convenience of the Board, I have formed a note of these results* and the peculiar specula-

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* ANALYSIS OF THE BLOOD.

This blood, on applying the tongue to it, had no taste, nor any particular smell; I also tasted it again, sometime after it had been drawn. I afterwards tasted the colouring matter, the coagulated albumen, and the fibrin, but in them I found no taste, nor any smell. It contained no gases of any description; was black as tar. I followed the plan which I have taken the liberty of recommending my professional brethren to pursue in the investi-

tions thereupon constructed; but however ingenious they may appear, I cannot detain the Board by investigating their merits, since they rest upon "facts" not yet demonstrated, and since the analysis of the abnormal blood,

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gation of typhus and other diseases, and which I intend henceforth, for many reasons, to pursue.

I shall give the results of an analysis of the blood of one of our sailors, which was taken in October last. This blood contained one cubic inch of carbonic acid in the 16 ozs. which were taken.

	The Sailor.	Elliot Todd.
Water	756	644
Albumen, coagulated	121	31
Colouring matter	59	253
Free carbon	32	66
Fibrine, pressed and dried	18	6
Muriates of soda and potassa, carbonate of soda and animal extractive	14	0
	1000	1000

In continuation of the above communication I beg to say, that having completed my investigation of the blood, and *workable* fluids of epidemic cholera patients, I am satisfied that the first impulse which the system receives is from the atmosphere, through the medium of the respiratory system; but whether it be from the distemperature of the atmosphere only, or from contagious miasmata floating in the air, or from both, is not at this moment the question.

From direct experiments I am assured that this impulse causes, in the first instance, the circulating blood to part with its free carbonic acid by an excited respiratory process. When the blood is in this state, the circulation necessarily becomes languid, for the carbonic acid is to the blood in its circulation what the spiral spring is to the balance wheel of the watch. The free carbon of the blood is gradually restrained in its progress into the air-cells of the lungs, as well as throughout the whole circulation; for at this time not only is the whole circulation retarded, but the surface of the body also is chilled, or collapsed.—From this time the animal heat, of course, declines, not half the usual quantity of carbonic acid being formed in the air-cells of the lungs. We even find the patient heavy, melancholic, and listless; the blood, leaving the extremes of the circulatory system, distends the large blood-vessels and viscera, and should the patient be worn out by poverty, old age, disease, or drunkenness, he may die from excess of carbon in the blood, by which the coronary arteries of the heart,

if conducted according to Dr. CLANNY's own directions, must have led to fallacious quantitative statements.

Reviewing, then, the whole summary of results obtained by the several experimentalists, we find them *all* coinciding in the deficiency of water, while diminution or absence of saline matter in the blood is admitted by two, while the third (M. HERMANN) overlooked the necessity of prosecuting this part of the inquiry.

In addition to the above, I am happy to say that, in a number of the *Gazette des Hôpitaux*, which has fallen under my notice since this part of my report was commenced, there occurs a letter from *Warsaw*, written during the prevalence of cholera in that capital by M. FOY to Professor MAJENDIE, and detailing some results of ana-

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or the sensorium commune, or both, may be, to use a newly-coined medical phrase, poisoned by his own blood; or if the patient should be rashly raised up, he may die from inanition, as the blood now finds much obstruction in its circulation, particularly when it has to be carried against its now excessive specific gravity, by reason of its scarcely fluid state. When epidemic cholera takes its usual course, the blood-vessels of the coats of the stomach are generally influenced by the diseased blood; sometimes those of the stomach first, sometimes those of the intestines, and often of both at the same time. In some instances I have remarked that such has been the severity of the attack, that blood, if drawn from a vein, was mixed with the substances ejected by vomiting. The serum of the blood is thrown off from that fluid in both instances, and the salts of the serum not only cause this impulse in the extreme branches of the arteries, but also give those acute pains and cramps which are so characteristic of the disease,—in which pains and cramps the inferior and sometimes the superior extremities sympathise—similar in many respects to what we find takes place when, under ordinary circumstances, looseness, or costiveness, produces cramps of the legs, particularly when the individual is warm in bed.

When all the salts, such as are contained in healthy blood, are purged out of the system, the cramps and pains, as a matter of course, leave the body, the patient looks like an animated corpse, and we have, at this time, lymph circulating with the crassamentum, in lieu of serum, as the case of Elliot Todd, detailed in my accompanying letter, testifies.—*Lancet, Jan. 7.*

lyses of the blood and dejected matters, which amply corroborate my own observations.

In the next and last section of my Report I shall inquire into the extent to which the preceding facts entitle us to reason physiologically or therapeutically on the chemical pathology of this malignant disease.

SECTION III.

BRIEF INQUIRY INTO THE EXTENT OF THE PATHOLOGICAL AND THERAPEUTIC CONCLUSIONS DEDUCIBLE FROM THE PRECEDING STATEMENTS.

In order to reason correctly on the indications afforded by a series of asserted facts, it is absolutely necessary, first, that the reality of these facts should be ascertained; and, secondly, that their universality should be determined. Thus alone can we be entitled to regard such facts in the light of *essential causes* or *essential effects*, and to consider them as distinct from any *accidental* phenomena. Again, even though the veracity and universality of the facts in question shall have been satisfactorily established, it will still remain a subject for investigation whether it be to the class of "causes" or to that of "effects" that they are to be referred, and if to the latter, whether they are essentially or intimately connected with the progress of the malady, or whether they are merely the insignificant, though constant, companions of other more important, though less obvious, circumstances.*

Unless we follow this strict system of ratiocination in all inquiries like the present, though we may accidentally

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* Laplace lays it down as an axiom, that in order to prove that any phenomenon is the effect of an alleged cause, it must be proved to be the *inevitable* result of that alleged cause.

stumble upon true conclusions, we can never reach them by a certain path, or by one which subsequent inquirers can ever unhesitatingly and effectually pursue.

The first question, then, for deliberation is, have the *facts* asserted in the analyses I have laid before you been satisfactorily *ascertained?* In reply, I shall only say that the experiments were openly performed, and were witnessed by competent and respectable persons. Moreover, I trust that the diffidence and caution with which I have drawn or may subsequently draw any conclusions from my observations will not detract from the confidence with which I desire them to be regarded.

Assuming then that my facts are accurately stated, the next question is, are these facts *universal;* and if such, are they entitled to be regarded as *essential causes* or *inevitable effects* of the disease?

I am at once prepared to admit that the universality of the facts regarding the deficiency of water and absence of saline matter in the blood is *not proved;* but I at the same time hold that it is rendered more than *probable* by the circumstance of three experimentalists having arrived at nearly the same results on one of these points, viz., the deficiency of water, by very different modes of investigation. Neither do I regard the alkaline state of the dejections as *proved* to be universal, but it is rendered very *probable* by the fact that Dr. Morries, of Edinburgh, has informed me, that Dr. Abbs, of Sunderland, had in numerous cases tested the dejected matter with turmeric paper, and always found them possessed of an alkaline reaction. Moreover, M. Foy has made the same observation at *Warsaw* during the irruption of cholera in that city.

The universality of these facts being thus rendered *probable,* are we, according to the strict rules of induction,

to consider them as causes or effects? If as the latter, are they the *first* effects of the original cause, or should they be placed lower down in the scale, or do they themselves become the proximate causes of any remarkable events in the disease?

That they should not be regarded as primary *causes* is rendered evident by the fact that the blood *per se* not being endowed with an inherent power of locomotion, cannot be altered in *quantity* within the vessel by any internal force, the moving power must proceed from without. Neither, I believe, can its *quality*, while life exists, be impaired by any really intrinsic causes. External means may, however, during life operate powerfully upon it, either by the introduction of substances alien to its constitution, or by the subtraction from or addition to the amount of the ingredients it naturally contains. It is, moreover, rendered evident by pathological facts, to which I need not particularly advert, that the vessels in which the blood is contained, or the nerves distributed on these vessels, influence both the proportionate and the qualitative state of the contained fluid, in a manner which, in the present state of knowledge, we cannot satisfactorily explain, even though aided by the light which the brilliant discoveries of DUTROCHET have shed upon the nature of increased vascular action.

Regarding then, as we must, this alteration of the blood as the *result* of an external impression, we still remain in darkness as to the mode in which that impression is communicated,—of its nature we have more knowledge to a certain extent; every circumstance in the semeiology and anatomical pathology of the malady, the kind of its symptoms, the sequence in which they occur, and "period of incubation" between the application of the primary cause and the appearance of morbid symptoms, all coin-

cide in denoting that an organic poison is the first mover of the consecutive actions.

The absence of water and saline matters being thus regarded as *effects*, are these effects *essentially* connected with the progress or the event of the malady?

In reply, I should observe, that many facts apparently indicate that these effects are *not* essential. Cases, for example, not uncommonly occur, in which death takes place in a very quick period of time, and without the occurrence of alvine evacuations. A most remarkable example of this kind has occurred within the last month at Haddington, North Britain. The case is detailed in the second edition of Mr. BELL'S *Treatise on Cholera*, which I have just received; and it elicits from Mr. BELL the annexed remarks.

"In this case of DUNBAR, what was the cause of the want of serum in the blood? It surely could not have depended on the abstraction of serum by profuse discharges from the stomach and bowels. From the former there was none, from the latter very little. This would seem to countenance the idea that some remarkable change takes place in the constitution of the blood, previous to the invasion of vomiting and purging. I merely hazard this conjecture from the observation of this case, as few cases are treated at so early a period."—Appendix, p. 238.

Some explanation of these singular cases may perhaps be derived from the recollection that there are many outlets for water and saline matter besides the intestinal vessels. I have indeed reason to believe, that in some cases, these ingredients have been eliminated by the kidneys and skin, before the commencement of any very remarkable symptom. An accurate observer has informed me since my arrival from Newcastle, that the cutaneous exhalation is frequently alkaline in this disease. Again,

during the prevalence of the Newcastle irruption, numerous friends of mine as well as myself have experienced *temporary diabetes* and trivial cramps after attendance on Cholera cases. I am at liberty to particularize Mr. MELIN of the 9th Lancers, who was particularly affected in this way, and whose attention was excited by it before I acquainted him with the results of my experiments. In my own case, the urine contained *free* soda. I will not, however, enter any further on this point. I submit it to further investigation with all the other facts and opinions in this report, and I only adduce it here, in order to show that deficiency of water and saline matter may occur as a consequence of the remote cause of Cholera, in the most rapid cases, and without alvine evacuations of any description.

Supposing then that we assume the universal occurrence of this deficiency, are we entitled to regard the loss of water, or of saline matter, or of both, as *essentially* connected with the progress of the malady or the production of the fatal event?

The most obvious manner in which the loss of water could occasion death, is by the physical obstruction it would necessarily occasion in the passage of the blood through the capillary vessels, especially in the pulmonary circulation. The motion of the blood would, therefore, be confined to the great vessels which would become distended to excess—gradual asphyxia should supervene, and death be occasioned with all the phenomena of impeded respiration and circulation. How accurately this description coincides with the events in Cholera, it is unnecessary for me to point out. In short, this mode of death in this disease might at once be admitted, had we previously accurate ideas regarding the precise density of fluid which the capillary system will receive and permit

the passage of, and had we found that the density of Cholera blood exceeded this standard. I regret that my inquiries among some distinguished anatomical friends have failed to procure me any conclusive evidence on this subject.

I shall therefore content myself by expressing my conviction that this deficiency of water in the blood, is, at any rate, in many cases *a powerful adjuvant* in the production of the fatal event. This view of the case as well as the general semeiology of Cholera, are strongly illustrated by the following brief quotation from the chaper on Hyperœmia, in ANDRAL'S splendid work on *Pathological Anatomy*.

"When the mechanical hyperœmia is carried to a "certain extent, other phenomena may arise as its "consequence. Thus the serous portion of the blood "may escape from the over-distended vessels, just as "water or any other liquid transudes through the per-"meable sides of a vessel in which it suffers compres-"sion. * * * *

"* * * * And although these effusions have "really nothing active in their nature, yet are they con-"siderably diminished, and sometimes altogether re-"moved by blood-letting," &c.

Giving thus to probable mechanical hyperœmia as much importance as I think it is entitled to receive in the consideration of this disease, I also cannot, or will not, conceal my conviction that the remote cause of Cholera *may*, and FREQUENTLY DOES, *produce death, independently of this auxiliary, by the unknown agency it exerts on the nervous system.*

Having thus disposed of the state of density of the blood in this disease, I arrive at the next part of the inquiry, namely, the question as to the probable influence of the

absence or diminution of quantity of saline matter on the progress of the malady. To argue correctly on this point, it is necessary to bear in mind the observations I have already made on the supposed functions of these agents, and which seemed to establish that the salts of the blood exercise an important, though certainly not a defined control, on the changes effected from the venous to the arterial states, and the functions thereon dependent. Admitting this proposition (and I cannot see how it can be disputed by any strict logician), it seems evident that, no matter how we reason on the mode in which the salts become separated from the blood in Cholera, whether, as I believe, it is a mere effect *dependent on the transudation of the water in which these salts are naturally dissolved*, we must still take the subject into our most serious consideration, in the institution of our therapeutic measures, should subsequent and more extensive inquiries render the absence of these salts in Cholera blood, *an ascertained and universal fact*.

I should add, that from all I have been able to learn on the subject, the diminution in quantity of saline matter in the blood is not immediately incompatible with life, and that the injury resulting is rather of a chronic character, that is to say, that it takes hours or days for its production; I consequently would not attribute to the absence of the salts any important share in the inducement of sudden death in Cholera, but I am inclined to believe such diminution or absence to be closely connected with the fever stage of the malady, an opinion at the same time suggested and substantiated by Dr. Stevens's experiments on the state of the blood in the yellow fever of Santa Cruz.

THERAPEUTIC CONCLUSIONS.

The commentary I have adjoined to the chemical inquiries, has shown sufficiently, that even though their results were shown to be universal facts, still that their remote *causes* yet remain open for investigation. I shall not consequently attempt to draw any practical indication from premises which, though *perhaps* correct, are nevertheless liable to share the fate of all hypothetical speculations.

The consideration, however, of the presumed *effects* of these causes, recognised in the alteration of the blood, leads to two important therapeutic conclusions, in the event of the universality of these effects being proved by subsequent inquiries.

These indications are;—

First. To restore the blood to its natural *specific gravity.*

Second. To restore its deficient *saline** matters.

The first of these can only be effected by absorption, by imbibition, or by the injection of aqueous fluid into the veins. The same remarks, with sufficiently obvious modifications, apply to the second.

In the milder cases, or preliminary symptoms, *ere yet absorption is impeded,* I would expect much benefit from the injection of copious enemata of warm water into the intestines. It should, however, be remembered, that in *mild* cases the necessity for such dilution is not imme-

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* In order to prevent misconception of my meaning I must again emphatically repeat, that I do not consider this deficiency *essential* to Cholera, but that it occurs as an accidental effect in a vast majority of cases; and that this effect must be obviated before we can accomplish a cure.

diately urgent,* inasmuch as the changes which give rise to the indication are not yet completed. The injections may, however, cut short the progress of the sanguineous alteration, which *may* otherwise supervene.

At the same time that this diluent injection is practised, a consideration of the state of the patient in each individual case will direct the competent practitioner as to the choice of the other remedies: such as stimulants, opiates, external warmth, &c. which may be calculated to re-excite the circulation and promote the required absorption—a function so intimately connected with the state of circulation, that the mobility or inaction of one is almost essentially connected with those properties in the other.†

The tepid water enemata may contain a certain proportion of the neutral salts. It will not be forgotten, however, that in the majority of cases these salts already pre-exist in the intestinal canal.

In the severe cases in which absorption is totally sus-

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* In the preliminary symptoms strictly so called, *astringents* may *prevent the inspissation of the blood* by the alvine discharges. The best astringent I ever knew the effects of, is the following, which saved many lives during the dreadful *dysentery* which prevailed in Edinburgh in 1829, while I was Dr. Alison's clinical assistant. I have strong expectations that it would be also found of decided utility in the Cholera diarrhœa:—

Take of Opium ..... 12 grains.
Acetate of Lead .... 24 grains.
Conserve of Roses.. a sufficient quantity.

Make into a mass and divide into 12 pills, one to be taken as occasion may require.

† Since this Report was drawn up, tepid water enemata have been employed in Newcastle with the best effects. See Dr. Gibson's Report, and Mr. Caton's Letters, in the *Cholera Gazette*, No. 2. In the Cholera of 1688, Sydenham exclusively employed diluents, in order "to dilute the spirituous parts of the blood;" and, he states, with the utmost success.

pended, and when stimulants, however varied or energetic, fail to re-excite the circulation, I would not hesitate to *inject some ounces of warm water into the veins.* I would also, without apprehension, dissolve in that water the mild innocuous salts which nature herself is accustomed to combine with the human blood, and which in Cholera are deficient. Let it be remembered, that if this experiment be not practised, death is inevitably close at hand, and that the proposal does not rest on idle or frivolous opinions. It should also be remembered, that this mode of medication has, in many a desperate disease, been practised with success, and that by some of the most cautious and experienced physicians in the world.

I beg, however, that I may not be misunderstood, so as to be thought to recommend this proceeding indiscriminately. On the other hand, I would deem that practitioner little better than a homicide who would perform the operation without the sanction of a numerous consultation.

With respect to the treatment of the fever stage, I would expect much benefit from the frequently repeated use of the neutral salts by the mouth or by enemata, and dissolved in large quantities of tepid water. I should prefer the subjoined combination,* as it imitates to a certain extent the composition of the materials in which the blood is presumed to be deficient. Besides meeting the chemical indication, these remedies will also assist the cure by their aperient properties, &c.

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* Take of Phosphate of Soda.... ten grains.
Muriate of Soda..... ten grains.
Carbonate of Soda .... five grains.
Sulphate of Soda ..... ten grains.

Dissolve in six ounces of water. The mixture to be repeated every second hour.

While this practice is pursued, I would also obey every local indication, and use cold applications, leeches, &c. according to the symptoms of the case.

I have now completed my statement of facts, explanations, and deductions; and I have little left me to add, further than the expression of my regret that I have been forced to draw so largely on the patience of the Body I have the honour of addressing. My object throughout being to present the Board with a review of all that has hitherto been learned of the chemical pathology of Cholera, it was impossible for me to effect greater condensation without the omission of details essential to the correct understanding of the entire subject.

In conclusion, I deem it my duty to solicit the attention of the Board to the urgent necessity of making further investigations of this description. At best I have but given the clue to the complete pursuit of the inquiry, and I feel satisfied that its extensive repetition would not only lead to an increased knowledge of the malady, but perhaps unravel many a mysterious problem in the functions of life and aberrations of disease. In order to contribute in some degree to the acquisition of the desired knowledge, I have taken the liberty of placing in the Appendix some plain and intelligible manipulatory instructions, by which, in the remotest village, any intelligent person may perform such an analysis of the blood and dejected matters as will suffice to confirm or refute the statements and opinions I have now submitted to your consideration.

I have the honour to be,
Gentlemen,
Your obedient servant,
W. B. O'SHAUGHNESSY,
M.D., &c. &c.

London, 7th January, 1832.

APPENDIX, No. I.

ANALYSIS OF A SPECIMEN OF SERUM TAKEN FROM A PATIENT LABOURING UNDER SEVERE BLUE CHOLERA.

Mrs. Barras, ætat. 39 (see Report), a widow of excellent habits, general good health, and comfortable circumstances, was seized with cramps, epigastric pain, and giddiness, about 10 p. m., on the 17th December. The attendants state that she became deadly cold, her features altered, and her strength overpowered, and that she spent some hours in this condition (having vomited and been purged about six times) during the night. At 9 a. m. a vein was opened at the bend of the elbow; the blood issued with great difficulty, and was at first extremely viscid and dark, but subsequently flowed more freely and brightened in colour.

The blood was placed in a basin, and at half-past 11 a. m. had separated into a loose, bulky, crassamentum, and transparent, but unusually viscid, serum. The crassamentum having been broken and much exposed to the air, the serum alone was removed for analysis.

From the commencement of the attack to several hours after the bleeding the patient passed no urine. The hot-air bath having been assiduously applied during the day, she rallied a little, but sunk and died during the night.

Examination of the Serum.

The serum having been carefully preserved in a stoppered phial, was examined 24 hours after removal. In colour and degree of transparency it differed in no respect from ordinary serum.

1. Its *spec. gravity*, compared to water (1 . 008), was 1,0 . 41.

2. It did not turn turmeric paper brown, or restore the colour of reddened litmus.

3. Five hundred grs. by weight were evaporated in a water

bath, until a small mirror was not dimmed by the discharge of aqueous vapour. A transparent, brittle mass was obtained, weighing 73 grs., indicating a loss of water of 427 grs.

4. The powdered residuum was introduced into a flask and boiled for ten minutes with distilled water. No diminution in *bulk* was sustained, and the mixture was filtered.

Examination of the Filtered Fluid in No. 4.

5. Evaporated to dryness in the water bath, left a brownish deliquescent residuum, weighing 5 grs.

6. Residuum treated with strong boiling alcohol, lost 2 . 50 grs.

7. Alcoholic solution did not affect litmus or turmeric paper.

8. Alcoholic solution evaporated to one-eighth its original bulk and allowed to cool, deposited groups of minute, white, cubic-shaped crystals.

9. The superjacent fluid was cautiously transferred to a watch crystal.

10. The crystals (No. 8) were dissolved in a drop of distilled water, and the drop divided into three parts by a capillary tube, and transferred to three separate watch crystals, marked *a*, *b*, *c*. A minute drop of the *nitrate of silver* caused a white, clotted precipitate in *a*, which redissolved in *ammonia*, and was again precipitated by *nitric acid*. *Chloride of platinum* produced no effect in *b*. *Oxalate of ammonia* no effect in *c*.

11. A very minute crystal (No. 8) was heated on platinum foil, over the spirit-lamp flame. It blackened at first, and then left a white stain, which did not turn turmeric paper brown, and which gave a yellow tinge to the blow-pipe flame.

12. The fluid (No. 9) was diluted with a few drops of distilled water, and a drop of nitric acid added. A white, flaky precipitate was formed.

13. The fluid (12) being separated from the flakes by a capillary tube was transferred to a watch crystal, and evaporated to dryness at a temperature of 100°. A numerous group of radiated crystals was formed, of a strong urinous smell, of yellow colour, soluble in water, and solution reddening litmus, soluble in alcohol, from which it was again crystallised, retaining its urinous odour.

Heated on platinum foil it blackened, and was totally consumed.

14. The flaky precipitate (12) was washed with water, and divided into three minute portions. It was found to be soluble in ammonia, acetic and muriatic acids, and to be totally destructible by heat.

Conclusions.—1 Shows the serum to be deficient in alkali. 10, 11, 12, 13, and 14, denote the presence of muriate of soda, urea, and albumen.

Examination of the Solid Residuum No. 5.

15. The residuum (5), insoluble in alcohol, and weighing 2 . 20 grs., was calcined on platinum foil, and left 2 grs.

16. The residue was dissolved in distilled water, and the solution slightly tinged turmeric paper brown.

17. Solution (16) divided into several drops, previously acidulated with nitric acid; was precipitated by nitrate of baryta, nitrate of silver, and chloride of platinum; by oxalate of ammonia and ammoniaco-phosphate of soda. It gave no blue colour with chloro-ferro-cyanate of potash, and a drop evaporated to dryness, and the residue held on platinum wire in the blow-pipe flame, gave it a fine yellow colour.

Conclusions.—16 indicates traces of alkali; 17 shows the presence of sulphates and muriates of soda, potassa, traces of combinations of lime, and magnesia—no iron.

Examination of the Solid Residuum No. 4.

18. The solid substance remaining on the filter (4) was again dried in a water bath, and then boiled for ten minutes in an excess of strong alcohol, and filtered while warm.

19. The alcoholic solution was clear while hot, but became opaque and turbid as it cooled.

20. Evaporated to dryness in a water bath, left a yellow oleaginous residuum, weighing seven-tenths of a grain, insoluble in water, and totally soluble in sulphuric ether, forming a yellow tincture.

21. The ethereal solution (20), allowed to evaporate sponta-

neously to one-eighth its bulk, deposited numerous crystalline scales, surrounded by a yellow fluid of the consistence of olive oil.

22. The residue (21) washed in cold alcohol, the oily matter was dissolved, and the crystalline scales remained behind.

23. Boiling alcohol dissolved the scales, and again deposited them unchanged. The hot solution did not tinge turmeric paper.

24. When crystallised and drained, the crystals were quite white, fused at a gentle heat, and were totally insoluble in, or unchanged by, caustic potassa. They were decomposed by heat, and left traces of earthy matter, which did not tinge turmeric paper.

25. The oily matter (21) floated on water in globules, was totally soluble in cold alcohol and ether, and, on incineration, left a trace of earthy matter, which did not tinge turmeric paper.

Conclusions.—All these experiments indicate the presence of the crystalline and oily fatty principles of the blood described by Lecanu.

Examination of the Residuum No. 18, *insoluble in Alcohol and Water.*

26. The residuum (18) was again carefully dried in a water bath.

27. Incinerated in a platinum crucible, weighed 0 . 80.

28. Residuum (27) insoluble in alcohol and water, and did not redden turmeric paper.

29. Dissolved completely, without effervescence, in diluted muriatic acid.

30. Muriatic acid solution, divided into several parts, was tested by the following re-agents.

31. The red prussiate, or chloro-ferro-prussiate of potash, gave a faint blueish green precipitate.

32. Ammonia a white precipitate, which was re-dissolved in muriatic acid.

33. The solution (32) formed a copious precipitate with oxalate of ammonia; the precipitate was thrown on a watch crystal, and the fluid separated by a capillary tube, and transferred to another crystal.

34. The precipitate by oxalate of ammonia (33) was calcined on a slip of platinum foil, and the residue stained turmeric paper a deep brown.

35. The fluid (33) was precipitated white by caustic potassa.

Conclusions—26, 28, and 29, show the absence of carbonate of soda, 31 denotes a trace of iron, 32, 33, 34, and 35, concur to show the presence of minute traces of lime and magnesia.

GENERAL CONCLUSIONS.

From the preceding experiments, it is evident that the serum analysed consisted of — in 1000 parts.

Water	854. 00
Albumen	133. 00
Urea	. 40
Fatty matter, *a* oily, *b* crystalline	1. 40
Albumen combined with soda. Organic matter soluble in alcohol and water	4. 80
Muriate of soda and potassa	4. 00
Saline combinations, viz. sulphates, muriates of lime, magnesia, and iron	1. 60
Loss	0. 80
Total	1000. 00

N.B. To save space and prevent confusion to the general reader, I have not alluded to the manipulation and calculations connected with the weighing of each product. Additional reasons for this omission will be found in the sequel.

Errata in Table No. I, page 30, in the column containing the analysis of Mrs. Barras' blood, *for* urea 1. 40, *read* urea 0. 40.

..... In Table II. p. 33, *for* urea 1. 40, *read* urea 0. 40.

..... In Table III. p. 37, in column headed "Lecanu," *for* albumen combined with soda, 2. 20, *read* 2. 10. In column "Barras," *for* urea 1. 40, *read* 0. 40. In column "Bilious Diarrhœa," *for* water 921. 00," *read* water 921. 75.

I regret the occurrence of these errors, which were occasioned by the supervision of the proof having been entrusted to another person.

APPENDIX, No. II.

MODE OF ANALYSIS OF THE "TARRY" BLOOD NOT SEPARABLE INTO SERUM AND CRASSAMENTUM.

In the analysis of cholera blood, when the disease attains its maximum pitch of severity, it will generally happen that no serum whatever will be separated, and that the chemical examination will relate solely to a dense clotted "tarry" mixture, consisting of dense though fluid albumen, fibrine in a semi-coagulated state, colouring matter, and small traces of the usual saline ingredients. To estimate the quantities of these ingredients, I have found the following to be the most effectual method. It is nearly the same with that pursued by M. Lecanu, in his examination of the crassamentum in the normal state.

1. A thousand grains by weight (or a corresponding quantity) of the blood to be examined, should be divided into two equal portions by weight.

2. One portion should be dried in a water bath until a mirror is no longer dimmed by the exhalation of watery vapour. The dried residuum weighed,—the weight multiplied by two, and then subtracted from 1000, the resulting number shows the quantity of *water* contained in 1000 parts of the blood.

3. The second part (1) should be diluted with thrice its bulk of distilled water accurately weighed, introduced into a stoppered phial containing a few pieces of thick gold or platinum wire twisted into a loose net-work. The whole should be agitated for about a quarter of an hour. The fibrine will then be found collected in white cords around the wires, and is to be separated, dried in a water-bath, and weighed. The agitation is to be continued as long

as any separation of fibrine takes place. The amount of fibrine thus obtained, multiplied by two, gives the quantity in 1000 parts of blood.

4. The quantity of fatty matter is recognised by treating the residuum (2), with boiling alcohol, filtering, and evaporating the filtered fluid to one eighth of its volume. The weight of the resulting oily matter multiplied by two, shows the amount in 1000 parts of blood. In some cases, however, we may expect to meet urea in the blood analysed, the alcoholic solution in this experiment may therefore be divided into two parts before it is weighed; and nitric acid added to one, if urea be present, a confused crystallisation of nitrate of urea will occur mixed with flakes of albumen. The quantitative analysis is very simple, by which the precise weight of the urea, and albumen, and of fatty matter, may be distinctly ascertained. The process is, however, unnecessary in the present inquiry.

Having thus determined the amount of water, fibrine, fatty matter, and urea, we have next to ascertain the quantity of salts, albumen, and colouring matter.

4. The residuum (1) should therefore be heated to redness, in small portions at a time, on a little tray of platinum foil, over the spirit-lamp flame. The amount of the residuum adding to that of the calcination of the alcoholic extract (3), expresses, when multiplied by two, the quantity of salts in 1000 parts of blood.

5. The mode of qualitative analysis of these salts may be learned from the perusal of the analysis in the case of BARRAS, Appendix, No. I.

6. Deducting the quantity of saline matter obtained by incinerating the dried residuum (2), and also the quantity of fibrine as estimated by experiment (3), from the weight before incineration —the remainder multiplied by two gives the quantity of albumen and colouring matter in 1000 parts.

7. In the analysis of "the thick tarry" cholera blood, it is a matter of great difficulty to appreciate separately the quantities of colouring matter and albumen. I have generally followed M. LECANU'S method, which, although I am aware it is not precisely exact, nevertheless will always afford the truth within two or

three grains per 1000, and moreover possesses the great advantage of being very expeditious in its indications. M. LECANU's method consists in diluting freely the blood with water (8), and then cautiously adding the sub-acetate of lead. The albumen is thus precipitated in combination with oxide of lead, and the colouring matter remains in solution. The fluid is next to be filtered—a little sulphate of soda is to be dropped in order to precipitate the lead remaining in solution, and the mixture then to be again filtered, and the filtered fluid heated to 212 Fahr. The colouring matter is coagulated, and its weight may be ascertained by filtering and drying in the water bath. The amount multiplied by two gives the quantity in 1000 parts.

Having thus ascertained the amount of water, fibrine, colouring matter, and saline matter in 1000 parts, the difference to 1000 shows the quantity of albumen and completes the analysis.

APPENDIX, No. III.

ANALYSIS OF ALVINE DEJECTIONS.

CASE I.—JAMES DEWAR. See Report.

The appearance of these evacuations was most characteristic. They were perfectly colourless, very fluid, and containing numerous white flakes, which subsided when the fluid was allowed to stand. The fluid having been filtered *through fine gauze*, the solid flakes and the liquid part were separately examined.

Examination of Liquid part.—Sp. gr. 1 .008.

1. Turmeric paper immersed in the fluid was strongly reddened, and reddened litmus paper restored to its blue tint.

2. Did not coagulate by boiling, or by the addition of the mineral acids.

3. Evaporated to one-eighth at a boiling temperature, still reddened turmeric paper, and turned reddened litmus blue.

4. Before evaporation, was precipitated by corrosive sublimate and ferro-cyanate of potassa.

5. Alcohol added to the reduced fluid, occasioned a precipitate of white flakes, soluble in water.

6. Two hundred and fifty grains of fluid evaporated to dryness left 1 . 1—45 grs.

7. Boiling alcohol took up from the residuum No. 6, 0 . 30 grs.

8. Alcoholic solution (7) evaporated to dryness, blackened when exposed to heat, and left a crystalline residue of the muriate and acetate of soda.

9. Permuriate of iron occasioned no red colour in the fluid (1).

10. 1000 grs. of the fluid were evaporated to dryness and cal-

cined on a platinum tray: $5\frac{1}{10}$ grs. of saline matter were obtained. The saline residue reddened turmeric paper powerfully; effervesced with acetic acid. Sulphuric acid caused the evolution of fumes of acetic acid.

11. No precipitate in its aqueous solution by tartaric acid, chloride of platinum, or perchloric acid.

12. The aqueous solution precipitated nitrate of silver white: precipitate insoluble in nitric acid, and soluble in ammonia.

Conclusions.

These experiments indicate that the liquid portion of this evacuation consisted of water, mucus, albumen in small quantity, and muriate, acetate, and carbonate of soda. It contained no caseum, sulpho-cyanate of potassa, or bile.

Examination of Solid Portion.

Insoluble in alcohol and water; totally destructible by red heat. Readily soluble in acetic acid and alkalies, and not precipitated from its alkaline solution by acetic acid. *Not reddened when treated by* STRONG SULPHURIC ACID, *and cautiously warmed.* Precipitated copiously, of a yellow colour, by ferro-cyanate of potash, from its solution in acetic acid.

From these experiments, I am inclined to conclude that the flaky matter in *this case* was principally composed of *fibrine.* All practical animal chemists are aware of the extreme difficulty of distinguishing this substance, in the solid state, from coagulated albumen. The only mode of distinction I have ever been satisfied with, is the experiment with *sulphuric acid*, which, when properly conducted, affords strong evidence of the nature of the substance under examination.

We thus find that the dejections in the case of DEWAR contained the most remarkable of the principles deficient in his blood.*

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* For additional facts relative to the constitution of these dejections see Appendix, No. 6.

APPENDIX No. IV.

MODE OF ANALYSIS OF CRASSAMENTUM.

WHENEVER the separation of the blood into serum and crassamentum has been complete, and where the analysis of the former has been performed, the constituent parts of the latter can be ascertained with great precision by M. LECANU's method; viz.—

"Divide 1000 parts of the clot into two parts—dry one, and estimate the loss—wash the other to ascertain the quantity of fibrine.* As the water given off from the first, exists therein *in the state of serum*, by subtracting the due proportion of solid matter (which the previous analysis of the serum has shown to be contained in 1000 parts thereof) in the interposed serum, from the weight of the dried clot, the difference affords exactly the weight of the fibrine and colouring matter together. Subtracting again from this the weight of fibrine, determined by another experiment, the amount of the colouring matter is accurately obtained. Incineration, finally, of a given weight of the crassamentum, gives the quantity of saline matter."—*Journal de Pharmacie, Sept.* 1831.

This was the process followed in the case of DEWAR. See Appendix, No. II. par. 3.

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* The manipulation I recommend in this part of the process increases considerably in the accuracy of its results.

APPENDIX, No. V.

PLAIN DIRECTIONS FOR THE ANALYSIS OF THE BLOOD AND ALVINE EVACUATIONS IN CHOLERA.

ACCORDING to the facts detailed in this Report, the principal objects in prosecuting the analysis of these fluids are, 1. To ascertain the quantity of water in the blood; 2. The quantity of saline matters in that fluid; and, 3. The presence or absence of the principles of blood in the alvine dejections.

To accomplish the first, a very simple plan is sufficient. Take 1000 grs. of the serum or blood to be examined, place it in a common bowl, resting on a saucepan containing water; place the saucepan on an ordinary fire, covering the bowl with a piece of gauze. Continue the boiling until a bit of mirror glass is not dimmed by being held over the bowl. The water is thus expelled, and its amount ascertained by weighing the residuum. According to LECANU'S analysis, *blood* contains from 780 to 785 parts of water per 1000; *serum* from 900 to 906. These estimations may be taken as the standard of comparison between the normal blood and that drawn from the cholera patient. The quantity of salts is best ascertained by drying the blood or serum completely in the water bath now described, and incinerating the residuum, in small portions at a time, on a little tray made of folded platinum foil. A spirit-lamp is the best mode of applying the necessary heat, and the substance should be kept at a red heat until it ceases to be black on cooling.

The normal quantity of salts in the human blood, is from 11 to 12 grains per 1000.

It is unnecessary to enter into a qualitative examination of the

saline mass thus obtained. It is only essential to touch it with a little moistened turmeric test paper, or with litmus paper previously reddened by an acid, in order to find if any alkaline carbonate be present. A red stain on the turmeric, and a blue on the litmus test papers, sufficiently prove the presence of the alkalies or their carbonates.

The alvine dejections may be simply examined by the immersion of these test papers—by the boiling of their fluid parts in a Florence flask, and the addition of the prussiate of potash. The evidence of alkalescence with the former, and coagulation by either of the latter, is sufficient to show their nature. Those who have leisure or inclination for further experimental details, may follow the processes adopted in the examination of the saline parts of the blood. See Appendix, No. I.

APPENDIX, No. VI.

CONSTITUTION OF THE BLOOD AND ALVINE EVACUATIONS OF THE DISEASE NOW PREVAILING IN LONDON.

Analyses by MM. ROSE *and* WITTFLOCK, *of* BERLIN, *of the same Fluids in the Disease recently prevailing in that city. Identity of Chemical Pathology of Cholera in* NEWCASTLE, LONDON, *and* BERLIN. *Consequent identity of Cholera in all these places.*

SINCE the preceding Report and Appendix were submitted to the Central Board of Health (7th January, 1832), I have received the *Gazette Médicale de Paris,* of the 14th January, containing a most important article on the chemical pathology of the malignant cholera.

The article alluded to is an epitome of the results obtained by the celebrated ROSE and by M. WITTFLOCK, in an extensive series of analyses conducted at Berlin. These results, I feel much gratified in stating, correspond entirely with mine, as the subsequent translation of the passage in the *Gazette Médicale* will sufficiently denote.

"MM. ROSE and WITTFLOCK have communicated to us the result of their experiments on the blood of persons labouring under cholera. Despite of all the exactitude of their researches, they could not find the acid character of the blood which M. HERMANN asserted to exist.

"They have observed, that when the blood contained in the right ventricle of the heart of the cholera patient is dried with great care, 30 parts per cent. of solid matter are invariably obtained;

while in the state of health the blood only affords 21 per cent. This morbid proportion has been constantly found, as well in the blood of children as of old persons; neither did sex occasion any difference of results.

"The *serum* of the blood of a young man, æt. 20, who died of intense cholera, had the sp. gr. of 1.447, and afforded, when dried, 16 parts per cent. of solid matter. In a young woman, in good health, MM. Rose and Wittflock found the sp. gr. 1.28, and that the serum only contained $9\frac{1}{2}$ p. c. of solid matter.

"The dejections were strongly alkaline, and contained albumen. These experiments, frequently repeated at the cholera hospitals of Berlin, have invariably been attended with the same results."

The identity of the chemical pathology of the cholera of Berlin and Newcastle being thus fully proved, I was naturally anxious for an opportunity of examining into the state of the blood and the alvine dejections in the disease now prevailing in London. I felt that my Report could not be complete, did it not comprise this important feature; and I also felt that the results of my experiments would throw great light on, and perhaps altogether decide, the professional controversies, which the irruption of the disease in London has unhappily occasioned.

I have now completed the analyses of four specimens of blood, drawn from persons labouring under this disease in its severest forms, and of eighteen examples of the peculiar dejections, obtained from patients in different quarters of London.

The results correspond so perfectly in every particular with those of the Newcastle and Berlin analyses, that to describe them would be but the repetition of the same terms.

I have also availed myself of the interval which has elapsed between my return from the North of England and the publication of this Report, to examine still further, whether in ordinary diarrhœa the blood experiences this particular change, and whether the dejections ever present the same properties in any other disease or condition of the system.

In all the cases of ordinary diarrhœa I had an opportunity of examining, amounting to seven in number, the blood preserved its normal proportions.

Of more than 100 cases of ordinary diarrhœa, the dejections presented no single property of the cholera evacuation.

Artificially I found in one experiment, that the administration of a powerful saline cathartic to a person while fasting, can produce the transudation of part of the constituents of the blood, rendering the dejections similar to the cholera character in kind, but widely different in the *degree* and quantity of the transudation.

I am therefore entitled to conclude, that the exudation of the colourless part of the blood constitutes one of the chief diagnostic characters of the malignant cholera.

That the inspissation of the blood is essential to its pathology.

That these conditions being alike in the diseases of BERLIN, NEWCASTLE, *and* LONDON, *the diseases in these three situations are entirely the same*, and lastly,

That in the fluidity, alkalescence, and albuminous nature of the dejections, we have the means of forming a certain and chemical diagnosis between this disease and others, with which on a general consideration of ordinary symptoms, it may be confounded. The chemical re-agents for the examination of these conditions are, 1st. Turmeric paper, which is turned brown by the dejections. 2nd. Boiling in a glass tube or flask, by which the albumen is coagulated. It is necessary to state that these conditions are, of course, more remarkable in the early period of the disease. When the diarrhœa has persisted for some hours, the alkaline character is frequently lost, from the simple fact of all the alkali of the blood having been passed away. The quantity of albumen also declines for a parallel reason, and requires the test of ferro-cyanate of potash for its detection.

In conclusion, I have only to state that one of our most eminent British chemists (Dr. TURNER, of the University of London) has also made an analysis of a specimen of blood, and one of the peculiar evacuations, and that his results are quite in accordance with those of MM. ROSE, WITTFLOCK, and myself.

THE END.

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