CONTRIBUTIONS
TO
MILITARY AND STATE MEDICINE

MARTIN

FIRST VOLUME

CONTRIBUTIONS

TO

MILITARY AND STATE MEDICINE

BY

JOHN MARTIN

SURGEON, ARMY MEDICAL DEPARTMENT

FIRST VOLUME

MEDICINA LITERIS

LONDON
J. & A. CHURCHILL, NEW BURLINGTON STREET
1881

TO

The Memory

OF

EDMUND ALEXANDER PARKES,

M.D., F.R.S., F.R.C.P.

PROFESSOR OF MILITARY HYGIENE

IN THE

ARMY MEDICAL SCHOOL,

THE FIRST VOLUME OF THIS SERIES

Is Dedicated

BY

THE AUTHOR.

THE EFFECTS

OF

HEALTH AND DISEASE

ON

MILITARY AND NAVAL OPERATIONS.

PREFACE.

I AM induced to write the following pages, in competition for the Medal founded by the Statistical Society in memory of the philanthropist John Howard; being for many years impressed with reverence and admiration for his virtues and labours. Circumstances have brought me much into contact with the "homes and haunts" of Howard, especially Watcombe and Cardington, the latter of which was associated with both the happiest and most painful passages of his life.

But, more particularly do I write on the subject of this Essay, as a military medical officer, whose special business it is to inquire into the interactions between each other of Disease and Warfare: and, as surgeon to one of the largest military prisons in India, where my attention has been much directed to those subjects with reference to which Howard laboured and wrote.

At Cardington, it was my duty, some years ago, to visit much among the poor and helpless; and, even now, it is not difficult to trace among such the effects of the philanthropist's influence. "It seems to have been the capital object of his ambition, that the poor in his village should be the most orderly in their manners, the neatest in their persons and habitations, and possessed of the greatest share of the comforts of life, that could be met with in any part of England. And as it was his disposition to carry everything he undertook to the greatest pitch of perfection, so he spared no pains or expense to effect his purpose. He began by building a number of neat cottages on his estate, annexing to each a little land for a garden and other conveniences. * * * He was careful to furnish his tenants with employment, to assist them in sickness and distress, and to educate their children. In order to preserve their morals, he made it a condition that they should regularly attend their several places of worship, and abstain from public-houses, and from such amusements as he thought pernicious; and he secured their compliance with his rules by making them tenants at will." *— "Cardington, which seemed at one time to contain the abodes of poverty and wretchedness, soon became one of the neatest villages in the kingdom; exhibiting all the pleasing appearances of competency and content, the natural rewards of industry and virtue." †

In the adjoining village of Willington not a single public-house exists (or did not in 1873) for the sale of any intoxicating liquor.

There are few spots of retirement in rural England more refreshing, as a retreat from the turmoil of the world, than Cardington, the favourite home of Howard. The orderly neatness of its cottages, the pleasing appearance, generally of prosperity, but always of happiness, of their inhabitants; its church, with its monumental brasses (the finest, perhaps, in Bedfordshire, a county particularly rich in these interesting relics), in which, although a dissenter, Howard so frequently worshipped; and where if one takes the trouble to inquire, are to be found so many mementoes of himself or his family, from the monumental tablets on its walls to the old faded crimson velvet cloth on its communion table—the peaceful dignity of the fine old trees, which, overarching the road, form one long shady avenue for the village; but, above all, the spot which more than any recalls recollections of virtue and universal benevolence.—" The still silence of this shady grove was his most favourite resort; and in its mossy path he spent many a solitary hour in devising (and many a social one when devised, in

* "A View of Howard's Character and Public Services," by Dr. Aikin, pp. 27–29.
† *Universal Magazine*, vol. lxxxvi.

communicating to his friends) those glorious schemes of benevolence, which will never cease to impart to every spot his footsteps are known to have traversed on so merciful an errand a charm more powerful than, without the magic influence of some such genius of the place, can dwell in Nature's loveliest or sublimest scenes." *

It will be found that in the consideration of the effects of Health and Disease on the Operations of War in the following pages, the remarks generally apply equally to Military and Naval Warfare. Yet, in some particulars, the military side of the question is more fully discussed, being that with which I am most familiar.

Except where otherwise stated, the tables and figures are compiled from information obtained from the Parliamentary Reports of the Army Medical Department.

NOTE.—The references to Howard's Experiences made in this Essay are principally taken from "The Life of John Howard, with Comments on his Character and Philanthropic Labours," by the Rev. J. Field, M.A. (London: Longmans & Co., 1850), or extracts from other publications quoted in that work.

* Brown's "Memoirs of Howard," p. 29.

CONTENTS.

CHAPTER I.

CHAPTER II.

CHAPTER III.

THE EFFECTS

OF

HEALTH AND DISEASE

ON

MILITARY AND NAVAL OPERATIONS.*

CHAPTER I.

PRELIMINARY OBSERVATIONS.

THE term *operations*, is usually applied to those duties which are performed by the Army and Navy on active service. The preparations for war, the entry on war, and actual warfare, differ in many respects from the duties performed by soldiers and sailors during peace.

The monotony of the usual drills and parades, the routine of barrack life and of garrison duty, are replaced by the stirring incidents of rapid movement and encampment; the excitement of emergency and surprise; the recklessness of imminent danger; the hope of victory, and the depression of defeat; the calls on skill and bravery in assault and defence; the enthusiasm of success, and the daring of despair. The magnitude of the operations necessary in warfare—and particularly modern warfare—demands the co-operation of considerable numbers of men. It is a fact known since the dawn of history that, where large masses of human beings are collected for any purpose, there disease springs up in their midst. This results from the operation of many natural agencies. The larger the number assembled, the greater is the probability that some bring with them the germs or agents of disease propagation; or even assuming that all come healthy, and without the

* Awarded the Howard medal and prize of the Statistical Society of London.

means of propagating disease, the results of aggregation combine in various ways to originate disease *de novo.* Besides the effects of aggregation, the circumstances under which men on active service (especially soldiers) are placed, and the varied and arduous duties they have to perform, tend both to the origination and reproduction of disease, and the development of latent morbid tendencies. In the case of men thus engaged, constant action and reaction, after the manner of cause and effect, exist between their duties and physical states. The conditions of their service produce disease, and disease reacts upon their service.

It is a fact patent to the most casual observer that the vicissitudes and duties of campaigns produce results deleterious to health, and certain to engender disease. It is a fact no less true, although not so apparent to superficial notice, and not receiving so much attention from the general public, that the physical conditions of soldiers and sailors operate no less surely on the duties they have to perform, and the success or failure of the operations on which they are employed.

The first point which comes prominently into notice, in studying this question, is the great decrease in our times, as compared with former times, in the influence exerted by health and disease on the operations of war. The brevity of modern campaigns; the celerity of transport of men and munitions of war; the rapidity with which, in engagements, sick and wounded are removed to permanent hospitals at the base of operations; the effective surgical aid possible in the first line of assistance, resulting from the increased distance at which modern fire is effective; the loose order in which troops now engage, and the cover available in modern tactics, tend to this end. Besides this, the increased range of small arms to a great extent diminishes the influence and weakens the action of cavalry.

Not only is exposure to incidental disease less, but the actual effect of action is reduced in fatality.

This is an indirect result of improved firearms. The old smoothbore of short range, and inaccurate aim, and slow loading, required close quarters to do any work; our present accurate rifles sighted to 1,000 yards, with their rapidity of loading, render it necessary to engage at long distances in loose order, instead of the old massed columns, and under any available cover. Within the past twenty years tactics have undergone a complete change;

and the result is much less loss from wounds and disease than heretofore. This is a historical fact. The effect of improved firearms is evident even in the battles of the latter half of the last century.

At Talavera sick and wounded was 1-8th of the strength engaged, at Austerlitz 1-7th, at Prague and Jena 1-6th, at Friedland and Waterloo 1-5th, at Marengo 1-4th, at Leipsic the French loss equalled 1-3rd, at Preussisch-Eylau the loss exceeded 1-3rd, and at Zorndorf, where 82,000 combatants engaged, some 65,000, or nearly 4-5ths of that strength, were left dead upon the field.

In this period, which comprises from 1809 back to 1758, the regular gradation in the diminution of mortality from military action is striking.

In recent battles we find the same results of modern tactics. At Solferino, where rifled firearms were first used, 1-11th of the engaged strength fell; and at Koniggratz (breechloaders) only 1-15th.

It seems, however, as if, in this trial of skill between weapons and tactics, the former were tending to outstrip the latter; for at Worth the loss was 1-21st of the strength, at Gravelotte 1-12th, and at Sedan 1-10th. The same result of changes in tactics resulting from increased range of fire, is true of loss in naval operations, but not to so great an extent. We see plainly, then, from these results, taken with modern rapidity of transport and communication, the efforts of public and private charity, and philanthropic societies, the advancements made in military medicine and surgery, and the application of hygienic science to the amelioration of the sanitary condition of armies, that the influence of health and disease on military and naval operations has been reduced to a very great extent. We find that disease likely to influence warfare has itself been absolutely diminished of late years, and also that as a consequence of the changes which have been necessitated in the operations of war—transport, strategy, and tactics—these latter are now less likely to be affected by the physical conditions of those engaged on them, than they were formerly.

To economize space, the reader is referred to the prefixed Table of Contents for information regarding the manner in which this subject is divided and treated of in the following pages.

CHAPTER II.

"Battles are dangerous operations, and a trifle may lose them."
WELLINGTON.

MILITARY AND NAVAL OPERATIONS LIABLE TO BE INFLUENCED BY HEALTH AND DISEASE—CONSIDERED WITH REGARD TO THE RESULTS OF SUCH INFLUENCE.

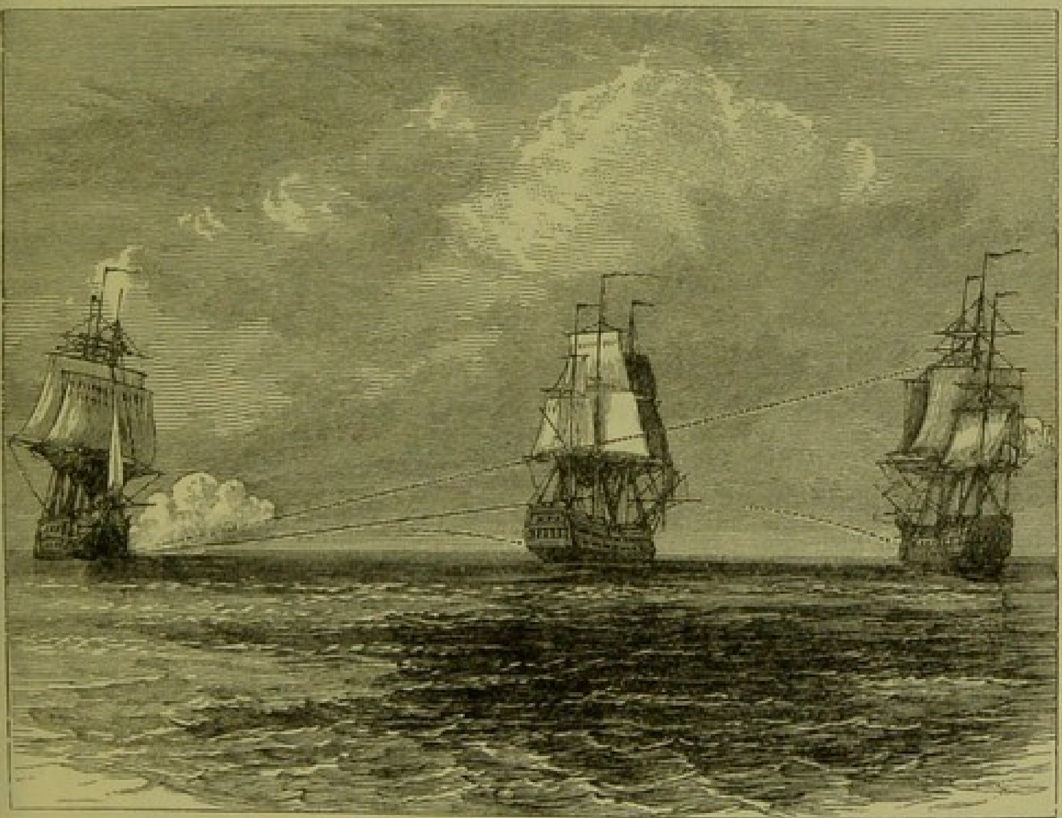
SECTION I.—PREPARATIONS FOR WAR DURING PEACE.

ALTHOUGH the term *operations* in military and naval matters is usually applied to actual warfare, it is considered well to include here and briefly refer to those military and naval duties, which during peace, are a preparation for war.

The physical condition of men engaged on them have a very definite effect on the manner in which those duties are performed; and thus, in this manner, indirectly on the operations of actual war, for which these duties are a preparation.

They may be classed generally as ordinary drills and parades, which instruct in movements and use of weapons, and (in the army) periodical manœuvres in the field, which are intended to accustom troops to the various conditions incidental to warfare. Ordinary drills and parades which consist in instruction and practice in the movements of bodies of men, of course, require to be regularly attended both by officers and men individually, because absence through illness, or other cause, tells very materially on the accuracy with which such movements are performed by the individual. And if one or more men have spent much time in hospital, the effect of their want of practice may often be seen in the movement of the entire corps. Much more is this the case as regards mechanical manœuvres (service of ordnance, &c.), and the use of weapons (bayonet and gun practice, &c.), and of the various instruments of construction, demolition, attack, and defence used in warfare. The efficiency of each individual man, soldier or sailor, with his own weapon, depends on the amount of practice which he has been able to give to the use of it. This amount of practice is much affected by his health, and the effect of its use in action depends on the physical conditions of the man as existing at the time; so much, at least, as this effect depends on him at all.

The accuracy so essential to success in action and which is alone secured by constant unremitting practice, and its dependence on physical condition, will be understood by an illustration from naval gunnery. It will be seen, by reference to the accompanying diagram, that the pointing of naval ordnance is a matter of the greatest importance; the accuracy with which it is performed having a much more direct bearing on the effect than is the case with field ordnance. This would be so even in close action in smooth water, and were the ship's side and the object aimed at steady, and were the distance known.



From Sir Howard Douglas' "Naval Gunnery."

But add to this, that the piece is to be discharged with effect in a swell; that the effect is different if the gun be fired with or against the roll; that a wind in the sails, perhaps, at the same moment counteracts what had been calculated on as the power of the swell; that the moment a vessel is steadiest, that is, when she is in the trough of a sea, is the moment she has the least commanding view and *prise* of her enemy; that when in the most commanding position, she is also in the unsteadiest, namely, on the crest of a wave; and some slight idea may be obtained of the wonderful co-ordinating power between brain, nerve, and muscle requisite for the working of ordnance—judging distance, pointing the gun, and the instant of firing—in naval action.

In the case of military operations, although, perhaps, these do not always require so fine a development and readiness of perception as naval operations, a great amount of practice is requisite to produce efficiency.

This is especially the case with those operations which require skilled labour for their performance; as the duties of the Royal Engineers and Royal Artillery.

Duties performed by infantry, and generally by cavalry, working as they usually do in line or massed columns, although, of course, requiring special knowledge, practice, and aptitude, do not depend so much on conditions of individuals as those performed by the so-called *scientific corps.* But there are other important duties of the soldier, besides those for which he is more especially trained, which are much influenced by his health and physical conditions.

"Whenever the public service may require it soldiers are liable to be employed on working-parties as a duty. Under this head are comprised the levelling of ground in and about camp or quarters, the opening up and making communications whenever necessary, and the ordinary construction and keeping in repair of fences for cavalry, and ranges, butts, &c., for rifle practice. The liability to be so employed must necessarily be enforced on service in the field where manual labour becomes a most important duty, where the bodily exertion of all ranks is required to strengthen positions and ensure the general safety of the army; and when the use of the spade, pickaxe, and barrow is quite as essential as that of the rifle and bayonet." (Queen's Regulations.) The number of men who break down under this fatigue work, as far as my experience goes, is much in excess of the number who go sick at ordinary duty.

We see from these considerations that, disease acts chiefly in two ways in reducing efficiency in operations of preparation for war during peace—1st, sickness produces inefficiency by want of practice; 2nd, the physical condition at the time of the performance of the duty affects its efficiency. The last column of the accompanying table shows to what extent the first of these occurred in our own army in 1875.

WHITE TROOPS. 1875. Annual Ratio per 1,000 of mean strength.	Admitted into Hospital.	Died.	Sent Home as Invalids.	Discharged as Invalids.	Constantly Non-effective from Sickness.
Troops at Home and Abroad	986·3	12·47	19·36	19·80	45·38
United Kingdom . . .	831·3	9·36	—	25·80	40·47
Gibraltar	620·9	5·50	22·25	12·29	38·36
Malta	906·1	10·43	33·73	18·86	42·21
Dominion of Canada . .	658·6	8·90	19·60	13·06	30·40
Bermuda	601·4	11·05	19·93	10·52	33·08
West Indies	842·6	8·84	23·00	21·22	41·78
Cape of Good Hope & St. Helena	775·6	7·30	12·76	6·57	37·68
Mauritius	1322·9	14·46	50·60	2·41	40·92
Ceylon	780·3	13·56	42·59	12·68	46.11
China & Straits Settlements	955·7	13·97	42·45	14·56	40·28
India	1303·8	18·52	46·24	11·61	56·06
On board ship	431·0	8·63	—	—	—

We find that in that year the *constantly non-effective from sickness* was greatest in India—namely, 56·06 per 1,000 of mean strength. So that if an army of 10,000 takes the field in India, the general officer in command must calculate that he is carrying with him, feeding, and providing in every way, over 500 men who will be valueless—who will turn out to be inefficient, either from want of practice in their duties, as a consequence of constant previous sickness, or who may be expected to break down from present sickness, and be constantly in hospital, *per annum* of his campaign. And this quite independently of all causes incidental to warfare. But taking the ten-year period, 1865 to 1874, we find that China and the Straits Settlements give a larger ratio of *constantly non-effective* than India.

Here we find it 66·28 per 1,000 of mean strength, and this came very near the fact of inefficient men, in the late Perak expedition (1876).

WHITE TROOPS. Ten-year period 1865-74. Average Annual Ratio per 1,000 of mean strength.	Admissions into Hospital.	Died.	Sent Home as Invalids.	Discharged as Invalids.	Constantly Non-effective from Sickness.
Troops at Home and Abroad	1040·9	14·50	19·32	22·21	45·30
United Kingdom . . .	833·5	9·06	—	27·39	30·01
Gibraltar	656·6	8·31	29·49	18·76	32·61
Malta	828·4	14·24	24·92	16·57	41·66
Dominion of Canada . .	642·3	9·19	16·43	13·58	30·75
Bermuda	716·5	15·04	21·92	14·25	35·39
West Indies	1044·9	17·59	31·82	15·05	46·80
Cape of Good Hope & St. Helena	1056·3	11·12	28·41	20·85	51·55
Mauritius	1419·4	18·97	44·15	16·18	53·58
Ceylon	1180·1	19·06	40·82	18·42	54·18
China & Straits Settlements	1703·6	29·92	72·49	31·07	66·28
India	1441·0	23·44	43·55	17·12	57·10
On board ship	610·8	10·52	—	—	—

It is found that during peace operations in camp, such as our autumn manœuvres, different conditions of service in camps and towns give a change in the "constantly sick" average. For instance, compare the ratio of constantly ineffective from sickness for the United Kingdom in 1875, and the same given for camps for the same year in the following:

(UNITED KINGDOM) 1875.	Average Strength.	Admitted into Hospital.	Died.	Average constantly sick.	Ratio per 1,000 of mean strength. Admitted.	Died.	Constantly Sick.
Camps . .	20,939	18,936	154	924·79	9 4·3	7·3	44·17

We find, from the table on page 17, that the ratio of constantly sick per 1,000 of mean strength of the United Kingdom for the same year was 40·47, and here that the same in camp was 44·17. Also that the ratio of admissions was 831·3 in towns and camp, and in camp 904·3. Therefore there is an increase of sickness when in camp. This must be caused altogether by the *duties*, as the sanitary conditions of men living in camp are better than when living in towns. This is proved by the smaller death rate being in camp 7·35 per 1,000, and 9·06 in towns for the same year. These differences are small, yet they are found to be constant. Hence the commander of a force must calculate on a greater rate of "constantly non-effective" during a campaign than is shown by the ordinary returns in barracks during peace. And this quite independently of wounds or other causes of disease incidental to actual warfare, this reduction in his strength having a corresponding effect on his operations.

To these causes of inefficiency in preparations for war during peace, caused by disease, must be added the loss by invaliding, seen from the following:

United Kingdom.	Troops generally.	Household Cavalry.	Dragoon Guards and Dragoons.	Royal Artillery.	Foot Guards.	Infantry Regiments.	Depots.	Royal Engineers.
Strength	92,802	1,204	10,389	15,870	5,309	40,870	8,80	3,664
1875 Number Discharged as Invalids	2,394	7	310	428	105	1,093	481	65
1875 Ratio per 1000	25·80	5·81	29·84	26·97	19·78	26·74	54·61	17·74
1865-74. Ratio per 1000	27·39	15·30	28·00	30·30	23·34	25·67	42·18	14·66

The depôts, as might be expected, bear the greatest loss from invaliding, next the Royal Artillery, and least loss from invaliding the Royal Engineers. On examining the returns as far back as they go, it is found that these proportions of loss from invaliding are remarkably uniform in each year for each arm of the service.

Why each arm of the service should preserve so uniformly its own proportionate average of invaliding loss, and why this should be so comparatively high in some corps and low in others, are problems which cannot be said to be as yet satisfactorily solved (see page 41).

Another effect of disease on military operations during peace is that, sometimes, certain military stations, or positions, have to be evacuated because of some disease becoming incorrigibly endemic in it. As, for example, Hazarabagh and Sabathu, in Bengal, which are about to be evacuated as military stations, all sanitary measures failing to eradicate from them typhoid fever, that Protean disease so fatal among young soldiers during the first three years of Indian service.

If a commanding position on a frontier had to be given up in this way, the result may be very serious to military operations. These are a few of the most important points bearing on the liability of preparations for war during peace, to be influenced by disease. The effect of health on them is, of course, that all are performed efficiently, and that the men in good physical condition are in much better preparation for active service than those who are debilitated or diseased; not only as regards each individual's bodily fitness, but also as regards his technical efficiency in the performance of the operations of war.

Section II.—Warfare.

SUBSECTION A.—ENTRY ON WAR.

(*a.*) *Mobilization of Forces, including Centralization.*—" The troops of every branch of the service are at all times to be kept in readiness to turn out at the shortest notice. It is expected that in half an hour from the time the troops receive the order to march either by day or night the army shall stand formed at the head of its encampment, with baggage packed, and the whole force prepared to move. This state of preparation is equally essential in cantonments and in camp; and in both the troops are to be accustomed to march without any previous notice." (Queen's Regulations.)

Perhaps there are no military operations to which heavy sickness and full hospitals are more disastrous than *movements*. It is, of course, most felt in the strategical movements of actual warfare, but it is also (not however so much as in former years) very embarrassing in the mobilization of troops on the outbreak of war. Formerly, before troops were transported to scenes of action by railway, and when long marches were undertaken to meet the enemy, the movements and length of time taken in transport were much interfered with by the carriage of sick.

Of course this is as much as possible avoided by weeding out of each regiment, before it takes the field, all material likely to break down or become inefficient from any cause. There are always, in every corps, some men who are tolerably efficient for a peace establishment, but who are certain to succumb to the vicissitudes and fatigues of war. These must be held back from the campaign; and formed—*en disponibilité*—into depôts; whence they may be drafted for active service, if augmentation of the field force is imperatively required. The mobilization of forces, and particularly of reserves, at the entry on war, is frequently interfered with by *disease*—that is, by diseases other than wounds, injuries, &c., which are caused by actual warfare.

In this case the effect is simply *absence by sickness*, a reduction in the numerical strength of the force. The strength of an army of reservists theoretically, as shown on paper, is always very different from the actual fact. When mobilization properly so called has terminated and centralization is about to begin, the question must be asked—How many men has mobilization produced? And it sometimes happens that the answer is rather startling.

The causes of differences of numerical strength between a reserve army on paper, and the strength which is actually available for service, are multiple.

But we have only to do with those caused by disease. Let us take, as a tolerably fair average, the official report of the conscription of 1875 in France. There has been improvement in the management of mobilization since, but the effects of disease must be tolerably constant, and the following may be taken as an average of them.

Total number of men available in 1875 . .		283,768
Of these	29,797 were physically unfit.	
	42,268 were dispersed during peace for family and other reasons.	
	19,508 were postponed.	
	25,778 were already in the army as volunteers.	
	4,295 were conditionally released as professors, teachers, &c.	
	121,646	121,646
Remaining for service		162,126

From this, it appears that "physical unfitness" reduces a reserve force of 283,768 to 253,971; or withdraws from the force ten per cent. (roughly) of the theoretical, or estimated reserve strength.

I am not sure what cases are included under those returned as *postponed*, but it is probable that a good deal of the postponement was caused by transient illness. If this be so, about twelve per cent. of the available reserve strength may perhaps be taken as the average held back on account of permanent physical unfitness *and* transient disease. This would vary in different countries slightly, according to the age at which liability to military service ceases.

In France all young men between 20 and 25 should *theoretically* be found in the active army; those from 26 to 29 should form part of the reserve; and all men between 30 and 40 ought to be found in the territorial army and its reserve; whereas, in Germany, liability to army service lasts until 42 years of age. Three years are passed in the standing army, four in the reserve, and five in the Landwehr; and in time of pressure the Landsturm, or entire able-bodied population up to the age of 42, can, in addition, be called to arms.

It is to be expected, therefore, that in Germany a slightly higher percentage of what is estimated as the reserve strength, would be kept back by physical unfitness and transient disease, than in France.

Now when the mobilization of an army is completed, and the time for its centralization arrives, how do we find this latter operation affected by disease? As regards the actual centralization we cannot find, either in theory or practice, that physical unfitness can affect it much.

Men who are mobilized can also be concentrated. Perhaps centralization may be affected slightly as to the time it would occupy, but this effect will be inappreciable. But centralization may be said to include all the period from the termination of mobilization until the army arrives on hostile ground; until the movements of strategy commence. In these days of rapid railway transport of men and supplies much disaster from disease cannot occur during this period.

In former times, however, when long marches had to be undertaken, great losses were sustained through disease during centralization. But, it must be assumed that *now*, in European warfare, at least, such disaster would be rare, except, perhaps, in the case of Russia, where the distances over which troops have to be moved is so considerable. Indeed, some years ago General Todleben, stated in a speech at Woolwich, that the immense tracts of country through which Russia has to transport her troops, prior to centralization, does and must always effect a considerable reduction of the strength of a force she puts into the field, as compared with the strength of the force she is capable of mobilizing.

The effect of disease on such centralization is, in these times, most experienced when troops are taken into a hostile country, to the climatic conditions of which they are unused, and which, quite apart from the conditions of warfare, produce disease among them.

We have illustrations of such in many of our own recent campaigns, particularly that of Abyssinia in 1867-8; the Ashanti expedition of 1873; and in several of our recent Indian expeditions, as in that of Perak, in 1876. The duties of campaigning during what may be called the period of centralization — that is, prior to the commencement of the strategy and tactics of actual warfare—often serve as a test of efficiency. As thus:—"The first five marches proved of great advantage to the efficiency of the advancing force, inasmuch as the difficult nature of the road through the Koomaylee Pass at once tested the physical capabilities of the troops, and enabled us to eliminate those who were sickly or unfit for hard work. The mountainous aspect of Abyssinia was alone sufficient to convince anyone that none but men in the highest state of health were fit to campaign in such a country; and the small experience already gained enabled medical officers to select from the different corps, on their arrival at Senafe, 100 men who were either sick, or too weak for field service." (A. M. D. Report, vol. ix). In the Ashanti campaign the strength of the expeditionary force was much reduced by disease before strategical operations were commenced.

It will be remembered that when the proposal of sending a force to the Gold Coast Protectorate was made, a great outcry was raised against it; and many of the organs of public opinion were filled with predictions of the certain disaster in which such an expedition must culminate, owing to the deadly nature of the climate.

Although, fortunately, these prognostications were not fulfilled, they were, no doubt, warranted by all our previous experience of intercourse with the country. They were also justified by the history of the Royal African Regiment, a detachment of which, serving at Cape Coast in 1824, consisting of 225 men, lost, in that year, by death from disease, 221 of their number, or in the ratio of ninety-eight per cent. of their strength; and fifty-four per cent. of the same, completed in strength by a fresh detachment, died in the following year. Taking the three years 1824 to 1826, the average annual mortality from disease in this corps was sixty-nine per cent. of the strength. Referring to this excessive mortality, the official Report says:—"It has on this account been necessary to enumerate the strength in each quarter, as the troops were cut off with such rapidity that few lived to complete one year in the command."

And in 1864, an expedition designed to undertake the same operations as those contemplated in 1873 returned to Cape Coast without encountering the enemy, sickness having occasioned an enormous loss, although no loss had been sustained from causes referable to military operations. In the expedition of 1873 we find that, although great precautions were taken to preserve the troops in the best state of health compatible with the trying conditions in which they were placed, yet the loss sustained from disease before any military operations were attempted was great, and had a distinct effect on the subsequent operations of the campaign.

"The conditions inherent in residence at Cape Coast are so inimical to health, that under the most favourable circumstances sickness can only be averted incompletely and for a short time. In the case of the Marines everything was adverse. Sent out in a hurry to a place where the arrival of white troops had never been contemplated as likely, and where no preparation for them had been made, they arrived at the most unhealthy season of that most unhealthy place; their duties entailed great exposure, and sanitary precautions were necessarily subordinated to the military exigency which required and justified their presence. . . . Sickness among the Royal Marines greatly increased: on the 23rd of July 56 men were in hospital or on board ship, sent there for convalescence, and only 47 were at their duty. On the 31st of the month (sixty-two per cent. of the Marines being ineffective), H. E., the Governor, after having caused a reference to be made to the principal medical officer if it was probable the health of the Marines might be restored by a short cruise at sea or not, decided that the detachment should be sent home.

"There being a great likelihood that the Ashantee army would be emboldened to recommence hostilities on hearing that the European soldiers could not remain, it was necessary to keep some of the artillerymen, and seventeen were ordered to remain—no more were found to be effective. . . . It had been anticipated that the men of the West India Regiment would suffer severely from climatic sickness after their arrival, and so it proved—the sick rate soon reached twenty-three per cent. of the strength. . . . . The efficiency of the European officers and non-commissioned officers was still more impaired than that of the West India Regiment, one-third of their number had been attacked by illness within three weeks of their landing, and one officer had died. . . . . Although heavy rains had unexpectedly recommenced they were fitful and short, and there were days of dry hot weather, the power of meteorological conditions to influence existing disease, and to change the character of newly-arising diseases, was clearly shown.

"The health of the troops, both European and West Indian, was now improved, though this was to be inferred, not from the number actually under treatment, but from the smaller number reporting sick. Of the seventeen Royal Marines stationed at Elmina eight were in hospital; of the European sergeant-majors three in eight; of twenty-nine officers in the two garrisons, nine were sick and seven were ailing, though able to be about. Through timely invaliding deaths were few, but it probably depended on the presence on the roads of a homeward bound steamer whether a patient, weakened to the last degree, should survive or die" ("Army Medical Department Reports," vol. xv.). The enormous reduction of strength here described, materially modified the contemplated operations. Such an excessive reduction of strength by disease must always be anticipated when troops are taken over long distances in countries climatically unfavourable. Many illustrations may be cited showing how frequently this tells disastrously on the future operations and final success of an expedition, as in the expeditions sent by Russia against Khiva, under Bekovitch in 1716 and under Perovski in 1839, both of which resulted in miserable failure. And the campaign undertaken by the same government against the Khanate in 1873, although successful, was very nearly a failure from the reduction in its strength by the same causes; indeed, it was only a strategical error of the enemy which saved it from culminating in disaster and wholesale death. We hence learn that the sanitary improvements of the present day are comparatively powerless to protect armies from disease arising out of such conditions.

(*b.*) *Transport.*—Disease is more affected by transport than transport is by disease. Yet we occasionally find that the conditions we have just considered as causes of reducing strength, interfere most disastrously with transport of men and supplies. This particularly happens when large tracts of barbarous or semi-civilized countries have to be traversed. Such expeditions resolve themselves almost entirely into questions of transport. If the transport of supplies should break down the failure of the expedition is a certainty. Such a breakdown may be caused by disease among men attached to the commissariat; but this can rarely have an appreciable effect. It is disease among cattle, particularly epidemics and those diseases caused by the want of proper food, especially good water, which are to be feared. The fewer animals taken the better will they be looked after and fed. But then the less supplies can be carried, and this involves the necessity of having a small force; and, as a consequence, the liability of discomfiture by the enemy in the field. On the contrary, if a large force be taken on an expedition of this kind, the number of baggage and stores animals must be considerable for the conveyance of its supplies, and the probability of their perishing quickly for want of food and water is increased. Sometimes disease of men or cattle causes straggling or slowness, which lengthens the line of the baggage train. In such cases it is often difficult for the baggage guard to protect the train from a flank attack by the enemy, particularly if the latter have irregular cavalry. In the transport of troops by rail, when lying-down accommodation is required for sick or wounded, more carriages are required for the corps or detachment, and if the supply of these be limited the troops will have to go in separate lots if the number of sick be large. And thus more time is occupied in the transport. The same inconvenience sometimes occurs when carrying a large number of sick in carts.

SUBSECTION B.—ACTUAL WAR.

(*a.*) *Strategy.*—"Strategy is the art of moving troops in a theatre of war, so that by the direction of their march, and apart from their powers of manœuvring and fighting, an advantage may be obtained over the enemy." The first great principle of strategy is that communication must be kept open between an army and its base of operations, and that the latter must be securely defended, this being imperative for the constant reinforcement of troops, horses, clothing, tents, hospital stores, and the transport of those disabled. Now, can this security of communication between army and arsenal be imperilled by disease? There are circumstances in which it may. The more artificial the defence of the base of operations, the greater is this danger from disease. England's base of operations or communications with it cannot be so influenced, because it is protected by natural defences—"England's base of operations is the Ocean."

So, generally, in the case of France, her bases of operations in a continental struggle would be the Rhine on the east, and the Alps toward Italy. But where no such natural defences exist—where such are artificial, as fortresses, and intrenched camps—the case is very different.

Suppose that France has to supply an army in the field with magazines from such.

The successful holding of them would depend as much, if not more, on the skill and power of artillery on the ramparts, as on the tenacity and strength of the garrison. And considering the organization of her *artillerie de forteresse*, even as now improved, it must be admitted that such a base of operations would be weak and liable to disaster, both from storming by an enemy, and from disease. The nineteen brigades of gunners, including three batteries in each, give but fifty-seven batteries for such a defence. Then, suppose these distributed to a number of such garrisons; and suppose the diseases so frequently found to accompany sieges, or a fatal epidemic, should with the effects of battle reduce their strength, as it may do, to any conceivable extent; how disastrous such a result must be, in a force small to begin with! As regards the security of communication between an army and its arsenal, this also may be jeopardized by disease.

It is manifest that communication between an army and its base of supplies is best protected when the army marches in a line perpendicular to that of its base. Its communications are thus covered from an enemy in front. But, suppose that, having marched some distance, the army is attacked by a severe epidemic of some fatal disease—cholera, typhus, yellow fever, &c.—and that the prevailing wind is blowing in the same direction as the line of march. In such cases the rule is to march at right angles to the wind. "Should cholera follow the troops, they will be moved short distances at right angles if possible to the prevailing wind and track of the disease." (" *General order by Sir H. Rose, Commander-in-Chief in*

India," dated Simla, 7th April, 1862.) Now, if the prevailing wind be perpendicular to the line of the base of supply, a commander must move his army in a direction more or less parallel to his base; he turns a flank to the enemy upon which he was marching; and being *en échelon* to his base, he leaves his communications therewith open to attack. It may be said such a movement is justifiable only when strategy permits. But, what choice is there in the matter? Only between keeping the army where it is, or continuing to march in the direction of the wind, running the risk of being decimated by disease and certain tactical disaster; and changing the direction of march to save the troops, thus risking an attack on base and communications—a strategical disaster.

The direction of a march may have to be changed, or a march may be altogether interrupted by disease, either existing or imminent, arising from want of good water, or by the entire absence of water *en route.*

"The last part of the march—viz., that from a place called Adam-Krylgan to the banks of the Oxus, was the most desperate of all. Twice were the troops brought to an absolute stand still in the desert through want of water, and were only saved by the timely discovery of wells and springs at some distance from the line of march. In one of these instances it was found that the water thus discovered was only sufficient for half the troops of the column, and all the cavalry and artillery horses and the camels had to be sent back one long march in order to remain near some wells, till arrangements could be made for their onward march. On both these occasions the situation of the troops was most critical. The camels perished by dozens; everything in the shape of baggage, except what was most rigidly and absolutely necessary, had to be abandoned in the desert (some burnt, some buried), in order to lighten the loads of those animals which were able to proceed." ("The Russian Campaign against Khiva in 1873," *Journal United Service Institute,* No. lxxvii.)

Again, an army may have to make a *detour* from a town or district infected by an epidemic disease, as cholera; and this may have a serious effect as regards time and strategical position.

Disease may in various ways delay troops in coming up to a rendezvous by a particular time, and many historical instances will occur to the reader, of engagements being won and lost by this cause. Generally, for political or other reasons, an offensive army endeavours to obtain occupation of some particular position in an enemy's country—a disputed tract, some fortress or natural stronghold of strategical importance, or the enemy's capital. Disease may in various ways oblige a commander to stop short of proceeding far enough into the enemy's territory to accomplish such an occupation, apart from tactical considerations.

There are in all countries, certain natural positions, the occupation of which is of much importance ; such as the issues of defiles, positions convertible into advanced bases, &c. Should epidemic or endemic pestilence necessitate the evacuation of such, this strategical accident may have a very definite effect on the issues of the campaign.

When the sick and wounded from a former engagement are present in the field during action, it is a serious strategical encumbrance, embarrassing tactical movement. This infrequently but occasionally happens; as in the taking of Coomassie. "The enemy's attack being thoroughly repulsed about 2 P.M., the troops continued their advance on Coomassie, which place, after a severe and most exhausting march, was reached at 6 P.M. Close to the town the troops had to wade through a swamp, knee deep, for about a quarter of a mile. The sick and wounded were obliged to accompany the column, there being no means of sending them to the rear." ("Army Medical Department Reports," vol. xv.)

(*b.*) *Tactics.*—A few of the most important tactical questions which are liable to be affected by health and disease—that is, by the physical condition of troops—are as follows :—1. Is the enemy to be engaged now, if not, when ?—2. Shall the tactics be offensive or defensive ?—3. The choice of position ?—4. Questions of the strength and composition of outposts, &c.—5. Questions of tactical formations. The first three questions refer equally to military and naval warfare, the others to military tactics only.

During active service, military and naval, the reports furnished by medical officers in charge differ somewhat from those required during peace.

In peace, averages are calculated on the mean strength, which includes men in hospital, because the information then chiefly required is—the loss caused by disease to the entire army in a given period. In war, the averages must be calculated, in addition, on the strength actually under arms, or efficient : because the information required relates not only to the mean strength, and to given periods, but also to the loss the field force is immediately suffering, and the varying proportions between effective and non-effective, up to the present hour. Definitely stated, the questions a general officer in chief in the field or an admiral commanding a Squadron requires to have answered are :—How many men am I losing daily from the effective strength actually under arms? How many are replaced by discharge from hospital? What is the balance, gain or loss? If, when this balance is struck, my effective force loses daily such and such a percentage, what will be its loss in a week, in four weeks, in six weeks, &c? What are the causes of this loss, and how are they influenced by special circumstances? (Professor Parkes). On the answers to the above questions depend (if there be any choice in the matter) :—

1st. *The time at which to engage the enemy with most advantage?*—It is obvious that if there have been much sickness from which the force is recovering—that is, if day by day the effective strength is being increased by discharges from hospital, and at the same time few men are going sick—engagement should, if possible, be put off until the time the force is as effective as it can reasonably be hoped to be.

This time will be roughly calculated by taking the average rate *per diem* at which men are being discharged from hospital. If the contrary be the case—that is, if the effective strength is becoming daily lessened by disease; and if this loss arises from causes (such as climate or epidemic disease) which are not under control, and not likely to be ameliorated—then, obviously, the sooner an engagement comes off the better.

2nd. *Shall the tactics be offensive or defensive?*—It may be stated generally, as regards military engagements, that a larger effective force is required for offensive operations than for defensive; because, *now*, under ordinary circumstances, the difficulties of attack, and advantages of defence, are much increased by the introduction of breech-loading rifles. That if the defensive be adhered to, it is at least three to one that the enemy will merely make feigned attacks on the front, and will make the real attack on the flank. And, that if the offensive be assumed the chances of the success of a well-ordered flank attack by the enemy are at least treble the chances of the success of a frontal attack, these probabilities being calculated from the results of recent battles (Major Griffiths, "Artillerists' Manual"). In naval engagements the defensive is also, under ordinary circumstances, to be preferred, but for a different reason.

Here, there is no cover available; preparations for attack are seen and justly estimated; surprise is no factor in the attack. "In naval operations the point of attack is so undisguised, that, if the defensive operation be well managed, the attacking force must be exposed to great and often decisive effect previously to close action." (Sir Howard Douglas, "Naval Gunnery.")

Hence it may be said generally that, if a force be in a high state of efficiency, the offensive may be assumed if rendered advisable by tactical or strategical considerations: but if, on the contrary, the efficiency of the force have been much weakened numerically or physically by death or disease, then it is not advisable to risk offensive operations.

3rd. *The choice of position* is also influenced by the efficiency of a force. Of course there are certain advantageous positions which a commander will always endeavour to secure.

But, under some circumstances, if his force be very efficient, and numerically stronger than the enemy, he can afford to engage, if the enemy be *exegeant*, without having obtained such advantage from position. But if his force be weak, numerically or physically, he will abstain from battle until a strong position be obtained. knowing that an advantageous position is in itself equivalent to a superiority of numbers. That is, if he be acting on the defensive; if he is attacking, there will be very little option as regards position. So also in naval tactics, numerical or physical efficiency will, or used to, determine the question of manœuvring with a view to obtaining the most favourable position for action, as, if a ship's crew be inefficient, her antagonist's efficiency must be well reduced by preparatory fire at a distance, previous to engaging her more closely.

4th. *The strength and composition of outposts, &c.*, is sometimes affected by considerations of the physical conditions of the troops. If operating in an unhealthy climate, where the men are much affected by excessive heat or cold, or are reduced in physical condition by malaria or other endemic causes of disease, then a sentry's beat must be reduced to less than the average length according to circumstances; he must be a shorter time on guard; and he must be on guard less frequently, and have a greater proportion of nights in bed. And in countries where there is danger of sunstroke, or the benumbing influence of cold, the sentries must be double as a precaution. This is one of the cases in which the physical condition of an individual affects the safety of the entire force—" the grand object of outposts must always be to prevent the main body from being surprised before it has time to prepare for action." (" Prussian Regulations.")

If men be in a weakly state, all such duties must be lightened—that is, must be more distributed. As, if a sentry's time at a post be four hours, this will take a guard of six men: but if it be reduced to two, a guard of twelve will be required. And so for other duties.

5th. *Tactical formations*, however admirably planned, and actual fighting in the field, are, of course, influenced by the physical condition of troops, by the efficiency of the whole force, and that, consequently, of the individuals who form it.

It may, perhaps, be too much to say that the break down of a few individuals, at the part of a line attacked, may cause a line to be broken through, or a flank to be turned. Yet, we know that in this, as in everything else, the strength of a chain is only equal to that of its weakest link, and that the slightest wavering may give an opening to a charge from a watchful and intrepid enemy. This recalls to mind the brave deed of the Swiss patriot, who, rushing on a line of bayonets, grasped as many as were within his reach, and drawing them with outstretched arms towards his breast, fell transfixed; while his comrades charged and broke the line through the breach thus made.

It would be out of place here to enter into fuller details of all the military and naval operations which are liable to be influenced by the physical conditions of those engaged upon them.

As a matter of fact, *all* such operations are, in various ways, more or less, affected by such causes. I have merely indicated slightly, above, the *kinds* of operations which more manifestly than others seem likely to be materially influenced by health and disease.

SECTION III.—PIONEERING.

Although not strictly speaking an operation of war, Pioneering must be included among military and naval operations. Pioneering expeditions may fail through the effects of diseases; more especially those produced by fatigue, privation, and the effects of climate.

Amongst the diseases produced by privation, *scurvy* is pre-eminent in its effects on such enterprises. It has been the cause of failure of many Arctic Expeditions. More especially some of the voyages of Sir Edward Parry at the commencement of the present century.

Among the causes of disease due to climate, as affecting such operations, are particularly to be mentioned malaria, excessive heat, and excessive cold. When war is carried into uncivilized countries, strategical movements, and the time spent on them, are a good deal influenced by the efficiency of those engaged on pioneering, such as road making, cutting jungle, &c.

The celerity with which such operations are performed being influenced by the number available for such work, and the individual strength or fitness of those employed; we see that disease influences the time which is occupied by such operations, by reducing the number of labourers, and by reducing the strength or physical energy of those who are actually employed. The effect of Disease operating in this way was manifested in the Ashantee and Perak Expeditions.

CHAPTER III.

"The Angel of Death spreads his wings in the blast,
And breath'd in the face of the foe as he past,
And the eyes of the sleepers wax'd deadly and chill,
Their hearts but once heaved, and for ever grew still."
BYRON.

CONDITIONS OF HEALTH AND DISEASE WHICH AFFECT THE OPERATIONS OF WAR.

SECTION I.—CONDITIONS OF HEALTH WHICH AFFECT THE OPERATIONS OF WAR.

IN recruiting reports the question has to be answered—Is he in every respect likely to become an *efficient* soldier?—after a medical examination of the recruit has failed to detect any physical defect.

In commanding officers' confidential reports, the question has to be answered—Is such and such an officer *fit* for active service? Now, what does this *efficiency* or *fitness*, independent of and over and above the freedom from physical defects or disease, mean?

It means a condition of health, or physical aptitude especially suited to bear the duties which are demanded of men in warfare. It is a rather indefinite complex idea, which we will now proceed to examine and attempt to define.

A man may be perfectly healthy, that is free from organic and functional disease, and yet not (*a.*) *Strong*. We find that occasionally the duties of warfare require not only health, but, in addition, a large amount of pure physical force—mere brute strength. In warfare men are sometimes called upon to do the work of beasts, if the latter are not just at the time available. And in proportion to the strength, mere muscular force of the men, the number required to be told off for such duties will be large or small. This is of importance when few men can be spared from other regular duties; particularly if the latter are pressing.

Many such instances of emergency will occur to the reader. At Spicheren, on August 6, 1870, the success of the German army in that engagement seems to have depended altogether on the exertions by which their twelve guns were hauled up to the crest of the plateau, opposite the Spicheren-Steiring heights, just in proper time to silence the "murderous fire of the Chassepôt" from Frossard's corps.

It makes much difference in the result if the number of men available for such duty possess among them the required strength to do it in a given time.

In fact the success or failure of such an operation may be calculated beforehand, at least in raising known weights through known elevations, by taking the amount of foot-tons and the given time, and calculating an ordinary man's strength as averaging one-seventh one-horse power, taking the latter as 33,000 pounds raised one foot per minute, or 8,839 tons raised one foot in ten hours. If the men's physical strength is below the average, numerical strength must be increased. Physical strength is much influenced by *age*. We know this both from actual experience of war, and also from physiological consideration of the immaturity of the ossification of the human skeleton before the twenty-fifth year. (Aitkin's "Growth of the Recruit and Young Soldier.")

The muscles are equally immature before this age; and their action is further weakened by the resilience of their leverage—certain points of the bony structures yielding if much force be used.

Young men under 20 years of age should never be put to the duties of actual warfare except in times of great pressure. Napoleon said that such "Merely strew the roadside and fill the hospitals" (Parkes). Age also exercises an important influence on the second factor of fitness—namely (*b*.) *Endurance*. Endurance seems to increase directly as age. A soldier of from 40 to 50, though he may be not so strong, can endure more fatigue, privation, and hardship, and for a longer time, than a soldier of from 20 to 30 years. So that, as regards *strength* and *endurance*, age plays a very important part in the efficiency of men for the active duties of war, and on the manner in which the operations of war are performed. The question of this condition of health, as regards recruits, has been well summed up by the late Professor Parkes, thus:—"If the State will recognize the immaturity of the recruit of 18 years of age, and will proportion his training and his work to his growth, and will abstain from considering him fit for the heavy duties of peace and for the emergencies of war till he is at least 20 years of age; then it would seem that there is not only no loss, but a great gain by enlisting men early. At that most critical period of life, the recruits can be brought under judicious training, can have precisely the amount of exercise, and the kind of diet, best fitted for them; and thus in two years be more fully developed, and be made more efficient, than if they had been left in civil life." (" Practical Hygiene.")

The next component of fitness would seem to be (*c*) *Independence*, both physical and moral.

Physical independence is a condition of health which depends to a great extent on previous life and training, and the social conditions in which men have been placed before their service.

It may be taken as a general rule that the physical conditions and social independence of troops taken from a sparsely populated country render them less liable to the effects of fatigue and inconvenience arising from the conditions of warfare than is the case of those levied from thickly populated districts. No doubt the physique of the men is improved by the segregation in which they have lived; and the same condition renders them independent. They are better able to help themselves at a push.

Regiments composed of such men can "run alone," and forage for themselves in times of pressure. They have their camp, and want nothing more—no buildings, no permanent staff, no nurses, no elaborate commissariat organization.

Their mobilization is much simpler and more perfect; and difficulties of transport inconsiderable.

Each individual has a sense of confidence and responsibility for his own personal welfare which is the true germ of a military spirit. Take for example the war which has just concluded. There can be no doubt that the troops of Western Europe would have succumbed much more to the deleterious influences of warfare, than did the troops engaged in that war on both sides. The armies of Western and Central Europe, high as they may stand in matters of discipline, drill, movement, appearance under arms, &c., are to a great extent dependent and helpless, because of the system under which they are organized; and the civilization of the countries where they are raised, and have always lived—

the result of the large population of such countries. What a contrast to this we find in the countries from which Russian and Turkish armies are levied. On an average there are 36 inhabitants to the square mile in European Russia; 29 in the Caucasus; 3 in Central Asia; and only 0·7 in Siberia; the average for the whole empire amounting to only 10 inhabitants per square mile. In Turkey, with the exception of the Constantinopolitan district with 623 inhabitants to the square mile, there is but one sanjak, namely, that of Berat, a small district of Albania facing the Adriatic, where there are 160 inhabitants to the square mile. In the Dobruja (Talcha and Varna) the number is but 37; in Bulgaria, 65; in Albania, 65. Only in five sanjaks does the population exceed 100 inhabitants to the square mile. (*Journ. Statist. Soc.* 1877.) This aptitude for military service caused by the physical training and social independence resulting from a life of segregation, I believe to be a most important matter, and deserving much more attention than is bestowed on it. It certainly should be an element in calculation when putting the army of a civilized country against that of a comparatively uncivilized one. It must be held in view that the efficiency of the latter is much less jeopardized by commissariat disaster, or being cut off from its base of supplies, than that of the former. We find from the early history of armies, that this consideration for a long time delayed the regular use of cavalry. "The Franks who issued from the forests of Germany and conquered Gaul, seem generally to have fought on foot. And as the victories achieved over their Gallic adversaries, who were mostly horsemen, tended to strengthen their belief in the superiority of infantry, their armies long continued to be mainly composed of that favourite force." That *superiority of infantry* in early times was no doubt due to the greater facility of providing supplies, independently of organization. (Mitchell, "Historical Sketch of the French Army.")

With reference to this subject Professor Parkes remarks:—"It is the nature of war to reinduce a sort of barbarism. The arts and appliances of peace, which tend, almost without our care, to shelter, and clothe, and feed us, disappear. The man reverts in part to his pristine condition, and often must minister as he best may to his own wants. No doubt the State will aid him in this; but it is impossible to do so as completely as in peace. Often, indeed, an army in war has maintained itself in complete independence of its base of supplies, and in almost every campaign there is more or less of this independence of action. In peace, the soldier, as far as clothing, feeding, shelter, and cleanliness are concerned, is almost reduced to the condition of a passive agent. Everything is done for him, and all the appliances of science are brought into play to save labour and to lessen cost. Is this the proper plan? Looking to the conditions of war, ought not a soldier to be considered in the light of an emigrant, who may suddenly be called upon to quit the appliances of civilized life, and who must depend on himself and his own powers for the means of comfort and even of subsistence. The English soldier is not helpless, he is simply untrained in these things; and so long as he is untrained, however perfect he may be in drill and manœuvre, he is not fit for war. The campaign itself must not be his tutor; it must be in the mimic campaigns of peace, in which the stern realities of war are imitated, that the soldier must be trained. Our present field-days represent the very acme and culminating point of war; the few bright moments when the long marches and wearisome guards are rewarded by the wild excitement of battle; but the more common conditions of the campaign ought also to find their parallel" (*loc. cit.*). Physical independence, then, is a condition of health, without which a soldier is not fit for service, no matter how free from disease he may be. And this condition of health has a very material influence on the operations of war. I have dwelt at some length on this subject because I consider it a matter of urgent importance, and look upon it as a source of real danger to this nation, more especially to our Indian Empire.

Equally important is mental and moral self-reliance. This our present system of organization absolutely forbids. It is nevertheless a most desirable object to be attained. And this necessity is beginning to make itself felt. "If we persist in tying gunners and drivers always to non-commissioned officers, we take away all sense of responsibility and destroy the germs of a military spirit; we take a course which no employer of labour, or master, would dream of doing, and we reap our reward in the helplessness and ignorance of many men who, under a better system, would make excellent soldiers. Naturally, recruited as our army is, there are some men incapable of being influenced by any good motive; but they can be got rid of, and I am confident the *morale* of the regiment would receive an enormous impulse if all ranks were in their degree more trusted and made responsible for their actions." (Col. Strangways, R.A.: *Proc. R.A. Inst.*, vol. ix.)

The last component of the health condition we term *fitness*, to which I shall allude, is (*d*) *Courage*.

It is hardly requisite to state that this quality has and must always have a most important effect on the operations of warfare. It is perhaps, more than any other moral quality with which the human race is endowed, dependent on conditions of physical health. With regard to courage, the late Prof. Parkes (from whose eloquent and philosophical writings on the subject of the interdependence of health and disease and warfare I have already so frequently quoted) observes :—" Without attempting to analyze the complex quality called courage, a quality arising from a sense of duty, or love of emulation, or fear of shame, or from physical hardihood, springing from familiarity with and contempt of danger, it may well be believed that it is capable of being lessened or increased. In modern armies there is not only little attempt to cultivate courage and self-reliance, but the custom of acting together in masses and of dependence on others actually lessens this. It is, then, a problem of great interest to the soldier to know what mental, moral, and physical means must be used to strengthen the martial qualities of boldness and fortitude. The English army has never been accused of want of courage, and the idea of pusillanimity would seem impossible to the race; but drunkenness and debauchery strike at the very roots of courage, and no army ever showed the highest amount of martial qualities when it permitted these two vices to prevail. In the army of Marlborough, the best governed army we ever had, and the most uniformly successful, we are told that the 'sot and the drunkard were the object of scorn.' To make an army perfectly brave, it must be made temperate and chaste. Good health and physical strength, by increasing self-confidence, increase courage; and self-reliance is the consequence of feeling that, under all circumstances we can face with strength the dangers and difficulties that present themselves" (*loc. cit.*).

These are a few of the elements of fitness, or conditions of health, most important in their influences on the operations of war.

Section II.—The Prevalence, Nature and Causes of Diseases most likely to Influence the Operations of War.

SUBSECTION A.—GENERAL REMARKS ON ARMY DISEASES, WITH A COMPARISON OF THE SANITARY CONDITION OF ARMIES AT THE PRESENT TIME AND THAT WHICH EXISTED IN THE EIGHTEENTH CENTURY, AS DESCRIBED BY HOWARD.

The comparative perfection to which many of the social institutions of civilized countries have attained, frequently causes us to forget that such perfection is comparative; that it is the result of progress. Being familiar with the prosperity and easy working of such institutions, we look upon this as a matter of course, and too much ignore the labours of those who founded or fostered the infancy of such institutions.

It is only when we are inconvenienced by some failure or hitch in the working of such that we realize the benefits we derive from the labours of past generations. These remarks are suggested by reviewing the past and present of our Sanitary Science. Doubtless it is far from perfect, but vast progress has been made in this direction within the past century. We fail to appreciate that progress perhaps, and realize it only when the mind is arrested by some calamity—a severe and fatal epidemic, death resulting from some adulteration or disease-cause in some widely used commodity, an outbreak of endemic or preventable disease in a Royal household, &c.; such occurrences leading us to reflect on the past and present of Sanitary Science, and to congratulate our age on the fruits of progress which it enjoys. It is no doubt true that, as Sir John Herschell expressed it, "To produce a state of things in which the physical advantages of civilized life can exist in a high degree, the stimulus of increasing comforts and constantly elevated desires must have been felt by millions, since it is not in the power of a few individuals to create that wide demand for useful and ingenious applications which alone can make great and rapid improvements." Yet we must experience a sense of gratitude to those pioneers of civilization who, by exposing evils existing in their times, have forced a consideration of them on the public mind.

These considerations apply especially to sanitation in the public services of this country. While fully appreciating the progress made in this direction, with admiration of those to whose zeal and labours this result is due, we are aware that much more remains to be done to bring army and naval sanitation on a par with the civilization of the present age.

We find that the health of our soldiers and sailors contrasts unfavourably with that of the civil section of the community. We must hence conclude that the conditions under which they are placed exert a deleterious effect on their health, or that for some reason certain diseases, or classes of disease, are more prevalent among them than among civilians. The prevalence of disease among soldiers and sailors (considered apart from mortality) is calculated from—(1) the number of admissions to hospital; (2) the number sent home from foreign service as invalids; (3) the number constantly non-effective from sickness; and (4) the number of days spent in hospital per head, per 1,000 of strength.

(1.) The numbers of admissions to hospital per 1,000 of strength are given in the second column of the Table on page 17 for the year 1875, and for the ten-year-period, 1865-74, on page 18. (The Army Medical Department Report for 1876 has not been received in sufficient time to make a comparison of details; but the admissions into hospital for that year were, taken altogether, lower than for 1875.)

The average for 1875 was 986·3, a decrease of 54·6 from the average for the ten-year-period, which was 1,040·9. Thus there has been a considerable improvement even in this short period.

This improvement is much more realized when we compare these figures with the returns of former years:—

In 1859. Admissions . . . 1,066	In 1861. Admissions . . . 1,025
In 1860. Admissions . . . 1,053	In 1862. Admissions . . . 989

The foregoing figures are per 1,000, calculated on the whole army strength. The average rate of admission varies considerably for different arms of the service. I have before (page 19) alluded to this fact, and stated that these different averages are not satisfactorily explained by the different duties of the corps. It is to be expected that cavalry and artillery would have increased admissions arising from contusions and such slight injuries occurring at drill, &c.; but on going into details of the diseases returned, it is found that these are not the causes of the increased admissions, and higher average of invaliding in these corps.

This *Admissions to hospital* is fallacious when alone taken as an indication of the health of a community. When we take the average admissions to hospital for the past twenty years, we find that the decrease is insignificant as compared with the decrease in mortality for the same period, which is enormous. "Admissions to hospital" are also quite useless as a standard of comparison of the military and civil sections of the community. Because, in the services, men are admitted to hospital for trivial complaints, which a person in civil life would never think of asking a medical opinion for, such as a cut finger or a bruised eye. The regulation being, "He (the medical officer) will examine all non-commissioned officers and soldiers who report themselves sick, and will cause such men as are found to be in an unfit state of health to attend parades, or to perform the ordinary duties of a soldier, to be removed to hospital for treatment." The authorities seem to think that a man should be either in hospital or at his full duty. This system swells up our number of admissions enormously, as compared with the sick-rate of the civil population; and also as compared with the admissions to hospital of other armies.

For example, in the French army, slight cases of illness are treated either in the barrack-rooms or infirmaries, and only the severer cases admitted to hospital, and returned as such. Another source of fallacy in this average of admissions is, that if there occur any change of symptoms while in hospital, or if any error in diagnosis has been discovered, the man is shown *discharged otherwise*, and re-admitted under the fresh disease. Thus, if a man has been exposed to wet and cold on a night guard: suppose he is admitted to hospital with a feverish cold under *febricula;* in a few days he complains of faceache, is discharged, and shown as an admission for *neuralgia;* within a week pains in the back and limbs supervene, and he is again discharged and re-admitted as *rheumatism*. Although all these symptoms arise from the same cause, and in civil returns would be described as one illness, our army returns show it as three admissions. Of course there are good reasons for adopting this plan, such as showing the prevalence of different diseases, &c. I merely wish to indicate how our estimate of the *sickness* is vitiated, if we go by the *admissions to hospital* taken alone.

(2.) The number sent home from foreign service as invalids is hardly of any value as a standard of comparison with the statistics of civil life, and may be omitted here.

(3.) The number constantly non-effective from sickness (given in last column of Tables on pages 17 and 18), means the daily number of sick in hospital per 1,000 of strength. We find improvement here when we compare the earlier returns with the present.

In 1860 the number constantly ineffective = 54·72 per 1,000 mean strength.
„ 1861 „ „ „ = 54·54 „ „
„ 1862 „ „ „ = 52·45 „ „
10 years 1865–74 „ „ = 45·30 „ „

The following averages per 1,000 of strength of the constantly ineffective through sickness of foreign armies are copied from Dr. Parkes (1864):—French, 40·2; Prussian, 44; Austrian, 45; Belgian, 54·2; Portuguese, 39·4. These figures all contrast favourably with those of former years. For example, in 1846 the French average was 45·5. When we compare the *constantly sick* average of the service with that of civil communities it brings out very plainly the excess of disease prevalent in the army. In England the number of members of friendly societies, between 20 and 30 years of age, constantly sick, being about 16 per 1,000.

(4.) The number of days spent in hospital per head per 1,000 of strength (which is practically the same as the "mean duration of cases") also brings out in a strong light the prevalence of disease in armies.

For the ten-year period, 1865–74, the average sick time to each man was 16·53 days distributed thus:—

Ten-Year Period, 1865–74.	
(White Troops.)	Average sick time to each man. Days.
Troops at Home and Abroad	16·53
United Kingdom	14·81
Gibraltar	11·90
Malta	15·20
Dominion of Canada	11·22
Bermuda	12·91
West Indies	17·08
Cape of Good Hope & St. Helena	18·82
Mauritius	19·56
Ceylon	19·77
China & Straits Settlements	24·19
India	20·84

In friendly societies the average sickness per man per annum is less than seven days, in those under 40 years of age.

No doubt a good deal of this seeming unhealthiness of our army is caused by the regulation referring to hospital admissions above quoted. And this causes an unfavourable contrast with other armies, not only in the number of admissions but all through the returns of sickness.

For example, in 1862, the French average sick time to each man was only 7·84 days, approaching very closely the average of the civil population; while in the same year the average for our own army, according to the different arms, were as follows:—

Days in Hospital to each sick man (= Duration of Cases).	
Arms.	1862.
Household Cavalry	20·43
Dragoon Guards and Dragoons	16·07
Royal Artillery	19·22
Military Train	17.76
Foot Guards	24·04
Infantry	20·03
Depôt Battalions	19·24

(Compare the difference between these results and those given on page 19 as loss from invaliding of the different arms of the service.)

It would appear from the Statistical Medical Returns for the Navy, that the admissions are generally rather above the average for the Army, but the average sick-time to each man is somewhat less. From the report for 1876 it appears that in that year the total number of cases *admitted* for disease or injury was 53,908, the strength being 45,010, which is in the ratio of 1,197·58 per 1,000 of strength. This is an increase of 38·58 per 1,000 over the previous year. The average duration of each case was 14·58 days. The total number of days of sickness was 786,517, giving an average of loss of service from sickness equalling 17·47 days per man calculated on the total strength, being an increase of 0·57 days over the average for the previous year.

As we are dealing in this Essay only with the Effects of

Health and Disease on Warfare, it is not necessary to give details of the *mortality* of the public services.

We see from the above statements that the *prevalence* of disease in them is still very considerable, but we must also admit the vast improvements which have taken place when we compare their sanitary condition at the present time with that of former years. The first systematic inquiry into the health of our army was instituted by the late Sir James M'Grigor, shortly after his appointment as Director-General of the Army Medical Department in 1814.

In 1835 Dr. H. Marshall, associated with the late General Sir A. Tulloch, K.C.B., was appointed by Lord Howick (then Secretary for War) to put these various accumulated statistical records into shape.

In 1836 Dr. T. Graham Balfour, F.R.S., with Sir A. Tulloch, brought out a statistical report on the health of the army, and continued to do so at various times up to 1853. From these we are able to judge of the improvements in army sanitation which had already at that time commenced.

A condensed history of the earlier sanitation of the British Army is given in Dr. Graham Balfour's "Summary of the Reports on the Health of the Army published previous to 1859," which was prepared by him for the Statistical Congress by order of the Secretary of State for War, and published in the Army Medical Department Report for 1860; and from which the table on the following page is taken.

The Crimean War prevented further publication till 1859, since when the sanitary statistics of the army have been regularly published in the form of annual Parliamentary Reports.

While we have no official or systematic inquiry on the prevalence of army diseases, to compare with our present returns, prior to those of 1814, we find occasional records of the results of private observations mentioned promiscuously by various authors. As, for example, in Robert Hall's "Reflections on War":—"We must remember, however, that as a very small proportion of a military life is spent in actual combat, so it is a very small part of its miseries which must be ascribed to this source. More are consumed by the rust of inactivity than by the edge of the sword; confined to a scanty or unwholesome diet, exposed in sickly climates, harassed with tiresome marches and perpetual alarms,

STATIONS.	Periods of Observation.	White Troops. Annual ratio per 1000 of strength.		Black Troops. Annual ratio per 1000 of strength.		Periods of Observation.	White Troops. Annual ratio per 1000 of strength.		Black Troops. Annual ratio per 1000 of strength.	
		Admitted.	Died.	Admitted.	Died.		Admitted.	Died.	Admitted.	Died.
United Kingdom. Household Cavalry	1830–36	—	14·5	—	—	1837–46	—	11·1	—	—
United Kingdom. Cavalry of the Line	—	929	15·3	—	—	,,	961	13·6	—	—
United Kingdom. Foot Guards	—	—	26·6	—	—	,,	862	20·4	—	—
United Kingdom. Infantry of the Line	—	—	—	—	—	,,	1044	17·9	—	—
Gibraltar	1818–36	966	22·3	—	—	1837–56	976	12·9	—	—
Malta	1817–36	1142	18·7	—	—	,,	1128	18·2	—	—
Ionian Islands	—	1201	28·3	—	—	,,	1168	17·9	—	—
Bermudas	—	1310	35·4	—	—	,,	1080	35·5	—	—
Canada	—	1097	20·0	—	—	,,	950	17·2	—	—
Nova Scotia	—	820	17·8	—	—	,,	836	15·1	—	—
Newfoundland	1825–36	—	37·6	—	—	,,	698	11·0	—	—
Windward and Leeward comb[d].	1817–36	1903	81·5	820	40·2	1837–53	1892	62·5	804	32·2
Jamaica	,,	1812	128·0	—	30·0	1837–55	1371	60·8	784	38·2
Sierra Leone	1819–36	2978	483·0	812	30·1	—	—	—	—	—
St. Helena	6 years	738	25·4	—	—	1837–56	906	12·3	—	—
Cape	1818–36	991	15·6	—	—	1838–56	875	15·9	—	—
,, Frontiers	1822–34	866	9·8	823	12·5	—	—	—	7 5	17·5
Mauritius	1818–36	1249	30·5	—	—	1838–55	909	24·0	—	—
Ceylon	1817–36	1678	74·9	1059	26·9	1837–56	1407	38·6	1005	22·7
Bengal	—	—	75·6	—	—	1838–56	2047	76·2	—	—
Madras	—	—	76·1	—	—	,,	1741	41·5	—	—
Bombay	—	—	62·8	—	—	,,	2117	60·9	—	—
Tasmania	—	—	—	—	—	1839–56	726	11·8	—	—
New Zealand	—	—	—	—	—	1844–56	529	12·8	—	—

their life is a continual scene of hardships and dangers. They grow familiar with hunger, cold, and watchfulness. Crowded into hospitals and prisons, contagion spreads among their ranks, till the ravages of disease exceed those of the enemy."

The first systematic investigation of the sanitary condition of armies was undertaken by John Howard, who may be called the pioneer of Sanitary Reform.

Those who have laboured and written on these matters—Tulloch, Balfour, Lord Herbert of Lea, Professor Parkes, and others of our later army statisticians and sanitary reformers—may, as compared with Howard, be called workers in the mint; but Howard was the worker in the mine. It is one thing to sit in a comfortable apartment drawing up Tables and calculating averages; and quite another to visit personally the offensive and loathsome (as they were in the days of Howard) scenes of sickness and crime; to obtain ocular demonstration of such facts as may arouse public attention to them and cry out for their amelioration.

In the last of these journeys undertaken by Howard for the purpose of visiting the abodes of wretchedness, and which was brought to an abrupt close by his death at Cherson, in January, 1790, we find that he paid particular attention to the condition of the military prisons and hospitals of the countries through which he passed.

We find him passing through Holland, visiting the penal and charitable institutions of Amsterdam, Osnaburgh, and Hanover; then proceeding on the same mission, to Berlin, Spadan, and Konigsburgh; thence, through Memel and Mittau, to the Russian territories, which he entered by way of Riga.

Here, on visiting the military prison, he found "three hundred sick crammed into two dirty and offensive wards; and the arrangements in general so faulty that he felt no surprise when told that five hundred recruits had lately died there" (Field).

Passing on from Riga, Howard visited the various institutions of St. Petersburg. He had gone over this ground before in 1782, and had published his observations in his "Foreign Prisons," and in the appendix to the second edition of his "State Prisons." On the 9th of September, 1789, he visited the Marine Hospital of Cronstadt. The suffering he here witnessed, caused by the carelessness and neglect of those supposed to be in charge of these institutions; the want of light and ventilation, and extreme filthiness of the wards; together with the scanty and improper food supplied to the wretched patients, drew from Howard the observation, "With what concern must a feeling mind be struck, when many objects are looking up for help, and the possibility of a cure is thus cut off."

Continuing his journey, he reinspected the Prison of Tver, which he found in a miserable condition. On reaching Moscow he visited the "Ostrog," then recently established, in which the sick were much neglected, although not to so great an extent as in the other institutions.

He left on record that the Military Hospital at Moscow was at this time very defective, both as regards its organization and accommodation. In a letter to his friend Dr. Price, dated Moscow, Sept. 22, 1789, Howard states:—"I arrived a few days ago in this city, and have begun my rounds. The hospitals are in a bad state: upwards of seventy thousand sailors and recruits died in them last year." Another letter written about this time, and containing the same statement, is thus referred to in the *Gentleman's Magazine* for January, 1790:—"Mr. Howard, we are happy to hear from a friend of his, who has received a letter from him, was in good health and spirits on the 17th of November, at Cherson, in Little Tartary, visiting the Army and Naval hospitals in that part of the Russian dominions, after having visited those of Riga, Cronstadt, &c., which he found throughout in such sad order as would have given credibility, had it wanted it, to the information he had received from good authority that no less than the shocking number of seventy thousand recruits, sailors, and soldiers had died in that country in the course of the preceding year; owing, undoubtedly, in a great measure, to inattention, ignorance, and inhumanity, whose influence is always checked, at least, if it cannot be overcome, by his persevering benevolence, his fortitude, and his skill; wherever human misery attracts this friend to every clime, this patriot of the world." While at Moscow, Mr. Howard was informed of the deplorable condition of the Russian Military Hospitals; and of the neglect, and altogether infamous treatment, to which the patients in them were subjected. With the object of ascertaining for himself to what extent these reports were correct, he undertook the journey to Cremeutsehnok, on the Dnieper, at that time a considerable garrison, where he found four hundred recruits, suffering from scurvy, crowded together in a small building, and receiving as their only food "a sort of water gruel, sour bread, and still sourer quas." He learned that the mortality in this institution (which can hardly be called a *hospital,* except on the principle of *lucus a non lucendo*) was excessive; and that it was, from time to time, nearly emptied by epidemics of putrid fever. Referring to this place, and to the hospital at Cherson, Howard remarks, in his notes, that the only attendants on the patients were "men sent from the different regiments, as being useless from stupidity or drunkenness."

He further remarks, "The primary objects in all hospitals seem here neglected viz;—cleanliness, air, diet, separation, and attention. These are such essentials that humanity and good policy equally demand that no expense should be spared to procure them." Howard passed on from Cherson to Witowka, where he witnessed the greatest suffering in a military hospital, owing entirely to the culpable neglect of the authorities.

He observes, "When I saw so many brave fellows, who had fought so well for their country before Otschakow, suffered to perish here with filth, neglect, and vermin, how did my heart melt within me!"

Field states, "The few who survived such cruel negligence, and who when convalescent were discharged from these wretched abodes, were destined to further perils, and exposed to death in a still more frightful form. Howard saw a number of these, just recovering, miserably clad, wet, and shivering, under orders to walk several miles to the next town; and felt no surprise when he learned that some were seen lying dead by the roadside. Nor could he wonder at the crowded state of the hospitals, when he became acquainted with the miserable quarters in which the Russian soldiers passed the winter. Instead of being lodged in barracks, or even encamped, as the troops of other countries, they were encaved and buried in damp and dismal holes covered with sticks and earth; with only a hole at top for the supply of air and the escape of smoke."

These glimpses of the miserable neglect of all sanitation—not so much from want of knowledge as from the utter carelessness of those in authority—is almost incredible in an empire at all removed from savage barbarism, when we reflect on the resulting waste, both of life and political power.

I quote again from the narrative of Field: "Howard's attention was next directed to a hospital for recruits and prisoners of war, consisting of four rooms, near the new town of St. Nicholas, which was then building. Three hundred poor objects were crammed into these confined apartments; their food was black and heavy, and their quas sour. The inspection was made by appointment, and our humane countryman was accompanied by the Brigadier Falagef and a physician sent by Prince Potemkin. Preparation was, therefore, made for the reception of the visitors. A partial cleansing had taken place, new coverlids had been distributed, and the surgeons were in attendance.

"To the government officials all seemed satisfactory; but such an inspection was little in accordance with Howard's practice. Experience had taught him that it was well to make some previous inquiries, and then to pay an unexpected visit. The latter had been prevented in this instance, but the former preliminaries had neither been forgotten nor neglected. He ascertained that, instead of the reported three hundred, there were five hundred patients in this hospital.

"Suspecting, therefore, that some were concealed when those prepared for inspection had been shown, he requested to see the remainder. Surprised at the demand, but perceiving that their sharp-sighted visitor was not to be deceived, after a time permission was granted, and several officers accompanied him in his further investigation." ("Life of John Howard," p. 462.) The result of this further investigation, which led to the discovery of about two hundred patients hitherto concealed from the inspection, is thus described by Brown: "He found fifty objects of such extreme wretchedness as, in the whole course of his extensive visits to the abodes of misery and vice, he had never before seen together. Most, or all of them, were recruits, in the prime of life, many of whom were dying upon a bed of hard, coarse reeds, without linen or coverlids, with nothing, indeed, but a few remnants of their old clothes to cover them, their persons dirty beyond description, and with their shirts in rags. With every kindlier feeling of his nature shocked at so barbarous a scene, our intrepid countryman turned to the officers at his side, and, directing their attention to their fellow-creatures who were thus inhumanly treated, told them, in a tone of the bitterest reproof, that 'in none of the countries he had ever visited had he found so little attention paid to the military as in Russia. He knew, however,' he added, 'that what he said would have no other effect on them but to make them despise him, but he should assuredly relate what he had with so much concern and indignation beheld.' As he had anticipated, his military auditors immediately left him." ("Memoirs of Howard," p. 621.) Mr. Field further states that drunkenness was common amongst the few attendants employed in these hospitals visited by Howard; and that on seeing one of them with a bottle holding two quarts of brandy, the sight drew from Howard the following remarks:—"How many patients do I see with many disorders, which, I am persuaded, proceed from the use of spirituous liquors! What strict care should be taken that the attendants do not bring any to sell in the hospital! Have I not seen unmixed spirits served round to sick and dying patients, by persons intoxicated themselves, when, to my great surprise, I was told that the physician had ordered it as a treat to the patients! If my visits had any share in promoting this, I fear I killed half a dozen of them, or, at least, put them some days sooner out of their misery." And, again, referring to the recruits of the Russian army, the same authority quotes the words of Howard:—"Compare the condition of these poor destitute wretches, forced from their homes and all their dearest connections, with that of those to be seen cheerful, clean, and happy at a wedding or village festival: let them be viewed quitting their birthplace, with all their little wardrobe, and their pockets stored with roubles, the gifts of their relations, who never expect to see them more; now joining their corps in a long march of one or two thousand versts, their money gone to the officer who conducts them and defrauds them of the Government allowance; arriving fatigued and half naked in a distant, dreary country, and exposed immediately to military hardships, with harassed bodies and dejected spirits; and who can wonder that so many droop and die, in a short time, without any apparent illness? The devastations I have seen made by war among so many innocent people, and this in a country where there are such immense tracts of land unoccupied, are shocking to human nature."

During the few last months of his life, Howard witnessed the effect of the disease and unsanitary condition of the Russian army on its strategical operations. After the fortress of Bender had been taken from the Turks, an epidemic of a putrid form of fever raged with much virulence among the Russian troops, and the Russian commander found it impossible to prosecute further operations at that time.

This same epidemic has a melancholy interest for those who appreciate the character and labours of Howard. Military operations being for a while suspended, the officers of the Russian army were given permission to visit Cherson, where Howard had taken up his residence. As a consequence, the epidemic from which the army suffered spread to the town, and Howard was requested to visit a lady attacked by it, as his knowledge of medicine had gained some repute. He complied with this request, and almost immediately was attacked with the same disease, which proved fatal (Field).

It would be unprofitable to dwell further on the observations made by Howard on the very unsanitary condition of the military prisons of Russia and other countries. From what has been said it will be seen that matters could not be much worse than they were. The sanitary arrangements of our own military and naval establishments were not much to boast of at the same time, nor, indeed, for some years after. The disasters of the British army in Holland, in 1748; of the ill-fated Walcheren expedition in 1809 (although these were certainly in some measures attributable to climatic causes); of some parts of the Peninsular campaign; and, I fear I may add, of certain passages of our Crimean War, are episodes in the history of the military sanitation of this country over which, perhaps, it is as well to draw a veil. Yet we must bear in mind that, however good sanitary arrangements may be, they must to some extent fail in actual warfare; or rather, I should say, the sanitary conditions of peace being inadequate to the requirements of warfare, have to be largely increased and supplemented by measures with which nations in peace are not conversant, and which for want of practice it is difficult to have properly carried out. Thus it is that in war the best organized and most carefully planned sanitary arrangements must seem to fail to some extent.

The military sanitary arrangements of the seven days' war of 1866 were on a well and largely organized scale on both sides, yet fell very far short of the actual requirement. A letter dated Pardubitz, July 28, 1866, relates what was only an apparent instance of disorganization and neglect: "It is, I think, the side-scenes which make the drama of war so very horrible. About actual fighting there is something grand at all times. The parade of a great army has a true grandeur of its own, and even the muster-roll of dead and wounded is not altogether a mere bill of bones and blood, as sentimental declaimers against the horrors of war like to describe it. So even in a great military hospital there is an air of order and self-command which modifies its horrors. But all this is wanting in one of these roadside halting-places for the sick, of which I have seen so many on this journey. At Prague, for instance, the railway waiting-rooms, the hall, and passages of the station were all occupied by wounded men when I arrived there last night. The men were to be moved on again in the morning, and so straw had been littered everywhere about for them to take what rest they could. Turning out of the brilliantly-lighted glass-covered platform where the train arrived, I had to grope my way in dim half-light across this temporary hospital. There, upon the straw, moaning, writhing, breathing heavily, lay a dense mass of human beings. I daresay they had been attended to, but it seemed as if there was nobody to look after them.

"In the half-lit, lofty halls you could distinguish little except the white linen bandages bound around heads and arms. You could hear nothing except an indistinct, guttural murmur of men in pain. The only sign by which you could tell that the men around you were wounded was the sickening smell peculiar to wounds slow at healing. And what was going on there in Prague was doubtless going on in well nigh every station from Vienna to Berlin."—Dicey's "Battle Fields of 1866."

Even now, as I write, in spite of our increased sanitary improvements; of our well-organized relief societies; of all the care and help which the civilization of to-day holds out for the relief of the sick and wounded in war · the Angel of Death is as peremptory as ever in directing the strategy of campaigns. The *Times* publishes a telegram dated Giurgevo, January 16: "On passing the hospital tents which cover the plain around Fratesti station . . . . we found a party of Russian soldiers digging huge graves, and near them four ghastly piles of Turkish dead, numbering at least fifty bodies, lying in heaps of four or five feet in thickness, with their ragged and half-clad forms entangled in horrible confusion, as they had been emptied out from the dead-carts. The mounds in other places showed that this fearful pile represents days of mortality, and the frost-bites visible on the naked limbs gave evidence of the hardships these poor fellows had suffered on their march from Nikopol. The mortality has been fearful among these prisoners of war, and the plains of Fratesti are becoming thickly dotted with mounds covering the mortal remains of those who defended Plevna. The station of Fratesti is becoming a terror to travellers between Bucharest and the Danube; and, even if there is nothing worse than typhus lurking round those canvas dwellings, the mortality, induced by the graves of those who have passed away, justifies the feelings of apprehension felt by those who pass by this vast abode of sickness and death." And the *Lancet*, of January 19, writes: "Of the existence of serious typhus among the Turkish forces, and of its rapid dissemination, there is no longer any doubt. There is little doubt, moreover, that the disease exists also among the Russian forces in Bulgaria, and it is to be feared that the recent successes of these forces will tend to give the malady a larger development, for the large number of prisoners captured will, not unlikely, lead to an importation of the serious typhus prevalent among the Turkish armies within the Russian lines. Unless, indeed, peace be now declared, it will scarcely be possible to prevent typhus being scattered broadcast, not only among the belligerents, but throughout the tracts of country traversed by them."

As regards the sanitary arrangements of armies in time of peace, we find a vast improvement in the present day as compared with the times described by Howard. Ordinary sanitary duties, the duties of medical officers in garrison and camp, and those relating to hospitals and prisons, are carefully laid down and strictly adhered to. A reference to the new Medical Regulations for the Army, recently published, will show how carefully our troops are cared for in the matter of general health, food, exercise, clothing, habitations, cleanliness, drills, &c.

And the same as regards men in military hospitals. "The officer in medical charge will daily attend to the sanitary condition of the hospital under his charge. He will, in communication with the proper authorities, take steps to ensure that the vicinity of the hospital is in a good sanitary state; that the surface is properly drained, cleared, swept, and free from nuisance; that the water supply is good and abundant; that the latrines are in an efficient state, &c.; that the ventilation of the wards is at all times sufficient and unimpeded; that the hospital is kept in a proper state of repair; that the walls are cleaned and lime-washed according to regulation."

As regards military prisons, the medical officer's duties consist in "visiting daily the cells or prison, and inspecting the inmates, ascertaining that the sanitation of the establishment is in all respects thoroughly attended to; that the work or exercise is in no case excessive or unsuitable to the constitution or bodily condition of the prisoners; that the diet is good in quality, and strictly according to regulation; and he will arrange that all sick men, whom it may be absolutely necessary to remove, are taken to hospital for treatment." The following sanctioned scale of diet for soldiers under sentence of imprisonment in military prisons is a satisfactory contrast to the sour bread and quas, and deficiencies in quantity and quality of diet recorded by Howard:—

Days.	Breakfast.	Dinner.	Supper.
Sundays ... Wednesdays Fridays ...	Bread . . 8 ozs. Milk. . . 8 ozs. Soojee* . 2 ozs. Sugar . . 1 oz.	Meat with bone . 1 lb. Barley and onions 4 ozs. Potatoes . . . . 12 ozs. (The above to be made in soup.)	Same as breakfast
Mondays ... Saturdays	Tea . . . 1 pt. Milk. . . 2 ozs. Sugar . . 1 oz. Bread . . 8 ozs.	Meat with bone. 8 ozs. Split peas. . . . 4 ozs. (The above to be made into soup.) Bread . . . . . . 8 ozs.	Ditto
Tuesdays......	Same as Sunday	Rice . . . . . . 6 ozs. Milk. . . . . . . 1 pt. Bread . . . . . 8 ozs.	Ditto
Thursdays ...	Same as Saturday	Potatoes . . . 20 ozs. Milk . . . . . 1 pt. When good potatoes are not procurable, 1 lb. of bread may be substituted.	Ditto

As regards the *causes* of army diseases we find that some of them are capable of removal, or are amenable to sanitation.

They have been thus classified by Dr. Parkes ("On the Causes of Sickness in English Wars," *Journ. of Roy. Unit. Serv. Inst.*, vol. vi.).

1. A defective commissariat, especially as to food and fresh vegetables, causing diseases, but mainly predisposing to many more.

* In India.

2. Undertaking military operations in an unhealthy site, and with an unhealthy season impending.

3. Exposure to cold, with insufficient clothing and food.

4. Propagation of disease poison, favoured by bad ventilation, overcrowding and filth.

5. The enlistment of boys as soldiers, whose bones are not yet matured.

6. Want of cleanliness, excessive use of spirits, debauchery, &c.

It will be observed that the majority of these disease causes are, to a great extent, subject to control.

It is to be hoped that, with our increased knowledge of the nature of these causes, the future of our army sanitation will continue to present an ever-increasing contrast to that of the past, which we have shortly sketched in this section.

SUBSECTION B.—DISEASES WHICH ARE THE INEVITABLE RESULT OF WARFARE.

Under this heading come wounds—gunshot and incised. The subject of this Essay being the effect of health and disease on the operations of war, and disease generally meaning sickness, and a distinction being popularly made between *sick* and *wounded* in war; perhaps, strictly, the subject of wounds does not come within our province. Nevertheless, I briefly allude to it here for completeness in our review. I have before stated that the changes which have been necessitated in tactics by the use of firearms of increased range and precision, have reduced the number of killed and wounded in action. This, of course, it will be understood, is the indirect result of improved rifled arms; the direct result would be a great increase in killed and wounded. This results not only from the increased range and precision, but also from the nature of the projectiles used with rifled ordnance. The effect of the conico-cylindrical bullet of the rifle is very different from that of the round bullet of the old smooth bore. In the days of the old "brown Bess" it was not unusual to see the bullet turned aside by soft and yielding obstacles, and it frequently happened that a bullet, having struck the chest, ran round the ribs externally beneath the skin, making its exit at the back; or, having entered a limb, it ran round the bone without shattering it. Now, however, it is very different. The conico-cylindrical bullet pierces right through where it strikes, and when a limb bone is struck, the wedge-like point of the projectile splits the bone up to its extremity, and thus opens the joint. The losses (other than immediate deaths) resulting from the introduction of rifled firearms are mainly due to the latter cause. The effect is that the number of men carried away to hospital at bases of operations are more numerous, and the loss afterwards, not on the field, but from the effects of wounds, is excessive.

Such wounds have usually to be treated by secondary amputation, and in this way permanent loss, although not necessarily by death, results from the use of the rifle. During the siege of Sebastopol, among 80,000 wounded Russians, there were 3,000 such amputations. The number of wounded, whether from ordnance fire or bayonet charge, has a very decided effect on future strategical operations, unless there is ample facility for sending them out of the way to some point of defence. Hence the necessity of improved or altered tactics to counteract the terrible effects of modern arms. Other diseases invariably result when a large number of wounded men are brought together, such as erysipelas, hospital gangrene, &c., so that no very distinct line can be drawn between the diseases included in this section and those of the next.

SUBSECTION C.—DISEASES WHICH ARE FREQUENTLY COINCIDENTAL WITH THE CONDITIONS OF WARFARE.

The diseases which most frequently occur in armies on active service may be grouped according to their causes as follows :—

(*a.*) Effects of aggregation.
(*b.*) ,, climate.
(*c.*) ,, privation (failure of supplies).
(*d.*) ,, fatigue.
(*e.*) ,, youth and age.

(*a.*) The most important diseases of the first group are erysipelas, hospital gangrene, typhus, yellow fever, and cholera. The mortality from erysipelas and hospital gangrene should be put down to wounds. The deaths from wounds and resulting disease are much more numerous than those from slaughter in the field. The length of time such cases have to be kept in hospital, as well as the loss of strength caused by them, has a very decided influence on a campaign. "Typhus fever is a disease as old as the disputes of nations," and is the effect of overcrowding in temperate climates, as yellow fever and cholera are the effects of aggregation, in tropical climates. The history of the effects of this one disease on military operations is in itself an extensive study. It has always been the scourge of Continental armies. It destroyed, in 1489, 17,000 of the troops of Ferdinand besieging Granada. In 1552, it devastated the army of the Emperor Charles V. during the siege of Metz. In 1556, we find it under the name of the notorious *Morbus Hungariens*, decimating the army of Maximilian II., and spreading over the whole of Europe. In 1620, the Bavarian army, serving in Bohemia, lost 20,000 men from typhus. In 1628, and again in 1632, it ravaged the Swedish army under Gustavus Adolphus, and almost annihilated the population of Northern Germany. In 1643, typhus broke out in the garrison of Reading, commanded by Charles I., and in the Parliamentary army, under the Earl of Essex. In the wars of Louis XIV., this disease caused enormous loss in the French army. In 1790, the Russian army had to suspend operations against the Turks in Little Tartary, owing to the losses it sustained from putrid typhus. And it was from a form of this disease, here, and at this time, that the philanthropist John Howard died. In 1799, an epidemic of typhus decimated the garrison of Genoa and the French army which was besieging it. The greatest ravages of this disease, which have been recorded, occurred in the armies of the first Napoleon, particularly at Saragossa, Torgan, Dantzic, and Wilna in 1803, and during the retreat from Moscow in 1812–13.

Typhus was the chief destroyer of the Bavarian army serving under the French at the same time. In May, 1812, it was 28,000 strong; in February, 1813, there were only 2,250 men under arms. In August, 1813, the first Prussian army consisted of 37,728 fighting men, having lost 16,000 men by the sword, and 10,000 from an epidemic of typhus. In Mayence, alone, of 60,000 French troops composing the garrison in 1813–14, there died of typhus in six months 25,000 men.

In the French and Russian armies in the Crimea, during and after the siege of Sebastopol, typhus committed great ravages. It had prevailed in the winter of 1854–55 amongst both the English and French troops; but in the following winter it was mainly confined to the French and Russian armies.

"In the spring of 1856 it was computed that more than 17,000 men of the French forces perished in less than three months, chiefly from typhus; and the highest authority stated that the safety of the whole French army was endangered by the outbreak" (Prof. Aitkin). (The above statements are principally taken from a Paper "On the Causes of Sickness in English Wars," by Prof. Parkes, in the *Journal of the Royal United Service Institution*, vol. vi.) The *Lancet* of Feb. 16, 1878, states:—"It is well known that for some time typhus and dysentery have existed among both the Russian and the Turkish soldiers; and there is little doubt that both these diseases have, during the winter campaign, assumed formidable proportions in both armies. Under the recent reverses of the Turkish armies, indeed, these grave maladies threaten the entire disorganization of the forces not yet disabled.

"On the Russian side, the ravages of these diseases are scarcely less serious. Of the number of sick among the Russian forces in Roumelia and Bulgaria, there is no precise information; but as to the sick of the Russian army in Asia Minor, a telegram from Tiflis to the journal *L'Agence Internationale*, dated the 30th of January, states that at that date the number of sick in hospital of the army in the field—the greater number suffering from typhus—was 24,000; 7,000 at Hassan-Kale, 5,000 at Kars, and 12,000 at Erzeroum. . . . . . Not a few of the military hospitals in Trans-Caucasia have been disabled by the medical and nursing staffs having been attacked with the disease. . . . . There is now, in fact, taking place at the seats of war a phenomenon similar to that observed at the close of the Crimean war."

Yellow fever has not much influenced the operations of actual warfare. Its ravages have been principally experienced by the navies of France, Spain, and Portugal; and in the West Indies. It is not likely to influence the operations of future wars.

The endemic area of cholera—which is described as bounded on the east by the 91° or 92° of east longitude, on the west by the 85° or thereabouts, passing a little to the west of Patna, on the north by the 27th parallel of latitude, on the south by the coast of the Bay of Bengal, including the deltas of the Ganges and Mahanuddy rivers or from about Chittagong round to Porree—being in British territory, the effects of this disease on our army is of importance. Its visitations have, and still do, frequently reduce the strength of our Indian army.

It has not caused as much disaster to our armies on active service as may have been expected. Still it has at times exerted considerable influence on our military operations; as at the commencement of the Crimean war. "Another and more dreadful enemy had now entered the camp of the allies. From the period of its arrival in the Levant, the French army had been suffering much from sickness. In the British army, on the contrary, though slight complaints were not infrequent, the bodily condition of the men had been, upon the whole, very good; and so it continued up to the 19th of July. On that day, out of the whole Light Division, there were only 110 in hospital. But it seems that one of the omens which portend the visitation of a great epidemic is a more than common flush of health. With the French, the cholera first showed itself on board their troop-ships while passing from Marseilles to the Dardanelles. It then appeared among the French quartered at Gallipoli and followed their battalions into Bulgaria. There its ravages increased, and before the beginning of the last week in July, it reached the British army. By the 19th of August our regiments in Bulgaria had lost 532 men. But it was amongst the three French divisions marched into the Dobruja, and especially in General Canrobert's division, that the disease raged with the most deadly virulence. In the day's march, and sometimes within the space of only a few hours, hundreds of men dropped down in the sudden agonies of cholera; and out of one battalion alone, it was said that besides those already dead, no less than 500 sufferers were carried alive in the waggons. On the 5th of August it was computed by an officer of their staff, that out of the three French divisions which marched into the Dobruja no less than 10,000 lay dead, or struck down by sickness. If the cholera had been confined to the land forces, the generals would not, perhaps, have allowed it to delay their embarkation; but it now reached the fleets. In a few days the crews were in such a state that all idea of attempting to embark the troops was, for the moment, out of the question; and on the 11th and 12th of August, the admirals put out from their anchorage, in the hope of driving away the disease with the pure breezes of the sea. But they had scarcely done this, when, on board some of the ships, the mysterious pest began to rage with a violence rare in Europe. The *Britannia* alone lost 105 men. The number of those stricken, and of those attending upon them, was so great, that it was impracticable to carry on the common duties of the ship in the usual way; and if the disease had continued to rage with undiminished violence for three days more, there would have been the spectacle of a majestic three-decker floating helpless on the wave for want of hands to work her" (Kinglake's "Invasion of the Crimea," vol. ii. p. 133). Later on in the campaign this pestilence had its effect, not only in reducing the numerical strength, but in its moral influence on the army. "After a while many of the battalions which had landed were ordered forward to occupy the hill on our right; and thenceforth all day the acclivity was sparkling with the bayonets of the columns successively ascending it. But what were those long strings of soldiery now beginning to come down from the hill-side and to wind their way back to the beach; and what were the long white burthens horizontally carried by the men ? Already ? already, on this same day ? Yes; sickness still clung to the army. Of those who only this morning ascended the hill with seeming alacrity many now came down, thus sadly borne by their comrades. They were carried on ambulance-stretchers, and a blanket was over them. Those whose faces remained uncovered were still alive, those whose faces had been covered over by their blanket were dead. Near the foot of the hill the men began to dig graves" (*idem*, p. 180).

One who has not witnessed a cholera panic (as the writer has on two occasions) cannot form any adequate idea of its moral effect on those who are unattacked, and the influence of this on the duties to be performed. Should there at any time arise a necessity for our occupying Afghanistan or the Valley of the Tigris and Euphrates, the influence of this disease on operations there is to be feared, as such occupation would probably be made from India through Peshawar and Kyber Pass, or through Scinde by the Bolan Pass, and these are the old paths of cholera epidemics, being the great routes of carvans and pilgrimages.

Among the results of (*b*) Climate come the large group of malarial or paludal fevers, hepatic diseases, and the effects of heat and cold. Under the same heading may be considered the effects of (*c*) Privation, because the facility of obtaining food and the quality of supplies is principally influenced by climate. Malarial and liver diseases always occasion a large amount of loss to our army serving in India. Their effects, however, would not be great on active service (as all men likely to suffer from them would first be removed from their corps) unless in a prolonged campaign in unhealthy seasons in unhealthy territory. In the tropics heat must always seriously affect all military operations, both the routine duties of peace, and the movements, strategy, and tactics of actual war. I need not give examples of this. Any one familiar with the military history of British India can recall many instances. But it is not only in the tropics we find excessive heat influencing the operations of war. Many instances of this have occurred in continental Europe. In 1778 the Prussian army, by forced marches, marched in four days to Dresden ; each man carried a weight of eighty pounds ; the weather was hot, the officers were afraid to let the men drink, and no care was taken to obviate the effects of the intense heat. They halted at midday on a burning plain where no water was procurable. " In one night the soldiers looked as if they had aged ten years. Almost at every step lay a fainting man, and entire troops lay on the roadside. The horses also were ruined by this march, almost every hundred steps lay a dead packhorse. In such fashion the entire army marched in four days to Dresden, and getting there exhausted even to death, found the Saxon army fresh and lively." (Riecke, quoted by Parkes.) Prof. Champouillon used to mention the following case in his lectures :—" A regiment of Chasseurs de Vincennes marched twelve miles into Paris on a hot July day. The men carried their packs, and wore their stocks, which at that time were thick and stiff, and compressed the throat. Hardly a quarter of the men reached Paris ; the rest were left in villages, or lying on the roadside. Many died of what was called *coup de soleil*."

Instances of the effects of disease produced by heat on actual (continental) warfare, are to be found in Sir William Napier's " Peninsular War."

Diseases which are effects of cold and privation generally go together. Lebaume has given a graphic account of the effects of such diseases in his account of Buonaparte's campaign in Russia. The defeat of the French army in many battles, noticeably that known as the passage of the Beresina, and the disasters of his retreat, were due to this cause. " Our soldiers, pale, emaciated, dying with hunger and cold, having nothing to defend them from the inclemency of the season but tattered pelisses and sheepskins half burnt, and uttering the most mournful lamentations, crowded the banks of this unfortunate river." Climate, privation, and (*d*) Fatigue, acting either separately or together, produce a state of degraded health, to which, perhaps, no definite name can be given, but which exercises a very marked effect on the operations of war.

It is experienced, more or less, in all campaigns. Its influence was marked at the commencement of the Crimean war. "Grievous sickness fell upon that part of our camp which had been pitched in the midst of the beauteous scenery of the lake of Devna, but the whole English army at this time began to show signs of failing health. It appeared that, even of the men out of hospital and actually present under arms, hardly any were in the enjoyment of sound health—hardly any were capable of their usual amount of exertion. This weakly condition of the men was destined to act with other causes in bringing upon the army cruel sufferings; and it may be asked whether, with the soldiers in this condition of body, it was right to undertake an invasion. The answer would be this: the medical authorities thought, and with apparently good reason, that for troops sickening under the fierce summer heats of Bulgaria, the sea voyage, the descent upon another and more healthy shore, and, above all, the animating presence of the enemy, would work a good effect upon the health of the men, and although these hopes proved vain, they seemed at the time to rest upon fair grounds. And, after all, it is hard to say what other disposition of the troops would have united the advantages of being better and possible. To remain in Bulgaria, or to attempt to operate in the neighbourhood of the Danube, was to linger in the midst of those very atmospheric poisons which had brought the health of the army to its then state; and on the other hand, our people at home would hardly have borne to see the army sent back to Malta, and forced to recede from the conflict, for the bare reason that some of the men were in hospital, and that the rest, without being ill, were said to be in a weakly condition." If such were the effect of climate merely, what would have been the result had the effects of privation and fatigue been added?

To the effects of (*e*) Youth and Age among soldiers and sailors, I have already alluded (p. 35). I have stated how different ages operate on the conditions of health, particularly on strength and endurance. Youth predisposes to some diseases, especially typhoid fever, the average age of which is 21 years. Besides this, young men under 25 years of age are more susceptible to the effects of those insanitary conditions which must always, to some extent, exist as a result of the exigencies of warfare.

SUBSECTION D.—DISEASES WHICH MAY AFFECT THE OPERATIONS OF WAR, BUT WHICH ARE INDEPENDENT OF THE CONDITIONS OF WARFARE.

Under this head, we have Diseases of the Heart and Lungs, and the various fevers which cannot be said to be due to the insanitary conditions of warfare. Men who are known to suffer from organic affections of the heart and lungs are, of course, never sent on active service, nor, indeed, retained on the peace establishment. But diseases of these organs are, not infrequently, developed by the duties of war, where a latent tendency to them exists; or those duties develop those diseases fully in cases where they before existed in so slight a degree as to escape notice.

The duties of peace—running drill, &c.—do not always bring out these diseases of the chest. Any weakness of this kind which interferes with *wind* is serious in active operations, especially skirmishing. It would seem desirable that specially trained and selected men should be set apart for such duties. The men of our Rifle Brigade are not specially selected nor suitably trained, as compared with the French *Chasseurs à Pied,* who are picked active strong men, taken from mountainous districts, such as Auvergne, the Vosges, the Ardennes, the French Alps, the Pyrenees and Corsica. They are practised much in the *pas gymnastique,* or running considerable distances in a peculiar step, which strengthens the lungs, and brings out any chest weakness, points of great advantage in all troops intended to act as skirmishers.

Sometimes febrile diseases, other than those before referred to, occur largely during campaigns, and influence operations.

Formerly, before vaccination was practised, epidemics of small-pox caused great losses in armies in the field. The earliest record we have of small-pox influencing the operations of war, is in the following passage from the Korahn:—"Hast thou not seen how thy Lord dealt with the army of the Elephant? Did he not cause their stratagem to miscarry? And he sent against them flocks of little birds which cast upon them small clay stones, and made them like unto the stubble which the cattle have eaten." This passage refers to the discomfiture of Abraha's army in the year of Mahomet's birth. In its train was an elephant which gave its name to the event commemorated. The seeming miracle of the birds and stones was poetic language for an epidemic of small-pox, which, according to Wâckidi, the oldest of Mahomet's biographers, " first began" on this occasion. The Arabic word for small-pox likewise means small stones, and on this coincidence tradition began to build, and the poetic language of the Korahn gave birth to the above legend. (Sir W. Muir, LL.D., "Life of Mahomet.")

CHAPTER IV.

THE MANNER IN WHICH HEALTH AND DISEASE AFFECT THE OPERATIONS OF WAR.

"At Flores in the Azores, Sir Richard Grenville lay,
And a pinnace like a flutter'd bird came flying from far away;
'Spanish ships of War at sea, we have sighted fifty-three.'
Then sware Lord Thomas Howard, 'Fore God I am no coward,
But I cannot meet them here, for my ships are out of gear.
And the half my men are sick; I must fly, but follow quick;
We are six ships of the line, can we fight with fifty-three!'"

TENNYSON.

It now remains for us to consider the manner in which Health and Disease affect the operations of war. There are various ways in which the mode of action of disease may be considered. It may be broadly defined as consisting of three modes of action—namely: (1) Preventing operations; (2) Retarding operations; and (3) Causing failure of operations.

Health or fitness acts, of course, in the contrary direction; so that the operations of war, military and naval, are performed more quickly and successfully.

Perhaps, for our present purpose, a more practical and convenient method of viewing the effect of Disease on the operations of war is to consider it as producing its effects in two ways:—

(1) By reducing the number of men available for duty; and

(2) By reducing the efficiency of men actually at duty.

SECTION I.—THE EFFECT OF DISEASE IN REDUCING NUMERICAL STRENGTH.

The numerical strength of the armies which contending nations put into the field influences the operations which they perform by equivalence or disparity of numbers, principally as regards the points mentioned before on page 29; and for all practical purposes it matters little to a commander why his force is small, or how it is reduced, if the reason of its inferiority, or the causes of its reduction, are brought about by agencies beyond his control. Under such circumstances he can only accept the position, and work what force he has as best he may.

But if these agencies are known, and their effects remediable, it becomes an important study to ascertain how they act, and how such effects of them may best be obviated, if the causes themselves cannot be altogether removed. Disease acts in reducing strength in various ways: through conditions of the individual, through conditions of age, or particular service, or arms, or other causes. But the point of practical importance is that Disease produces *Absence*, which may be considered under three divisions :—(*a*) Absence by death; (*b*) Absence by inefficiency from wounds or disease; (*c*) Absence in attendance on sick and wounded.

(*a*) Death causes absence either before the commencement of action or during action.

In former days, when long journeys had to be performed before arriving at the scenes of action, many men died before they saw the enemy. This occurs even in modern times, as we have already seen in the case of the Crimea.

It was a striking feature in the wars of the first Napoleon. In the invasion of Russia in 1812, Alison thus describes the condition of the French army :—"From the want of magazines and the impossibility of conveying an adequate supply of provisions for so immense a host, disorders of every kind had accumulated in a frightful manner on the flanks and rear of the army. Neither bread nor spirits could be had. The flesh of over-driven animals and bad water constituted the sole subsistence of the soldiers ; . . . and before a great part of the army had ever seen the enemy it had undergone a loss greater than might have been expected from the most bloody campaign. . . . . When the stragglers and sick were added to the killed and wounded, the total reached 100,000 men."

The same had occurred in 1810, when Masséna entered Portugal; and having spent some time in the examination of Torres Vedras, recrossed into Spain in April, 1811. After being about six months in the country, he had lost 30,000 men "by want, sickness, and the sword" (Alison). Now, during that six months the only action of any importance was Barrosa, at which the French loss was estimated at under 1,000; so that, of the above loss, some 29,000 must be put down to "want and sickness."

The effect of reduction in strength by death before action is, of course, the same, or perhaps worse in its effects on the action, as if the men dead had never been. The same as if the field had been taken without these men, except that, perhaps, if they had not been counted on, the commander would have avoided or postponed taking the field. Their loss is unexpected, and modifies his plans. Upon them have been spent provisions for which there is no return, and these provisions would have been available for the effective force. Their death also causes panic, or further disease. Death or disabling during action is, of course, the loss which most apparently tells on the results of engagements—that is, actual field work; and formerly in naval encounters, which were terminated by boarding. Loss in the field, or in a naval encounter, does not, however, necessarily influence the success of an entire campaign.

(*b*) Absence by inefficiency from wounds or disease is, in its practical effects on an engagement, the same as absence caused by death. It is seriously embarrassing, however, as regards strategy and supply, when a number of sick or disabled men have to be kept with an army during active operations.

(*c*) Absence in attendance on sick and wounded cannot affect operations now, as it used to do before the formation of hospital staff battalions. There may, however, even still arise necessity for telling off men for the purpose of looking after sick and wounded, which must interfere somewhat with other duties. It is sometimes found imperative to send a force to look for and collect men who have fallen out of the ranks from sickness on a march. It was so after the first march in the Crimean invasion. "Yet now, before the first hour of march was over, the men began to fall out from the ranks. Some of them were in the agonies of cholera. Their faces had a dark choked look; they threw themselves on the ground and writhed, but often without speaking and without a cry. Many more dropped out from mere weakness. These the officers tried to inspirit, and sometimes they succeeded; but more often the sufferer was left upon the ground. It was vain to tell him, though so it was believed at the time, that he would fall into the hands of the Cossacks. The tall stately men of the Guards dropped from their ranks in great numbers. It was believed at the time that the men who fell out would be taken by the enemy, but the number of stragglers at length became very great, and in the evening a force was sent back to bring them in" (Kinglake). Before the duties of looking after the sick were delegated to corps specially trained for this purpose, men left other important duties to attend on sick and wounded. "In the cause of humanity, which may not be neglected even in the most embittered warfare, we are glad to have it in our power to state that now nearly every European army has established a corps of hospital staff. There is no longer any occasion to withdraw line troops from their duties to take care of the wounded. We consider such an employment of fighting men most injurious to the service, and, unfortunately, it is an evil which rapidly spreads, without any chance of being able to check it. During the Schleswig-Holstein campaign more than half the army made it a practice to fall out of the ranks under one pretext or the other. The hospital staff will, henceforth, prevent such laxity of discipline." (Wraxall, "Armies of the Great Powers.")

That this was a real evil, and had a disastrous effect on tactics, may be judged of from the fact that before the 42nd, 79th, and 93rd Regiments of Highlanders advanced up the slope of the Kourganè Hill, Sir Colin Campbell addressed them thus:—"Now, men, you are going into action. Remember this: whoever is wounded—I don't care what his rank is—whoever is wounded must lie where he falls till the bandsmen come to attend him. No soldiers must go carrying off wounded men. If any soldier does such a thing, his name shall be stuck up in his parish church. Don't be in a hurry about firing. Your officers will tell you when it is time to open fire. Be steady; keep silence; fire low. Now, men, the army will watch us: make me proud of the Highland brigade" (Kinglake).

These various effects of disease are, for all practical purposes, simply reduction of numerical strength—absence from duty.

Section II.—The Effect of Disease in reducing the Efficiency of Men at Duty.

Having seen how disease acts in causing absence from duty, we now pass to consider its action on men at duty, those actually present under arms. The working efficiency of a man present with the colours may be reduced by disease in his own individual person, or in the persons of other men. An example of the first is a man placed *hors de combat* by being shot down or disabled; of the second, a number of men (each of whom is himself free from disease and physically in his own person efficient) rendered inefficient by their officers being shot down; or a body of infantry (in itself efficient) being rendered inefficient by death or disablement in the battery which should cover its advance.

These effects of disease may also be conveniently considered under these heads—namely (*a*) Effect on quantity of work done; (*b*) Effect on quality of work done; (*c*) Impediments to work by sick and wounded.

(*a*) When a number of sickly or half-cured men get into the ranks of an army in the field, the result is that the army's efficiency is diminished in proportion to this weakness.

If among 1,000 men there are 100 whose physical strength is only half what it should be—that is, is equal to the strength of fifty healthy men; then the strength of that 1,000 men only equals the strength of 950 healthy men, or, for all practical uses, is only equal to 950 men; and can only perform the amount or quantity of work to be expected from 950 men.

So that, if this 1,000 men be put against 1,000 healthy and robust troops, they are fighting under the same disadvantage as if they were numerically inferior by fifty.

So that for all practical purposes, in actual fighting, inferiority in physical strength is the same as inferiority in numerical strength.

Hence it is evident what a fallacy as regards the probability of the success of his operations a General is led into, who looks only at the numerical strength of his force, without taking into consideration the physical strength of its individual components.

The chance of such miscalculation of success is much increased by the practice of putting men discharged as convalescents from hospital immediately to work with the colours. It may be said, "Then don't discharge men as convalescents; keep them in hospital till fit for the ordinary routine duties of an efficient soldier." But what hospital establishment, even in peace, to say nothing of a field hospital, could stand this strain on its resources? Besides, why should a man be detained in hospital who does not require medical treatment; and be kept on hospital diet, and cooped up in the inactivity and diseased atmosphere of a hospital, when he would become much sooner efficient on barrack rations, and doing some light work outside with his comrades? It would seem a preferable plan to allow an interval to elapse between discharge from hospital and the performance of full duty.

If it is not considered well to do this in peace, it should certainly be done on active service. Men should be sent from rear hospitals to a sort of convalescent depôt, and employed there on some light duties which would fit them for the hardship of work in the front; and should not be put to perform ordinary duty immediately on leaving the comfort and ease which has probably formed an essential part of their treatment in hospital.

Inefficiency by disease, or inferiority in physical strength, is also disastrous in operations other than actual fighting, when time is a factor in the success of these operations. Time is sometimes a very essential element of success in operations of war. Here as everywhere else in physical work the power is in an inverse ratio to the time. If there be plenty of physical energy available to be spent on the work in hand, the less time is required to perform it; if that energy be deficient or inefficient, time is lost.

Take, for example, "quantity" in operations of movement. In the often referred to march of Crawfurd bringing up the 43rd, 52nd, and 95th Regiments of Foot to reinforce Sir A. Wellesley, at the Battle of Talavera in July, 1809, the brigade marched sixty-two miles in twenty-six hours, carrying arms, ammunition and pack—in all a weight of between fifty and sixty pounds. Now, assuming that each man carried sixty pounds, and that a man's weight with his clothes is 150lbs., and that walking on a level is equivalent to raising one-twentieth the weight of the body through the distance walked (Haughton), then walking one mile equals lifting 24·75 tons one foot.

Therefore, in the march referred to, the work done by each man equalled raising 1534·5 tons, one-foot in twenty-six hours; or 590·192 tons in ten hours.

Now, the "one-horse steam power" is 8839 tons, raised one foot in ten hours. So, by a simple calculation it is found that every fifteen men of this brigade kept up, for twenty-six hours, an amount of exertion distributed among them, equal to (slightly over) the one-horse power of a steam engine. The feat is not so much the work as the endurance. But, *the weakly men were left behind*, and the men *had constant training in marching for the previous month.* (Napier, "Peninsular War," vol ii. p. 400.) If the brigade had not been trained or had been physically inefficient, it may not have been up in time to assist at Talavera, and the fortunes of that day may have been reversed.*

One of these same regiments—the 52nd—had to work against time in the same way during the Indian Mutiny. They marched from Umritsur to Godaspore, a distance of forty-two miles, in twenty hours; and the following morning marched ten miles, and fought the mutineers.

Many other examples will occur to the student of military history; as Blucher coming up to Waterloo: and the Peninsular campaign was a series of such.

In most of the operations of warfare the "*quantity of work done*" affects the result. Take field engineering, a class of operations becoming daily of more importance in modern warfare. The sieges of Metz and Paris show that accidents of ground may have to be turned to account at the shortest notice to convert them into a system of investment or defence, where a line of abattis, or of shelter trenches is sufficient to make a successful assault impossible. The success will depend on the sufficiency of *quantity* of work of all such engineering. Had the Moselle been sufficiently bridged in Bazaine's retreat through Metz, on August 15, 1870, he probably would not have had to suffer the morrow's defeat at Rezonville. And the quantity of work done depends in a great measure, on the physical efficiency, as affected by health or disease, of the men engaged.

Then, again, in tactics, perhaps more than in any other class of operations, is the quantity of work done, with reference to the time in which it is done, an essential to success. Illustrations of this abound in every engagement. Thus, we find that, largely through the *quantity* of work done and the *time* which is occupied in doing it, do the physical conditions of men, arising from Health and Disease, affect the operations of war.

(*b*) The operations of war are also in a like manner affected by the efficiency or inefficiency of those engaged on them as regards the *quality* of the work done, as to whether the operation has been properly or improperly performed.

* They took charge of the outposts. Admitting that a man is capable of work equal to 1-7th one-horse power, for a day of twelve hours (which average is generally considered too high), work of 1-15th one-horse power continued for twenty-six consecutive hours is far above the endurance of an average man. Besides, this march was commenced after a march of twenty miles, there being but a few hours halt between. So the march of the brigade was really eighty-two miles.

(c) Disease causes inefficiency of men at duty through impediments to work caused by sick and wounded. This is so when panic from excessive loss spoils an operation. And, also, when loss destroys a formation, particularly a massed column: for example the wavering and defeat of the left Kazan column at the Alma. "The men must be ceasing to feel that the column they stood in was solid. The ranks which had been straight as arrows became bent and wavy. The Russian officers well understood these signs. With drawn swords, moving hither and thither as actively as they could in their long grey melancholy coats, they seemed to become loud and vehement with their orders, their entreaties, their threats. Presently their gestures grew violent, and more than one officer was seen to go and seize a wavering soldier by the throat. But in vain; for seemingly by some law of its own nature, rather than any new stress of external force, the column began to dissolve; the hard mass became fluid. It still cohered: but what had been, as it were, the outlines of a wall were becoming like the outlines of a cloud. First some, then more, then all, turned round. Moving slowly, and as though discontent with its fate, the column began to fall back." In this way, disease, or disabling, is not likely to interfere with the efficient operation of a British line.

These are a few of the ways in which Disease reduces the efficiency of men at duty.

Section III.—The Effects of Diseases of Cattle on the Operations of War.

The diseases of cattle exert a considerable influence on the operations of war. The cattle most used in military operations are—horses, chargers and draught, mules, ponies, donkeys, elephants, camels, and bullocks. The diseases of horses operate in the same manner as diseases of men, that is by causing *absence* and *inefficiency*. Lack of horses influences the number of regiments of cavalry and batteries of horse artillery put into the field.

In an article on "England's Field Army," a recent issue of the *Times* states:—"As to the horses, omitting the draught horses, the establishment is only 572 above our requirements. Practically the supply would not be equal to the demand, for we buy our cavalry horses so young that they are not fit for work till a year later. Besides, some of the horses are too old, others are unfit for hard work. More than thirteen regiments of cavalry, therefore, we could not send out. . . . . Of field artillery we have thirty-nine battalions, including depôt batteries, at home, with about 6,120 of all ranks and 3,900 horses. Twenty-four batteries on a war footing would need, if the *etappen* batteries were 9-pounders, 4,534 of all ranks and 3,508 horses. From this it will be seen that, deducting young and sick horses, the batteries left at home would be practically quite unhorsed." A good deal of the inefficiency of our horses on service arises from the same cause as that alluded to as causing inefficiency or unfitness in men when speaking of "physical independence." "The horses are generally very much spoilt: they require regular food, careful nursing, protection from draughts and cold, especially when heated, and in any severe campaign, when such attention cannot be paid them, they fall sick and die off by dozens. A lamentable proof of this was furnished in the Crimea; the half-bred horses of the cavalry and horse artillery nearly all perished, so that only a few men could be mounted; when the French *Chasseurs d'Afrique*, continually employed on outpost duty, suffered scarcely any loss among their little Moorish stallions, and even the clumsy French cavalry and artillery horses stood the weather better than ours. When it was found necessary to remount our regiments in the Crimea, Turkish and Hungarian horses were bought up, and these endured the fatigue much better than our expensive horses." (Wraxall, "Armies of the Great Powers.")

Inefficiency of draught cattle occasionally causes serious impediments to strategy by delay in bringing up camp equipage, munitions, &c. Even in transport during peace, in India, serious inconvenience and delay is caused in marches by the sickness of elephants, which is very common. Disease in draught animals affects military operations much more seriously than disease of cavalry horses. This was proved in Napoleon's campaign of 1813, when even his cavalry horses were in a deplorable condition.

CONCLUSION.

In the foregoing pages I have briefly indicated the more important of the effects of Health and Disease on Military and Naval operations. This vast subject—embracing as it does the Sciences of War, Hygiene, Medicine, and Surgery—can be but dimly outlined in such an Essay as the present. I have not attempted to do more than sketch such an outline faintly; yet—because I have as much as possible aimed at condensation—I venture to trust with some completeness and accuracy.

There are two views to be taken of the importance of the subject of the physical conditions of soldiers and sailors—a moral one and a financial one. It was the moral side of the question which chiefly concerned Howard. He laboured to ameliorate the sanitary conditions and the moral and mental training of those whom their country, to a great extent, deprives of individual action in these matters. He was shocked at the amount of suffering and waste of life caused, unnecessarily, by the conditions in which such were placed. It was the morality of the question which stirred Howard, who yearned for the happiness of all men:

> "Who trusted God was love indeed,
> And Love creation's final law,
> Though Nature red in tooth and claw,
> With ravine shrieked against his creed."

But when we consider the effects of the physical conditions of soldiers and sailors on military and naval operations, we view the question in another aspect—a politico-financial one. We apply a general law of Social Science to a particular class. We consider their efficiency as labourers, as the "costly servants of the State," and not their happiness.

I may observe, that I do not myself believe that practically any good can follow taking these views of the subject as distinct from one and other.

I believe that, however perfect and well carried out legislation may be concerning the welfare of communities which ignores their individual happiness and comfort, or what they consider their happiness and comfort, it will be ineffectual. The letter of the law may be obeyed, but the spirit of it will not. Discipline is no less discipline when it appeals to the reason, or even to the feelings, than when it requires obedience to orders, the immediate purpose of which is not perceived. Acting on this belief I have always endeavoured to instruct men under my charge in the principles of hygiene—of their own individual welfare, explaining to them the *reason* of each sanitary regulation. Here, as in most other cases, the conclusion of the moral argument, and the conclusion of the financial argument is one; and, by omitting to appeal to the reason, we lose a vast power, which, if properly used, would do more for the health and welfare of our public services than years of legislation, or volumes of "regulations." It will be found that the individual Will is the most effective means of making soldiers and sailors efficient, in every sense, as regards their duties. That is, as much as possible, to merge Human Law in Natural Law.

It is seen from the brief account I have given of the history of the sanitation of our Public Services, that there is good cause to hope for a still further amelioration of the physical conditions of those engaged in them. Experience teaches us that to a great extent these matters rest with ourselves. It may not, perhaps, be possible to eradicate the causes of material and moral disease, which seems so intimate a part of our nature; yet, so far as past experience goes, there is no reason to suppose that we have yet approached to the limits of our power in this direction. In the words of the late Professor Parkes :—" In the scheme of Providence it may not be meant that man shall be healthy. Diseases of mind and of body may be the cross he has to bear; or it may be the evil against which he is to struggle, and whose shackles he is finally to unloose. The last disease will disappear, we may believe, only when man is perfect; and as in the presence of the Saviour all disease was healed, so, before perfect virtue, sorrow and suffering shall fade away. Whether the world is ever to see such a consummation no man can say, but, as ages roll on, hope does in some measure grow. In the midst of all our weakness, and all our many errors, we are certainly gaining knowledge, and that knowledge tell us, in no doubtful terms, that the fate of man is in his own hands."

We must hope that States will continue more and more to perceive that they are bound morally and politically to protect those whose services they employ from the effects of the conditions they impose on them, so long as international social conditions necessitate the employment of such labour—

> "Till the war-drum throbs no longer, and the battle-flags are furl'd,
> In the Parliament of Man, the Federation of the world."

THE

INFLUENCE OF DRINKING WATER

IN

ORIGINATING OR PROPAGATING

ENTERIC FEVER, DIARRHŒA, DYSENTERY,
AND CHOLERA.

CONTENTS.

PREFACE.

Evidence which suggests that a disease is or may be influenced in its origin or propagation by a cause so widely spread as water drinking, at once marks such disease as of special importance from its necessarily universal prevalence, whether it be one generally fatal or not. But when the same disease, in addition to its being so universally prevalent, is also one producing a large amount of human suffering and a high rate of mortality, the investigation of its nature, etiology, and treatment becomes a matter of the gravest importance to all members of the community. And such may be said to be the characters of the four diseases mentioned in the subject of this Thesis. Taken as a group, they are universally prevalent and cause a high rate of mortality.

Importance of the subject of the Thesis.

The records of the Army Medical Department show that diarrhœa, dysentery, enteric fever, and cholera are diseases vastly fatal in all climates and in all parts of the world.

Diseases of the digestive system are second only to febrile diseases in prevalence and mortality in the portion of the army serving in India; and if we remove enteric fever from the febrile class, and place it in a group with diarrhœa, dysentery, and cholera, we find that the group thus arbitrarily made includes the diseases which are the most prevalent and most fatal of the dangers to which our troops are exposed during service in India. As the cause of a large mortality among soldiers, the consideration of the means of averting or lessening the prevalence and fatality of these diseases, is a subject of the greatest import to the civil section of the community. The civil population has sacrificed for ever the industrial employment represented by each of our soldiers; the produce of its own industry goes in a great measure to feed and clothe and provide for him; and that he may not die but live, so that he may some day pay back the price he has cost, by doing for the State good service as a standing defence in peace, or by adverting from the country the distress of actual war, is a matter of interest to all, if not considered on the higher grounds of religious feeling and humanity, yet at least on the lower, of personal safety and personal profit and loss.

Another view of the importance of the inquiry is the practical result which has already been attained by the investigation of the etiology of disease, and of the causes of epidemics within the last few years. The result is satisfactory, and encourages us to pursue these investigations. It must also be remembered that sanitary science has very nearly caught up, and has almost entirely done the bidding of etiology; and unless rapid advance be made by the latter science, sanitation must come to a stand as regards progress, in the failure of indication from the physician and pathologist as to the directions in which farther advance is to be made.

Reasons for these diseases being thus grouped together.

In considering the verbal structure of the subject of the present Essay the question arises, Why are these four diseases—enteric fever, diarrhœa, dysentery, and cholera—thus grouped together? They are not arranged in the same category of diseases in the nomenclature of the College of Physicians—they differ widely in their symptoms, as well as in the morbid changes observed in them after death—and the methods by which they are severally treated are equally divergent. There exist at least three broad bonds of relationship which seem to justify their being thus regarded as to some extent kindred diseases. They are as follows: two natural points of relationship—namely, in all there occurs an altered alvine flux, and each is seen frequently to complicate, or run into, or supervene upon one or more of the others; and one artificial point of relationship—namely, that of all of them the drinking of impure water has been suggested as a cause, with more or less corroboration from established facts.

In general the coexistence of a common symptom occurring invariably in a number of diseases cannot be said to justify the arranging of these diseases in a distinct class, as a consequence of this one symptom being common to them, except where such symptom is a direct and distinct pathognomonic sign. For example, in the febrile class we have elevation of temperature, and evidence of rapid tissue destruction as evinced by increased elimination of urea. On these two common symptoms the class is formed; and legitimately, because it is based on the primal cause, so far as we know, of the diseases contained in the febrile class. Not so, however, with regard to the class of diseases now under consideration. In these the common symptom, *increased and altered alvine evacuation,* does not represent to us the primal cause of these diseases, it is not pathognomonic of them; and we are not justified in classing them together, with reference to this symptom, in any attempt at strictly scientific classification. But in our ignorance of the primal cause or causes of these diseases, we are justified in thus classing them together, merely for convenience of description and argument. It must, however, be remembered that this classification is simply arbitrary and artificial and provisional, and in no sense at all scientific, serving only until we shall have obtained more accurate information as to the primal cause of these diseases. Just as in chemistry we class under the term *katalytic* a number of processes of the nature of which we are wholly ignorant, calling them so in consequence of one condition or symptom apparently common to all. The tendency of these diseases to run into or complicate each other is a more justifiable condition by which to class them together; but still quite arbitrary, and manifestly provisional, awaiting increased knowledge as to their nature and cause. But the one great bond of relationship of interest to us in the present inquiry, and of any apparent practical importance, is that the drinking of impure water is, or may be, the cause of their origin, or if not of their origin, at least of their propagation.

It is evident, then, that in the class of diseases we here treat of we do not include all cases of diarrhœa, dysentery, enteric fever, and cholera, but only those cases of these diseases in which we can show, with some plausibility of fact or argument, that the ingestion of impure water may be the cause of their origin or of their propagation. As an objection to this limitation of the subject it may be stated by some, that if impure drinking water can be proved to be the cause of origin or propagation of any one or more cases of diarrhœa, dysentery, enteric fever, or cholera, this amounts to proof that all cases of these diseases are originated or propagated by the same cause. This being a matter on which it is necessary that the writer of an Essay like the present should distinctly state his views (to be understood), it seems expedient to me to place before my readers the position I occupy with regard to this question here, as the subject cannot well be introduced in the body of the Essay.

I must distinctly state at the outset, that *I do not believe that specificity of a disease necessarily implies specificity of its cause.* I shall first state the facts and arguments which appear to me to defend this position, and afterwards consider the objections which are sometimes advanced against it.

Does specificity of disease necessarily imply specificity of disease cause?

That specificity of disease does not necessarily imply specificity of the disease cause, appears to me not only reasonable to suppose, but manifestly apparent, by instituting an analogy between diseased specificity and healthy specificity.

If specificity exist at all in nature, it is surely most emphatically expressed in healthy physiological function. The influences by which external agencies act through the nerves of sensation are surely specific, if there be any meaning in the word.

But are the agents producing these influences in themselves specific? In other words, the influences producing sensation and the produced sensation are specific, but are the agents influencing and producing sensation also necessarily specific? Here the agent producing sensation is the analogue of disease cause, and the sensation produced the analogue of disease. We must consider the nerve influence producing sensation identical in health and disease, being, as far as we know, molecular disturbance of nerve tissue in both. *Primâ facie* there is a delusion of sense, which prompts an affirmative reply to the above question. But is any one prepared to state, or prove, that the sensation produced on, say, the olfactory nerves by a certain odour—for example, that of a rose, cannot be produced by any other combination of elements, or by a combination of the same in any other form? The wonderful results of organic chemistry in our own day should make us hesitate in such an assertion. It is reasonable to suppose not only the possibility but the probability of the existence of some other combination of elements which, in some other form or called by some other name, "would smell as sweet." Many other examples will occur to the reader—delusions of the tactile sense, optical and aural delusions, and delusions of the palate—which seem to render it in a high degree probable (and argument from analogy can do no more) that in healthy physiological function specificity of sensation does not imply specificity of the cause producing it.* And not only does specificity of sensation appear independent of specificity of its producing agent, but specificity of the kind of nervous influence by which it is produced seems independent of specificity of the agent producing such specific influence. If we compare a case of death from tetanus with a fatal case of poisoning by strychnine, we find the symptoms during life and the effects post mortem as similar as we find them in any two cases of what we would call one disease—as similar as in any two cases of cholera, or any two cases of enteric fever.

And we have no reason to suppose that the nerve influence carrying the agent of death by tetanus differs from that which carries the agent of death by strychnine. Here, then, we have two distinct agents or disease causes producing similar results as to nerve influence (mode of action), symptoms during life (phy-

* "The difference in the sensations due to different senses does not depend upon the actions which excite them, but upon the various nervous arrangements which receive them."—JOHANNES MÜLLER (quoted *Indian Medical Gazette*, August, 1878, p. 227).

siological function), and as to post-mortem appearances (anatomical sign). If we were not aware by actual experience that the agents producing these two similar conditions were different—were the case of strychnine poisoning as obscure in its origin as the case of tetanus, and were the history of the cases unknown—we would, in all probability, be guided by the similarity of symptoms and post-mortem appearances to pronounce the disease the same, and produced by an identical cause. And we would call the symptoms pathognomonic of it, and the lesions noticed after death the anatomical sign of it. And I am not sure that this would not be as correct a view as any other, even with a full knowledge of the difference of cause.

Because I apply to this particular case the doctrine above stated in general terms, that supposing the two states to be so similar that *they are* for all practical purposes identical, it does not hence follow that the causes of them are identical.

Because there may be a constitutional state or combination of effects—x (hitherto unnamed by physicians), which may be produced alike by the agent in tetanus and the agent in strychnine poisoning; and there may be a constitutional state or combination of effects—y (named cholera by physicians), produced alike by (a) some atmospheric influence, or (b) by the ingestion of impure drinking water, or (c) any other cause.

It is this gratuitous and altogether unnecessary assumption of a one specific cause which is, I believe, the explanation of the controversies on the origin of cholera, enteric fever, and other diseases; some of the cases of disease being really due to the cause which each etiologist defends, while there is opposed to him, by another etiologist, equally forcible proof that this cannot be the cause in certain other cases of the disease.

Although no less an authority on those matters than Professor Aitken states, "When it is found that a series of phenomena occurs in (thousands, millions) x number of instances, in the same order, within similarly uniform periods of time, and altogether with so much regularity, that those who are instructed on visiting a patient for the first time can not only affirm what has gone before, but may predict what is to come after, it is impossible to avoid concluding that such an invariable sequence has as constant a cause."* I think it must be admitted that this uniformity in the sequence of phenomena can only argue specificity of the morbid condition or state of which these phenomena are pathognomonic signs, and not the specificity of the cause which gives rise to this morbid condition or state.

Take the disease or morbid condition known as *cancer* : the disease is truly specific,† but can we thence conclude specificity in its cause ? The causes producing cancer—depressing emotions, a blow, injury, irritation—are known to be various, and in no sense specific.‡ And in the same manner, I maintain, various non-specific causes or agents may produce the diseases, or morbid conditions, or train of uniformly sequential phenomena known as enteric fever, or dysentery, or cholera. But it may be objected that what I here call the causes of cancer are merely the excitants of a series of phenomena, dependent on a constitutional idiosyncrasy before existing. Well, but the same may be said of all diseases, and certainly may be said of dysentery, enteric fever, and cholera. There may be a condition of the intestinal glands — constitutional, hereditary, connate, or accidentally acquired—giving rise to, or favouring the occurrence of, these specific diseases, but evolved or called into play by causes which are various and non-specific.

But to object that the cause of a disease is due to an already existing tendency, "seems to me," observes Professor Erichsen, in speaking of the pathology of cancer,§ "a begging of the whole question. There is no proof of the existence of any constitutional affection preceding, or even concurrent with the outbreak of the disease, nor until it has existed for a sufficient length of time for the lymphatics to be affected, and the blood to be poisoned; and it certainly appears to be more reasonable to look upon the local

* "Science and Practice of Medicine," part iii. chap. i.

† Not "specific" as used synonymously with "miasmatic," but specific as implying that the disease preserves its individuality through all time.

‡ Velpean observes that those organs most exposed to irritation or violence are most liable to cancer.

§ "Science and Art of Surgery," vol. i. p. 553.

disease as the primary affection, occurring originally in a healthy constitution, but after a time infecting the system generally, than to regard the mere local manifestation as in itself a proof of the constitutional nature of the disease. It is true that we are ignorant of the manner in which a cancer germ can be produced by the local action of the part in which it is generated, but we are as little acquainted with the essential mode of production of an exudation cell, or of a pus-corpuscle, which we know to be the result of strictly local action; and it seems to me that the difficulty is in no way solved, but simply pushed back a step, by the attempt to prove that in all cases of cancer formation, a constitutional cause or predisposition exists, which impresses the cancerous character upon local action, taking place in such a system. The hereditary tendency to cancer in certain families is not necessarily a proof of its being a constitutional or blood disease. We see the tendency to the hereditary transmission of malformations in strictly local diseases, as supernumerary fingers or exostoses, but we do not regard such conditions or such diseases as constitutional, merely because the tendency is transmitted from parent to offspring."

But, if it be required, I think it may very well be granted that a constitutional tendency to it exists in cases of enteric fever. There is some evidence from facts of the tendency to it being hereditary, and occurrence only between the age of the development and the age of the atrophy of certain intestinal glands seems to favour the idea that a pre-existing *condition* is essential to the evolution of the disease.

Although I am myself thoroughly convinced of the truth of the doctrine that specificity of disease does not necessarily imply specificity of its cause, I would not presume to advance the theory in this place if I did not consider my views supported by more competent authority. As far as I know, this doctrine has never being stated thus, in so many words, but I have within the past few years noticed the idea underlying or running through the writings of accomplished etiologists. It is evident that on this idea are based the views expressed by Dr. Murchison in his observations on the origin of fevers, and more especially of enteric fever. He believes that cases of this disease are caused independently of any prior case by "air or drinking water polluted with the products of putrefying sewage,"* at the same time admitting "that enteric fever is in some way communicable by the sick to persons in health."† These statements involve belief in three different causes for one specific disease—namely, (1) direct contagion; (2) ingestion; and (3) inhalation of particles of putrefying sewage, altogether independent of a foregoing case of the disease. I know also, by personal acquaintance and correspondence, that the doctrine I have stated is held (to a great extent, if not in its entirety) by many intelligent physicians of extensive practical experience and accurate observation in the United Kingdom and in India. This multiple origin of specific disease is a postulate which Dr. Murchison and his school must have, and which Dr. Budd‡ and his school will not grant. Until they come to some terms about this postulate, argument between them is merely waste of time and words.

The continental schools are divided in an exactly similar way as regards this postulate. Pettenkofer, Ranke, and Wolfsteiner§ range themselves with Murchison, believing that the disease sometimes arises from causes independent of a foregoing case, as well as sometimes by contagion; while Gietl and Friederich believe, with Budd, that a prior case is necessary, and refuse the postulate assumed by the others. It matters little, however, whether they refuse it or not, since it is granted by common sense and by experience. Murchison and his school believe in the origin and propagation of disease, while Budd and his school believe only in the propagation, and do not acknowledge any origin for specific disease.

* "A Treatise on the Continued Fevers of Great Britain." By C. Murchison, M.D., F.R.S., p. 428.

† *Loc. cit.*, 465.

‡ "Typhoid Fever." By Wm. Budd, M.D., F.R.S.

§ "Ueber die Actiologie des Typhus" (Vortrage gehalten in den Sitzungen des ärztlichen Vereins zu München, 1872). I am unable to explain the statement by the late Professor Parkes (Report on Hygiene, Army Medical Department Blue Book, 1872) with reference to Dr. Wolfsteiner's views: "He looks at enteric fever precisely as Budd does." Professor Parkes is not likely to have made such an assertion in error.

I would here also refer, in support of my views, to a Paper by Deputy Surgeon-General (now Surgeon-General) Dr. Munro, C.B., published in Army Medical Department Blue Book for 1872, "On Malarious Fevers and Cholera."

Dr. Munro's reputation and experience as a pathological anatomist attach considerable importance to his opinion on the subject. He observes: "If, in the same localities, during the same season, amongst the same class of men, almost under similar circumstances, several so-called different diseases, as remittent, intermittent, congestive remittent fevers, heat apoplexy, cholera, and yellow fever, are prevalent; and if there is in all a similarity in the symptoms or stages, progress, treatment, and terminations; and, finally, if the post-mortem appearances are in many points (and these the chief points) similar, may we not suppose that such diseases are originated by the same agency; and that, if not all the *same*, they are at least *phases* of the same disease, and may we not therefore class heat apoplexy and cholera among febrile diseases?"* And further on he asks: "How many kinds of malaria are there? Can there be one for every form of fever or pestilence; or is there but one, which, acting in different ways, according to local or accidental circumstances, on different habits and constitutions, produces the same effect, with greater or less intensity, which intensity is considered a distinct disease?"

It will be seen that the doctrine here implied is the exact converse of the proposition I have enunciated.

Dr. Munro's doctrine is, that specificity of cause does not imply specificity of disease. Now, whoever admits this, must, of logical necessity, also admit that specificity of disease does not imply specificity of cause, for each proposition is a corollary from the other.

Again, in the Army Medical Department Report for 1875, under the head of "Sickness and Mortality in each Corps serving in Madras," the remark occurs: "Taking the three diseases, dysentery, diarrhœa, and hepatitis, together, as being probably only different manifestations from the same generating cause," &c. &c.

* Army Medical Department Blue Book, 1872, p. 269.

I would refer to many other authorities holding the same views,* but that I cannot devote further space to the considera-tion of this subject.

In conclusion of it, the principle here maintained may be recapitulated; it is briefly as follows:—Specific diseases may or may not arise from specific causes—most probably they do—but it cannot be inferred from their specificity, that other non-specific causes do not initiate the same specific diseases or specific train of phenomena.

Certain theories bearing on the subject.

As a navigator marks upon his chart certain rocks and quicksands which he must avoid as dangerous; so I here notice certain speculations in etiology which, in the present state of our knowledge, cannot, in a systematic inquiry into the nature of disease, be assumed as data without danger. We must first consider the germ theory of disease, or more correctly the theory of the germ-nature of contagion. This matter is of special importance in the present inquiry, because if this theory be correct it furnishes overwhelming theoretical evidence that certainly typhoid fever and cholera, and perhaps diarrhœa and dysentery, are propagable by drinking water into which the dejecta from persons suffering from these diseases pass. Stated as a formal syllogism, the argument is as follows: *Major*—All entities which breed true are propagated by a specific germ. *Minor*—The specific fevers, whenever they do breed (because occasional *de novo* origin is admitted by some germ theorists), breed true. *Conclusion*—Therefore specific fevers may be sometimes propagated by a specific germ. The holders of this conclusion are divided into those who believe that such germs are always organisms foreign to the human economy; and those who believe that such may be derivants from human tissues—constituents of the economy, the vital energy of which has been vitiated or modified—the degraded bioplasm of Professor Beale.

* Simon in "Public Health Reports," and Pettenkofer, Professor Acland, and Thiersch, as quoted in his Report for 1860; "What is Malaria?" by Surgeon-Major Oldham, M.D., B.M.S.; "Similarity or Identity of Malarious Fevers, &c.," by Surgeon-Major Smith, M.D., A.M.D.; and *passim* throughout periodical medical literature.

The theory as above stated is thus answered by its opponents: the word *all* in the major premiss cannot stand unchallenged in the face of modern scientific investigations. The conclusion is thus invalidated. Some persons found the germ theory of disease, I think weakly, on an analogy between the propagation of disease and the propagation of organisms—as an acorn is to an oak, so is a cholera germ to cholera.

This analogy is of course open to the imputation of being a *petitio principii.* No one disputes that a cholera germ would produce cholera in the same way as an acorn produces an oak, if the cholera germ existed. If a cholera germ be at all mentally conceived, that conception of necessity includes the idea that the cholera process is the only disease process which this germ will produce—not the enteric fever process, nor the small-pox process, nor the process of any disease other than cholera. So that, assuming the existence of a germ begs the whole question. We believe that every oak is the product of an acorn, and every horse the product of a horse, because the reverse is contrary to our experience, and we know that these organisms are never formed *de novo.* But we do not believe that every case of specific disease is the product of a foregoing case of the same, because the reverse of this is not contrary to our experience, and it is not a matter of knowledge that specific diseases do not originate *de novo,* or that they do not originate from more than one cause. Thus the natural history problem and the etiological problem are not comparable in any of their terms. The analogy does not hold; and it is not a case in which the method of analogical reasoning can be admitted.

It is often stated, even by the defenders of it, that the germ theory is based on analogy. This is not so: it depends on the argument (which is not analogical) first above given, and if the premisses of this are substantiated, the conclusion is not a theory but a fact.

Another view of the nature of contagion very recently proposed (indeed, I believe not as yet published, but to which I am permitted to refer) is obtained by instituting an analogy between the action of contagion and the development of hereditary tendency. Some most interesting points are brought out by this analogy; but on the whole it seems to lead back to the old metaphysical doctrines of disease cause, in which the word influence was so much and so indefinitely used.

But, indeed, it may be questioned whether modern science is justified in pushing aside the older doctrines except those which have been actually disproved by modern experiment.

Recent investigations have certainly contributed some little to our knowledge of the action of contagion, and hypothetically from this, perhaps, to our knowledge of its nature; but whether any data have been obtained on the question of the source of contagion is a matter of opinion. One very practical outcome of the theory last referred to is, that if such an analogy exist, we are probably as far as ever from a knowledge not only of the source but also of the nature of contagion.

As opposed to the Germ Theory of Disease we have the Physico-chemical Theory. According to this, the minute organisms found in the tissues after death are the causes and not the results of pathological change; and the material *viri* of specific diseases arise directly from combinations of inorganic matter. These theories on the nature and source of contagion have been promulgated in definite form during recent years; chiefly as a consequence of the impetus given to biological research by the influence of the evolution theory of life on the scientific mind, and the results of recent investigation on the nature and causes of fermentative and putrefactive change.

How far and into what recondite speculations the etiologist may be led if he take modern scientific thought for his guide, it seems difficult to conjecture. "The conception of struggle for existence must be extended from the race to the individual; and not only do races and individuals compete with one another, but there is a competition between parts in the individual organism, and every permanent variation is the result of a successful struggle on the part of some special group of elements."*

* "The Physical Basis of Mind." By George Henry Lewes.

As regards the subject with which this Essay deals—the origin and propagation of disease—these modern theories regarding the nature and source of contagion affect only our views of propagation, and have nothing to do with the origination of disease.

When it is considered that disease cause consists of a threefold agency—the predisposing, the originating, and the propagating—and that contagion can have to do only with the latter, it will be seen that any germ theory of disease must be accorded but a subordinate place in etiological inquiry.

A predisposing cause, as distinct from contagion, is admitted by modern teachers of the germ theory. Dr. Maclagan distinctly alludes to it thus:*—"This something we shall for convenience sake refer to as the Second Factor—the contagium being the first. Without this second factor the contagium is not reproduced; and no results follow its reception into the system—the contagium being *per se* absolutely innocuous."

An originating cause as distinct from contagion is also admitted, for the same writer observes:†—"It seems to us that one may attribute the phenomena of disease to the propagation in the system of minute organisms without being bound down to the belief that these organisms can never originate *de novo*." And Dr. Lionel Beale writes:‡—"The phenomena which occasion the formation of ordinary pus, may, if they continue to occur for a long period of time, determine the development of a specific pus, which has still more marvellous powers of vitality. So it may, I think, be reasonably argued that if the ordinary feverish state be prolonged for a considerable time, and be severe in degree, it is likely that the bioplasm in the blood collected in the capillaries may give origin to bioplasm with marvellously increased powers of retaining its vitality, of growing and multiplying. The particles of this, making their way through the vessels and escaping, may live for a considerable time, and having entered the blood of another person may excite in it changes which accompanied their own development. . . . . Without venturing to state

* "The Germ Theory of Disease," p. 46. † *Opus cit.*, p. 2.
‡ "Disease Germs," p. 253.

positively from what particular kinds of germinal or living matter of the body the germs of contagious disease are actually derived, or attempting to decide definitely whether they come from the very minute blood bioplasts, or ordinary white blood-corpuscles, or mucus or epithelial or other particles, I think I am justified in advancing the doctrine that the germs originate in man's organism, and that they have descended from the normal bioplasm of his body."

It will be seen that in the following pages I have set forth two kinds of evidence—theoretical and experimental—giving to each what I consider its due place and value. I have taken trouble to focus accurately to the mental vision the experimental evidence, while the speculative, although indicated, is left out of focus if it in any way interferes with the clearness of the former.

But while we avoid giving too much value to hypothetical evidence used as premisses from which to draw conclusions, we must remember that we cannot afford to disregard altogether those views of our subject which are discerned only by the dim ray of speculation.

We must to some extent recognize probabilities, "for to us probability is the very guide of life." In the words of the Rev. Professor Haughton, "We resemble spiders on a summer morning, floating their gossamer threads into the air, in the hope that some passing breeze may waft them to a resting-place, and find a firm foundation from which to build."

In conclusion, I express regret that circumstances have prevented my dealing with this important subject in as carefully detailed a manner as I had wished.

The writing of this Essay has been frequently interrupted by the exigencies of military service; and it has been written in a climate but little conducive to the prosecution of scientific research or literary labour.

NOTE.—The writer requests that readers will peruse the Table of Definitions on page 297 before commencing to read the Essay.

INTRODUCTION.

It may be well to give some outline of the arrangement which has been adopted in the bringing together of the various subdivisions of the following Thesis, so as to obtain a fuller comprehension of the topics which the author understands the subject to embrace, and a greater facility in its perusal. Since the most trustworthy evidence of the existence of a *materies morbi* is to be drawn from the effects produced in and by the disease, it seems most appropriate that the point from which to start in this inquiry should be the morbid changes found to occur in the diseases under consideration. We shall then proceed, first, to inquire into the abstract correlative symptoms of diarrhœa, dysentery, enteric fever, and cholera; that is, the symptoms which are common to them, with reference to the influence of impure drinking water on their origin and propagation.

To start by the consideration of some symptoms common to these diseases seems not only a convenient artificial arrangement, but one justified by nature, since these diseases are so frequently met with complicating or running into one and the other.

Having then obtained (as I believe we shall) some idea of the *effects* of this influence, or at least of the reality of its existence, we shall proceed to consider the *agent* in the chapter entitled *Hygienic Meaning of the term Impure Drinking Water.* We will then briefly, and chiefly for the sake of completeness, review our knowledge of the *object* acted upon, in the chapter headed *Physiology of the Intestinal Canal, with reference to the Influence of Impure*

Drinking Water on the Origin and Propagation of Disease. We will then pass to consider the *action,* under the designation *Rationale of the Operation of Impure Drinking Water on the Health of Individuals and Communities.*

Having thus, in the first part of the Essay, treated very generally of the pathology, and to some extent of the etiology of diarrhœa, dysentery, enteric fever and cholera in the abstract, and also of the general influence of impure drinking water on health, we will, in the second part of the Essay, endeavour to present a complete view of the same subjects in the concrete form—"*The special Pathology, Etiology, Endemiology, and Epidemiology of Enteric Fever, Diarrhœa, Dysentery, and Cholera, with reference to the influence of Impure Drinking Water on the Origin and Propagation of these Diseases.*"

I consider it expedient not to adhere to the order in which the names of the diseases occur in the title of the Essay. Enteric fever has been placed first, as belonging to the febrile class, and as separate from the three following, which are classed together as diseases of the digestive system. But I arrange them according to the severity of their common symptoms. I prefer to treat first of diarrhœa, as being the simpler malady, and, so to speak, composed of fewer symptoms; next of dysentery, as perhaps occupying a position, as regards complexity of symptoms and morbid changes, intermediate between diarrhœa and typhoid fever and cholera; and I shall then treat of the two last-named diseases, enteric fever and cholera. Not only in this arrangement do we pass from the simpler pathological condition to the consideration of morbid changes of greater complexity, but this order seems expedient with regard to the present condition of our knowledge of the nature and etiology of these diseases. We proceed from the partially known to the wholly unknown. We have here a very complete gradation of disease—a gradation as regards the severity and complexity of the *influences* of origin and propagation; a gradation as regards distress and complexity of *symptoms;* the same as regards intensity and complexity of *morbid changes;* and a gradation in *fatality.* In practice this gradation is seen more regularly and completely than can appear from a systematic description of these diseases, because of their tendency to complicate or run into one or the other.

There is a border-land between each, into which each frequently encroaches, well enough known to the practical physician, but hardly admitting of an accurate description. As above observed, this gradation from simplicity to complexity is well reflected in our knowledge of these maladies.

In the second part of the Essay, which deals with the etiology of these diseases as occurring in their entirety, or concrete form, I will not bring forward evidence with any adherence to the chronological order in which the various parts of it have been promulgated, but, dividing the subject into sections, each dealing with a particular division of it, I will group together the evidences relating to each.

I consider it preferable to present a series of views of the subject from different standpoints, and give the various evidences relating to each phase in close proximity. We will with greater facility be able to judge of the value of each of the evidences when they are thus compared and weighed against each other. I shall then briefly summarize, at the end of each section, the evidence adduced, and endeavour to draw a just conclusion. Subsequently, in a general summary, I shall collect these conclusions, and submit a tabular statement of the results arrived at, which I shall endeavour to make a faithful miniature of what is known—not what is supposed—of what is known regarding this subject.

And here it may be well that I should describe, and with some detail define, what I mean when using the terms *origin* and *propagation*, as applied to disease, in this Essay.

The term origin, as used in medicine, may imply either (primarily) the first cause of disease in the abstract, or (secondarily) the cause of, or influence producing, disease in any one given case. To explain: assume two consecutive cases of any entero-zymotic disease, and assume that the first is independent of any foregoing case of the same disease, and that it is due to $x\,y\,z$; and that the second case is due to ingestion of particles of dejecta of the first case, taken in impure drinking water. The influence of *origin* of the disease in the second case (if this phrase is applicable to the case at all) must manifestly mean the influence of *propagation* of the disease; so that origin in its secondary (and strictly erroneous) sense is synonymous with propagation. Hence in the phrase "origin and propagation," as occurring in the title of this Essay, *origin* must be restricted to mean the primal cause of a first case* (imaginary or real) of the disease or diseases under consideration. I can hardly avoid here briefly alluding to the question, Do these original cases (strictly so called) ever occur now? I ignore the term *spontaneous*, as inaccurate and misleading. If such cases do not occur, then our efforts should be directed against the propagating causes; if they do occur, we should endeavour to counteract the originating cause; and if this object be attained, the propagating cause will cease to exist.

I think most persons, after due consideration and a careful study of epidemics, will agree with me in believing not only that such original cases do now occur, but also that they are of frequent occurrence; and, moreover, that propagation holds a secondary place in epidemics.

An objector may reply by pointing to our improved sanitation, and the lessened mortality of epidemic disease as cause and effect. But this is no valid objection, unless he prove that sanitary improvements have operated solely against propagation, and have not influenced the origination of disease.

The question of the *origin* of disease by ingestion of impure drinking water is a question standing *per se*, unconnected with, uninfluenced by, and not included in any other fact or inquiry relating to the etiology of disease of which I am aware; while the question of *propagation* of disease by impure drinking water is, of necessity, practically contained in the broader question of the influence of diseased dejecta in reproducing their specific diseases.

The term *propagation* must be also defined and used in a somewhat restricted sense, as here applied. The word signifies generation, including the idea that the disease generated must be of the same genus as that generating it; so that propagation presupposes a foregoing case of disease, in which the morbid condition is identical with that in the case due to propagation. Although this difference in the signification of these terms is apparently clear enough, there are very few writings on the etiology and epidemiology of the zymotic diseases in which these two terms, origin and propagation, and the distinct ideas represented by them, are not jumbled together. For example, this

* "Spontaneous," as sometimes erroneously called.

passage which I have just opened: "While some medical men believe that the water, in order to produce cholera, must be contaminated with actual poisonous matter (cholera dejecta), there are others who are persuaded that water *plus* filth, under certain conditions as to decomposition, &c., is quite sufficient to cause a case of cholera, and to propagate an outbreak."* Now, it is manifest that the word propagate in this sentence is not intended to convey its strict meaning, which presupposes a pre-existing agent, identical in its nature with the object or effect produced by such propagation. What the writer of the above extract means is, that water *plus* filth is sufficient to originate a case of cholera, or to *foster* (not propagate) the development of an epidemic. The correct use of the word propagate implies the generation of like by like.

Finally, there will be found added, in the form of appendices, brief remarks on some matters closely correlated with the subject of the Thesis, which could not well be included in it, but without some notice of which a treatise on the subject would hardly be complete. For the sake of uniformity in nomenclature and terminology, and in order to avoid certain fallacies and misapprehensions to which I have alluded in the Preface, I affix a table of definitions of any words used in an ambiguous, or limited, or unusual sense, in the Essay, without an exact understanding or limitation of which terms, argument on the subject is merely a waste of words.

* "The Cholera Epidemic of 1875," *Ind. Med. Gazette*, February, 1877.

THE INFLUENCE OF DRINKING WATER

IN ORIGINATING OR PROPAGATING

ENTERIC FEVER, DIARRHŒA, DYSENTERY & CHOLERA.*

PART I.

GENERAL VIEW OF THE TOPICS EMBRACED IN THE SUBJECT OF THE THESIS.

CHAPTER I.

GENERAL PATHOLOGY OF ALVINE FLUXES, WITH REFERENCE TO THE INFLUENCE OF IMPURE DRINKING WATER ON THEIR CAUSATION AND PROPAGATION.—(*Effect.*)

THE consideration of the Pathology of Diarrhœa, Dysentery, Enteric Fever and Cholera, is included in the subject of this Essay only in so far as it bears upon the influence of Drinking Water on their Origin and Propagation. To this view of their pathology the following remarks are rigidly limited.

I have stated in the Introduction my reasons for, at first, grouping these diseases under the generic term of "*alvine flux with complications.*" It seems expedient thus to obtain an extensive bird's-eye view of the ground which we shall afterwards go over in detail, so as to perceive the more general relations or points of contact of the subjects to be treated of.

In dealing with an inquiry like the present it is necessary to reason, first, from effect to cause; because the produced effects are the only known *data,* while the producing cause forms the subject of question. Afterwards, when this method of inquiry seems complete, and when certain conclusions have been arrived at, it is allowable to employ the reverse method of inquiry—namely, from cause to effect—in order to test the truth of such conclusions by

* Awarded the Alexander Memorial Gold Medal and Prize of the Army Medical Department.

assuming them as premisses; when, if true, they should logically lead up to the *data* (the effects) from which the inquiry started.

I purpose employing this method in the present instance, and begin by considering the *data* at our disposal, the known effects produced—namely, the symptoms and lesions occurring in these diseases.

We first inquire, What are the effects produced by whatever may be the cause of Diarrhœa, Dysentery, Enteric Fever, and Cholera?

Firstly, we have HYPERÆMIA of some part of the intestinal canal; secondly, intestinal CATARRH—these two processes being in their simpler forms unaccompanied by permanent lesion; thirdly, EXTRAVASATION OF BLOOD from the intestine, accompanied or not by a permanent lesion; fourthly, PROLIFERATION OF CELL GROWTH ON THE SURFACE OF THE INTESTINE, rapidly generating formed material; and fifthly, PROLIFERATION OF CELL GROWTH POURED INTO THE CAVITY OF THE INTESTINE, and passing through it too rapidly to admit of its generating formed material *in situ*. The two latter phenomena we find never, or hardly ever, occur without a resulting permanent lesion, or "anatomical sign." In the diseases under consideration these phenomena are grouped as follows:—

1. *Hyperæmia and Catarrh* of the intestinal canal accompanied by symptomatic fever, constituting the various forms of *Diarrhœa*. (Hyperæmia and catarrh of the intestinal canal passing on to extravasation of blood, unaccompanied by pathognomonic lesion, constituting hæmorrhage from the intestines).
2. *Hyperæmia and Catarrh* of the intestinal canal passing on to extravasation of blood from the vessels of the mucous membrane, caused by a softening of the coats of the vessels and tissue through which they pass; with a *proliferation* of corpuscular, croupous or aplastic growth from the mucous surface, accompanied by an uniform permanent lesion and by symptomatic fever, constituting *Dysentery*.
3. *Hyperæmia and Catarrh* of the intestinal canal, with *excessive cell growth* in the intestinal gland structures (which is cast off from them), giving rise to an uniform permanent lesion, accompanied by fever of the typhoid type, constituting *Enteric Fever*.
4. *Hyperæmia and Catarrh* of the intestinal canal, with copious

transudation and proliferation of cell-growth, with infiltration of the follicular structures of the mesentery and intestine, accompanied by symptomatic fever, constituting *malignant Cholera.*

The above groups of phenomena constitute the pathological bases of the diseases named. There are, of course, other conditions, some of them very complex, added on to these; but the phenomena here enumerated form the fundamental beginnings of the disease processes, as far as our means of investigating them can show. Besides, it is to the morbid anatomy of these diseases we must more particularly direct our attention in endeavouring to arrive at practical conclusions concerning them, since these alone constitute the constant specific signs of them. The same symptoms do not always accompany these lesions, and sometimes these lesions exist without any symptoms of them being present during life. Two remarkable cases of enteric fever, throughout which alvine flux was altogether absent, although the intestinal lesions characteristic of the disease were found to exist in a well-marked degree, are related in the *Indian Medical Gazette* for 1877: and the author has recently met with two cases (one of phthisis, the other of Addison's disease) in which extensive recent ulceration of the large intestine was discovered, although in both *constipation* existed for some time previous to death. The lesions, then, and not the symptoms, must be taken as the constant characteristic and pathological basis of these diseases.

1. *Hyperæmia.*—The first change occurring in hyperæmia is an excess of bioplasm in the capillaries. This may be due either to direct increase of pabulum, and resulting excessive multiplication of blood bioplasts, or from some lesion of innervation, brought about by any cause of irritation.

So long as the multiplication of the masses of germinal matter in the blood does not exceed a certain amount, or continue over a certain time, there are no very distinct morbid results following, and the neighbouring tissue, but slightly affected, is repaired, if any actual damage have resulted in it.

The circulation soon returns to its normal rate, the increased number of white blood corpuscles in the capillaries soon pass on into the circulation, and become lost in the mass of the blood, where they undergo change. Here there is no permanent lesion, the morbid condition not lasting long enough or going far enough to place the tissues involved beyond the conditions of repair. When the same process continues for some time, or exists in an excessive degree, then the bioplasm of the blood multiplies to an enormous extent, and there occurs an exudation of a colourless transparent fluid which moistens the surface of the affected membrane. This fluid, besides containing albumen in solution, holds in suspension multitudes of minute particles of bioplasm, which grow and multiply on the affected surface.

2. *Catarrh.*—This exudation of fluid secretion from the mucous surface is characteristic of catarrh, and hence its name.

It is probable that when there exists extensive capillary congestion, minute fissures may result in the stretched capillary walls, as suggested by Dr. Beale, allowing minute particles of living germinal matter to pass through.*

The small protrusions upon the surface of the white blood corpuscles may also grow through the capillary walls, become detached, and pass into the tissue external to the vessel.

However brought about, the result is, that a vast number of cells like white blood corpuscles exist in the capillary vessel, and immediately around its external surface, sometimes extending a considerable distance in the substance of the surrounding tissue. During this inflammatory process these exuded particles of bioplasm may undergo change, and be metamorphosed into a form of germinal matter or bioplasm known as *pus.* Pus-corpuscles can generally be identified in microscopic examinations, not indeed by appearances actually characteristic, but by certain movements much more active than those occurring in other corpuscles, and by the absence of spherical cells. Although there are no very definite characters by which the pus-corpuscles can be absolutely identified, yet, as we know, the aggregations of these, which we call *pus,* are readily enough recognized. This change in the nature of the exuded bioplasm is a matter of much importance as regards contagion and the propagation of disease. As we occasionally see pus carried off in the alvine flux of dysentery and other

* This assumption seems to the author unnecessary. The phenomena here alluded to seem quite explicable as a result of osmosis, which permits the passage of infinitely minute particles of germinal matter, not yet formed into corpuscles, to pass through animal membrane of extreme tenuity, as demonstrated by the experiments of Onimus. With an abnormally excessive supply of pabulum, this germinal matter may be developed into exudation cells, after leaving the vessel, in the substance of the surrounding tissue.

diseases, so it is likely that in dysentery and in certain other diseases exuded bioplasm passes into some other specific form of exudation cell; and although not differentiable from other corpuscles by its physical characters, passes out of the body *endowed with a specific modification of vital energy.* It has been proved by experiment that living pus bioplasts, derived from normal bioplasm, may travel long distances, and after considerable lapse of time gain entrance to another organism, and may in it live and grow and multiply, and may establish changes in it of the same nature as those by which it was itself originated. There is no difficulty in understanding that the exudation cell of a specific contagious disease may in like manner set up in another organism the changes by which it had been itself produced.

This would be the explanation of an "*originated*" case, or, as sometimes called, a *sporadic* or *de novo* case of disease, giving rise to a *propagated* case of the same.

The former, or original case, arising from some metamorphose of healthy bioplasm, just as the pus-corpuscle is developed from the healthy bioplast; the second, or propagated case, being due to the carriage of the same into a healthy organism.

3. *Proliferation.*—In addition to this exudation containing corpuscles, which the observations of Cohnheim and others give some grounds for believing have actually migrated from the interior of the capillaries, there is also to be observed a local multiplication of material derived from the elementary structures of the part, particularly from the particles of the sub-epithelial connective tissue. This proliferation seems due to the increased access of nutrient material to the germinal matter surrounding the nuclei of this tissue, either the result of hyperæmia or of some disturbance of innervation.

When this occurs in the mucous membrane of the intestines, the rudimentary corpuscles are so rapidly developed that the superficial layers of the old and hardened epithelial structure are lifted from the surface, being displaced by the pressure of the new growth accumulated beneath.

Indeed, Dr. Beale states that this result may be caused even by excessive exudation direct from the capillaries, and shows how, in cattle plague, bioplasm, passing from capillaries, may accumulate sufficiently in the sub-epithelial tissue to actually strip off the superficial layer of epithelium separating the layers of formed tissue, like a dissecting aneurism.

When this external layer of mucous epithelium is thus stripped off, we find beneath a layer of spongy texture, occupying a position as regards development between the healthy formed material known as *epithelium* and the diseased form of bioplasm known as *pus*. This formation is seen when *sloughs* or *exuviæ* have been cast off from the intestine. Some careful investigations I have made as to the minute structure of the shreddy dejecta of dysentery and chronic diarrhœa (to which I cannot more explicitly refer in an anonymous essay) lead me to conclude that such are of two distinct kinds—namely, sloughs and *casts*. The first consist of the layer of epithelium cast off by excessive proliferation beneath, as just described; the second being hollow moulds of the intestine formed by the proliferated corpuscles united by a film of lymph, which becomes partially organized, constituting the false or adventitious membrane, as seen in dysentery, which becomes detached from the bowel, and passes away with the dejecta. It is unnecessary to point out how importantly such processes bear on the question of the propagation of diseases by water into which such bowel discharges may pass.

4. *Transudation.*—Accompanying the severer forms of proliferation an excessive exudation is frequently met with.

This seemingly differs from the exudation of catarrh not only in degree, but seems to be absolutely distinct from it both as regards the nature of the process and the nature of the product. I have above referred to this process as "proliferation of cell growth poured into the cavity of the intestine, and passing through it too rapidly to admit of its generating formed material *in situ*." It is to be observed that, in speaking of this aggravated form of exudation, we are unable to state its actual source, as we are able to do regarding the simpler exudation of catarrh.

This excessive exudation we find emphatically pronounced in the bowel discharges of cholera. Just as a pus-corpuscle is not absolutely recognizable by any constant affirmative characters, but aggregations of pus-corpuscles are easily recognized; so the corpuscular elements peculiar to the cholera discharges are not seen to have any distinctly definite characters on microscopic examination, but the aggregations of them known as the "rice-water stools" of cholera are distinct enough in their characters, and easily recognized. And as the pus-corpuscle is germinal matter, to which a specific modification of vital energy has been imparted by some morbid process of which we are ignorant, so in like manner there are good grounds for believing that the corpuscular elements of the discharges of specific diseases are *bioplasm endowed with a specific modification of vital energy.* And finally, as the pus-corpuscle is capable of carrying potentially the morbid process which gives birth to it, so are the corpuscular elements of the discharges of specific diseases capable, it would seem, of reproducing in a healthy organism the morbid processes which originated them.

I omit to consider here *extravasation of blood*, which is the commonest pathognomonic symptom of dysentery, and sometimes a complication of other intestinal affections, because it is not a pathological basis of disease process. It is an effect, and may be an effect of three distinct processes—namely, (*a*) mechanical vascular engorgement, (*b*) softening of the coats of the vessels and surrounding tissues, and (*c*) rupture of vessels after the separation of *exuviæ*, in the same manner as *post-partum* hæmorrhage occurs from the open mouths of uncontracted uterine vessels when the placenta has separated. The processes first and last mentioned—mechanical vascular engorgements and rupture of vessels — occur not infrequently in the acute intestinal diseases enteric fever and cholera; while the second, the softening of tissue, is frequently met with in the chronic forms of diarrhœa and dysentery. The almost constant occurrence of well marked amyloid degeneration in old standing cases of dysentery seems to explain this; but it is not easy to say whether the degeneration is prodromal to the dysentery, or the reverse. Out of these morbid processes seem to arise, as effects, other processes which are pathognomonic of these diseases.

And processes which are distinctly pathognomonic of the diseases in which only they occur, seem also to result from these.

The definite characteristic stamp impressed on each distinct disease process seems due to the definite modification of vital energy — the direction, or impetus of growth — given to the bioplasm in each distinct case. Suppose a particle of bioplasm is composed of ten (or x number) atoms, each of a different element, certain conditions, or a certain pabulum, or certain combinations or modifications of innervation currents, may develop one atom, giving to the bioplasm an impetus of growth in one direction; certain other conditions may develop another atom, giving an impetus of growth in another direction; and so on for every atom of each element, and for every combination of them.

Or the phenomena may be explained on a chemical theory, by assuming that the ultimate atoms of bioplasm are split up into their elementary constituents, and that a re-arrangement of these elements occurs, producing a particle capable of a growth in a different direction to that from ordinary healthy bioplasm.

Glover's observations on the chemistry of tubercle,* and those of Thudichum on the bile and other secretions in cholera, strengthens the probability of the correctness of such a view.

Lesions.—The processes above referred to as being the pathological bases of Diarrhœa, Dysentery, Enteric Fever, and Cholera, are accompanied by certain permanent lesions, or "anatomical signs," which may be grouped as—

1. Results of vascular engorgement of the intestine.
2. Inflammation of gland vesicles.
3. Granular exudation on mucous coat, or growth of adventitious membrane.
4. Rupture of gland vesicles.
5. Ulceration of glands.
6. Separation of exuviæ.

The four last may be again broadly grouped as *elimination of morbid bioplasm*, and it is these only we have to consider in the present inquiry. I have already, under the heads of *Proliferation* and *Transudation*, briefly referred to this process of elimination of a diseased product, but the permanent structural changes found in the glands have been supposed by some to throw some light on the nature of this eliminative process; it will, therefore, be well for us to glance cursorily at this question in passing, this process having a very important bearing on the subject of the origin and propagation of these morbid conditions by impure drinking water.

In the first place, some pathologists deny that the structural changes seen in diseased glands (their rupture and discharge of contents) afford any evidence whatever of elimination of a diseased

* Quoted in Prof. Aitken's "Science and Practice of Medicine."

poison or product, affirming that such changes are due merely to destruction of the gland. They argue that these glands can act only in one direction—that is, centripetally—absorbing material from the intestine to pass into the blood; and that they cannot hence act in a backward direction, pouring forth material into the alimentary canal. We can only consider this a theoretical objection, and believe that those who advance it have never seen a gland actually in the stage of rupture and discharge.

But even as a theory, any one who holds this view must also believe that because the office of the skin glands is to pour forth secretion on to the external surface, it is impossible they could absorb material from that surface, and that such measures as iodine painting, inunction of ointments, embrocations, &c., are therefore erroneous and obsolete methods of medication.

It has been objected that the term *elimination* should be restricted to the removal of non-living excrementitious matter from the blood, and should not be used to include the passage of living particles through structures whose normal office it is to remove such excrementitious matters. This objection seems also founded on a theoretical difference between the two processes. There may be such a difference, but until it is clearly indicated disease products may, I think, for all practical purposes, be described as *eliminated* by glands.

But the great fact pointed out to the etiologist by these structural changes, is evidence of increased afflux of blood to the intestine—the formation of a new and abnormal substance—and its passage out of the economy *per rectum*. The process is quite undoubtable, whether it be called "elimination" or "*x y z*."

I do not know of any grounds on which it can be denied that this disease product is formed direct from the germinal matter of the blood, or from the still undeveloped germinal matter of the tissues; or on which it can be denied that each particle of it is as potentially capable of carrying into another organism the changes which gave it birth, as a pus-corpuscle is experimentally proved to be.

We have now briefly and imperfectly glanced at the principal effects produced in, or pathological bases of Diarrhœa, Dysentery, Enteric Fever, and Cholera. It may, perhaps, be well to pause here, and review the ground we have traversed, to observe whether any evidence has transpired as to the possible causes of these phenomena.

Can we suppose these morbid processes and lesions to be results of the drinking of impure water?

In dealing with this question, having only the produced effects as *data*, we must be careful not to beg the whole question on one side or the other. Indeed we cannot possibly consider the matter justly until we have inquired what we mean by impure water. But we are in a position to judge whether such changes may or not result from certain characters of substances ingested to the alimentary canal.

We can predicate broadly that these results *may* be brought about first by (*a*) morbid changes in bioplasm without appreciable direct external agency; and secondly, by (*b*) direct external agency.

The first opens up the old question of the occurrence or not of *spontaneous* or *de novo* cases of specific disease. It would be unprofitable and foreign to the inquiry on which we are now engaged to go into this question. Probably most etiologists have their own views on the subject, which they would not be easily induced to surrender. However, it is worthy of remark that, considering the morbid processes and lesions by themselves (whatever that evidence may be worth), there are no grounds for denying the possibility of the occurrence of such cases. The second statement, that these morbid changes may result from direct external agency, practically includes two statements—namely, origination by deleterious substances ingested, and propagation by specific disease products ingested. It is impossible to draw any very practical conclusion from the slight evidence at present before us, and we pass on to collect further facts; but, so far as we have gone, it seems there are no grounds for denying that cases of the diseases under consideration may be *originated* by

Vital organic changes, and by
Ingestion of deleterious aliment;

and may be *propagated* by

Ingestion of specific disease product.

CHAPTER II.

HYGIENIC MEANING OF THE TERM "IMPURE DRINKING WATER." (*Agent.*)

Drinking Water influences the production of the diseases of which this Essay treats by being impure. Hygiene meaning that branch of medicine of which the object is the preservation of health, it follows the hygienic meaning of the term "impure drinking water" is water which, from its composition, is deleterious to health. I shall not, therefore, in this chapter consider all water impurities, but only such excesses or deficiencies of normal constituents, or the presence of such foreign matters as are known to be injurious to health, or those of which there exists a reasonable probability of their being so. But when is water fit for drinking purposes, and when unfit? Prof. Parkes, in his Report on Hygiene for 1867, observed—"At the present moment matters have been brought to such a pass that if a question of water analysis came before a court of law, hardly any chemical point could be raised on which a chemist might not be called on either side. This is unfortunate and unnecessary. I feel no doubt that a very safe opinion can be given of the hygienic properties of a water by the combination of a few simple chemical and microscopical tests." I shall therefore restrict my observations to those impurities of drinking water which can be determined by simple tests, without going into the details of the professional chemist or microscopist.

I have said that water is hygienically impure when its composition is deleterious to health, but such a water must not necessarily be chemically impure. In their Report the River Commissioners divide all river pollutions into *Sewage* and *Manufactory refuse*. Under the former term are included the solid and liquid excreta of man and animals, and also house and wash water, and in fact all impurities coming from dwellings. Under the head of "manufactory refuse" are included all impurities coming from dye, paint, and bleach works, chemical works, tanneries, paper-making, woollen works, breweries, and some others less frequent and less injurious, as metal works, &c.

This, however, does not include many sources of disease which may arise from impurity of drinking water. It refers only to affirmative or positive disease cause, omitting deficiencies of some of the normal constituents of water.

As a cause of disease, impure water must be considered in its threefold agency as a predisposing cause, an originating cause, and a propagating cause. This classification, however, refers more to its mode of action in causing disease, and I will postpone the consideration of these three modes of action until treating of the rationale of its operation in the Fourth Chapter. In this place it will be most convenient to classify impurities of water as deficiency of normal constituents, excess of normal constituents, and presence of foreign or abnormal matter, which may be subdivided as follows:—

Deficiency of normal constituents	. . . .		Predisposing to or originating disease.
Excess of normal constituents	1. Mechanical		,,
	2. Chemico-physiological . . .		,,
Presence of foreign matters	1. Mechanical		,,
	2. Chemico-physiological		,,
	3. Organic	1. Ordinary organic impurities . .	,,
		2. Extraordinary or specific organic impurities	Predisposing to, originating, or propagating disease.

This division has no pretensions to being an accurate scientific classification. It does not include *extraordinary non-specific organic matters*, and is open to other objections; but it seems convenient and sufficient for the practical purposes of the inquiry on which we are engaged.

The connection between deficiency of normal constituents of healthy water and disease origin is not well established.

However, it seems quite reasonable that the withdrawal of certain constituents, especially if their ingestion have been for some considerable time previously continued, may originate certain morbid conditions, and there seems some ground for believing that, as in the outbreak of scurvy referred to on page 163, deficiency of water supply—which of course includes withdrawal of usual constituents—originated or predisposed to the disease. A deficiency of calcic phosphate and sulphate seems to cause or predispose to rickets and defective nutrition of bone. The diseases under consideration in this Essay, however, do not seem to be predisposed to or originated by such deficiency; it is, therefore, unnecessary for us to dwell further on this matter.

Under the head of "Excess of Normal Constituents" we may include in the first division—*mechanical*—all suspended impurities, such as silica, alumina silicate, calcic carbonate, calcic and alumina silicate, particles of ferruginous soils, and débris of animal and vegetable matter. When such exist in excess of a soluble amount, they become mechanical impurities, and cause irritation of the alimentary canal, both predisposing to and originating disease. The action of these is considered further on, in the chapter on the Rationale of the Operation of Impure Water, and also under the head of "Irritation Diarrhœa" on page 141.

Excess of normal constituents, the action of which is *chemico-physiological*, is generally a result of the passage of water through certain soils. Strictly speaking, these are impurities, yet they are so usually present that they must be considered as normal constituents. A good description of such is given in Dr. Parkes' "Hygiene," in the section 'Summary of Drinking Waters.'*

These have no apparent etiological connection with the diseases we are specially engaged in considering.

The presence of foreign matters which act mechanically cause some forms of diarrhœa, and may predispose to, if not produce, the simplest varieties of dysentery. But these are not of sufficient importance to be considered here in detail. We are, in this inquiry, specially interested in the two last groups of the above classification—namely, "foreign matters of chemico-physiological action," and "organic foreign matters."

The members of the foregoing groups may predispose to or originate the simpler forms of diarrhœa and dysentery.

But it is doubtful that they ever do so without the concurrence of a second factor. The dissolved foreign matters which act by some obscure combination of the chemical and vital processes, and foreign matters of purely organic origin, are of far more interest to the etiologist. These two divisions may be again grouped as follows :—

* First Edition, p. 14.

1. Foreign matters of chemico-physiological action.
   A. Inorganic.
   B. Organic products.
2. Organic foreign matters.
   A. Ordinary organic impurities.
      a. Animals, } and primary products of their
      b. Vegetables, } decompositions.
      c. Excrementitious matters.
   B. Extraordinary or specific organic impurities.

Under *inorganic matters of chemico-physiological action* we have the various compounds resulting from the combination of nitric and nitrous acids (resulting themselves from the dissolution of the elements of organic matter) with metallic oxides, as those of lead, copper, iron, calcium, zinc, &c. The occurrence of such compounds in drinking water produces diarrhœa, and may cause or predispose to certain forms of dysentery. The view has been advanced, and will be considered further on, that enteric fever and cholera may in like manner be predisposed to by the use of certain impure drinking waters.

Organic products of chemico-physiological action (being the results of the breaking-up of organic matters) must be considered separately from the latter (organic matters), because they act as if they had been ingested as inorganic matters. It is not meant here that the action of organized animal and vegetable products in the economy is not chemico-physiological; of the nature of the action of organic matters prior to the occurrence of chemical changes we have no satisfactory evidence; but from what we have it would appear that their action is much more complex when acting in their integrity than after they have been broken up by putrefactive change.

With reference to this subject Dr. Macnamara has observed that water containing the evacuations of cholera patients, after the lapse of a day or two, when the vibrios will have disappeared and given place to ciliated animalcules, may be ingested with impunity. When the products of the putrefaction of animal and vegetable matter are examined, they are found to consist of some or all of the following substances :—

Sulphuretted hydrogen,
Phosphuretted hydrogen,
Ammonia,

Phosphorus and nitrogen bases of complex constitution,
Acetic, butyric, valerianic &c., acids,
Carburetted hydrogen,
Hydrogen,
Carbonic oxide,
Carbonic acid,
Nitrogen.

We now pass on to consider the *ordinary organic impurities* of drinking water, including animal and vegetable matters, and excrementitious product.

The presence of infusoria and animals of low type is of importance as indicating the existence of organic matter, but it has not been shown that they are in themselves very hurtful. There are, however, certain parasitic growths, entozooal and entophytal, which seem to possess a well-marked influence in predisposing to and originating, and possibly also in propagating diseases of the intestines. These will be referred to when treating of Entozooal Diarrhœa.

The existence of animal excrement in drinking water has long been recognized as a prolific source of disease, both when the water is contaminated by the actual solution in it of such matters, and when it is contaminated merely by the access of sewer air to reservoirs in which it is contained.

It is generally believed that the organic impurities of water which are excrementitious products of man or animals, exert a more deleterious influence on health than those which result from nitrogenous vegetable matter, which is usually termed "vegetable albumen." Such a belief seems well founded, because we find that even the healthiest drinking waters contain some vegetable albumen—the purest waters from granitic and clay-slate districts containing from 0·3 to 0·7 grains per gallon, and the purest limestone waters from 0·3 to 1 and 1·5 grains per gallon. The waters from sandstone formations usually contain more than this. Waters passing through certain bogs in Ireland are heavily charged with vegetable albumen—so much so as to be coloured by it. Yet we find that such waters are innocuous, and their ingestion unattended by detriment to health. So also in certain mineral waters, noticeably those of Baden, Ischia, Plombières; and in the sulphurous springs of the Pyrenees the nitrogenous substances known as zoögene and glairine are so abundant as to be visible as an iridescent film floating on the surface. Yet these also are ingested with impunity. If such an amount of true animal organic matter (that is, fæcal and biliary matters, and the products of decomposed flesh) exist in water, the use of such seldom fails to produce disease. Urinary contamination has not been proved to be of so much importance, probably from the metamorphose of urea into ammonia carbonate, and the well-known unstableness of the other constituents of urine.

Organic impurities simply suspended seem much more hurtful than those which are dissolved; and it is curious how the effect of such organic impurities is influenced by use and tolerance. Professor Parkes observes—"A water may contain as much as twenty or even thirty grains per gallon, and be considered good by those who drink it. In those who are unaccustomed to it, or in all, apparently, under certain conditions of decomposition, such as high temperature, water of this kind produces diarrhœa, and even choleraic symptoms, and there is some evidence to show that it predisposes to true cholera, as will be presently noted. Possibly the organic matter may, under certain conditions, commence to undergo fermentative changes, and then becomes suddenly poisonous; but it seems likely that the perfectly dissolved animal organic matter is not quite so injurious as that which is merely suspended."

It is still a question in etiology whether the ingestion of particles of sewage containing ordinary healthy bowel evacuations (not a specific contagion) can *originate* specific diseases. Some etiologists believe such is the case; while others hold that a specific contagious principle is necessary to originate a specific disease. All of course agree that a specific contagious principle is necessary to *propagate* a specific disease. Others, again, seem to think that ordinary putrid matters may give rise to a specific disease, containing a specific contagion, although independent of any foregoing case of the same disease.*

These matters will be more particularly considered in the chapters on Enteric Fever and Cholera.

Extraordinary or specific organic impurities, as separated from the *ordinary organic impurities*, embrace, as a class, the special *viri* of contagion. It is no part of our present purpose to consider the

* "La matière putride est plutot le vehicule, la compagne du principe contagieux de la fièvre lente nerveuse que ce principe même."—M. PIERRE FRANK.

intimate nature of contagion. Such an inquiry is quite beside the subject of this Essay.

It is sufficient for us to know that etiologists and pathologists have agreed to recognize an active principle or cause of propagation of disease, provisionally termed *contagion* or *virus*, and that this is believed to be contained in the secretions of an affected person. We must, however, remember that, as far as actual experience goes, contagion has only to do with propagated cases of disease. The principal points known concerning contagion are given in the following paragraph from Dr. Lionel Beale,* with which I close this chapter :—" The minute contagious bioplast is less than the $\frac{1}{100000}$ of an inch in diameter, and often so very clear and structureless as to be scarcely distinguishable from the fluid in which it is suspended. Such a minute particle may readily be transferred from the affected organism to an apparently sound organism. It may be carried a considerable distance from its source without losing its marvellous power of causing in the organism invaded a series of changes resembling, and often in very minute particulars, the phenomena which have occurred in the organism from which it was derived. And it is established that there exist different kinds of contagious living bioplasm, each capable of occasioning specific phenomena which distinguish it. The poison of small-pox will produce small-pox, not typhus fever or measles, &c.; nor will any of these produce small-pox. Without, therefore, pretending to identify the actual particles of the living bioplasm of every contagious disease, or to be able to distinguish it positively from other forms of bioplasm, healthy and morbid, present in the fluids, on the different free surfaces and in the tissues, in such vast numbers, I think the facts and arguments I have advanced prove—first, that the contagious virus is living and growing matter; secondly, that the particles are not directly descended from any form of germinal matter or bioplasm of the organism of the infected animal,† but they have resulted from the multiplication of particles introduced from without; thirdly, that it is capable of growing and multiplying in the blood; fourthly, that the particles are so minute that they readily pass through the

* " Disease Germs," p. 244.

† It is to be remembered that this limitation refers only to propagated cases (the effects of contagion), and not to originated cases of specific disease.

walls of the capillaries and multiply freely in the interstices between the tissue elements or epithelial cells; and lastly, that these particles are capable of living under many different conditions, that they live and grow at the expense of various tissue elements, and retain their vitality, although the germinal matter of the normal textures, after growing and multiplying to a great extent, has ceased to exist."

CHAPTER III.

THE PHYSIOLOGY OF THE INTESTINAL CANAL, WITH REFERENCE TO THE INFLUENCE OF DRINKING WATER IN ORIGINATING AND PROPAGATING DISEASE.—(*Object.*)

As in diarrhœa, dysentery, enteric fever, and cholera, the alimentary mucous membrane appears to be primarily affected, it is reasonable to suppose that the initial steps of these disease processes take place at this part of the organism, or in its immediate vicinity. Hence, to understand these processes and the nature of the influence by which drinking water may inaugurate them, we must consider the physiology of this tract with reference to the action of the various constituents of drinking water on it. It must, however, be borne in mind that this part of the organism, although so seriously affected in these diseases, need not necessarily be more than one of the portals by which the causes of these disease processes find entrance to the organism.

Indeed, experiments afford a probability, almost amounting to certainty, that one if not more of them may be produced by agencies acting through the function of respiration. But even when such is the case we find that the incidence of the disease process is to be observed chiefly in the intestinal tract; only in the simpler forms of diarrhœa is diseased action limited to the alimentary mucous membrane. In the more complex forms of this affection, and in dysentery, enteric fever, and cholera, a definite series of systemic phenomena accompanies their local intestinal lesions. We must, then, consider the alimentary canal as an organ, the normal function of which may be disturbed through the influence of drinking water; and, further, as an organ through the function of which the influence of drinking water may cause systemic disease. This hollow tube is really part of the external surface of the body, although hidden from view in the greater part of its extent; and substances contained in it are as much outside the body as a finger introduced into a finger-ring is outside the metallic structure of which the ring is composed. We must believe that the greater proneness of this hidden surface to disease than the visible surface, and its greater readiness to act as an avenue for causes of systemic disease, arise from the more abundant complexity of structure, and the part which this structure plays in the systemic function of nutritive absorption. Thus this tract is susceptible of local disease by the contact of drinking water; is capable of transmitting systemic disease by absorption of deleterious agency brought into contact with it by drinking water; and further, may act as an eliminator of disease products, which latter, passing into drinking water, may become propagators of disease when introduced by the same vehicle into another alimentary canal.

This "OBJECT," then, on which drinking water acts in producing disease must be considered here as—1st, An organ liable to local lesion (structural disease); and 2nd, An organ liable to (*a*) deranged absorption, and (*b*) deranged elimination (functional disease).

(*a*) Except in the more simple forms of diarrhœa, when the affection is merely a muscular contraction from irritation or an intestinal catarrh, these three groups of lesion—local structural lesion, morbid absorption, and morbid elimination—occur in the four diseases of which this Essay treats. Local lesion is produced principally by irritation of the nerve filaments ramifying in the mucous surface, which produces either simple muscular contraction and expulsion of the contents of the canal, or a flux of blood to the irritated part, which passes on to congestion or catarrh. In the first case the resulting alvine flux is simply the natural contents of the intestine, probably more or less altered in consistence; in the latter a product of disease is added. This added catarrhal mucus is the first and simplest of the disease products which occur in the morbid alvine fluxes of this group of diseases. Here it is simple and non-specific; but as we proceed to the more complex of this group of disease processes—enteric fever and cholera—we find that the disease product, of which their alvine flux chiefly consists, is complex and specific; this complexity being apparently proportional to the functional complexity of the glands which eliminate it. But non-specificity and specificity of disease-products do not necessarily mark any fundamental difference in the disease-processes of which they are the products.

For example, in peritonitis we can trace the disease product from a simple non-specific exudation through all its stages up to a most virulent specific contagious product. And this is one ground for believing that the so-called premonitary diarrhœa of cholera is in reality the first stage of the disease; and that cases of the diarrhœa always so prevalent in epidemics of cholera are themselves cases of cholera in which the cholera process stops short of the later stages of the disease, either in consequence of remedial interference or some other influence. "The communication of cholera by the so-called premonitary diarrhœa (*i.e.*, the early stage of cholera) is now beyond dispute" (Prof. Aitken, "Sci. and Pract. of Med.," vol i. p. 672). This view does not coincide with certain modern doctrines on the nature of *specificity:* but we must be guided more by facts than by dogma. Simple irritation diarrhœa is produced by muscular contractility, which is not a vital movement.

As we proceed in this scale of disease processes, we find that they increase in functional complexity, until we arrive at cholera, in which every vital function seems to be more or less affected. The muscular contractility which can be produced by drinking water, is a reflex effect of nerve irritation. The same effect is frequently produced by other causes acting as central irritation (as the diarrhœa of certain mental states); but we here consider only the effects of agents likely to be contained in drinking water.

Severe or long-continued irritation of the nerve supply of the alimentary canal may not only produce local lesion, but also bring about systemic pathological changes of great severity. This point is of importance in the consideration of the influence of drinking water in predisposing to some of those diseases. Indeed, so complex and severe are some of the effects of this irritation, that it is often difficult to trace them to their true cause.

This arises from the variety of sources from which the nerves of the alimentary canal are derived, and from the close relationship into which the great nerve centres are brought in the abdomen. Here branches from the pneumogastric nerve join the solar plexus and semilunar ganglia of the sympathetic, and supply not only the stomach and duodenum, but also the liver, suprarenal capsules and kidneys, and unite with the phrenic nerves.

This complexity of distribution of the pneumogastric, taken with the fact that it supplies the stomach and upper intestine with both afferent and efferent filaments, accounts for the difficulty frequently experienced in identifying the source of many functional derangements arising from nerve irritation at the intestine. This irritation is most frequently produced, as regards drinking water, by the groups of impurity named *Mechanical* and *Chemico-physiological* in the preceding chapter, and is the simplest effect of the *Organic* group.

(*b*) But disease causes find their way into the organism through the intestinal tract principally by means of capillary absorption. Several well-established facts regarding the absorption of toxic agency render it nearly certain that such absorption does not occur through the lacteals. The experiments of Prof. Högges seem to prove satisfactorily that the same result follows the administration of cholera discharges in food, and the inspiration of a stream of air passed over cholera discharges. Indeed, it is not likely that a particle of contagium (whatever that may be) can be absorbed by a lacteal and still preserve its integrity as a particle of contagium. We know from the experiments of Bernard, which were confirmed by Frerichs, that organic substances are quickly and entirely decomposed by the action of the gastric and other gland secretions in the process of lacteal absorption. A dose of emulsin is given to a dog, and shortly after a dose of amygdalin. No injurious result follows, because the emulsin has been decomposed before the introduction of the amygdalin. It is not likely, then, that a highly complex organic substance, such as a particle of diseased bioplasm, or whatever a particle of contagium may be, can escape decomposition, and be introduced by the lacteals into the circulating blood, in all the integrity of its pathetiological potentiality. This difficulty occurs only when we assume contagion to be a definite *materies morbi*, or particulate entity ; and disappears if we think of it as an influence, or specific modification of vital forces (an abnormal mode of vitality), which may reside in a fluid as well as in a solid particle of matter.* But Bernard found that if, before administering the above substances, he divided the pneumogastric nerves, the substances were absorbed into the blood undecomposed, and meeting there, poisoning by hydrocyanic acid, the product of their combination, resulted.† I

* For reasons to doubt the particulateness of contagion, see page 135.

† These experiments open up a wide field of the utmost interest to the student of contagion. Have we here an explanation of the facts—that many persons

think, on the whole, we must conclude that the majority of disease causes likely to be introduced into the alimentary canal by drinking water, are absorbed by way of interstitial absorption by the capillaries, and not by lacteal nutritive absorption. I shall have to return to this point when considering in the next chapter *the influence* by which drinking water propagates disease.

(*c*) Finally, as regards elimination of disease product, which is held to perform so important a part in the propagation of at least two of these diseases, I have, on page 113, alluded to the objection advanced against the doctrine of elimination of disease product. If this objection refer only to the use of the word *elimination*, maintaining that the term should be restricted to the normal removal of effete material from the organism, the objection may be admitted. But I do not use the term elimination in this connection in the ordinary meaning of the word, and few others do. I do not mean by the word as here used a removal from the blood of a certain definite substance by gland cells, whose proper physiological function it is to separate such a definite material from the blood. I use the term here (for want of a better) merely to express a shedding of living matter—not a definite substance, but any material which is still endowed with organic vitality at the time of this shedding of it from the body. (I avoid the word bioplasm, because it may be mistaken as representing some definite entity of constant composition and properties; although there must be many forms of bioplasm—that of bone, of muscle, of cartilage, &c. &c.) I consider it accidental, and in no way dependent on their normal function, that the intestinal glands so often happen to be the seat of this shedding. When we consider that *what we see* of organisms is really dead or formed matter—that in rigid actuality we associate and converse with *corpses*

exposed to contagion escape; that contagion is rendered innocuous BY DIGESTION, while in others, in whom the function of the pneumogastric nerve is impaired, contagion may be admitted to the circulation by interstitial absorption, in the integrity of its disease propagating power; and that susceptibility to certain contagious diseases (the so-called entero-zymotic group of them, as distinguished from the enthetic), seems to increase when persons go from temperate to tropical climates, and in temperate climates during hot weather; these circumstances being known to cause impairment of the function of the pneumogastric nerve? Read in this light, Dr. Habershon's recently published "Lumleian Lectures on the Pathology of the Pneumogastric Nerve" may be a valuable contribution to our knowledge of the influence of contagion. "Why the material, whether choleraic or non-choleraic, should exert its power in some instances and not in others, we cannot explain" (Lewis and Cunningham's Report, 1874. See also their "Experiments on the Section of the Splanchnic and Mesenteric Nerves," for the same year).

(because to produce formed material the vital matter of the blood must first die), we understand that such a shedding of material, *possessed of its organic vitality in integrity* AT THE TIME OF ITS SHEDDING, must be an occurrence of extreme rarity. In some specific fevers the closed intestinal glands (which for some function with which we are ignorant contained this unformed vital material, as revealed by reagents under the microscope) rupture, and admit of the escape of this material still endowed with vitality, or specific modification of vitality, resulting from the peculiar disease process going on in the organism at the time of its first elaboration in the blood.

It is true that in some diseases of the human organism, and in cattle plague, this unformed vital material is found in other situations, as on the tongue, fauces, mammæ, under the epidermis, &c.; but the shedding of this material by the intestinal glands is an occurrence of much greater frequency than in any other part of the organism. I am therefore of opinion that the so-called elimination of disease products is not a part of, and has no necessary connection with, the *normal* physiological function of the intestinal canal, and is not conducted by the intestinal glands as a part or in the course of their *ordinary* physiological business. This point has a very important bearing on our views of the nature of the influence of contagion, and I shall have to refer to it again when considering the influence of drinking water in propagating enteric fever and cholera.

CHAPTER IV.

RATIONALE OF THE OPERATION OF IMPURE DRINKING WATER ON THE HEALTH OF INDIVIDUALS AND COMMUNITIES.

We have now to consider the nature of the action of drinking water in causing—originating and propagating—these diseases. The word used in the theme to express this action is *influence,* and it would seem, advisedly, because this word does not commit those who use it to either of the views which so broadly distinguish those who concern themselves with the important questions of pathetiology under discussion at the present time.

The two great scientific problems of the day—namely the evolution of life, and the germ nature of contagium—have largely affected our ideas with reference to the intimate nature of the influences which cause disease, and the nature of that causation.

Within the last quarter of a century, medicine, in common with general philosophy in all its branches, has to a great extent, tended to pass into cosmology—into the science of the mere physical universe.

The tendency of modern belief is, that because experimental research has done so much, it is capable of doing all: that no influences exist but those which can be explained by physical laws.

This tendency has been fostered by our improved means and methods of physical investigation, as well as by the increased scepticism of our age, and its intolerance of faith in dogma, as "a creed outworn."

As Positivism reduces general philosophy to an acquaintance with physical sciences—astronomy, terrestrial physics, chemistry, biology, history, sociology—so, in the same way, recent speculative pathetiology would seem to assume that the special biological problems of the beginnings of life, the influences of the causes of death, the equilibrium of health, the origin and transmission of disease, the phenomena of heredity, the nature of nerve functions, &c., are altogether explainable as the actions and interactions of physical forces now known or hereafter to be discovered, and that they are capable of being formularized as physical laws. This may be termed the modern scientific view of the subject.

There is, however, another view still preserved by quite as advanced investigators and thinkers on these special biological questions—namely, that these problems are not so easily to be explained by any physical laws which present experience justifies us in believing it is or will be possible for the wisdom of man to discover. This is the philosophic view of the subject which endeavours to withstand the very free and rapid generalizations of modern science.

This view is continually open to modification by scientific discovery, and thus is never out of accord with any ascertained scientific truth.

Indeed, it is much less so than the other; the scientific view being scientific only in its methods of investigation, and being, as regards its conclusions, in reality the least scientific of the two.

Philosophy, if we accept modern definitions of it, being the systematizing of the conceptions furnished by scientific facts, and being based rigorously on fact, is unable, in the present state of scientific experience to accept all the conclusions which go by the name of modern scientific generalizations.

Thus this paradox has come to exist, that modern science is more speculative than modern philosophy. "The fundamental ideas of modern science are as transcendental as any of the axioms in ancient philosophy."*

The subject of the influences of the causes of disease or the causes of death is closely related to that of the beginnings or causes of life, and we are not at present in a position to prove that what we all of us believe, or think we believe, on these two subjects is true—that is, absolutely based on scientific fact. Hence, in the absence of experimental proof on all of these matters, what we mean by the *influence* of disease cause is merely our *ideas* regarding the beginnings and endings of life; so that in a great measure the question resolves itself into a metaphysical one.

* G. H. Lewes, "Philosophy of Aristotle," p. 66.

Whether we take any of the modern definitions of philosophy, or call it, as Plato beautifully does in one passage, "a meditation of death," the subject of the causes of disease must form a large portion of philosophical inquiry.

And this always has been so from the dawn of philosophy, even as it is to-day; with this exception, that formerly those who concerned themselves with the investigation of vital phenomena—physiologists, psychologists, phrenologists, or whatever name we may give them—preserved intact that distinction between mind and matter which, from its universality, would seem to be an idea inherent to our being.

But the system of Auguste Comte broke down, and for a certain class, to some extent at least, did away with this distinction. For his disciples there is no such old-fashioned idea as an Anima or vital principle. For them thought is cerebration; the beginning of life is merely the transformation of physical force into vital force; and disease and death simply the breaking-up of organisms in which such vital forces for a time operated, and the liberating of these to resume their actions as physical force in inorganic matter. That this system is a satisfactory one, even to its professors, seems doubtful.

"With all their devotion to their researches and conviction of their grandeur and importance, they regard themselves, so far, as physicists and nothing more; they do not deny the possibility of a philosophy as something over and above—nay, they either desiderate such a philosophy or already possess such a philosophy in private, as a spiritual background and relief from their physics."* But there are still some who think and hope that these views of life and death are mainly due to the impetus given to physical research at this time, and the exaggerated ideas of what is possible to such research.

"In the enthusiasm awakened by the discovery of some new facts or of some new forces, and in the freshness with which they impress the idea of such agencies on our minds, we sometimes very naturally exaggerate the length of way along which they carry us towards the great ultimate objects of intellectual desire. We forget altogether that the knowledge they convey is in quality and in kind identical with knowledge already long in our possession,

* Professor Masson, "Recent British Philosophy," p. 276.

and places us in no new relation whatever to the vast background of the Eternal and the Unseen. Thus it is that the notions of Materialism are perpetually swept away—swept away partly before the intuitions of the mind, partly before the conclusions of the reason. For there are two great enemies to Materialism—one rooted in the affections, the other in the intellect. One is the power of *things hoped for*—a power which never dies; the other is the evidence of *things not seen*—and this evidence abounds in all we see. In reinforcing this evidence, and in adding to it, science is doing boundless work in the present day. It is not the extent of our knowledge, but rather the limits of it, that physical research teaches us to see and feel the most. . . . . There is no point, short of the last and highest, at which science can be satisfied. Her curiosity is insatiable. It is a curiosity representing man's desire of knowledge. But that desire extends into regions where the means of investigation cease, and in which the processes of verification are of no avail. Above and behind every detected method in nature there lies the same ultimate question as before —What is it by which this is done?"*

It is not necessary for me to point out here that many of our advanced scientific etiologists in the present day seem to deny that it is possible to pursue scientific investigations up to a point at which the ordinary processes of verification are of no avail. The finiteness of the capacity of the human mind would seem to be ignored by many who, in the presumptuous vanity of science, rest placidly on a system of absurd phraseology and nomenclature, as if it were knowledge, the high-sounding pretension of which only serves to draw the more notice to the ignorance of which it is a cloak.

Thus, when considering here the nature of the influence of drinking water in originating or propagating these four diseases, we must be careful not to mistake an obscure phraseology for explanation. To say that the influence of drinking water in originating diarrhœa is due to *irritation* or *congestion*, or to say that the influence exerted by drinking water in propagating enteric fever or cholera is due to its acting as a vehicle of *contagion*, are no more explanations of its influence than to say that some chemical process is due to *katalytic* action. It is much

* Duke of Argyll, "Reign of Law," pp. 115 and 271 (People's Edit.).

better not to use any phrase to express such influence than to run the risk of resting satisfied on a word, and fancying we know all about an influence of the nature of which we are really profoundly ignorant, such as those influences grouped as *vital forces*.

Drinking water influences health, and thus (so far as the present inquiry is concerned) causes disease only when it is impure. And this it does in three ways, namely:—

1. By being the vehicle of predisposing causes of disease;
2. By being the vehicle of originating causes of disease; and
3. By being the medium of the transmission of disease.

1. The influence of drinking water in predisposing to disease.

Predisposing influence is often included in or confounded with originating influence. I do not say that this is always erroneous. There are grounds for believing that under certain circumstances these influences are identical in nature, differing only in degree, and that predisposing cause may sometimes pass imperceptibly into a cause of absolute origination of disease.

But in this place we have to consider them as distinct in their action, if not in their ultimate nature. The predisposing conditions with which we are most concerned here, which we find produced by drinking water, are those forms of lesion of nutrition often spoken of as *poverty of blood, impairment of general health, cachexiæ*, &c., which follow on the continued use of drinking water contaminated with putrid organic matter. These conditions predispose particularly to alvine fluxes and other forms of catarrh, inasmuch as they are accompanied by notable feebleness to resistance of the capillary vessels, and a yielding atonic state of the tissues through which these vessels pass. Further, such conditions render the system prone to succumb to the influence of disease excitants or poisons, whether these be the agents of disease origination or of disease propagation. Another predisposing condition frequently produced by drinking water is chronic irritation of the mucous surface of the intestinal tract.

It has been proved by experiment that certain disease poisons (noticeably the choleraic principle) act with increased certainty and severity when such a gastro-intestinal irritation pre-exists. This irritation is brought about by mechanical and chemico-physiological agency, as well as by the organic matters noticed as producing the cachexiæ above referred to. The theme of the

Essay dealing only with disease origin and propagation, it is unnecessary to dwell further on predisposition, which is here noticed only in so far as it is included in the subject of disease origination.

2. The influence of drinking water in originating disease.

Our knowledge of the carriage by drinking water of originating causes of disease is far from precise. It rests chiefly on the method of exclusion, and on what may be coincidence. As a type of this influence of drinking water may be taken the generally accepted doctrine of the production of bronchocele by this agent. The investigations of McClellan on this point, as given in his work on the "Medical Topography of Bengal," seem convincing. The same prevalence of this disease in regions in which the drinking water supply is derived from limestone formations is observed in polar regions, on the European continent, and indeed all over the world. Yet it must be remembered that the prevalence of bronchocele in such regions may be—so far as drinking water is concerned—only a coincidence; and that this geological peculiarity may operate as a disease producer in different ways—such as affecting the configuration of hills and valleys, and through these, atmospheric currents, &c.—quite independently of drinking water.

So in the same way there is hardly any case in which we can absolutely prove that the prevalence of a disease and certain peculiarity of water supply are more than coincidences. If such proof or certainty be at all possible, it is perhaps so only when the disease cause is mechanical, as in the irritation forms of diarrhœa following the use of drinking waters containing mechanical irritants. And even were such connection between drinking water and disease as cause and effect established, it would be difficult to determine absolutely whether such cause was an excitant or a predisposant, or whether this agent combined both these factors of disease causation. Although our knowledge is thus inaccurate regarding this influence of drinking water, when we find that the ingestion of a certain water is frequently accompanied or followed by the occurrence of a certain disease, we are bound in practice to ignore the possibility of mere coincidence and to accept such a connection of cause and effect. It will be found that this has been done in the following pages as regards the diseases of which this Essay treats. In like manner, our knowledge is limited with reference to the nature of the influences by which diseases may be thus originated; but they may be roughly classified as in the table of water impurities on page 116.

3. The influence of drinking water in propagating disease.

Diseases are transmitted from one organism to another, according to the teachings of modern pathetiology, in two ways, namely, infection and propagation. This division of the influences of transmission is to a great extent arbitrary, and there exist no very clearly defined characteristics of the two processes, if they be two processes, and not different phases or conditions of one. An attempt is made to characterize them according to the agents by which they are brought about, by saying that the process of infection is produced by an influence called infection, and propagation by an influence called contagion—and the agent of infection is described as miasmata, emanations, &c., and contagion is described, according to different schools, as germs, bacteria, fungi, parasites, degraded bioplasm, albuminoids in a peculiar state of molecular change, &c. &c. When disease is said to be transmitted by drinking water it is usually the propagation phase of transmission which is meant, although it is by no means certain that the agents of infection may not also act through the medium of drinking water. The theme of this Essay, however, refers only to the influence of drinking water in originating and propagating disease; and propagation, as strictly understood, being produced by contagion only, we have nothing to do here with infection.

I here assume, as is generally believed, that whatever the active influence of propagation of contagious diseases is, drinking water is capable of carrying it.

It would be equally impossible and out of place to discuss the question of the nature of contagion in this Essay; yet, inasmuch as part of the subject is the *influence of drinking water in propagating* disease, it is necessary to glance cursorily here at what this influence is or may be. The points considered under the three following headings seem to be closely connected with the subject of the propagation of disease by drinking water.

Is contagium particulate or absolutely soluble in fluids? That is, is contagium a suspended or dissolved impurity of drinking water?

A prominent doctrine of the theories regarding contagion most in favour at the present time is that contagium is particulate. The experiments said to prove this, and the arguments founded on them, are set forth as strongly as possible in Maclagan's "Germ Theory of Disease," chap. ii. pp. 5–32. The present writer is among those who incline to doubt the conclusions drawn from these experiments, on the ground that the general method of them is incorrect. I do not think we are justified in drawing from experiments on contagious fluids in glass vessels, in the pathological laboratory, conclusions regarding the nature and action of contagium in the living organism. But even admitting the accuracy of their method, it does not seem to me that these experiments prove all they are said to do.

The experiments of Chauveau (*Comptes Rendus*, 1868, lxviii., and 1871, lxiii.), of Burdon Sanderson ("Twelfth Report of Medical Officer of Privy Council"), and of Braidwood and Vacher ("First Contribution to the Life History of Contagium"), prove that the difference between pure contagious lymph and the same diluted consists solely in the relative number of successes and failures of inoculation with them; the number of failures bearing a direct proportion to the extent of dilution. It is hence justly concluded that contagium is indiffusible *in certain liquids outside the body*. But does this prove that contagium consists of particles? Because an oil is indiffusible in water, does oil consist of particles? Undoubtedly in one sense it does. If one part of an oil be shaken with ten parts of water, every drachm of the resulting mixture will contain a certain number of oil globules (particles of oil); and if further diluted with a thousand parts of water, such mixture will contain in each drachm some hundred times less oil globules than in the previous case—exactly as the failures in inoculation with diluted lymph bear a proportion to the amount of dilution: but does oil therefore consist of *solid* particles? It is divided into innumerable portions called globules, which in these experiments seem perhaps to deport themselves somewhat after the manner of solid particles, but they are nevertheless liquid. I incline to think that the experiments above referred to prove only the same regarding contagious lymph—that it is in them divided into a number of minute portions which deport themselves like particles, but that they do not prove anything one way or the other as to the existence of contagium in the form of solid particles in the lymphatic system of a living organism.

We have not yet learned the effect of diluting living contagious lymph with a liquid identical with it in specific gravity and other physical properties, and in vital movements. *So far as these experiments go,* I consider the particulateness of contagium, as generally accepted in Great Britain, *non-proven.* The power of contagium to reproduce itself, to multiply, is by some considered almost conclusive of its particulateness. But Robin, in his "Leçons sur les Humeurs Normales et Morbides du Corps de l'Homme," explains this multiplication on the supposition of contagium being an absolute liquid, in, I think, quite as satisfactory a manner. He believes that the smallest quantity of matter (solid or liquid) from a diseased organism, being in a certain condition of molecular change, or, as I have termed it in this Essay, a specific modification of vitality, and carrying in itself potentially that specific mode of life-action, is capable of inducing a gradual change from healthy vital action to this specific diseased vital action in the coagulable principles first of the blood, then of the tissues of a healthy individual—the phenomena of propagation. If these views be correct, microscopic examination and our present chemical tests are powerless to detect contagium in drinking water; and this has a most important bearing on practice—namely, that if there be any suspicion of a disease being propagated by drinking water, a total acquittal of the water by microscopic and chemical tests must not be allowed to allay that suspicion.

Does contagium ingested with food or drink enter the blood in the same integrity of its pathetiological potentiality as when introduced by inoculation?

Here is another stumbling-block in the way of our accepting the necessary particulateness of contagium.

If the integrity of its disease-propagating power depends on its existence as an organized particle, is it likely that this power would withstand digestion? But we have many reasons to believe that contagium ingested is as potent as contagium inoculation; hence we must believe that this potentiality depends on a specific vital action stamped in its elements, and not on any peculiar combination of these in an organized particle. I have in a former chapter (Part I., chapter iii.) dwelt on the probable action of the gastric fluid, and of capillary and lacteal absorption on contagium, and indicated that if contagium preserves its integrity of disease-propagating power under these influences, certain accepted doctrines regarding its nature must be received with caution, and that it is more in accordance with known physiological laws to consider contagium an influence, or mode of vitality, of which the minutest portion (solid or liquid) of an organism may be a carrier potentially, provided such portion of matter still preserves its vitality, in the strict sense of the word—that is, has not undergone the molecular or physiological death, which is a necessary part of its transition into formed material.

This question also has a very important practical bearing on the subject of the propagation of disease by drinkiug water.

Is the contagium of a disease the sole cause of that disease?

Throughout this Essay I endeavour to defend the doctrine that *specificity of a disease does not necessarily imply specificity of its cause.* With reference to this view, which accepts the possibility of identical effects being produced by a variety of disease-causes, I will here only cite some chemical processes which seem to bear some analogy to this etiological problem. Take, for example, the decomposition of water. By whatever means brought about, *the process is always the same.* The usual mode of performing it is by electrolysis. Do we therefore say that this process can only be brought about by this peculiar modification of force, of the nature of which we are so ignorant? Not so, for we are aware that the same result is produced by white-hot platinum, through the influence of another little understood modification of force—namely, katalysis. Do we hence conclude that the decomposition of water results only from the influence of certain obscure modifications of force of the nature of whose action we are altogether ignorant? Not so, for we know the same result follows the passing steam over red-hot iron; and here we can account for the rationale of the process unexplainable in the other two cases. In the same manner, I would maintain, a specific disease may be produced by some unknown influence, bearing more resemblance to katalytic action than to any other form of physical force with which we are familiar—namely, propagation by contagion—*and may also* be produced in a less mysterious way through the influence of agencies with which we are better acquainted, of the nature and action of which we have some definite knowledge. This view I hold to in the following pages, endeavouring to show especially that enteric fever may be produced by contagion and by mere putridity—may be both propagated and originated—the mysterious nature of the first influence not precluding an explanation of the second. We know that here, as in the decomposition of water, the process must in all cases be the same, or specific; but in one case we can explain the rationale of the inauguration of the process, while in the other case we cannot.

In concluding this portion of the Essay, which to a great extent deals with hypothetical matters, I would again impress the very speculative character of these questions, and the possibility of their always remaining so. In passing now to consider the more practical matters treated of in the second part of the Essay, we must endeavour to leave behind any preconceived ideas regarding these speculations. We must, above all, beware of the party cries of modern science, which divide us into factions, and degrade the knowledge of universal eternal laws to the level of little fitful local politics; and of that blight on all noble purpose which is eating its way like a canker through modern science—

> "The longing for ignoble things,
> The strife for triumph more than truth."

PART II.

SPECIAL PATHOLOGY, ETIOLOGY, ENDEMIOLOGY, AND EPIDEMIOLOGY OF DIARRHŒA, DYSENTERY, ENTERIC FEVER, AND CHOLERA; WITH REFERENCE TO THE INFLUENCE OF IMPURE DRINKING WATER ON THE ORIGIN AND PROPAGATION OF THESE DISEASES.

CHAPTER I.

THE INFLUENCE OF DRINKING WATER IN ORIGINATING DIARRHŒA.

The systematic study of the various forms of catarrh of the alimentary canal, grouped somewhat rudely under the term Diarrhœa, is a matter of no small difficulty.

When treating in a previous chapter of the general pathology of the diseases which we have to consider, I pointed out certain difficulties in the way of a systematic study of the different forms of diarrhœa inherent to the nature of the subject.

But these natural difficulties are added to by artificial ones of our own making. A student seeking knowledge regarding the nature of diarrhœa from books is at once repulsed by finding that altogether different affections are included in the word diarrhœa, and that, generally, no intimation is given of the form of the affection referred to. If diarrhœa be thus treated of in systematic medicine, there is no reason why dropsy should not be permanently retained in systematic nomenclature, ignoring the various conditions which may give rise to it.

It is especially to be regretted that the term diarrhœa is accepted as the permanent designation of a pathological condition in registered statistical records, such as those of the Army Medical Department, because we thus lose much valuable information which would be available had the various forms of the affection been distinguished. At present the statistics of diarrhœa are comparatively useless. We have no available data from its frequency and mortality as compared with other diseases, from its epidemiology or endemiology, or from the symptoms and after-death appearances, recorded of cases loosely grouped under the name diarrhœa. At least no trustworthy conclusions can be drawn from such as premisses — diarrhœa as thus used being what logicians call a distributed term.

For these reasons we are obliged, in endeavouring to consider the etiology of the various complex morbid conditions grouped as diarrhœa, to fall back, each, upon our own personal experience of them. As before observed, in treating of the diseases mentioned in the subject of this Essay, beginning with diarrhœa and ending with cholera, we pass from the partially known to the wholly unknown. Perhaps it may seem somewhat disparaging to the pathology of the present day to speak of diarrhœa as a disease the nature of which is only partially known. But I think those who follow me in the previous chapter on general pathology, and in the succeeding inquiry, will agree that our knowledge of the disease in all its phases is extremely limited.

Under the term diarrhœa, speaking of it as a disease *per se*, and not as a symptom merely of other diseases, I shall include and treat of the following forms of the disease :—The diarrhœa of irritation of the alimentary canal; the diarrhœa of congestion of the alimentary canal; summer or English cholera (so-called); choleraic diarrhœa (so-called); the entozooal forms of diarrhœa, including the mycosis intestinalis; and (if the paradoxical expression will be allowed) a constitutional form of the local disease, including malarial and scorbutic diarrhœa.

1. Diarrhœa of irritation.

The ingestion of impure drinking water plays an important part in the origination of the first form of diarrhœa. Irritation of the intestines producing diarrhœa is caused by (*a*) the mechanical impurities of unfiltered water; (*b*) by dissolved inorganic substances producing irritation by some chemico-physiological action; (*c*) by various organic impurities, animal and vegetable, suspended or dissolved; and possibly (*d*) by certain specific organic impurities (*disease germs*), which it will be more convenient to describe distinct from the organic impurities last mentioned. And first, as regards the mechanical irritation of the intestines by means of inorganic substances suspended in drinking water.

These may be classed as silica, clay (alumina silicate), chalk (calcic carbonate), chalky marl (calcic and alumina silicate) and ferruginous salts.

A small proportion of these substances in water—a proportion not sufficient to be detected by taste—produces generally more or less constipation, these constituents being for the most part astringent. But when these exist as suspended impurities, they produce primarily irritation, and, secondarily, congestion of the mucous membrane of the alimentary canal. It has been satisfactorily established that the hard water of the red sandstone formation, which was at one time in general use as a drinking water in some parts of northern England, produced constipation and lessened the secretions, while particles of the disintegrated sandstone suspended in drinking water produce diarrhœa by mechanical irritation. The ingestion of water taken from rivers at certain periods of the year produces diarrhœa of irritation from the amount of suspended impurities—principally clay and marl—washed into it by heavy rainfall. These substances are in so fine a state cf levigation as to refuse subsidence even after long standing of the water, and filtration even sometimes fails to arrest them. This is ɪ articularly the case in the large rivers of America and India. Dr. Hammond, of the United States army, specially refers to the Mississippi, Missouri, Rio Grande, and Kansas, as producing this effect at certain seasons, when the rainfall is or has been great.

The water of the Ganges is well known to act in the same way. In the case of the Hooghly water, with which Calcutta was some years ago supplied, the amount of suspended impurity prevented its filtration ; the result was, that the water was drunk in its natural unfiltered state, and epidemics, or rather endemies, of diarrhœa occurred.

An interesting paper on this subject was published in the Journal of the Asiatic Society of Bengal for 1873, in which it is shown that during the rainy season the water contains much less saline matter than in the dry weather. It is this saline matter which causes coherence of the particles of the finely levigated marl, and arrests it; but during heavy rainfall, owing to the absence or decrease of saline matter, coherence is not obtained, and the clay and marl, in a state of most minute division, penetrate and choke even the best sand and charcoal filters. Methods for the correction of this condition, by the addition to the water of alum and iron salts, are mentioned in the paper referred to. The matter is of importance, as explaining the discrepancy of reports made on filters, and as showing that a filter which acts well for one district or water, is not adapted to another.

The inorganic chemico-physiological agents producing diarrhœa by irritation may be divided into dissolved solids and gases. Certain liquid impurities which act in this way may, of course, find their way into water, the refuse of certain factories, &c.; but as they are easily detected, such water is not often used for drinking or culinary purposes, and we need not delay to consider them.

It will not be necessary either to take up space by citing evidence to show that drinking water impregnated with dissolved mineral matters and metallic salts, produce diarrhœa of irritation. It is the first, or at all events an early, symptom of poisoning by such substances, when these have not been rejected by the stomach. Calcic, magnesian and sodic carbonates and sulphates produce this effect. Nitrates frequently formed by the decomposition of organic matter combining with lime, magnesia and soda, give rise to irritation diarrhœa. Nitric acid formed in the same manner frequently combines with lead, mercury, arsenic, copper and zinc, giving rise to troublesome forms of irritable diarrhœa. As regards dissolved gases, hydrosulphuric acid is sometimes met with as a result of decomposition of sulphates, and produces, among other effects on the human economy, that of irritable diarrhœa. Other gases dissolved in water seem to produce this effect, but their occurrence is infrequent and comparatively unimportant. As to organic matters, Professor Parkes states that dissolved putrescent animal matter, to the amount of or over three grains in a gallon, will produce diarrhœa. There exists a very large amount of evidence to show that diarrhœa is very frequently caused by animal matter in water. There is not, however, any satisfactory evidence to prove that vegetable matter produces diarrhœa. There is, doubtless, a great probability that it does so, but the constant presence of other impurities renders it difficult to prove. As to organic gases, there exists an immense mass of evidence proving that the drinking of otherwise pure and healthy water from a cistern, the overflow pipe of which communicates with a sewer and is untrapped, causes serious and persistent diarrhœa, which is most frequently the irritation form, but may also produce other and more complicated forms.* Professor Parkes, in his "Practical Hygiene," quotes a typical case of this—the Salford Jail endemie—from Mr. Simon's Second Report to the Privy Council. There are many other instances of diarrhœa becoming endemic, from communication between water cisterns and sewers, given in the Reports of the Medical Officer to the Privy Council, and in Professor Parkes' Reports on Hygiene in the Reports of the Army Medical Department. We have no satisfactory evidence to prove that specific disease germs produce the diarrhœa of irritation.

2. Diarrhœa of congestion.

The diarrhœa of congestion is produced in the same manner as that of irritation; the latter cannot long persist without being supervened upon by the former. Of course, congestion of the mucous membrane of the intestine is produced by many other causes, as irregularity of circulation, especially as results from surface chills, deranged nervous influence, and various conditions of the viscera; but with these sources of congestion diarrhœa we have nothing to do in the present inquiry.

The forms of diarrhœa known as choleraic diarrhœa and summer or English cholera, although somewhat differing in their characters, are sufficiently cognate to admit of our economizing space by treating of them together, as regards etiology, under the term of Pythogenic Diarrhœa.

3. English cholera and choleraic diarrhœa.

This affection is not brought about by mechanical irritation, nor have we evidence of its production by the chemico-physiological agents. It seems to be due (so far as water can be said to produce it) altogether to organic impurities, and especially to sewage contamination. I do not include under the terms English cholera or choleraic diarrhœa (pythogenic diarrhœa) the so-called premonitory diarrhœa of malignant cholera. The latter may or may not be of the same nature, or identical, but in a systematic study of their etiology it will be preferable to consider them distinctly. European pythogenic diarrhœa is distinguished from the irritation and congestion forms of diarrhœa by its greater severity, by griping, by thirst and deficient appetite, by pneumotosis, by the presence in the stools of bile or unaltered ingesta, by slight fever and slight vomiting, and

* Carpenter; Parkes; Aitken; Virchow, *Archiv.*, Bd. xxvi. p. 117; Caustatt, "Jahresh." 1862, vol. ii. p. 31.

from the succeeding forms of diarrhœa by absence of blood and albumen (or only a trace of the latter) in the stools. As noticed in the chapter on the pathology of these diseases, we here first meet in our series of alvine fluxes with evidence of a constitutional nature of the local affection. Its occurrence in epidemics at certain seasons of the year is the first evidence of its being a general and not a local disease; and it is, in the first place, from its natural history as an epidemic disease we must obtain evidence as to its origin.

Epidemics of pythogenic diarrhœa occur at the end of summer and beginning of autumn, and in country districts which are swampy, and in ill-drained towns which are also overcrowded. Indeed, it may be stated generally that epidemics of pythogenic diarrhœa occur, or that the disease becomes endemic, under conditions favourable to putrescence; and hence its name. It appears to belong to a group of pathological phenomena which may be grouped as mal-assimilation, generally, but not always, dependent on deranged nervous influence, in which the healthy physiological functions of nutrition give way before ordinary physical processes of decay, through failure of vital energy—a condition named *molecular feebleness* in modern medicine.

No doubt molecular feebleness may cause (by a lowering of physiological activity) a continuance within the body of ordinary physical processes which in health molecular energy arrests. But there is some evidence to show that if food be ingested in certain physical conditions—for example, in certain advanced stages of decomposition—ordinary physiological activity is unable to cope with and arrest the continuance and advance of such physical process.

Under these circumstances food is not subjected to the ordinary function of digestion. For this reason I observed above that mal-assimilation is not always dependent on deranged nervous influence, this morbid condition being sometimes brought about by the physical processes already going on in the food at the time of its ingestion, resisting or overcoming the physiological processes known as digestion and assimilation, although the latter may be in the usual health condition.

This being the case, it is manifest what a large part food plays as an agent of origin of this disease.

And drinking water being the most frequent medium of the introduction of putrescence to the stomach, it is clear that impure drinking water must be considered a prolific source of pythogenic diarrhœa. I do not say that putrescence of ingesta is the sole cause of this condition. I have intimated that it may occur by a lowering of molecular energy which renders the physiological processes of digestion inert, and permits ordinary physical processes to occur in the stomach or intestine, although the food may have been ingested in a fresh and wholesome condition. Probably when the disease occurs in a well-marked form, both factors of its origin have been at work. Food has been ingested in an unwholesome condition into a system in which molecular energy has been lowered. With that form of the disease which is caused by weakness or vitiation of the natural physiological processes (dyspepsia), we have nothing to do in the present inquiry. We consider only that resulting from the ingestion of impure drinking water, which actually contains putrid organic matter, or substances which favour the occurrence of putrescence.

The impurities of drinking water which can be included under this head are as follows:—Putrid flesh, putrid animal excreta, putrid vegetable matter, and certain substances resulting from these, which seem to produce a form of diarrhœa having both the irritation and pythogenic characters of which I have treated in a former part of this Essay.

The effects of these agencies are noticed in their greatest activity about the course of Indian rivers when the water becomes low, and the banks or beds of these exposed to the influence of the sun.

Then a population which has used the water of such a river for culinary purposes with impunity while the water was high, and the organic débris contained in it unexposed, begins to suffer from diarrhœa and other diseases of the pythogenic type.

The like condition produces similar results in the neighbourhood of swampy jungle and terai, and more or less in all low-lying deficiently drained localities. These effects are especially manifest in the neighbourhood of extensive unopened swampy jungle, as in the pythogenic diarrhœa and fever of Cachar. The surface of this locality consists of what may be described as hillocks rising out of stagnant bheels. These latter are never wholly dry, and are full of pestilential jungle and of remains of carcases and of animal excreta. The water supplied for all purposes is taken from or runs in some part of its course through these bheels. The result is, that about six months is the average term of European life in this locality. I refer to this district because here and in the neighbouring stations of Sylhet, Ganhati, and Nowgong, garrisoned by native troops, and occasionally even at the elevation of Shillong, this pythogenic form of disease, pure and simple, is met with in its rank indigenous virulence.

The method in which ingested putrescence acts on the economy has been referred to in a former chapter.

From the consideration of the diarrhœa produced thus by the remains of organic life, the transition is natural to that produced by living organisms, animal and vegetable.

4. Entozooal Diarrhœa.

This form of the disease does not seem to be produced by the irritation or congestion of the mucous membrane of the intestinal canal, nor yet by partial poisoning, as in the case of the substances included in the chemico-physiological group; but apparently in some independent way by the existence of living organisms within the tube at any part of its extent from mouth to anus.

It is true that the irritation form of diarrhœa may be produced by the presence of such organisms, but the effect is not due altogether to this; the symptoms of entozooal diarrhœa being distinct and more pronounced, and, so to speak, superadded on those of the irritation and congestion forms of diarrhœa. The rationale of their operation has been noticed in a former chapter.

How these organisms find their way into the system, and whether or not they are ever spontaneously developed, are questions which, perhaps it is not too much to say, will never be completely answered in every case. At all events, we know that certain of these organisms which produce diarrhœa are (so far as we know) always ingested in water, and by this means only; and that others are sometimes taken into the stomach in this way. We have, then, to consider in this place the forms of diarrhœa produced by (1) entozoa, and (2) entophyta, which we have some evidence are or may be produced by the use of impure drinking water. In doing so, I give a tabular view of what is known regarding the action of these bodies, following the classification of them given in the Appendix to the Nomenclature of Diseases of the Royal College of Physicians.

It would occupy too much space to cite the authorities by which I arrive at the conclusions given in this Table. Perhaps it will suffice to say that I have carefully analysed all the evidence on the subject within my reach, from the writings of Allen Thompson, Heslop, Aitken, Parkes, Cobbold, Leuckart, Bastian, Bancroft, Lewis, Welch, and others; writers in the Army Medical Department Reports, and in periodical medical literature. It will be remembered the words *Yes, No, Probable*, &c., used in the following table, do not assert affirmation, negation, or probability of their facts, but only refer to the evidence at present at our disposal regarding these facts. To avoid a repetition of the Table in the chapter on Propagation of Diarrhœa, I here give a third column, which we have nothing to do with at present, but will refer to in the chapter on Propagation.

CŒLELMINTHA.	1 Evidence of being a cause of Diarrhœa.	2 Evidence of being originated by impure water.	3 Evidence of being propagated by impure water.
1 Ascaris lumbricoides . . .	Yes	Yes	Yes
2 A. mystax	Yes	Yes	Yes
3 Tricophyton dispar	Yes	No	Yes
4 Trichina spiralis	Yes	No	Probable
5 Filaria medinensis	No	...	...
6 F. oculi	No	...	...
7 Strongylus bronchialis . . .	No	...	...
8 Eustrongylus gigus	No	...	...
9 Sclerostoma duodenale . . .	Yes	No	No
10 Oxyuris vermicularis . . .	Yes	Yes	Yes
STERELMINTHA.			
11 Bothriocephalus latus . . .	No	...	...
12 B. cordatus	No	...	...
13 Tinea solium	No	...	...
14 Cysticercus of same	No	...	...
15 T. mediocanellata	No	...	...
16 T. acanthotrias	No	...	...
17 T. flavopunctata	No	...	...
18 T. nana	No	...	...
19 T. lophosoma	No	...	...
20 T. eliptica	No	...	...
21 Cysticercus of T. marginata .	No	...	...
22 Echinococcus hominis . . .	Probable	No	Yes
23 Fasciola hepatica	No	...	...
24 Distoma crassum	No	...	...
25 D. lanceolatum	No	...	...
26 D. ophthalmobium	No	...	...
27 D. heterophyes	No	...	...

STERELMINTHA.	1 Evidence of being a cause of Diarrhœa.	2 Evidence of being originated by impure water.	3 Evidence of being propagated by impure water.
28 Bilharzia hæmatobia . . .	Yes	Yes	Yes
29 Tetrastoma renale	No	...	...
30 Hexathyridium venarum . .	No	...	...
31 H. pinguicola	No	...	...
ACCIDENTAL PARASITES.			
32 Pentastoma denticulatum . .	No	...	...
33 P. constrictum	No	...	...
34 Æstrus hominus	Probable	Probable	Probable
35 Athomyia canicularis . . .	No	...	...
ENTOPHYTA.			
44 Leptothrix buccalis	Probable	Not clear; certain waters seem to favour its development	No
45 Oidium albicans	Yes	Doubtful	No
46 Sarcina ventriculi	No	No	No
47 Torula cerevisia	No	No	No

From an analysis of the above Table it will be seen that there is evidence that diarrhœa is produced AND originated through drinking water by five of these organisms—four of the animal forms and one of the vegetable: three of the Cœlelmintha—Ascaris lumbricoides (1), A. mystax (2), Oxyuris vermicularis (10); and one of the Sterelmintha—namely, Bilharzia hæmatobia (28); and one of the Entophyta—namely, Oidium albicans (45). It is only at the commencement of the disease that diarrhœa alone exists, or that the nature of the affection can be mistaken.

But at this time, before the pathognomonic symptoms are pronounced, the disease is, to most observers, *diarrhœa*; and it is well to remember these forms of it are originated by impure drinking water.

The Tricophyton dispar (3) gives rise to a form of diarrhœa to which the name *morbus mucosus* has been given.

About the year 1760 this disease occurred in an epidemic form in the French troops stationed at Göttingen. The entozoon was found in the bodies of soldiers who died of this mucous flux. I am unable to find any evidence, however, of the disease having been originated by drinking water. There seems to be some evidence of its propagation by this means, to which I will refer in the succeeding chapter.

In the same way Sclerostoma duodenale (9) produces diarrhœa, but we have no evidence of its origin by drinking water; we have, however, of its propagation, and shall allude to it again under this head. Sometimes temporarily uncomplicated diarhœa is caused by Bilharzia hæmatobia (28), but this affection is not sufficiently local to be more than mentioned in the present inquiry. There is evidence of this entozoon being both originated and propagated by drinking water. We will have to refer to it again when treating of dysentery. With regard to the Entophyton noted above as giving rise to diarrhœa—Oidium albicans (45)—the evidence of its origination by drinking water is extremely doubtful. The evidence in favour of it rests chiefly on the endemic character sometimes assumed by this form of the disease. Leptothrix buccalis (44) I have noticed as being a probable cause of diarrhœa of this form. The assertion depends altogether on the evidence of Klob of Leipsig.* Dr. Parkes suggests that the bodies which the former found in diarrhœa cases were Penicillium in the Leptothrix stage of development. Indeed, in the present day such terms as Leptothrix, Oidium, &c., are used only to express certain stages of fungoid development; but I adhere to the old nomenclature authorized by the College. Besides the Entophytes given in the above table, there exist other forms of intestinal vegetation, classed as Algæ, Fungi, Mycoderma, Leptomiti, &c., which are imperfectly known. It may be stated of them generally, that being, as they commonly are, introduced through impure drinking water, they form a *nidus* for the development of more highly organized vegetations, and sometimes for the lowest forms of animal life. This is also true of minute particles of woody fibre and of leaves, as well as of the various confervoid growths.

We must here specially notice a form of disease which Dr. Parkes aptly termed a *fungus embolism;* I mean the so-called "Mycosis intestinalis" of Buhl. Of this pathological condition diarrhœa is a primary and prominent symptom, but it is soon complicated with others. In advanced cases the disease assumes unmistakable

* "Path. Anat. studien über das Wesan des Cholera processes."

constitutional characters, but it seems well proved that the affection is intrinsically a local intestinal disease. Having described the pathological appearances after death, Buhl observes:—"It appears now no more doubtful to what we should ascribe the œdema of the intestinal wall of the mesentery, and of the retroperitoneal tissue, as well as, lastly, the exudation in the abdominal cavity. The abundant penetration of the plant fibres into the portal vein, and in the lymphatic vessels of the portal system, clear up perfectly the relations already noted. The cholera-like symptoms were the consequence of the mechanical working of the plant, the blockage of the portal blood and lymph stream, and the consequent transudation."* The intestinal villi are found to be permeated with fungoid structures (zoöglea heaps), referable to the group *Schizo mycetes*, the submucous tissue traversed by *Mycelium fibres*, the veins penetrated by the *mycelia*, while the blood contained *conidia;* these appearances indicating a sequence of phenomena which forces the belief that, etiologically at least, the disease is local.

The observations of Waldeyer go far to prove that the disease is in man a local intestinal disease, having some relationship to the "malignant pustule" of animals, which seems in some way to be transmitted to man. If so, its origin in man may possibly be by ngestion of impure water, into which the secretions of animals so affected have found their way. But by this means its origin must be rare. Its propagation by water carrying excreta of those human beings suffering from the disease seems very probable. I may mention here that M. Bavay has recently (July, 1877) described in the "Archives de Médécine Navale," under the name of *Anguillata intestinalis*, a parasite found in six cases of the diarrhœa of Cochin China. Five of these six cases ended fatally. The worms were found abundantly in the duodenum, few in the jejunum, and never in the ileum. His observations are not sufficiently extensive to warrant using them in evidence as to the etiology or possible propagation of the disease. He seems, however, to incline to the belief that their origin is due to the ingestion of impure water carrying them.

I must here notice some interesting and probably important facts which seem to bear on the subject of the parasitic origin of

* "Zeitsch. für Biologie," Bd. vi. p. 136.

certain forms of diarrhœa. During my service as medical officer in charge of the Military Prison at Fort William, Calcutta, some little time ago, endemics of two distinct (?) diseases set in. With the rainy season, towards the end of June, commenced a prevalence of diarrhœa and a prevalence of ringworm. On inquiry and reference to the medical officer's diary, I found these diseases were constant in their recurrence at this period of the year.

I have called them endemics of distinct diseases, and so they appeared to be at first, and to a casual observation; but as they progressed the fact forced itself on my notice that the prisoners who suffered from the skin disease did not suffer from the bowel affection, and those who were affected with diarrhœa, if they contracted tinea at all, had it transiently or in a modified form. Those in whom the diarrhœa occurred were weakly and debilitated men, with long Indian service; while the patients with tinea were generally young lusty men, a comparatively short time in India. The diarrhœa cases were not of sufficient severity to require hospital admission, but were treated at the fort dispensary, while some of the cases of tinea were sent to the garrison hospital. For this reason I am unable to refer to published statistics for the facts. The average number of diarrhœa cases comes out as 6·8 per cent.* of strength, and of tinea cases as 8 per cent. of strength.

Now, this proportion of diarrhœa to ringworm comes very near the proportion of debilitated to robust prisoners during the time. There was nothing peculiar in the symptoms of the diarrhœa. The men described it, and correctly, simply as looseness of the bowels, without pain. There was apparently no contamination of the water supply, the sewerage and drainage were perfect, and in fact the prison was in a state of model sanitation. Now, here were two diseases having apparently the same cause of origin (else how can their constant endemic occurrence at certain seasons be explained?) invading at the same time a body of men placed under exactly similar conditions, the only difference of circumstances being those of individual idiosyncracy and personal health. The young, healthy, and robust suffered from one of these diseases; the old, weak and debilitated from the other. Are we justified in concluding that one cause of origin produced a patho-

* Daily sick.

logical condition which in one set of cases was symptomatically evidenced by an affection of the outer surface of the body, and in the other set by an affection of the inner.

We can only say that some probability of this exists, the number of observations not being sufficiently numerous to justify any definite conclusion. The average strength of prisoners during the period referred to was 40.

The fact of these diseases becoming endemic, constantly at the season of the year when a deterioration in the water supply may reasonably be predicated, and the fact of water being constantly applied to the mucous membrane of the alimentary canal and to the skin (the parts affected), amount, so far as they go, to a presumption of the probability that both are originated by one cause—namely, the influence of impure water. Against this comes the fact of failure to detect any impurity in the water, the only change in it during this season being that before referred to, a diminution in the amount of its usual saline constituents.

It may be that excessive humidity favours a development of parasitic growth on the skin, on the inner surface of the alimentary canal, or in the blood; but, on the whole, the impure water cause is the more probable, notwithstanding the evidence against it. I may observe that I have communicated with a medical officer since in charge of the prison, and find that the same diseases have, under his observation, occurred synchronously, as I have stated; and I gather from his remarks, that although he had not before observed the immunity from one in cases affected by the other, he is of opinion, on reflection and reference to the diary and prescription book, that there exist some grounds for believing such to be the fact. The prescription book shows that men who were treated with external applications for the skin affection did not apply for astringent medicine; while at the same time a large number of other prisoners were being treated for diarrhœa. That the latter disease is vastly more prevalent in the autumn than in other seasons is apparent from the statistical records.

Quarters of the Year.	1874. — Cases of Diarrhœa.	1874. — Cases of Diarrhœa.	1874. — Cases of Diarrhœa.	1874. — Cases of Diarrhœa.	1875. — Cases of Diarrhœa.	1875. — Cases of Diarrhœa.	1875. — Cases of Diarrhœa.	1875. — Cases of Diarrhœa.
	Number admitted.	Number died.	Ratio per 1,000 admitted.	Ratio per 1,000 died.	Number admitted.	Number died.	Ratio per 1,000 admitted.	Ratio per 1,000 died.
First ...	231	1	6·4	·03	267	...	7·2	...
Second...	509	1	13·5	·03	554	1	14·9	·03
Third ...	831	2	22·2	·05	970	...	26·1	...
Fourth...	335	...	8·9	...	526	2	14·1	·05

This Table deals only with the admissions and deaths for diarrhœa for 1874–5, for Bengal only, but may be taken as a fair average. It would be interesting to know if a year characterized by a large rate of admission for diarrhœa is also remarkable for a correspondingly large admission for Tinea tonsurans, or a correspondingly small one, or whether any relation exists between them. We are unable to give accurately the statistics of Tinea tonsurans, these not being stated separately in the returns as distinct from other cutaneous diseases. No relationship is perceived when the admissions for diseases of the digestive and cutaneous systems are compared; but when the admissions for diarrhœa are eliminated from the digestive system admissions, we are able to detect one important fact. Possibly if Tinea tonsurans were in like manner eliminated from cutaneous system admissions, and given separately, the result would be more striking.

	Bengal.—Admissions per 1,000 of Mean Strength.					
	1870.	1871.	1872.	1873.	1874.	1875.
Digestive system	249·3	251·9	262·5	222·5	225·5	222·6
Cutaneous system	63·7	63·8	63·0	65·0	70·0	65·4
Diarrhœa	...	...	77·62	49·6	51·0	62·3

However, the fact remains, whatever it may mean (or if it mean anything), that in the four years 1872–5, the year characterized by the largest admissions for diarrhœa (1872) has the smallest number of admissions for the cutaneous system; and the year of largest admissions for diseases of the cutaneous system has, not the smallest, but a comparatively small admission for diarrhœa.

The diarrhœa admissions are not separately given in the Reports prior to 1871. This is a fact which ought not to be lost sight of. I am not inclined to attach much importance to it at present, the number of observations being so few. It must be remembered that Tinea tonsurans forms a large proportion of the cutaneous system admissions, particularly in Southern India.

The following remarkable case, bearing on this subject, has been recently under my observation:—Lieut. S., aged 28, healthy, strong, an athlete; seven years' service, six in India. For three consecutive years has suffered from an eruption during the rains. In the spring of this year he marched with his corps (to which the author was at the time attached for duty) from a plain station to one of a higher elevation. About a month after arrival he experienced a good deal of intestinal irritation, pain at epigastrium, nausea, loss of appetite, and diarrhœa. I attributed these symptoms to hepatic derangement, resulting from sudden change of temperature, he having resided during his Indian service in the plains until now. There were, however, no tenderness over the hepatic region on firm pressure, nor pain in the right side or shoulder; no jaundice, nor any trace of bile acids in urine. The fæces were not pale, and but little changed in consistence; in fact, the diarrhœa was simply *looseness*. The case resembled gastric catarrh with diarrhœa. Only once was there vomiting, which occurred after taking food, and consisted of the unaltered ingesta. The usual remedies were administered—bismuth, hydrocyanic acid, solution of lime, vegetable tonics, with various astringents, and blisters to the epigastrium. This was in June, the commencement of the rains. The patient was becoming decidedly worse, losing flesh rapidly, and there was extreme exhaustion. The senior medical officer at the station now saw the case in consultation, but no satisfactory diagnosis was arrived at. In July the rains had well set in, and my patient remarked to me that his old friend the pluvial rash was again visiting him, and showed me some small papules on his wrists and ankles. The eruption rapidly matured, and the diarrhœa as rapidly declined.

I allowed the cutaneous affection to take its course, without treatment, and in a few weeks he was at his duty, as healthy and active as ever. It is hardly reasonable, I think, to consider the cutaneous and intestinal affections in this case as merely coincident, without any mutual connection. There was no abnormality of temperature during the illness. Shortly after the first appearance of the eruption, I made a microscopical examination of the blood. The white corpuscles were greatly more numerous than normal, and all the corpuscular elements presented a somewhat shrivelled appearance, and were smaller than those of healthy bloods examined with the same powers.

On inspection of the vicinity of the mess-house where this officer lived, an old heap of stable refuse was found. The drinking water was derived from a neighbouring spring, which is, however, liable to contamination by surface drainage, but was always filtered. Two other officers lived in the same house; they were unaffected.

This case appears to me to be one of those, by no means rare, in which some condition of the drinking water (most probably dissolved organic impurity, due apparently to surface washings from its tendency to occur in heavy rainfall), or excessive atmospheric humidity, develops or vivifies latent minute organisms, or actually produces them on the internal mucous membrane, on the external surface of the body, or possibly in the blood. The subject of entozooal and entophytal diarrhœa is one in which much work yet remains to be done.

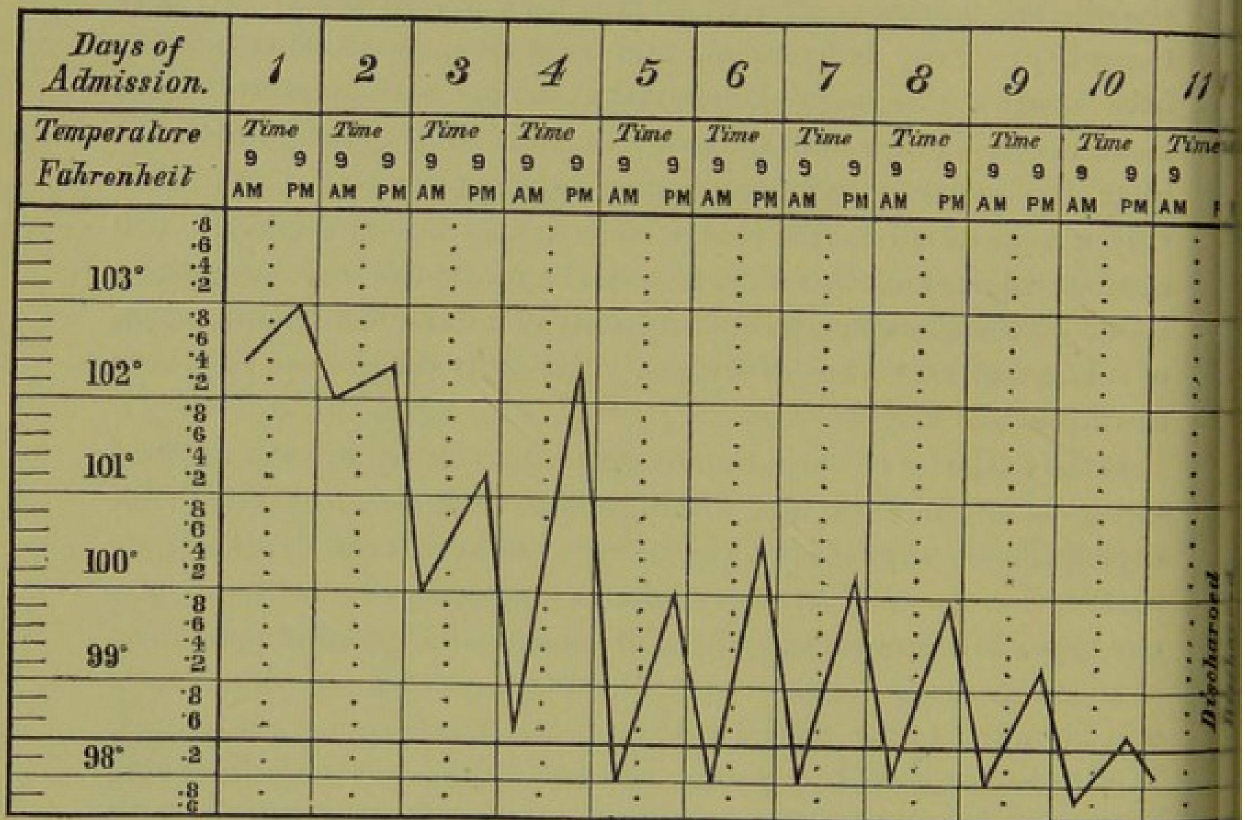
5. Malarial diarrhœa. I believe most medical men who have seen much of the diseases of tropical climates will readily understand what I mean by *malarial diarrhœa*.

I stated in the beginning of this chapter my intention of treating of the forms of diarrhœa as a disease *per se*, and not a symptom of other diseases.

The present form is not an exception. The diarrhœa we are now about to consider is not a symptom of malaria; the latter frequently, indeed generally, existing without it; but it is a result of malarial poison, frequently unaccompanied by the usual pathognomonic symptoms of malarial disease. To the inexperienced in tropical diseases it is simply *diarrhœa*. Before going intò the ultimate object of our inquiry—namely, the origin or propagation of it by impure water, I briefly define this form of diarrhœa, and indicate its pathognomonic symptoms, by relating a few cases which have come under my observation. Malarial diarrhœa may be defined as an intestinal catarrh, resulting from functional derangement of the nervous centres (especially the sympathetic system) by the specific action of malarial poison, distinguished from other forms of diarrhœa by occasionally manifesting phenomena of the malarial type, but sometimes occurring when the usual malarial phenomena are latent or have passed away. The symptoms are (when the affection manifests itself alone) a sense of oppression at the epigastrium, and an alvine flux of matter, more liquid and of lighter colour than normal. Occasionally this diarrhœa accompanies intermittent fever, but is then itself continuous, showing that it is not a symptom of the malarious disease, but a result of it. In nearly all cases of malarious disease some one organ is selected as a safety-valve for the disease poison—different organs in different cases. At first there is generally congestion, and subsequently exudation from such organs. In cases of malarial diarrhœa the intestine is the channel of discharge, and the morbid condition frequently continues after the malarial symptoms have passed away; or these latter may not be at all manifested if the flux be free and early. The anatomical sign of this affection is the œdema of the submucous tissue of the intestine, frequently seen in after-death examination of soldiers of long tropical service. Rarely is the disease so severe as to produce ulceration.

It fell to the lot of the author of the present Essay to have medical charge of many of the men who were sent back sick from the Perak expedition of 1875–6. They suffered either from dysentery or chronic diarrhœa. Most cases of the latter disease were of the malarious type. It is unnecessary here to describe the hygienic conditions of that expedition, as they have been related to me; no doubt they will be found in the official reports before this Essay is completed. It will suffice here to say that all the drinking water obtainable contained large quantities of dissolved organic impurity. And although the water was always boiled, and then twice filtered, officers and men ascribed the sickness of that campaign to impure drinking water. The malarial nature of the diarrhœa in some of these cases was so marked as to mislead. The clinical thermometer was the sole means of diagnosis. I here give what may be taken as a typical case of this form of disease.

Gunner J. A., aged thirty years; service seven years. (*Medical history.*—Ague, ten days hospital. Dengue, fifteen days. Dengue, twenty-two days. Dyspepsia seven days.) Was admitted with chronic diarrhœa of two months' duration. *States that he has no fever.* Was much exposed to damp while at Perak. Has constant bowel flux of slimy light-coloured evacuation; no symptoms of hepatic or splenic disease. Pulse varying from fifty to sixty beats a minute. Emaciated, and states he is losing flesh rapidly. Temperature shown by clinical thermometer to be subject to periodic exacerbations, of which the patient is unconscious. There is no shiver or sweating. Ten grains quinine are given morning and evening. Diarrhœa checked. Discharged convalescent. (See temperature chart.) This case is peculiar from the age of the patient, and coincidence of low pulse with the high temperature.



Another remarkable case I obtained permission to treat in the man's quarters, because the ground about the hospital was badly drained, and (this being in the rainy season) a good deal of water was stagnant about the building. When in hospital the diarrhœa was persistent, but when he went back to his quarters it became much better. Although there were here no ordinary symptoms of malarial disease, this extreme sensitiveness to miasm, and the indications of the clinical thermometer, left no doubt respecting the true nature of the affection.

Another somewhat similar case is at the present time under my observation. He comes to hospital occasionally complaining of diarrhœa. I do not give him astringents, knowing, from experience of his case, how useless such treatment would be. He gets ten grains of quinine sulphate morning and evening for two days, by which time the diarrhœa is checked. In this case, also, the patient is unconscious of any malarial phenomena.

I have met with three well-marked cases of malarial gastritis in which malarial phenomena were latent or masked, curable by quinine. I note this experience here with reference to the researches of Ritter on the origin of malaria, shortly to be considered. The question of the origin of malarial diarrhœa by impure drinking water, is of course contained in the broader question of the influence of impure *drinking* water in originating malaria. There is not very much evidence available regarding this question. After reviewing the subject carefully, the late Professor Niemeyer observes:—"I have no hesitation in saying decidedly that marsh miasm—malaria—must consist of low vegetable organisms, whose development is chiefly due to the putrefaction of vegetable substances. It is true these low organisms have never been observed. No one has ever seen malarious spores." Since his time, however, numerous experiments have been made which seem to indicate that malaria, or rather a substance capable of carrying it, exists in a visible tangible form. Surgeon-Major A. Deane Massy, A.M.D., concludes a very able report on "The Prevalence of Fungi at Jaffna," published in the seventh volume of A.M.D. Reports, with these observations—"The spermatiferous or most active phase of the aërial mucor at Jaffna is associated with partial moisture and a high temperature: such spores are wafted along with the land or shore breezes, which have been proved by experience to convey palludal or littoral poison; they may be intercepted by a belt of trees, and affect with black rust the leaves on which they impinge, and to which they adhere. Increased prevalence and intensity of intermittent fever are observed to be coincident in point of time with a definite phase of decomposition in fungi (associated with their degeneration, partial decay, and retrograde transition from a higher to a lower grade of existence),*

* See note in the Appendix.

under the special conditions of partial moisture, a high temperature, and recent disturbance of the soil, which are known by experience to be in a constant and direct relation to the development of palludal poison. This fungus product is invariably found in most intimate association with a dwindled condition of the sporules and resulting mycelia. The fungus product is constantly detected in the altered secretions of malarial patients, and has been removed in bulk from the bladder after it had occasioned the symptoms of stone; while the dwindled sporules have been demonstrated in the red corpuscles of the blood. It may be added that, since fungi are essentially parasitic, they are most abundant nearest the surface of the ground; and though they continue to grow by day, yet their force of growth and development is known to be most active at night.

"Hence it appears highly probable that the poison of intermittent fever is a special poison, conveyed and developed by fungi, of the nature of an organic virus, or product of organic decomposition in a transition state, which it would be desirable to complete the decomposition of as speedily as possible, in circumstances where the formation of the noxious product cannot be avoided, as seems to be naturally accomplished, in some degree, through the nascent oxygen evolved from the green leaves of plants, which latter may in turn succumb, and permit excessive development of noxious fungi that prey upon them, under the conditions of impaired vitality from excessive drought or damp, and the accidents attendant upon the various phases of their growth and development." This paper is a very valuable contribution to the etiology of malarial disease.*

In 1870, an Italian physician, Dr. Balestra,† examined the water of places notoriously malarious (Pontius, Maccarebe, Ostia, &c.), and found the water from their neighbouring marshes filled with forms of infusorial life which he described as busariens, trichodiens and vorticelliens, and in addition to these a plant, which was constant, and the abundance of which was always in direct proportion to the putrefaction of the infusoria. It was a constant and special form of "microphyte granule" belonging to the algæ, which floats on water, and when young resembles

* See also Report, "Prevalence of Malaria in connection with Water Supply," A. M. D. Report for 1875.

† *Comptes Rendus*, 1870, tome lxxi. No. 3.

minute oil globules. Its growth and development are much more active during night than day.

It rises after rain, and permeates mist and dew. Solutions of arsenious acid, sodic sulphate, and neutral quinine sulphate check its growth, as does also cold.

In the Army Medical Department Blue Book for 1869 Dr. Parkes mentions Ritter's opinion, that the malarions disease of the Elbe marshes is due to some impurity in the water. He states that, besides malarial disease, the water also produced catarrh of the stomach. I am of opinion that the latter is identical with the malarial gastritis, with masked malarial phenomena, which I have above stated that I have observed.

By reference to their works, it will be seen that the above observers believe, and state in almost so many words, that the malarial poison is swallowed, and sometimes swallowed in drinking water. Without denying the possibility of this being true, my own observations lead me to believe that the origination of malarial disease by means of drinking water is of comparatively rare occurrence.

6. Scorbutic Diarrhœa.

A form of diarrhœa associated with and apparently dependent on the scorbutic habit is occasionally met with among old soldiers. It may be mistaken sometimes for dysentery, but is, I apprehend, quite distinct from that disease. There is no doubt that the scorbutic habit gives a distinct type to intestinal diseases. This is manifest in the case of dysentery—the scorbutic dysentery being distinct from the ordinary form of that disease, both in its life symptoms and after-death appearances, the disease being impressed with the scorbutic *stamp*, to use Professor Aitken's expression. The same I believe to be true of diarrhœa. There may be distinguished two forms of the scorbutic diarrhœa—namely, (*a*) ordinary irritation diarrhœa, occurring in the scorbutic habit, and bearing the stamp of scorbutus; and (*b*) true scorbutic diarrhœa. The first may be brought about by any common impurity of drinking water which, under ordinary circumstances in a healthy individual, would, in common with any other irritant, produce but trivial and transient irritation diarrhœa. But when occurring in the scorbutic habit the disease assumes an altered and aggravated form. I have said it might be brought about by any irritant—cold and a bout of drinking are the most frequently seen. There is in the corps to which the author is attached for duty, an old and broken-down soldier of decidedly scorbutic habit, who occasionally indulges in a "big drink;" and who invariably comes to hospital in a day or two after the event with blood-streaked fæces. A short time ago, when on the march, the battery halted in a camping-ground where water is scarce. In fact, there is but one well near the ground, and a river some mile and half distant. This one well is close to a native Indian village, and unprotected from surface drainage, and within half a dozen yards of it, on this occasion, was a large accumulation of ordure. Precautions were taken that the water supply should be brought from the river and not from the well, but through the laziness of the *bheesties* and carelessness of the men, some water from the latter source was brought into camp. The old soldier above mentioned used some of this water, and the following day reported blood-streaked fæces. This is the symptom of the disease. The fæces are passed stained with blood, and but little, if at all, altered in consistence. Generally a small quantity of liquid fæces also pass, blood-stained, but there is no flux of unmixed blood. An examination of the gums revealing some sponginess, and a red line along the base of the teeth, sometimes disposes of any difficulty as to the nature of a case of diarrhœa of this form.

We now come to the second variety, True Scorbutic Diarrhœa. I do not mean the spontaneous blood fluxes which occur from mucous canals generally, in cases of well-defined scurvy, existing as an independent affection; but cases of diarrhœa in which the fæces are blood-stained, through the existence of scorbutic taint, where scorbutic phenomena are latent, or masked, or not easily recognizable. As regards the origination of the irritation scorbutic diarrhœa, it seems plain that any impurity of drinking water which would produce ordinary irritation diarrhœa, is still more likely to produce the scorbutic form of irritation diarrhœa where the scorbutic taint exists. The question of the origination of the true scorbutic diarrhœa by impure drinking water depends, of course, on the question of the causation of scorbutus by impurity in drinking water. There is some slight evidence in favour of a probability that in some constitutions scurvy is caused by agencies which in other constitutions produce malarial disease. If this be so, it of course follows that the impurities of drinking water which are said by some to produce malarial disease, may in certain constitutions produce scorbutus.

I am unable to find any evidence as to the direct origination of scurvy by impure drinking water, except in a notice of this subject by Professor Parkes in his Report on Hygiene, published in the Army Medical Department Blue Book for 1865, which I here give *verbatim* :—

"Scurvy is a disease which has hitherto not been ascribed to impure water by any writer; but Dr. Beckler,* who was the medical officer in Burke's exploring expedition in Australia, has brought forward the view that in that expedition the very early appearance of scurvy was not owing, as usual, to deprivation of fresh vegetables, or to want in variety of food, but to bad water, and to the restricted use of what water they had. Dr. Beckler enumerates the provisions, which appear to have been ample, and shows that the fault was not in them, though it is stated the preserved vegetables were bad. The water was found in pools; it had been standing for months, and was full of animal and vegetable life, and contained also suspended clay. When boiled it became covered with a brown slimy mass; tea boiled in it threw down a very bulky precipitate.

"Dr. Beckler states—'According to all that we could observe, it was this character of the water, or rather the want of fresh water, which was the cause of the scurvy. In favour of this view there is the early appearance of the disease—at a time, in fact, when its origin could be attributed in no way to the quality of the provisions, and when, in particular, the want of fresh vegetables, justly considered as the common and principal cause of scurvy, could not be used in explanation of its appearance.' As an additional argument, Dr. Beckler mentions that other exploring parties, with no better provisions but with good water, were not affected.

"The circumstance seems worthy of note, though I cannot but think too much importance has been ascribed to the water. Dr. Beckler mentions that the lime juice was left behind, but that they carried and used crystallized citric acid without the least benefit."

There seems to be no information to be gained on this point of the water origin of scurvy from the reports of recent Arctic expeditions.

* "Verhandlungen der Berliner Medicinischen Gesellschaft," 1866, p. 211.

We have now considered briefly, and only in slight outline (to do so in detail would only prolong this Essay to an unreasonable length), some of the forms of diarrhœa of the water origin of which there exists evidence. The last form treated of—scorbutic diarrhœa—constitutes a link between diarrhœa and dysentery, the disease next to be considered with regard to its origination by drinking water. Before discussing this question, however, we must devote a chapter to inquiring into the influence of drinking water in propagating diarrhœa.

SUMMARY.

Theoretical. It is unnecessary to take evidence from hypothetical considerations on this question, inasmuch as experience gives definite and sufficient grounds on which to base conclusions.

Experimental. Satisfactory evidence exists that irritation diarrhœa may be originated by drinking water. Satisfactory evidence exists that congestion diarrhœa may be originated by drinking water. Some evidence exists that pythogenic diarrhœa may be originated by drinking water. Some evidence exists that some forms of entozooal diarrhœa may perhaps be originated by drinking water. Slight evidence exists that malarial diarrhœa may possibly be originated by drinking water. Very slight evidence exists that scorbutic diarrhœa may possibly be originated by drinking water. It is to be noticed that, while we have evidence regarding this question with respect to the simpler forms, the quantity and quality of the evidence declines in proportion to the complexity of the forms of the disease. It may be that the fact of such complexity withdraws the mind from the consideration of the origin being due to so simple (common) a cause. From a consideration of all the evidence bearing on this influence of drinking water I conclude that—*Drinking water originates diarrhœa by disturbing the innervation or circulation of the intestinal canal, producing muscular contraction, or increased afflux of blood. This irritation and congestion, either simple or passing on to more complex results, is often merely a mechanical or non-vital process, but must also be frequently referred to chemico-physiological action—a combination or interaction of physical and vital forces of the nature of which we have no accurate knowledge.*

CHAPTER II.

THE INFLUENCE OF DRINKING WATER IN PROPAGATING DIARRHŒA.

When we come to consider carefully the data at our disposal for the consideration of the question of the Propagation of Diarrhœa by means of impure drinking water, we find theory and practice very much intermingled. It becomes necessary, in the first place, to distinguish accurately between these two, so as to give each piece of evidence, as it arises, its proper place and weight. It is impossible, in the present day, that all should agree as to the value of any one piece of evidence, because of the want of concurrence between members of the medical community on certain etiological axioms according to which evidence must be judged. The author, however, stated distinctly his views regarding the principal of these, and will give value to evidence regarding disease propagation in accordance with these views, asking those who differ from him on those fundamental points, to alter, according to their own views, the values of the evidences which will be adduced. In the last chapter our attention was devoted to certain constituents of certain waters—in fact, to endemics, whether necessary or accidental to the locality; whether permanent or temporary. Now, it must be remembered, we are brought face to face with the phenomena of epidemics, and perhaps of pandemics; and also that we can only take evidence from cases of disease propagated from a foregoing case of the same disease, in which the connection can, with some show of probability, be established.

It must also be remembered that, in considering the question of propagation by drinking water, we have nothing to do with the more ordinary impurities of water, but only with those substances which have been an integral part of the body, or have been in contact with the body of the foregoing case, and which there is reasonable probability to suppose may be carried into the healthy economy by means of drinking water, and there propagate the same disease.

First, we have to deal with theoretical *à priori* evidence, arising from a knowledge of the following fact—that, in certain forms of diarrhœa, the cause of it may leave the affected body, and by means of drinking water find its way into an unaffected body; which may thereafter, as an effect, be affected with the same form of diarrhœa—the phenomena of true propagation.

Secondly, we have to deal with experimental *à posteriori* evidence, arising by practical experience, that a given case of diarrhœa is traceable to a foregoing case of the same, from the following facts:—(*a*) That drinking water, which we know by examination to have contained substances which formed an integral part of the affected body of the foregoing case, or which were in contact with it, was ingested in the given case; (*b*) that diarrhœa of the same form occurred after such ingestion; and (*c*) that the sequence of the two cases is unexplainable in any other way.

Manifestly, the *à priori* evidence, when complete, must be more satisfactory than the *à posteriori* evidence, because from the nature of things this latter never can be complete. It is always liable to be answered with *coincidence*, and put aside as a *post hoc ergo propter hoc*. In considering propagation of diarrhœa, all cases which are seen to occur in epidemic form must be noticed. Not that all of them are due to propagation (which is only one factor in the production of an epidemic), but it is reasonable to presume that some of them are—those especially which occur in sequence or synchronously in a family; or some domestic institution, as a school; or some public institution, as jails, asylums, hospitals, &c. Now, there is hardly an epidemic of diarrhœa on record in which the disease is propagated by itself. There are many epidemics of diarrhœa on record in which the diarrhœa is presumed to be due to other forms of alvine flux—more especially enteric fever and cholera.

We have absolutely no evidence, either from theory or from experience, that the irritation or congestion forms of diarrhœa are ever propagated by means of drinking water.

We therefore pass to consider what is known regarding the propagation of Pythogenic Diarrhœa. Both on theoretical and experimental grounds this seems to be a truly propagable form of diarrhœa. If the theory of the propagation of disease by *germs* be correct (we have here nothing to do with the question of its correctness or incorrectness), then we have some theoretical evidence, whatever it may be worth, that pythogenic diarrhœa is propagable by drinking water.

But putting aside the germ theory as being perhaps only applicable to the so-called specific diseases, there still remains some theoretical evidence on this matter—namely, the fact that pythogenic diarrhœa is originated by putrescent organic matter, taken with the further fact that the dejecta of a case so originated may pass in a state of putrescence into a healthy alimentary canal by drinking water, and there produce the same disease.

More than ordinary value must attach to speculative evidence on this subject, because of the fact that experimental evidence fails to account for the phenomena of epidemics of diarrhœa, especially as seen in children. In reviewing the recent weekly returns of the Registrar-General relating to the population of about 7,000,000 of persons living in twenty of our largest English towns, the *Lancet* (August 18, 1877) makes the following remarks:—"While it is beyond question that cool and wet weather in the height of summer is most favourable to the health of infants, it is equally evident that other than meteorological conditions operate powerfully in governing the mortality from diarrhœa. We have pointed out that during the week ending the 11th inst. the average annual death-rate referred to diarrhœa in the twenty largest towns was equal to 2·7 per 1,000 persons living; yet the rate in several towns during the same week ranged from 0·7 and 1·0 in Wolverhampton and Bristol; to 7·1 and 8·4 in Hull and Leicester. Leicester is again conspicuous for its excessive death-rate from diarrhœa, although its general death-rate has recently been satisfactorily low. These wide variations in the fatality of diarrhœa have still to be explained. It is a subject which demands further study and investigation at the hands of public hygienists." We now consider evidence derived from actual experience on the subject of propagation in epidemics of diarrhœa. As above remarked, there is hardly a record of an epidemic of diarrhœa in which we can be sure that propagation was the only or even a large factor in the epidemic. I may cite three large experiments to show how unsatisfactory such evidence is. The case recorded by Dr. Clouston of an epidemic of diarrhœa in the Cumberland Asylum, which seemed to be traced to emanations from a field over which the sewage of the asylum passed; the sewage was then taken into a brook; but after two years the plan of land sewage irrigation was again resumed, and the diarrhœa reappeared in the asylum as before. It is to be regretted that fuller information is not given in such reports, both as to the particular form of diarrhœa, or if there existed more than one form of the disease.

If we knew that at the time of letting loose the sewage over the land the asylum was free from disease, the case would refer to the origination of diarrhœa by organic contamination, as it is probable the sewage found its way into water by surface drainage. But this latter point is not noted.

It is, however, more probable that a case or two of the disease pre-existed; if so, the evidence refers to propagation. Dr. Parkes remarks:—"The case is certainly very striking, and, were it not for the numerous negative instances, would be quite convincing." Then, again, with regard to the sewage irrigation of Croydon, Dr. Carpenter finds that, amongst other diseases, diarrhœa is an effect on the public health. Here also the evidence is little worth. We are unable to determine whether the disease is produced by way of origination or propagation. The third case is a much more satisfactory piece of evidence. In his "Practical Hygiene," under the head of 'Effects of Impure Water,' Dr. Parkes quotes from Mr. Simon's Second Report the following case, in which, as he observes, "all the conditions of an exact experiment seem to have been fulfilled." In the jail at Salford there were two bodies of men—the prisoners numbering 466, and the officers and their families 53—living as far as possible under the same conditions. Of the former, 266 (57 per cent.) were attacked with diarrhœa of the pythogenic type, and not one of the latter. "The cause was not in the air, for both classes were on a par in that respect; the food of the prisoners was examined, and it was found to be good; the only other probable channel of the poisonous agent was the drinking water. It was discovered that, while the water was derived from the same source, the officers used the water of one cistern, and the prisoners' food was cooked with the water of another covered cistern, the untrapped overflow pipe of which communicated with a common sewer."

Now, out of 100 sick in this case, 73·68 per cent. fell sick within twenty-four hours, 21·02 in the second twenty-four hours, 2·63 in the third, 1·87 in the fourth, and 0·75 fell sick in the fifth twenty-four hours. We are not told that the communication between cistern and sewer was newly established just as this epidemic commenced; therefore we must presume—what is most probable—that it had existed for some time before any effect was produced. Again, the epidemic was declining, and in all probability would have passed away even if the communication between sewer and cistern had been allowed to continue; hence the epidemic could not have been produced by the simple communication. Some substance must have passed into the water within the twenty-four hours preceding the attack, over and above the usual products of sewer contamination, which must be presumed to have existed for some time before.

Then what was this substance? If the admissions to hospital in the jail, or cases of sickness for the week or so previous, had been given, the question may possibly be answered.

After careful search I am unable to find any evidence on this matter which is not vitiated thus—by being imperfect.

Neither do my own observations furnish any evidence with regard to epidemics of diarrhœa in which I have any proof that propagation was the chief factor. As regards *negative theoretical evidence* there is the fact that there does not seem to be any special constant characteristic of the dejecta, which, passing into a healthy intestine, would be likely to set up a similar action in it, apart from ordinary irritation by putrid organic matter; unless we assume that this putrescent diarrhœa is a form of fermentation —a zymotic disease, using that term literally—and that a particle of the zyme is capable of setting up the same action in another individual. There is no *negative experimental evidence* of which I am aware.

SUMMARY.

Theoretical. There are no *à priori* grounds on which to found a belief that the alvine flux of ordinary diarrhœa carries a specific modification of vitality capable of propagating the like phase of vitality in a second intestine. But considering the natural history of certain entozoa (as stated in the fourth column of Table on page 148) we would expect that diarrhœa produced by entozoa, which are capable of migration from one intestine to another by means of drinking water, is thus influenced in its propagation by this agent.

Experimental. Practical observation leaves no room for doubt that the diarrhœa which depends on the presence of certain parasites in the intestinal canal is influenced in its propagation by drinking water. This is especially the case with regard to those forms of diarrhœa produced by Ascaris lumbricoides, Ascaris mystax, Tricophyton dispar, Oxyuris vermicularis, Echinococcus hominis, and Bilharzia hæmatobia (see Table, pp. 148, 149).

There are also experimental grounds for believing that the forms of diarrhœa produced by *putridity* are in some way transmissible from one individual to another. But, it being almost impossible to exclude infection by atmospheric contamination in considering these cases, we are unable to say definitely whether or not propagation by drinking water is a factor in such transmission.

The following seems a fair conclusion from the evidence at our disposal, that—

The only form of diarrhœa regarding which we can be certain that its propagation is due to an agent which is carried by drinking water is that dependent on the presence of parasites in the intestinal canal. Although there are some slight grounds for believing that the pythogenic forms of diarrhœa may be transmitted in some manner from one individual to another, we have no good reason to believe that such transmission is due to propagation by the influence of drinking water.

CHAPTER III.

THE INFLUENCE OF DRINKING WATER IN ORIGINATING DYSENTERY.

Is dysentery a disease, or is it merely the name of a symptom, like diarrhœa or dropsy? If the word be used simply meaning an alvine flux of blood depending on some other known cause, also expressed—as in the phrase "scorbutic dysentery," meaning a flux of blood symptomatic of, or predisposed to, by scorbutus—then no doubt the term merely stands for a symptom. But when it is used in the sense in which the London College of Physicians place it in their nomenclature of diseases, among the diseases of the intestines, we must consider it a distinct disease, having its own definite and characteristic natural history, symptoms, pathological process, and anatomical sign.

Affirmative theoretical evidence.

This specificity as a distinct disease of the alimentary canal is the first *à priori* reason we have for believing that ingested matters may be a source of its origin. Then, again, its varying type, intensity, and effects in different countries, point to climate as an influence in its origin; and we know that the foods indigenous to a country, and its water supply, are frequently a large factor in those influences sometimes loosely described as climate. Again, its manifest connection with and frequent supervention on diseases of the water origin, of which there exists strong probability, suggest a likelihood of its also being originated by this cause.

The action of certain entozoa in producing a hæmorrhage from the bowel apparently specific to them, and distinct from the intestinal irritation and congestion common to the presence of all entozoa in the alimentary canal, must be remembered as perhaps originating a morbid condition upon which true dysentery supervenes under conditions favourable to its development.

The Sclerostoma duodenale, Oxyuris vermicularis, Bilharzia hæmatobium are noticeable in this respect, and are frequently (at least the two latter) ingested with drinking water. The Bilharzia gives rise to a formidable disease, which is often easily mistakable for dysentery, even as regards the anatomical sign.

The symptoms being those of inflammation and hæmorrhage from the intestines, and the after-death lesions, congestion of the intestine with extravasation of blood, deposit upon and beneath the mucous membrane, fungoid excrescences and croupy exudations, which occupy ulcerated patches of the bowel—it is suspected that the dysentery endemic to and so prevalent in Egypt is due to the action of this distoma. There is, then, some *à priori* probability of some cases of dysentery being originated by some entozoon ingested with drinking water. The well-marked putridity of some cases of dysentery may perhaps be taken as favouring the view that the disease is sometimes caused by the ingestion with impure water of organic matters in a state of putrescence.

Then, again, some views of the pathology of dysentery favour the idea of its water origin—namely, an analogy presumed by some to exist between this disease *and diphtheria.*

Recent observations of Oertel, Hueter, Nassiloff, Klebs, v. Recklinghausen, Eberth, Weber, Hetzerich and others,* go far to prove the doctrine that diphtheria is a *mycosis*, whatever local peculiarities it may assume from the tissue in which it is engrafted; in other words, that all the localizations of the disease are associated with the presence in the affected part of innumerable microzymes; and there exists much probability that such microzymes may be introduced through the medium of drinking water.

The probability of impure water originating the morbid conditions found in dysentery.

Considering the morbid processes with their products and lesions occurring in dysentery, there seem to be grounds for believing that such may be produced in subjects susceptible to the disease by the mechanical impurities of water.

Mechanical irritation causing hyperæmia and catarrh will produce the symptoms of dysentery when the mucous membrane, the capillaries and intercapillary tissue, are in a certain diseased condition. These simple processes seem complicated by a certain

* Ziemssen's "Encyclopædia of Practical Medicine."

modification of the inflammatory process acquired by the blood or textures from idiosyncrasy or previous disease—a dysenteric inflammation; just as we find wounds take on a systemic modification of inflammation in syphilitic inflammation and in erysipelatous inflammation. The commonest illustration of this is seen when residents in India who have suffered from dysentery, use food composed of oatmeal. In Highland regiments this is frequently seen, and some of the men are unable to take a small quantity of oatmeal without suffering in this way. This action can only be mechanical, and not caused by any peculiarity in the composition of this grain. And in the same way the mechanical irritation of certain suspended impurities of water give rise to the same symptoms. It would seem that ordinary irritation produces inflammation, which inflammation takes on certain characters by reason of some pre-existing condition either of the blood or of the intestinal tissues. I have noticed that this occurs from suspended impurities in water from limestone formations more that in any other.

With regard to the inorganic chemico-physiological impurities there are some grounds for believing that certain of them produce dysentery apparently by a specific action, apart from ordinary irritation, as, for instance, *sulphuretted hydrogen;* but there is not sufficient information on this point to warrant us in classing it in the experimental evidence.

Dysentery, or at least an epidemic of hæmorrhage from the bowels, with more or less ulceration, occurring among the workmen of the mines of Anzin, is said to be due to the effects of sulphuretted hydrogen.* This is of considerable etiological interest, and perhaps importance, when taken with the fact that epidemics of *boils* have been attributed to the same cause,† and the frequent co-existence of dysentery and hepatic abscess. Hepatic abscess being simply an aggravated visceral boil, it is possible that the connection between these two diseases may be that they are *sometimes* separate effects of a one cause, arising independently of one another, and not that one is a result of the other by absorption of pus, &c. Whether ordinary fæcal impurities are likely to originate dysentery is a question on which we have but scant evidence.

* Aitken, "Pract. of Med.," vol. ii. p. 956.
† Parkes' "Hygiene," pp. 60 and 89.

The evidence yielding an affirmative probability is briefly as follows:—We have seen that animal excreta which have undergone putrefactive change are split up into various constituents, all of them being more or less likely to set up irritation, and if their action be prolonged, hyperæmia of the mucous coat of the intestine, acting either locally on the bowel or systemically through the blood. We know also that these processes, when they occur in certain constitutions, or under certain conditions, run into a dysenteric inflammation and produce this disease. Further, we have seen that certain of these—noticeably sulphuretted hydrogen, and probably phosphuretted hydrogen—seem to exert a specific action in originating dysentery. We therefore conclude that, when fæcal matters which have putrefied are ingested with drinking water, they are probably capable of originating dysentery, but perhaps only when some predisposing condition co-exists. Whether fresh non-putridinous animal excrement possesses a similar power (further than setting up ordinary irritation), we have no theoretical grounds for believing.

Fortunately, experimental evidence, which we now proceed to examine, speaks very definitely on this point.

Affirmative experimental evidence.

Our experience of dysentery—isolated cases, endemics and epidemics—points unequivocally to impure drinking water as being one of the causes of origin of this disease. Impure water as a cause of origin is divided into (*a*) predisposing cause, and (*b*) exciting cause. The facts and arguments adduced by Prof. Lionel Beale in support of his views regarding the predisposing to cholera, founded on evidence of chronicity sometimes seen in the bowel lesions in rapid cases, are applicable (so far as they go) equally to dysentery. I have before, in the fourth chapter, dwelt in detail on this subject, so that it is unnecessary to do more than mention it here. Certain conditions which predispose to dysentery may, it seems, be themselves predisposed to by impure drinking water.

The use of drinking water for a considerable time which contains organic matter, even in small amount, produces a cachexia which predisposes to dysentery when an exciting cause comes into operation. Then with regard to scorbutus, which is a condition acknowledged by all to predispose to dysentery, the latest researches on its etiology and pathology show that certain conditions of water which has been used as drink for some time are powerful in predisposing to this condition. Dr. Ralfe, physician to the Seamen's Hospital, who has worked long and laboriously at this subject, thus sums up his conclusions in the *Lancet* of 1877 (July 21):—" 1st, That the primary change which occurs in scurvy is a *chemical alteration* in the quality of the blood; 2nd, That this chemical alteration, as far as can be judged from inferences drawn from the analysis of urine in patients suffering from scurvy, and analysis of scorbutic and antescorbutic diets, points to a *diminution in the alkalinity of the blood*; 3rd, That the diminution of alkalinity is produced in the first instance (physiologically) by an increase of acid salts (chiefly urates) in the blood, and finally (pathologically) by the withdrawal of salts having an alkaline reaction (chiefly alkaline carbonates); 4th, That this diminution of the alkalinity of the blood finally produces the same results in scurvy patients as happen in animals, when attempts are made to reduce the alkalinity of the body (either by injecting acids into the blood or feeding with acid salts)—namely, dissolution of the blood corpuscles, ecchymosis, and blood stains on mucous surfaces, and fatty degeneration of the muscles of the heart, the muscles generally, and the secreting cells of the liver and kidney."

I now proceed to give evidence of impure drinking water being an *excitant* of dysentery derived from experience. Prior to the year 1859 the Foot Guards quartered at the Tower of London invariably suffered from dysentery and diarrhœa to a great extent. In 1859 the admissions for these diseases were very much reduced as compared with the average of former years. " In the case of the Foot Guards there is satisfactory evidence that much of this reduction is due to the adoption of sanitary measures to improve their quarters in the Tower of London. The draining of the ditch and the supply of good drinking water were very likely to effect such an amelioration."

The Army Medical Department Reports for 1859 (from which the above is quoted) and 1860 show very clearly the immense reduction in disease, and especially in the digestive system diseases, consequent on the sanitary improvements introduced by the Barrack and Hospital Commission in the barracks and hospitals of the United Kingdom; and in these Reports many instances similar to the above are given. There is tolerably good evidence to show that water containing sewerage miasm is a frequent exciting cause of dysentery. The case of the Cumberland Asylum, before referred to, is a good illustration of this. The premonitions of these cases of dysentery are nearly identical with those known to arise from poisoning by sewer gases—namely, increased temperature, thirst, irregular and feeble muscular contraction, and intestinal irritation; these symptoms continuing so long as the exposure exists, and disappearing on removal of the cause, and not exhibiting any tendency to recur. A remarkable case is quoted by Prof. Parkes in his Hygiene Report for 1870, from Prof. Champouillon ("Recueil de Mem. de Med. Mil."). In 1852 two regiments—the 19th Light Infantry and the 44th of the Line—were barracked at Neuilly in Paris. During the month of August the first regiment was attacked with a grave form of dysentery, while the other regiment had only a few cases of diarrhœa. The water supplied to both was the same—namely, drawn from the Canal de l'Ourcq, which is known to be very impure. A hasty observer may for this reason have excluded the water in searching for the cause; but Champouillon discovered that the two regiments had different modes of dealing with the water. The 19th added brandy to it, and it was noticed that this caused it soon to become putrid if the water vessels were allowed to remain in the barrack-rooms. The spirit, in fact, precipitated the organic matter on the sides and bottom of the vessels. The other regiment took their water either as coffee or mixed with red wine, the tannin of which prevented any putrefaction. Red wine being substituted for brandy, the 19th Regiment soon became free from dysentery. We seem to have here all the conditions of an exact experiment. In the Report on Hygiene for 1873, an epidemic of dysentery at Nurnberg is noticed. It occurred in the summer of 1872, and was localized to the barracks. In four weeks thirty cases and four deaths occurred among the soldiers. On examination it was found that putridinous gases were absorbed by the drinking water from the sewers under the building in which it had broken out. The water contained abundant nitrates and free ammonia. But it seemed from observation during this epidemic that an individual predisposition to the disease was an essential necessary for its occurrence. The results of a somewhat large experiment, tending to show the influence of fæcal contamination of drinking water in originating dysentery, are given in the Reports of the Rivers Pollution Committee for 1870, describing the purification of river water by the irrigation system, and the influence of sewage irrigation on health.

It would be impossible to indicate in a categorical manner, even ever so briefly, without prolonging this Essay to an unnecessary length, the recorded experience of this disease. It must suffice to give the conclusions with regard to this matter arrived at by some of the most experienced observers.

From the experience of army surgeons extending over the last 250 years, obtained from the dysentery epidemics of the British army in Holland in 1748, the French, Prussian, and Austrian armies in 1792, the Walcheren expedition in 1809, in the Peninsular campaigns, in the allied armies in the Crimea, and during the American civil war, it is believed that this disease is originated by certain unfavourable hygienic conditions, acting with other conditions, either endemic or constitutional, which predispose to it. Moreover, it is generally agreed that the disease process known as dysentery is a specific process, although producible by various different and therefore non-specific causes. Records of this disease as it is seen in the East and West Indies, in China, the Ionian Islands, Gibraltar, Malta, the Canadas, and in Africa, go to prove the same causes of origin. It appears to be almost certain from these that there exists an intimate connection between dysentery and intermittent fever (in addition to that tendency which dysentery has to complicate the prevailing fevers in all climates), so close and constant as to amount to presumption that both may be sometimes due to a common cause—namely, paludal miasmata (Prof. Maclean, Dr. Grant). Proof is continually accumulating that paludal miasm can be absorbed by water, and that the condition known as *paludal toxæmia* may be thus produced. The Report "On the Prevalence of Ague and Malaria at Tilbury Fort in connection with the Source of Water Supply," by Surgeon-Major Faught, with the analysis of the water by Prof. de Chaumont, published in the Army Medical Department Blue Book for 1875, leaves no doubt that certain forms of organic water impurity derived from marshes produce malarial poisoning, and this cause would act with greater virulence in tropical climates. Hence it follows that impure drinking water derived from or passing through marsh land, or otherwise contaminated with paludal impurities, is one originating cause of dysentery.

The experience of Dr. Alex. Grant led him to form the same conclusion, and also to believe that sewage miasm—animal effluvia poison—may be a direct exciting cause of dysentery, constituting one form of the disease, as well as a predisposing cause, by reason of the cachexia induced by it. Prof. Aitken's opinion is as follows: —"Besides unwholesome food, water of an impure kind, and from an impure source, favours the development of dysentery; drained from swamps, and used for drinking and cooking purposes, as it was on the Chinese coasts, it exerted a marked injurious influence both in exciting and in maintaining the disease."

It is to be regretted that so experienced and accurate an observer as Dr. Morehead has not recorded his views of the origin of this disease, which he has so exhaustively described.* Annesley and Twining have both directed attention to impure drinking water as a cause of Indian dysentery. The same view we find advocated by the older army surgeons, Pringle, &c.; and in fact the animal effluvia idea of the origin of dysentery is as old as Cullen, if not older. Dr. Chevers has pointed out how much of the dysentery of Calcutta is produced by impure water ("Indian Annals," 1870). The great loss in the Peninsula, especially the epidemic at Ciudad Rodrigo, was attributed by Sir James Macgrigor to the use of water which passed through a cemetery where nearly 20,000 bodies had been hastily interred. Prof. Parkes observes: "Dysentery is decidedly produced by impure water, and this cause ranks high in the etiology of dysentery, though perhaps it is not the first." The Reports of Mr. Simon, late Medical Officer of the Privy Council, furnish many interesting and important facts, and many able arguments founded on them, in support of the view that impure drinking water is a frequent originating cause of dysentery.

In concluding this part of the subject it may be well to observe that, while strong evidence exists for maintaining that impure drinking water produces dysentery, it is by no means meant that this is the only cause, or that water may be more than one of many vehicles by which a *specific cause* of origin acts. The proposition is simply this—experience of dysentery warrants the belief that some cases of the disease are originated by impure drinking water.

* Dr. Morehead refers to climatic causes, merely mentioning the possibility of dysentery being caused by aliments.

Negative evidence, or evidence against the view of impure drinking water being a cause of dysentery origin.

Some facts upon which arguments may be founded against the view of impure drinking water being a cause of dysentery origin, are found to exist. These, so far as they go, rather tend to weaken the foregoing affirmative evidence, but do not disprove the proposition. These facts may be classed as (*a*) theoretical objections based on the nature of the disease, (*b*) theoretical objections showing the improbability of water impurity producing such an effect, and (*c*) practical experience that dysentery is actually produced by other causes.

We will briefly consider these. As regards the first, the nature of the disease, it is maintained by some that dysentery is caused by a certain condition of the coats of the intestinal vessels. This view is certainly corroborated to a great extent by reference to the necrological registers of the Royal Victoria Hospital at Netley. It is found, in the examination of old soldiers who have died from dysentery, that amyloid degeneration of the tissues almost constantly exists. And if this be the source of origin of dysentery, it is not likely that impurity of drinking water gives rise to it. Another view of the pathology of dysentery tending to the same conclusion is the relationship existing between this disease and hepatic lesions.

Dr. Parkes, in his work on "The Dysentery and Hepatitis of India," observes that, "if the functional morbid state of the liver is to be judged of by chemical analysis of the secretion of that viscus, the liver is found to be diseased more or less in every case of dysentery." Statistics show that undoubted hepatic lesions exist in 57 per cent. of dysentery deaths, and hepatic abscess in about 18 per cent. of the same (Grant, "Annals of Military and Naval Surgery"). We have no good reason for believing that serious hepatic lesions are produced by the influence of impure drinking water (apart from its action as a carrier of malaria poison referred to in a former chapter of this Essay), so that if dysentery be looked upon as an effect of liver disease, we cannot well maintain that it is originated by the effects of impure drinking water. Of course the question rests in a great measure on which—hepatic lesion or dysentery—is the cause and which the effect. The sequence of the phenomena does not throw much light on the matter.

With regard to the second class of objections, that water impurities are not likely to produce the symptoms of true dysentery, it is stated that the effects of sulphuretted and phosphuretted hydrogen, and other products of decomposition, and the entozoa said to produce dysentery, do in reality cause merely hæmorrhage from the intestines.

This may be true of some, but there certainly exists strong evidence (however unlikely it may seem in theory) as regards the Bilharzia hæmatobia, from the reported observations of its discoverer, Bilharz of Cairo, Griesinger, and Reinhart, that when this entozoon exists in the intestines, its effects so closely resemble those of ordinary dysentery in symptoms and post-mortem appearances, that they are not distinguishable from these by expert observers.

It is further objected that the varying conditions of water impurity in various localities in which dysentery is endemic, and in some of them the excellence of the water supply (as in some hill stations, where dysentery is frequent), go against the idea of drinking water being the cause of this disease; and further, that if so, the effect should cease on the removal of its cause, which is not so, dysentery sometimes continuing persistently, although the locality of residence, and most of the conditions of climate and life, are changed.

The experimental negative evidence is as follows:—Individual cases of dysentery occur, and dysentery is endemic, in some localities where the purity of the drinking water is above suspicion. And in certain cases the disease has been traced to other causes of origin, as cold, damp, fatigue, or unwholesome food acting on cachectic constitutions.

These propositions any one having experience of tropical disease must at once confirm, but they only go to prove that dysentery has other causes of origin besides impure drinking water, and by no means disprove that sometimes drinking water exerts an influence as a cause of dysentery origin.

SUMMARY.

Theoretical. There is some evidence, from the conditions which are found uniformly associated with dysentery, to believe that certain impurities of drinking water, especially those arising from putridinous organic matter, predispose to this disease. That their ingestion, continued for some time, produces a constitutional state prone to develop into dysentery when an excitant comes into action.

There is also evidence, from the same considerations, that certain impurities of drinking water, especially irritants, both mechanical and chemico-physiological, and certain entozoa, act as excitants of dysentery when the necessary constitutional conditions exist.

This leads to the conclusion that when a water containing both the disposants and the excitants—the primal and second factors—is used for drinking for a considerable time, this water may perhaps in itself be capable of exerting an influence in originating dysentery.

But it would seem that this can be only rarely a cause of dysentery, from the evidence that this disease is frequently accompanied by amyloid degeneration of the tissues, and particularly of the intestinal coats; and impurity of drinking water not being known to produce this change, it is argued that if dysentery be the result of this degeneration, drinking water is not likely to influence its origin. This evidence is not to be disregarded, but still is weak, unless it be proved that amyloid degeneration is a constant and essential condition of dysentery. The like argument, based on the frequent co-existence of dysentery and hepatic lesion, is in the same manner weak, without proof that hepatic lesion is essential to dysentery origin.

Experimental. There is satisfactory evidence from experience that drinking water is an occasional cause of dysentery origin. With regard to the experimental negative objection, that certain cases of dysentery have been proved to arise from other causes, the affirmative proposition, that certain other cases have been originated through the influence of drinking water—that this agent occasionally exercises an influence in originating the disease—is still unaffected. There is no experimental evidence going directly to prove that drinking water is never an originating cause of dysentery. Hence it is concluded that—

Our knowledge of dysentery warrants the belief that this disease may sometimes be caused by impurity of drinking water. But it would seem that drinking water produces this disease chiefly by furnishing an exciting cause when other predisposing causes coexist.

At the same time, the possibility is recognized, that drinking water, under peculiar circumstances, may furnish both a predisposing and exciting cause, and thus become a true originator of the disease: but such a combination of circumstances is probably rare.

CHAPTER IV.

THE INFLUENCE OF DRINKING WATER IN PROPAGATING DYSENTERY.

Before going into the question of whether or not dysentery is propagated by drinking water, and, if so, what the probable nature of such an influence may be, it is perhaps well to inquire whether dysentery is a propagable disease.

It would appear from the natural history of dysentery that this disease is closely allied to the entero-zymotic febrile group. In common with other members of this group, it is frequently found that many cases of this disease occur simultaneously. Are these so-called epidemics of dysentery real epidemics due to true propagation, or are they merely epidemics—that is, accumulations of originated cases of this disease?

From a pathological view, as regards its morbid action and product, dysentery is closely allied to what are commonly considered contagious bowel fluxes. The product of the disease being constant, it is presumed to be specific, and carry with it potentially the diseased vitality which originated it, which process it may propagate to a healthy intestine, when applied thereto through the medium of drinking water into which it has passed. According to recent speculations, especially those associated with the names of Hueter and Billroth, which have been brought forward regarding the pathology of this disease (corroborated by facts, more or less according to our interpretation of these facts), and the views entertained regarding the fundamental basis of the disease, it seems probable that the dejecta of dysenteric patients ingested with drinking water propagate this disease. These speculations assume that dysentery is a local disease, in the same way as diphtheria and erysipelas may be considered local diseases; that it is due to a contagium which is particulate and living; and that in the mucous membrane of the intestines this contagium meets with the conditions necessary to its development, its nidus, or the so-called second factor of the disease.

Affirmative theoretical evidence.

This localization of the cause of dysentery was evidently present to the mind of Morehead (who, if not a scientific etiologist, was at least a shrewd observer of facts) when, in considering dysentery, he instituted an analogy between diseases of the alimentary canal and those of the cutaneous system as follows :—" The opportunity which we enjoy of observing inflammation of the skin from its earliest appearance to its close, has enabled us to determine certain facts of its pathology. They may in theory reasonably be assumed to some extent of the mucous membrane of the large intestine, but for very evident reasons they are and must always be unsusceptible of proof, except in a very limited degree. If the skin during life were removed from the sphere of our senses, and all that we know of its inflammations were derived from certain symptoms caused by deranged function or constitutional sympathy, and from the appearances which the results of inflammation exhibit on inspection after death, we should be, in respect to the pathology of the skin, very much in the position in which we now stand in respect to the pathology of the mucous membrane of the large intestine. In this state of hypothetical comparative ignorance of inflammation of the skin, we should probably find that our positive knowledge might be fully expressed by some such single term as dermitis, just as we find our present positive knowledge of inflammation of the mucous membrane of the large intestine sufficiently expressed by the single term dysentery." And again: " It is believed that some inflammations of the skin are caused by the reception into the blood of some specific poisons. Such are the eruptions of measles, small-pox, scarlatina. To apply a similar theory of causation to some forms of dysentery, and to invest them with contagious or infectious properties, is quite within the limits of rational speculation. But it may be very safely affirmed, that as yet such opinion is altogether unsupported by facts. Again, it is sufficiently probable that the influence of a specific poison, or of blood vitiated by retained or altered excretions, may be operative in causing other cutaneous inflammations, as erysipelas, or some of the squamous, vesicular and pustular eruptions, and that such theory may also be reasonably applied to some forms of dysentery."

If this analogy between the pathology of these two classes of disease be allowed (and there seems to be a fair probability of its correctness) it is justifiable to extend the analogy to the methods in which skin diseases are propagated, and the propagation of dysentery.

Mr. Jonathan Hutchinson has followed up this analogy between the etiology and propagation of certain cutaneous diseases and certain entero zmyotic diseases. A condensed resumé of his views of the subject is given in his speech during the discussion on the Germ Theory of Diseases, at the Pathological Society, in April, 1875.

The doctrine he enunciates is as follows:—That the inflammatory products of these diseases (cutaneous and entero-zymotic) are themselves the media of contagion, the means by which the affection is propagated from one tissue to another, in an affected organism, and by which it is propagated from one organism to another; and that contagion is not propagated by means of any substance foreign to the animal economy. The term inflammatory product, as here used by Mr. Hutchinson, seems synonymous with "degraded bioplasm," as used elsewhere by Dr. Beale.

This is chiefly founded on the fact that results of different degrees of severity can be obtained by inoculation with matter taken at different stages of the disease. These results, he states, are not explainable on the theory that they are due to specific germs existing without the body, but that they are easily accounted for on the theory that they are due to the contagium of living cell-material supplied entirely by inflammatory processes, deranged processes of nutrition which go by the name of inflammations in the patient's own tissues.

He says: "I believe it is a general law that almost all cell elements arising in connection with inflammation in the patient's own body are under favouring conditions contagious, and that they produce in the recipient a disease of the same type—that is, differing in type, as gonorrhœal inflammation, as erysipelatous inflammation, as porrigenous inflammation, as diphtheria inflammation do in the recipient; also that it is necessary for cells to be produced by tissue similar to that in which they are to be transplanted; that gonorrhœal pus, for instance, manifests its potency as regards contagion very differently according to the mucous membrane to which it is applied; not all mucous membrane can grow it.

"The pus produced by the skin appears to grow only on the skin; or, at any rate, it is with great difficulty that it produces any inflammation on a different kind of tissue."

On this view the dejecta containing exudations from an intestine which is the seat of dysenteric inflammation passing into drinking water, and thus applied to the mucous membrane of a healthy intestine, may set up therein a morbid process identical with that which produced this exudation, provided, of course, that the latter have preserved its vitality, or rather that specific modification of vitality which distinguishes it from healthy secretion.

These are a few of the theoretical arguments favouring the view that dysentery is propagated by the exudations of the dysenteric inflammatory process passing with the dejecta into drinking water, and thus gaining access to the mucous membrane of a healthy intestine. It will be observed that they are for the most part based on the assumption that dysentery is a specific local inflammation.

Affirmative experimental evidence.

Professor Aitken observes: "It does not seem to be so clearly understood as it ought to be that dysentery is contagious, or rather that it is capable of being propagated from person to person. Being a frequent complication or concomitant of contagious fevers, it has been believed to inherit similar contagious properties. In the severe form of dysentery for which the old Infantry Barracks of Secunderabad, in the Deccan, have long been notorious, it has been observed that men labouring under other diseases, who happened to be exposed to the putrid effluvia of the excretions of dysenteric patients, were often severely affected by the disease (W. C. Maclean). There is, therefore, good reason to believe that the exuviæ of dysenteric patients, as passed by stool, may, like those of typhoid fever, propagate the disease; and the observations of Budd and Goodeve give support to this view. Niemeyer also entertains this belief." The opinion of the late Prof. Parkes is given as follows:—"The observations which prove so satisfactorily that the dysenteric stools can propagate the disease, make it probable that, as in the case of typhoid fever and cholera, the accidental passage of dysenteric evacuations into drinking water may have some share in spreading the disease." The observations here alluded to, and on which this opinion seems to be founded, are those of the old army surgeons, especially Pringle, Donald Munro, and Lemprière. In the *British* *and Foreign Medical and Chirurgical Review* for 1849 Dr. Parkes contributed a valuable Essay on Dysentery. In that article are given important details of an epidemic (?) at Guadaloupe in 1847. These furnish some strong evidence of true epidemicity—that is, of true propagation. It is to be remarked, however, that Dr. Parkes did not arrive at any conclusion as to the propagability of dysentery from his own experience of the disease, which seems to have been considerable. Dr. Morehead, who has dwelt with considerable detail on the causes of dysentery in his "Researches on Disease in India," does not so much as mention propagation. I have seen a great deal of dysentery in India and on board ship, but I have not observed any facts leading me to the belief that the disease is propagated by contagion; but there are some facts which suggest that it may possibly be infectious. (See Table of Definitions.) On the whole, it must be admitted that the experimental evidence telling in favour of the propagation of dysentery by drinking water is exceedingly slight.

Theoretical evidence against the propagation of dysentery by impure drinking water.

Arguments against specificity of dysentery, and therefore against the specificity of its dejecta, and therefore against its propagation by these, are derived from (*a*) the nature of the dysenteric fever, (*b*) constancy of susceptibility, (*c*) alternations with chronic diarrhœa, (*d*) inconstancy of type, and (*e*) the different results of remedies. It would appear from the following considerations that the fever of dysentery differs in its nature, in some important particulars, from the fever of other diseases which are known to be propagated by contagion.

Of course, it has not been proved that fever is a necessary accompaniment of contagion propagation, but from what little is known of the nature of contagion, this seems probable; and when we know that the fever which accompanies a disease (as in dysentery) is wanting in the usual phenomena of specificity, we must consider this evidence (for as much as it may be worth) against the specificity of the disease.

Now, the fever which accompanies diseases, the propagation of which is due to contagion or is specific, is characterized by certain fixed phenomena, amongst which may be mentioned a definite period of incubation, the phenomena of defervescence, and evidence of increased waste of nitrogenous tissue.

The definite period which elapses between the time of the reception into the system of a specific virus, and the time of its action first becoming evident, seems to be of a uniformly exact duration for each of the specific diseases.

The definiteness of this period is explained by the nature of contagion, and it is generally held to be essential to true propagation by contagion. It is believed that this phenomenon depends on the "multiplying" or "self-propagating" power of the disease germ. "In the early days of the period of incubation these particles exist in comparatively small quantity. They very quickly increase, however; and during the last few days of that period must be very abundant. The more abundant they are, the more rapidly will the general mass of the contagium particles increase. Suppose that each produces four others, and that these, in their turn, give rise to as many more; it is evident that the number of germs will soon be so great that it cannot be quadrupled without causing almost an explosion of disease germs in the system. The advent of this inevitable result terminates the period of incubation, and ushers in the stage of invasion" (Maclagan). It is therefore believed that those diseases in which the phenomena of incubation are absent are not propagated in the true sense of the word—that is, that they are not transmitted from person to person by means of a particulate and self-propagating germ; but that if they are so transmitted at all, they are so by means of some *influences* (or perhaps *conditions*) which are not particulate and not self-multiplying, and which are not contained in organisms palpable to or appreciable by the senses.

To express such influences or conditions we at present use the terms *emanations*, *exhalations*, *miasms*, &c., and the phenomena of such transmission we call *infection;* these expressions being simply confessions of our ignorance of the nature and mode of action of such influence. We know little of contagion, but we know far less of infection.

It would seem, from the absence of any definite period of incubation in dysentery, taken with what evidence we have of the so-called contagion or propagation of this disease—as in the case of the Secunderabad Barracks (referred to on page 185), where it is observed "that men labouring under other diseases, who happen to be exposed to the *putrid effluvia* of the excretions of dysenteric patients, were often affected with the disease"—that it is to be classed among those diseases the *carriers* of which are not particulate or self-multiplying—that is perhaps *infectious*, but not truly propagable by contagion.

If this be so, the probability of its being propagable by means of drinking water is, of course, lessened.

We are not in a position to say how much, or if at all, such *miasms*, or *infectious influences*, are capable of being transmitted by water; nor have we even any evidence to prove that dysenteric evacuations *per se* (*i.e.*, without the putridity) possess any power of causing disease.

The indefinite duration of an attack of dysentery, the gradual amendment of a patient recovering from this disease, the indefiniteness of the time this may occupy, and the absence of a definite crisis, as well as a fixed period of defervescence in this disease, also point to the same conclusion.

These, as well as other differences between the fevers of specific diseases and the fever of dysentery—such as the absence of preternatural heat in the latter, the little increase in the patient's consumption of fluids, and in the amount of urea eliminated, and ordinarily the absence of delirium, indicate a fundamental difference between dysentery and other diseases of which the propagability by a particulate contagion is known.

Some, therefore, argue that as the febrile symptoms which are found to accompany dysentery are non-specific, so dysentery is in its entirety non-specific; and, as such, is incapable of being propagated by drinking water or any other means.

These objections to the propagability of dysentery are answered by saying that the febrile symptoms of dysentery are caused merely by systemic disturbance, resulting from the local intestinal lesion—just in the same way as non-specific fever may result from the local irritation of a cutaneous disease, about the specificity of which latter there can be no doubt.

The arguments against the propagability of dysentery which are derived from the *constancy of susceptibility* to it are much more satisfactory. It is maintained that dysentery is not propagable by a specific virus, because the susceptibility to it is constant; the advocates of this view holding that exhaustion of susceptibility is an essential of specificity on the following grounds:—They believe (as already stated) that such diseases depend upon the co-existence of two factors—namely, the disease germ, called the first factor, and the second factor; these two, probably, being nearly identical with what are usually described as the exciting and predisposing cause.

This position is chiefly defended by the fact that matter may be taken from a vaccine vesicle and inserted into the arm of one who has never been vaccinated, with the certain result of producing a similar vesicle; and that some of the same matter may at the same time and in the same way be inserted into the arm of one on whom the operation had previously been performed with success, with the certainty that there will either be no result at all or only a modified one.

And so with other diseases—scarlet fever, measles, &c.; those who have once suffered from them enjoy a future immunity, although much exposed to contagious influence.

These phenomena they explain by stating that, after one attack, all the second factor which is necessary for the development of the first has been used up, and not again reproduced.

It is hence argued that no second factor is necessary for the development of the exciting cause of dysentery, and that therefore it is not propagated by a specific first factor, or exciting cause. The only attempt at an explanation of the nature of this hypothetical second factor is made by the followers of Professor Beale.

In a note on page 115 of his "Disease Germs" he remarks that, while embryonic bioplasm ordinarily proceeds to form tissue; a small portion of it does not undergo metamorphosis into masses of the next series of tissue, but remains in the tissues in an embryonic state. "Many cancers and other morbid growths probably originate in these masses of embryo bioplasm, which remain for a long time in a quiescent state, embedded in some of the fully formed textures of the adult." And others extend this theory by believing that certain of these embryonic bioplasts are capable of passing into certain states, producing or predisposing to certain specific diseases; and that when all the bioplasm capable of being transformed into a certain disease cause has been used up by the supervention of such disease, then the susceptibility to that disease ceases, and an immunity regarding it is henceforth enjoyed by such an organism. Some doubt is also thrown on the specificity of dysentery by its occasional supervention on other diseases, its alternation, especially, with diarrhœa.

We often find dysenteric symptoms subside and give place to chronic diarrhœa, until some cause of intestinal congestion or irritation—chill, error in diet, &c.—reproduces the dysentery, and so on from time to time, the patient never being quite free from one or the other form of alvine flux.

Dysentery is also a frequent concomitant of remittent fevers; and in the Army Medical Department Blue Book for 1864 we have recorded a case in which the pathological signs of both dysentery and enteric fever were well marked. We sometimes find also symptoms and post-mortem appearances closely resembling dysentery and cholera coexisting. Some cases of this are related in the *Indian Medical Gazette* for December ,1876. I quote one autopsy of the intestines :—

"The mucous coat of stomach arranged in fourteen or fifteen long folds, running from the cardiac to the pyloric end, of a rather pale grey colour.

"Small gut contains some fluid semifæcal matter and three small tape-worms. The mucous membrane healthy throughout. The great gut for the first six inches beyond the cæcum contained some soft yellowish formed fæces; the rest of the gut was empty, and its mucous coat was of a dark greenish mottled appearance, thickened, rugose, and sloughing.

"In the middle of the transverse colon a slough of mucous membrane of the size of a rupee had separated, exposing the muscular coat; and in the last three inches of the rectum the mucous membrane had been shed, and the gut had so wasted that it was quite translucent."

The paper concludes as follows :—" I am informed that about a twelvemonth ago a ship had, when three days out from Calcutta, to turn back on account of the illness on board; and that some half-dozen of the crew were brought to the Presidency General Hospital to be treated for the excessive diarrhœa that had prevailed, of whom two presented symptoms so resembling those of cholera that they were sent down to the cholera ward, where they shortly died; and it was only at the post-mortem inspection that the diagnosis could be definitely settled, when they were found not to have suffered from cholera, but from sloughing dysentery. Again, in the Presidency jail, in August, 1872, two men were struck down with symptoms so resembling cholera as to be diagnosed as such; of these men one died, and on inspection was found to have died of sloughing dysentery; in the other the symptoms gradually developed into those of pronounced dysentery, from which he recovered." Some would argue from these cases that dysentery is not specific, because they maintain that two specific diseases cannot exist at the same time in one organism. I have, however, more than once, and so no doubt have many others, successfully vaccinated men suffering from acute syphilis, by way of experiment on this point.

Inconstancy of type in dysentery (scorbutic, malarial, &c.) is also said to throw doubt on the specificity of this disease; as is also *the different results of remedies used.*

Whatever it may be worth on this point, it is certain that the latter is true. I have already twice alluded to my experience of the dysentery of the men from the Perak expedition of 1876. I was surprised to find how useless the ipecacuanha treatment was with those cases;* the most successful remedy being an emulsion made with poppy oil and liquor potassæ containing laudanum. But this treatment has since frequently failed me, while the ipecacuanha has been successful.

Experimental evidence against the propagation of dysentery.

The practical evidence at our disposal on this question is very meagre and defective. It consists chiefly of facts which go to prove that seeming epidemics of dysentery are not epidemics, but aggregations of cases due to endemic origin—that such are due to origination, not to propagation; and that the seemingly epidemic character of such an outbreak is only due to the fact of such cases occurring at or about the same time.

Their synchronous occurrence is, however, explicable on other grounds than that of the disease being propagated by contagion; as, for example, on that of its being produced by atmospheric or other conditions, occurring at certain seasons, which influence a community at large, and cause a number of cases of the disease to occur at the same time.

This explanation of outbreaks of dysentery is fully borne out by experience, at least in the dysentery of tropical climates. Morehead states :—" When I turn to my hospital experience, I find that the proportion of admissions from dysentery is greatest in those months of the year in which the atmospheric state is most likely to

* Since writing the above I find the same fact was observed by the medical officers serving with the troops at Perak. It is mentioned in the Memorandum of the Sanitary Commission, published in the "Report on Sanitary Measures in India in 1875–76."

be one or other of those which I have just described (absolute lowness of atmospheric temperature, considerable diurnal ranges, much moisture of atmosphere, and currents of dry or humid air), and in this category I am careful to include June and November—months in which marked atmospheric changes occur in Bombay. In June the hot season terminates, the rains begin to fall and humid winds to blow; in November the sultry heat of October ends and north-easterly winds set in. The following statement shows the proportion of admissions for dysentery per cent. of the total admissions in the European General Hospital, and the Jamsetjee Jejeebhoy Hospital at Bombay, in different seasons of the year :—

SEASONS.	European General Hospital.	Jamsetjee Jejeebhoy Hospital.*
Cold months—November, December, January . .	10·8	10·2
Wet months—June, July, August	7·0	10·7
Transition from cold months—Feb. and March .	6·3	6·4
Transition from rains—September and October .	5·4	8·9
Hot months—April and May.	5·1	7·2
Annual proportion	7·4	9·1

We find that these aggregations of dysentery cases occur, not among persons between whom much intercourse takes place, or who are by any medium particularly exposed to a propagation of the disease from one to another; but among those who are exposed to like influences, persons living under the same or similar sanitary conditions.

To quote again from Dr. Morehead :—" Exposure to continued elevation of temperature, habitual residence in an atmosphere vitiated from excess of carbonic acid or the effluvia of decaying vegetable or animal remains or too much moisture, will induce cachexia ; so will also the habitual use of food defective in quantity or quality, intemperance of all kinds, too much bodily fatigue, and the influence of depressing passions, as anxiety, fear, &c. A cachectic state may also arise from long-continued disease of any kind; from injudicious and too prolonged antiphlogistic medical treatment; from mercurial influence and the poison of syphilis, &c. The occurrence of dysentery in crowded barracks,

* This column gives the proportion of dysentery and diarrhœa combined.

transport ships, jails, standing camps, besieged garrisons, beaten and retreating armies, are illustrations of the importance of considering cachectic states in our explanation of the causes of dysentery. There can also, I apprehend, be little doubt that close inquiry into the history of the kind of event to which I have just referred will always clearly demonstrate the influence of atmospheric states, consequent on defective clothing and houses, as an exciting condition of the disease. The acknowledgment of this law also renders unnecessary the use of such terms as *camp*, *jail*, and *hill* dysentery."

When we come to consider outbreaks of dysentery on a large scale, we find that such individual exciting causes as those just referred to must be secondary and subordinate, if they have anything to do with the causation of such outbreaks, and that propagation by contagion must have less or nothing to do with such; as, for example, in the well-marked prevalence of dysentery and other complaints of the bowels all over India in 1872 (see Ninth Annual Report of the Sanitary Commissioner with Government of India). The admissions for dysentery were considerably augmented in all three Presidencies among Europeans and natives, and the deaths as well as admissions much increased among natives, showing the influence of some widely spread pandemic condition of the nature of which we are ignorant.

A good deal of attention seems to have been given to the prevalence of dysentery in the Berar district, with a view of ascertaining the causes to which it may have been due. The result points altogether to endemic or meteorological conditions as being the causes of such prevalence, and no trace of evidence appears to be forthcoming favouring the idea that such outbreaks of dysentery partake at all of the nature of epidemics in which the disease is propagated by contagion or infection. There is some slight evidence showing that surface drainage into drinking water may be a cause of *origin* of the disease; but in no case does it appear to have been produced by contamination of drinking water with dysenteric evacuations.

"With a view to throw some light, if possible, on the effect of drinking river water as compared with well water in producing disease (bowel fluxes), Dr. Abbott tabulated the deaths for five years in six towns (Berar district), with the result that, though the total mortality is higher where well water is used, the mortality from bowel complaints is less, except in the months of July, August, and September, when the increased mortality from the latter cause is probably due to water contamination, the wells in Berar being filled from surface drainage only" ("Report on Sanitary Measures in India in 1874–75," p. 48).

In the Memorandum of the Army Sanitary Commission on the Medical History of the Laroot Field Force (Perak), published with the "Report on Sanitary Measures in India in 1875–76," all the important facts which could be ascertained regarding the causes of the great prevalence of dysentery in that force are given. It must be noticed that in this case, predisposing cause—that is, a pre-existing constitutional cachexia—cannot have had much to do with the prevalence of dysentery, unless such were caused by the conditions of service at Perak; and if so, they certainly acted more rapidly than such causes usually do, because the corps engaged were in exceptionally good sanitary condition on taking the field. The 1st Ghoorkas went from Dhurmsala; the battery engaged, 10-8th R.A. (then 3 of the 5th), had shortly before been two years at Darjeeling; and the Buffs went from Calcutta, which cannot be called a bad station as regards those conditions which produce cachectic states or other predisposing causes of disease as ordinarily understood.

The Memorandum referred to states (p. 190):—"Considering that this force was sent into the field during the least unhealthy time of the year, it becomes a matter of importance to learn why it suffered so much from sickness in so short a time. The facts already stated show that the most important causes of death and inefficiency were dysentery and diarrhœa, which gave rise to 40 per cent. of the disease admissions, and 71 per cent. of the deaths from disease.

"The fever admissions were not so great proportionately for a tropical climate, so that the reply to the question must be sought in determining the cause of prevalence of these bowel diseases. . . . . The following is an important statement of the medical officer in regard to the causes of dysentery. After describing the great severity of the cases, he says:—'A very moist atmosphere, temperature high and heat rather oppressive during the day, occasional puffs of a cool north breeze, a close evening, followed towards early morning by a sudden fall in the temperature, and the advent of a cold northerly breeze; the proximity of a large river, and the occurrence of morning fogs; exposure to the sun, with the body perspiring profusely under exertion; a thirst that frequent and copious draughts of water could only allay; slime in the water, of itself causing a tendency to diarrhœa; and indigestible food;—these various agents acted on the system in such a manner that a chill was all that was required to excite morbid action, and as large quantities of cold water were swallowed daily, the bowels suffered."

With regard to the effect of impure water, the medical officer of the Ghoorkas states that these men did not drink the river water, but dug wells for their own use, which yielded good water. They, nevertheless, had fifty-eight cases, and two deaths from dysentery and diarrhœa.

But, on the other hand, there is no information as to the chemical or microscopical characters of either water, and the facts, such as they are, lead to grave suspicions as to the purity of the river water at least, which we are told deposited a colourless and transparent gelatinous slime, which rapidly choked the filters. Subsequently all the drinking water was boiled, with decided improvement in its quality.

Now, it will be observed that in those referred to, and in other outbreaks of dysentery, the disease has been, as far as investigation can show, produced by endemic or pandemic influences, acting on congregations of persons who are exposed to like sanitary conditions; this influence resulting in the occurrence, at or about the same time, of many cases of the disease, imparting to the outbreak a pseudo-epidemic character.

In none of these outbreaks which have been reported on has anything like propagation been detected; not a trace of contagiousness has been observed. Indeed, the idea of the propagability of the disease hardly seems to have occurred to the minds of the observers.

SUMMARY.

Theoretical. Taking all the theoretical evidence for and against the propagability of dysentery which we can find, the following are the conclusions to which it seems to lead:—The evidence or arguments in favour of its propagability appear to rest altogether on the idea that the disease is a local specific inflammation of the intestinal mucous membrane.

Those who hold this view have, I think, failed to prove the proposition—it remains a mere assumption.

Therefore, the arguments founded on it must be greatly questioned. But, even if this assumption were proved to be true, the arguments founded on it in favour of the propagation of dysentery are, I believe, fairly answered by those who hold the opposite doctrine. It is true that the latter fail to prove that dysentery is not propagable; but the *onus probandi* should rest with those who assert that it is so.

The idea that water containing the dejecta of dysenteric patients can propagate the disease is quite unsupported by argument, even if it be admitted that dysentery is a local specific disease.

Therefore, without dogmatizing on the question of whether or not dysentery is a propagable disease (using that term in a wide sense), I believe we must conclude that the balance of theoretical evidence is against its propagation by water, and therefore that drinking water has no influence in propagating dysentery; at the same time admitting that the evidence is very slight, and that further light brought to bear on the subject may necessitate a modification of this conclusion.

Experimental.

The experimental evidence leads, I believe, to very much the same conclusion as that above deduced from the theoretical. There is some evidence that dysentery is infectious; as, for example, that of the Secunderabad Barracks, referred to on page 185.

But this seems to be capable of being explained in other ways. It is said that "men *labouring under other diseases*, who happened to be exposed to the *putrid* effluvia of the excretions of dysenteric patients, were often severely attacked by the disease." Now, it may be that the other diseased conditions from which we are told they were suffering had as much, or more, to do with the dysentery attack than the exposure. Or it may be that this exposure gave rise to a cachexia which, acted on by other conditions, such as endemic influences or unsanitary state of the barracks, produced dysentery.

Or, if the disease were really excited by the exposure to putrid dysenteric evacuations, how much had the putridity to do with it? Would fresh dysenteric evacuations have produced the same effect?

Or, putting aside the putridity, would other fæcal evacuations, such as emanations from the stools of simple diarrhœa, have produced dysentery in persons already cachectic—"men labouring under other diseases."

There seems only a small degree of probability that dysentery is a propagable disease, and there is not a trace of experimental *proof* that it is propagable by drinking water. We find that considerable attention has been paid to the etiology of dysentery by the compilers of the Sanitary Reports for India; and it is only reasonable to suppose that, were dysentery propagable by drinking water, some evidence of the fact would have been brought to light before now.

So that, as in the case of the theoretical evidence, the evidence from experience also leaves it doubtful whether dysentery may or may not be a contagious, or at least an infectious disease.

But in the entire absence of all evidence to show that it is propagated by drinking water, I believe, from a consideration of all the evidence, theoretical and experimental, it is legitimate to draw the following conclusion :—That *drinking water has* NO *influence in propagating dysentery, so far as our present knowledge affords us means of judging.*

CHAPTER V.

THE INFLUENCE OF DRINKING WATER IN ORIGINATING ENTERIC FEVER.

General observations.

We now come to the consideration of the influence of impure drinking water in originating Enteric Fever. The etiology of this disease is perhaps one of the most important and interesting medical problems of the day.

The subject is especially important as a question of army sanitation, inasmuch as the prevalence of this disease is a large factor in the fatality and sickness from which our army suffers in the United Kingdom, the Colonies, and in India. Of such importance is it considered, that for the past few years the *prevalence of enteric fever* has been specially reported on in the Parliamentary Reports of the Army Medical Department. The problem of the nature and cause (or CAUSES) of enteric fever is one which has hitherto baffled the inquiries of etiologists, because some of the phenomena of the disease are truly *epidemic*, others as truly *endemic*, and yet, again, others most emphatically pronounced as being due to causes identical with *idiosyncracy*, or the individual health condition of the persons attacked. A satisfactory explanation of the phenomena must therefore include all these phenomena, to the exclusion of none. Propagation does not explain the occurrence of the disease at certain seasons, and its persistent endemicity at certain stations. Endemicity does not explain the phenomena which characterize this disease as truly and most unmistakably epidemic. And, again, neither of these explain why young soldiers during the first three years of Indian service should be specially liable to be attacked by enteric fever. Until etiologists recognize and accept the necessity for accounting for all these groups of phenomena—the epidemic characters, the endemic characters, and those evidently associated with individual idiosyncracy—which go to make up the natural history of enteric fever, there is little hope of arriving at a satisfactory knowledge, or explanation at least, of its nature and causes.

Up to the present this has not been done. Etiologists have described exhaustively the epidemic phenomena of the disease, quite excluding or ignoring the remaining phenomena, or using much ingenuity to deny them altogether, or persuade us that cases apparently due to endemic influence or individual conditions —so-called sporadic or spontaneous cases—are really due to epidemic influence or propagation.

This seems to be a mistaken method of investigating the subject. The explanation of the propagation of enteric fever is mistaken for an explanation of the cause and nature of that disease, and for those phenomena of it which most certainly are not due to propagation.

We will endeavour here to examine all the groups of phenomena together, giving to one as much importance as to another, and see if they be explained by a common cause; always with reference to the immediate subject of our inquiry—namely, the influence of drinking water in causing enteric fever.

It must be remembered that in the present chapter we have nothing to do with the contagiousness (or, to use the less clumsy expression of the French, the contagiosity) of enteric fever. The greater part of what has been written on the etiology of this disease refers to its propagation; and this division of the subject is reserved for consideration in the succeeding chapter. We have in this place to elicit the facts and arguments which bear on the origination of the disease—its spontaneous or *de novo* occurrence.

When treating in the foregoing chapters of the influence of drinking water in the origin and propagation of diarrhœa and dysentery, it was necessary to state the facts and arguments for and against the idea that any such influence existed; because this question has not received as much attention, perhaps, as it deserves, nor are such facts and arguments as definitely stated as they might be in standard medical works.

But, as regards enteric fever, the case is different. The facts and arguments for and against the doctrine of its *de novo* origination have been very definitely stated and discussed of late years, It seems, therefore, unprofitable to go into this matter in the present Essay, or to bring forward the vast mass of experimental evidence which can be adduced on both sides; the question with which we are concerned being, not so much whether enteric fever is ever originated, as by what influences drinking water *may* originate enteric fever.

This question, however, to some extent includes the broader one of the probability or otherwise of origination.

In considering this subject in the following pages I make two assumptions, viz.:—

1. Enteric fever is sometimes originated; and

2. Enteric fever *may* be originated by impure drinking water.

I distinctly state at the outset that these two assumptions are, in the present state of our knowledge, incapable of absolute proof. I, however, proceed to justify the idea of their probability, noticing the controversy on enteric fever origination only so much as is necessary for the purpose, without going into details of the facts and arguments on each side.

For some years past etiologists have arranged themselves into two sects, as regards the question of the spontaneous origin of enteric fever—those who deny its spontaneous origin (of whom Dr. Budd may be taken as the representative), believing that every case of this disease is propagated, and to whom I shall here, for brevity, refer as *propagists;* and those who believe in its occasional spontaneous origin and its occasional contagion propagation (represented by Dr. Murchison), whom I will here briefly call *originists,* although this term does not fitly designate them, holding, as they do, both the origination and propagation of this disease.

The views of the followers of Budd (or *Buddhists,* as they are sometimes playfully called) may be stated in the form of the conclusions summed up by Dr. Budd in the last chapter of his book on Typhoid Fever:*—

"1st. Typhoid fever is in its essence a contagious or self-propagating fever, and is a member of the great natural family of contagious fevers, of which small-pox may be taken as the type. 2nd. The living body of the infected man is the soil in which the specific poison which is the cause of the fever breeds and multiplies. 3rd. The reproduction of this poison in the infected body, and the disturbance attaching to it, constitute the fever. 4th. This reproduction is the same in kind as that of which we have in small-pox ocular demonstration. 5th. The

* "Typhoid Fever; its Nature, Mode of Spreading, and Prevention." By W. Budd, M.D., F.R.S.

disease of the intestine, which is its distinctive anatomical mark, is the specific eruption of the fever, and bears the same pathological relation to it which the small-pox eruption bears to small-pox. 6th. As might have been anticipated from this view, the contagious matter by which the fever is propagated is cast off chiefly in the discharges from the diseased intestine. 7th. As a necessary result, sewers, and the cloaca which, under existing sanitary arrangements, are the common receptacles of these discharges, are also the principal instruments in the transmission of the contagion; and consequently, in many instances, the infected sewer, and not the infected man, appears as if it were the primary source of the specific poison. 8th. When once cast off by the intestine, this poison may communicate the fever to other persons in two principal ways—either by contaminating the drinking water or by infecting the air. 9th. As an inevitable consequence of the impalpable minuteness of the contagious unit, and the many invisible and untraceable ways in which it is transmitted, cases must be constantly occurring exactly as in other contagious fevers, whose lineal descent cannot be followed, and which spring up, therefore, under the semblance of spontaneous origin. 10th. The occurrence of such cases obviously constitutes no proof whatever that this fever ever does arise spontaneously. 11th. The exceeding speciality of the conditions attaching to the reproduction of the specific poison in the living body itself, as well as the facts relating to the geographical distribution, past and present, of this and the other contagious fevers, constitute evidence as strong as such evidence can ever be, that none of these fevers originate spontaneously, but are propagated solely by the law of continuous succession. 12th. By destroying the infective power of the intestinal discharges by strong chemicals or otherwise, the spread of the fever may be entirely prevented; and by repeating this process in every fresh case as it arrives, the disease may in time be finally extinguished."

In general terms, these conclusions amount to this, as if those who hold them were to say: "We are able to trace certain cases of enteric fever to a preceding case; therefore all cases of enteric fever must result from a preceding case, or are propagated, and the disease is never a result of any other cause." Now, by a parity of reasoning, they should also say: "Puerperal fever is sometimes propagated by an attendant going from a case of this disease to what would, but for this circumstance, have been a healthy child-birth. It sometimes, indeed, *seems* as though puerperal fever occurred from a practitioner or nurse going from a case of scarlet fever, typhus, erysipelas, or small-pox, to a labour; but this is a fallacy consequent on the impalpable minuteness of the contagious unit, and the many invisible and untraceable ways in which it is transmitted, and the necessary difficulty in always tracing back to a foregoing case of puerperal fever. But this hypothetical foregoing case must, nevertheless, be the true cause; and the scarlet fever, typhus, erysipelas, or small-pox have nothing to do with it."

But this is not said, because here the absurdity is manifest. The views of those who believe, with Murchison, that enteric fever is capable of being both originated and propagated, are embodied thus by Dr. Murchison in his conclusions:*—

"1st. Enteric fever is either an endemic disease, or the epidemics are circumscribed. 2nd. It is most prevalent in autumn, and after hot weather. 3rd. It is independent of over-crowding, and attacks the rich and poor indiscriminately. 4th. It may be generated independently of a previous case, by fermentation of fæcal and perhaps other forms of organic matter. 5th. It may be communicated by the sick to persons in health, and even then the poison is not, like that of small-pox, given off from the body in a virulent form, but is developed by the decomposition of the excreta after their discharge. 6th. Consequently, an outbreak of enteric fever implies poisoning of air, drinking water, or other ingesta, with decomposing excrement."

Professor Aitken combats these views of spontaneous origin thus:—He says that "the belief in spontaneous origin rests on evidence entirely negative—namely, the fact that cases do spring up in which it is impossible to trace the disease back to a personal source of specific propagation and dissemination; an event which is inherent in the very nature of these diseases. For the active principle of the poison is invisible, although the matter that is known to contain it may be capable of isolation and inoculation, as in small-pox; yet the existence of the specific disease poison is know to us by inference only. Again, we know that ample provision is made and ways are open for the dissemination of the

* "A Treatise on the Continued Fever of Great Britain." By C. Murchison, M.D., LL.D.

active agent of propagation in a thousand unseen modes; so that it is obvious that the precise source of infection and its track must often baffle the wisdom of man to discover or trace out. Cases thus constantly arise which appear to give countenance to the belief that the disease has had a spontaneous origin—sporadic, as it is termed. Numerous cases of small-pox occur which can never be traced to their source, or to communication with persons similarly diseased; yet the history of small-pox is decisive against the notion of its spontaneous origin, and if of small-pox, so for all the other specific diseases of the same nature."

Now it appears to me that the same reasoning may be used against the occasional origin of puerperal fever, and against the spontaneous origin of erysipelas, phlebitis, or peritonitis, because puerperal fever is occasionally propagated, and because the specific virus of erysipelas, phlebitis, and the severe forms of peritonitis may be transmitted by inoculation, as in a dissecting wound. But this would be an evident absurdity; for we know that puerperal fever is sometimes orginated as well as propagated. And in the case of peritoneal inflammation we can actually trace the specific inflammatory product or virus, inoculation with which may produce disease (not, indeed, always characterized by the same symptoms as the case from which it has sprung, but which, doubtless, would be the same were the virus applied to the peritoneal membrane),* through all its stages of decreasing virulence, till we come to the innocuous exudation of slight peritoneal inflammation.

This objection, I think, can only be replied to by maintaining that puerperal fever and the other affections above named are not specific diseases, which few would be inclined to maintain; or by saying, "Yes, they are specific, but not specific in the same sense that enteric fever is specific; the specificity of the latter meaning that each case must spring from a foregoing one, after the manner of species in the animal kingdom."

But this is manifestly reasoning in a circle. To say that the term specific, applied to enteric fever, must necessarily include the idea of origin only by propagation, and that the same term applied to puerperal fever does not include this idea, is absurd. And if the

* See Lewis and Cunningham's "Experiments on Transferring Inflammatory Products from one Serous Cavity to another" (Indian Sanitary Reports, 1874).

idea of propagation must be included in the term specific, then to say of enteric fever, that because it is specific it must be due to propagation only, is the same as saying enteric fever must be due only to propagation, because, from the nature of the disease, propagation can be its only cause.

If origination *as well as* propagation be admitted of puerperal fever, &c., on what grounds can it be denied of enteric fever, to which it is so closely allied?

Nay, more; if, as generally admitted, the causes of puerperal fever are multiple, how can it be denied that the causes of enteric fever *may* also be multiple?

I believe that puerperal fever, erysipelas, and peritonitis, are much more closely allied generically to enteric fever than small-pox is.

This view of enteric fever being possibly due to both origination and propagation is frequently described (sometimes with some attempt at ridicule) as being illogical, and as if the view of occasional spontaneous origin necessarily contradicts the facts of occasional propagation, as in the following passage from Dr. Gueneau de Mussy:—"Convaincu de l'origine pythogénique de la fièvre typhoide, M. Murchison ne pouvait accepter qu'avec une certaine répugnance des faits que attestent d'une manière si péremptoire la transmission de la maladie par l'intermédiaire des malades, il a cherché à atténuer par des arguments et des interprétations auxquels son nom et son talent donnent une autorité considérable, les conclusions qui semblent sortir, avec une evidence qui s'impose, des observations que je biens de rapporter, pour établir la contagion et la spécificité de la fièvre typhoide. Cependent on seul qu'il est impressionné par le témoignage de ces faits qu'il ne peut pas éluder; son amour de la vérité lui impose de prudentes réserves, qui frisent la contradiction; la logique le porterait à repousser absolument la contagion; il le fait dans certains passages de son livre, et dans d'autres, en face des observations qu'on lui oppose, il semble moins absolu *en admettant*, dit-il, *que dans certaines circonstances, la fièvre enterique soit communicable, il est certain que, dans beaucoup de cas* elle a une origine indépendante. Je vais analyser toute cette argumentation remarquable, comme tout ce qui est sorti de la plume de cet éminent confrère, avant d'exposer la doctrine des contagionistes exclusifs à la tête desquels s'est placé le docteur W. Budd, et je discuterai ensuite cette opinion mixte, dont j'ai parlé plus haut, qui admet la possibilité d'une double origine; produit de miasmes crées par des conditions accidentelles, comme la putréfaction, la maladie pourrait revêtir un caractère spécifique et se transmettre d'un organisme malade à un organisme sain."*

Now, it is not a "*mixed opinion*" that typhoid fever is originated as well as propagated, any more than it is a mixed opinion that puerperal fever is originated as well as propagated; and a belief in occasional origination does not of logical necessity deny occasional propagation. Nor can I admit that Dr. Murchison's logic, "*le porterait à repousser absolument la contagion.*" Indeed, as regards the formal logic of the question, I think most logicians would say that the originists have the best of the argument.

Because, Murchison predicates of certain cases origination, and of certain other cases propagation; while Budd predicates propagation of *all* cases, *because some* are proved to have been propagated; this latter conclusion being clearly an infringement of a fundamental logical axiom, that while it is possible to argue from the whole to a part, it is not possible to argue from a part to the whole.

With great deference to the undoubted authority of Dr. Aitken, Dr. Budd, and the other celebrated English and continental physicians and etiologists who, with them, hold that every case of enteric fever must be propagated, and deny its occasional spontaneous origin, I must accept Dr. Murchison as my leader in this matter.

And principally for this reason: that I believe our knowledge is at present neither full enough nor precise enough to admit of dogmatism on this question; and I consider Budd's views the more dogmatic of the two.

Murchison (in effect) says:—"Those cases of enteric fever which we cannot trace to propagation may be originated." No dogmatizing here. Budd says (in effect):—"Those cases which we cannot trace to propagation are, nevertheless, propagated by my hypothetical germ, although I have no proof to offer." Strong dogmatism there. Note, too, the "*may be*" in the conclusions above quoted from Murchison, and the "*is*" in the conclusions quoted from Budd.

* "Recherches sur l'Etiologie et la Prophylaxie de la Fièvre Typhoide," par le Docteur Noel Geneau de Mussy (Paris, 1877), page 39.

Budd assumes in his premisses the very conclusion which he thinks those premisses prove.

But while I accept Dr. Murchison's views on the occasional spontaneous origin of enteric fever, I carry them much further than he does.

I object to the limitation implied by the term "*fermentation*" in his fourth conclusion; and also to that implied by the term "*decomposing excrement*" in his sixth conclusion; believing not only that enteric fever may be originated, but also that the originating causes of it may be much more multiplex than he lays down.

Since writing the first rough copy of the above, I observe, with satisfaction, that very much the same views have been advocated by a distinguished etiologist—Dr. Bouchard—in a paper read before the International Congress at Geneva, and published in the *Revue Mesuelle de Médecine et de la Chirurgie* for November, 1877. I have not yet had an opportunity of perusing the article, but the *Lancet* of December 29, 1877, in referring to it, says:—"After reviewing the three doctrines of spontaneity, infection and contagion, the author concludes that both infection and contagion are too exclusive, and that the doctrine of fæcal origin is too limited, whilst that of spontaneity is not proved. He holds rather that typhoid is a specific miasmatic fever; that the *materies morbi*, not necessarily arising from a previously infected organism, may impregnate the air, the soil, or water, and may be disseminated by man, by contaminated objects, by air, and especially by water taken as food."

As an anonymous writer stating views opposed to those generally held on the subject of the origin of enteric fever, I must here state that I am far from wishing to do so with any undue semblance of authority.

At the same time, I think it right that, after many years of careful study and personal investigation of the originating causes of enteric fever, I should state the conclusions to which I have been led, especially in this place, as these conclusions have a very important bearing on the question of the influence of drinking water in originating the disease. I will first state the conclusions at which I have arrived, and then give the facts on which they are founded—not that a theory was first formed, and afterwards facts manufactured or strained to fit it; but because it seems to me unreasonable first to bring forward a mass of evidence while the reader is in ignorance of what such evidence is intended to prove.

Affirmative theoretical evidence.

Years ago, as a student, my attention was arrested on reading the following case given by Mr. Erichsen in his "Science and Art of Surgery" (5th edit., vol. i. p. 152), under the head of *Wounds with Inoculation of Decomposing Animal Matter*:—"That the poisonous influence from the bodies of persons who have died of these diseases"—erysipelas, phlebitis, and peritonitis—"is transmissible to others by contact or infection cannot be denied. . . . . Thus I have known a body seriously infect in different ways six students who were working at it. Two had suppuration of the areolar tissue, under the pectorals and in the axilla; one was seized with a kind of maniacal delirium; a fourth had typhoid fever; and the remaining two were seriously, though not dangerously, indisposed."

Such cases as the above are necessarily, from their nature, rare; yet they are on record, clear and well authenticated. So far as they go they prove two things:—

(1) That enteric fever is actually produced by at least two causes—namely, the virus of decomposing animal matter, and (as we know from the ordinary facts of the propagation of the disease) the specific virus of contagion.

(2) That enteric fever may be in some cases, but certainly not in all, the result of a specific virus—that infection from decomposing animal matters may produce enteric fever, and at the same time other affections as well as enteric fever.

A logical corollary from this last is, that the virus of enteric fever contagion may produce a variety of other affections; and even this very heterodox doctrine I do not mean to deny.

It will now be seen why I devoted so much space, in the Preface to this Essay, to enunciating and endeavouring to prove the proposition that *specificity of a disease does not necessarily imply specificity of its cause*. It is the process which is specific, not the cause. The specific process which will result is inaugurated, or developed, or determined, not so much by the nature of the cause as by the accidental peculiarities of the conditions under which that cause acts. I believe, then, that enteric fever is a form of putrefactive impregnation of the system, or one of the results of putrefactive impregnation.* I will be asked—Why does putrefactive impregnation of the system not always produce the specific process known as enteric fever, and leave behind it the specific anatomical sign of that process?

I answer, for the same reason that pyæmia sometimes results in a lesion of the lungs, sometimes a lesion of the liver, at other times of the areolar structures, at others of the serous membranes. For the same reason that in some cases a cold acts on the Schneiderian membranes, in others on the bronchial and in others on the intestinal membrane. For the same reason that a drug which, under certain conditions, acts as a diaphoretic, under other conditions acts as a diuretic. Because it is the accidental conditions under which the agent acts, varying in and peculiar to each individual case, which determine in what manner, and then in what organs, the results of its action are to be evidenced.

In those cases in which putrefactive impregnation results in enteric fever it is certain conditions of the organism which determine this result. What are these conditions? Hitherto I have only stated the *pythogenetic* doctrine of Murchison. I now go farther, and state what I believe those determining conditions to be, namely—

(1) Hepatic insufficiency.

(2) An abnormal functional activity of the intestinal glands, consequent on and vicarious or supplemental to this hepatic insufficiency. And

(3) An idiosyncranal proneness of these glands to assume this abnormal function.

Dr. Sander, in his "Handbook of Public Health," recently published, states that the only diseases proved to be due to putrefactive impregnation of the system are septicæmia and diphtheria. But what does septicæmia mean in the present day? Is it to be restricted in its meaning as it was in former years; or are we

* Some years ago, Prof. v. Gietl suggested that enteric fever may be a form of (so-called) "putrid intoxication;" and formally stated this opinion at the debate on enteric fever at the Medical Society of Munich in 1872.

justified in using the term as a designation of a *genus* of disease; and if so, may we not also be justified in classing enteric fever as a *variety* under that genus?* I believe we are justified in doing this; and I proceed to state some of the observations on which this opinion is based. These may be divided as—(*a*) clinical observations; (*b*) statistical observations; (*c*) observations on the determining conditions of enteric fever; and (*d*) necrological observations.

(*a*) The most important of the clinical observations on which this view of the nature of enteric fever is based refer to the condition of the urine and the body temperature in this disease. The first of these are certain valuable and interesting contributions to the urinology of enteric fever—chiefly the work of French observers; particularly MM. Gubler, Jaccond, Parrot, and Robin—which will be found described by the latter in his work on the subject,† the importance of which, when taken with the other observations referred to, cannot fail to be perceived. The chief of them are briefly as follows:—*Urea.*—A careful consideration of the discrepancy in the statements of writers as to the amount of urea eliminated in enteric fever, renders it probable that variations in the amount of nutriment ingested account to some extent for this diversity of results. It was previously taught (chiefly on the authority of Becquerel and Anstie) that there existed a direct relation between the mildness of the attack and the smallness of the quantity of urea eliminated. It seems, however, that a careful repetition of their experiments reverses this conclusion; at least, it appears from the observations of the above-named French writers, and Sigmund and Vogel in Germany, that the proportion of urea is increased during the early stage; but that, as the disease advances, the amount of urea falls below the normal standard of health, and, according to Robin, there is in fatal cases an excessive diminution in its amount.

Now the observations of Cloetta, Meissner and Bullard, Stokvis, Cyon and others, show that urea is formed by the liver. The experiments of E. Cyon, particularly, demonstrate indubitably

* Genus, *Septicæmiæ;* families, *Typhoideæ*, &c. &c.; varieties (under Typhoideæ), *Enteric Fever*, &c.

† "Essai d'Urologie Clinique; la Fièvre Typhoide." Par Albert Robin, Ancien Interne des Hôpitaux de Paris, Chef des Travaux Chimiques au Laboratoire de la Charité. Paris, 1877.

its formation in this gland. He examined carefully the blood of the portal and hepatic veins, and found that blood, after passing through the liver, contained more urea than it did before entering that organ; and his experiments also demonstrated that blood which was three or four times passed through the liver contained more urea than that passed through but once. Tending to the same conclusion are the observations of Frerichs and Murchison, who have shown that in acute yellow atrophy of the liver, in which the hepatic cells are destroyed, there is a great diminution in the quantity of urea eliminated, and in some cases there is absolutely none eliminated. The excess of urea, therefore, in the early stage of enteric fever points to exalted functional activity of this viscus; and its diminution later on indicates insufficiency of the hepatic cells to perform the increased work.

Albumen.—The occasional occurrence of albumen in the urine at the height of enteric fever (in 33·3 cases per cent. according to Parkes) has been long known; but Gubler insists on its constant presence in cases of any severity.* This may have been expected, because, owing to the incompetence of the liver to further eliminate urea, the albuminous waste of the tissues (which is increased by the fever) is eliminated by the kidneys as albumen and albuminose.

Colouring Matters.—*Urochrome.*—"The pigment, *at first*, is enormously increased, measured after Vogel's method (by comparison with a scale of colours). It has sometimes amounted to 80 or 100 in twenty-four hours, the normal amount being three to six (Vogel). This, Dr. Parkes says, is to be referred to increased disintegration of blood cells. It is, therefore, much more highly coloured than the mere concentration will account for" (Aitken). Urochrome is proved by recent experiments to be a product of the combustion of hæmoglobulin, and is said to occur in the urine in direct proportion to the activity of the liver and spleen, being increased in quantity when the functional activity of these glands is accelerated, and when there is rapid destruction of the corpuscular elements of the blood. Thus the excessive colora-

* Gubler states that albumen cannot always be detected by the usual tests, but maintains that its constant presence may be demonstrated by filling a glass vessel three-fourths full of the urine of a patient suffering from enteric fever (at the height of the disease), and gently adding nitric acid, until the latter occupies the lower two-fifths of the vessel; when, at the surface of contact, a layer of albumen is always seen, together with a diaphragm of a blue colour.

tion of the urine in the early stage of enteric fever indicates increased hepatic function. Hemapheïne and uroërythrine are present in enteric fever urine at the height of the disease, when the attack is severe. These substances are stated by Gubler to indicate hepatic insufficiency; this insufficiency arising from increased work for the liver when the destruction of the red globules is in excess of its transforming power, or when, from disease, the activity of the gland is diminished, and it is thus unable to perform its normal amount of work. We may hence conclude that one of these conditions exists in the maturity of enteric fever—most probably the former, because on examination of enteric fever cases after death no signs of organic hepatic disease are found.

It is unnecessary to dwell further on the elaborate analysis of enteric fever urine alluded to above; the few points here noted show that they indicate that an intimate connection exists between enteric fever and disturbance of the hepatic function, the other results of these analyses bearing out this conclusion. The whole indicate clearly that the condition of enteric fever is an engorgement of the tissues with effete material, resulting from failure of the lymphatic system to carry on an increased amount of work.

These conditions of the urinary secretion, occurring, as they do, uniformly at certain fixed stages of the enteric fever process, indicate that in this process a series of sequential phenomena occurs somewhat as follows:—The initial disturbance seems to be an excess of work thrown on the hepatic system. At first this demand is replied to by an increased functional activity of the liver; but as the demand on its activity still increases, the liver is unable to come up to time (so to speak), and breaks down (functionally) under the pressure of work.

We find that, coincidently with these conditions, the intestinal glands take on an abnormal functional activity. Can this new and abnormal function of the intestinal glands be vicarious, or supplemental to or consequent on the hepatic functional insufficiency?

The facts above stated, taken with others which will follow, appear to me to yield some probability of this being the case. Indeed, these points on the relationship between hepatic activity and enteric fever, taken with certain statistical facts, seem to show that exposure to an intensified temperature produces, under one set of conditions, enteric fever; under another, *hepatitis*. It would almost seem as if enteric fever were a kind of safety-valve, preventing serious organic lesion of the liver.

The body temperature in enteric fever indicates the different stages of the disease no less definitely than the urine analysis. At the end of the third or beginning of the fourth week the difference in the *kind* of temperature variation is well marked. This is the period when, according to the urine analysis the hepatic insufficiency occurs; and the great differences between the morning and evening temperatures, and altogether the general likeness of the temperature of the fourth week of a severe case of enteric fever, and the milder forms of pyæmia—not the traumatic form of the disease, but *the chronic pyæmia* of Aitken —taken with other clinical similarities of these diseases, cannot fail to suggest an etiological relationship between them. During the first three weeks we have the temperature of febrile disturbance, consequent on blood poisoning; and immediately following comes the peculiar temperature variation so characteristic of local suppurative inflammations, marking enteric fever as a suppurative enteritis—as closely allied to the family *Metastatical Dyscrasiæ*, in which the blood is altered systemically, becoming abnormally coagulable by the action of putrid animal substances. "These substances may act in the form of gases, fluids, or solid particles, which so disturb its relations with the living tissues as to induce coagulation of the fibrin of the blood, in some part, during life, within the blood-vessels. These changes are associated with fever, and the formation of local abscesses in one or more of the viscera and other parts" (Aitken). I shall subsequently describe why, in enteric fever, the intestinal glandular structures are peculiarly prone to being selected as the seat of such lesion (p. 215).

There are other clinical observations bearing on the causal relationship of enteric fever and *the septicæmiæ;* such as the detection of gas in the veins at the root of the neck, in severe cases of the former disease (Closs, Frank, and Jeffery Marston, in *Medical Times*, February 7, 1857, quoted by Professor Aitken), &c., but I am unable to devote further space to their consideration.

(*b*) We know from statistical returns that enteric fever is most prevalent and most fatal in hot weather, especially autumn, in northern climates, and spring in southern;* and from the same sources we gather that in the "tropics the chief cause of it seems to be the prolonged exposure of an alien and unripe constitution to an intensified and unaccustomed temperature" (Report by Surgeon-General Ker-Innes, C.B. See Appendix, p. 298).

Now we know from practice that excessive climatic heat produces in some way derangement of hepatic function.

It is true we cannot satisfactorily explain the *rationale* of this effect. We know nothing of the condition of the hepatic function under these circumstances. The quantity of bile secreted is not changed if the stools be taken as a guide (Marshall, Davy, Morehead, Parkes). We only know that an excess of colouring matter passes with the stools (Lawson). Nevertheless, it cannot be denied, in the face of large experience corroborating the fact, that hot weather and residence in tropical climates in some way affect liver function. We find from Indian statistics that enteric fever is especially prevalent among young soldiers during their first three years of Indian service, and we find that after that period the effect of climate is principally evidenced on the liver function, in the form of digestive derangements, hepatitis, and liver abscess. This is of importance when taken with the observations next following.

(*c*) I have examined the medical history sheets of many men who have suffered from enteric fever, and I have never found any functional liver disease recorded in them prior to the attack of enteric fever. I only take this evidence *quantum valeat*, for necessarily the experience of one individual on such a matter must be narrow.

* Deaths from Typhoid Fever in	1868.	1869.	1870.	Total.
January	1	1	...	2
February	1	3	4	8
March	...	...	6	6
April	1	2	6	9
May	4	5	18	27
June	14	4	4	22
July	2	3	5	10
August	6	3	5	14
September	4	3	5	12
October	1	2	4	7
November	...	1	...	1
December	1	...	5	6

(*Bryden's Vital Statistics.*)

Yet I consider it remarkable that in many cases of enteric fever I have observed, no liver disease was found in the previous medical histories. I have lately gone over the sheets of two regiments of infantry, two batteries of artillery, and of a convalescent depôt, and found that in twelve medical history sheets enteric fever was recorded, and that in none of these twelve was there any record of previous hepatic disease. I had before noticed the same in many instances, but had not kept any record in figures. It may perhaps be said that this is naturally to be expected, because, as enteric fever is a disease of early Indian service, it would be most likely to appear on medical history sheets before any entry of hepatitis. This may be a plausible theoretical explanation as regards the *hepatitis of deterioration* (Bryden), which is observed in those who have served long in India; but it can hardly hold in the face of the invaliding rates in first and second years of Indian service, as given in Dr. Bryden's "Vital Statistics."

According to this report, the average of *fevers* in first years of service is 1·66 invalided per 1,000, in the second 2·26; while the rates of invaliding for hepatitis are in excess of this, being for first year of Indian service 3·38, for second year 3·53. The above figures relate to all *fevers*; so that, if enteric fever be eliminated, the hepatitis of first and second years of Indian service would show largely in excess of it; so that this is no explanation.

If, when investigated on a large scale, my observation were found to be true, that enteric fever occurs *only* in those who have *not* suffered from hepatitis, I think the only explanation of it is to suppose that enteric fever is caused by influences which would cause hepatitis, but that, under certain conditions, enteric fever supervenes, and prevents the occurrence of hepatic results. That enteric fever occurs only in cases where the liver has not become capable of the increased function demanded of it, by residence in tropical climates, or during the hot seasons of northern latitudes.

(*d*) The *post-mortem appearances* in enteric fever also resemble those which result from putridinous poisoning. It is known that in septicæmia no very distinctive lesions are found. There is not, ordinarily, evidence of extensive local or multiple suppuration. The spleen and liver are slightly enlarged and engorged with dark coloured blood. The pulmonary tissue is usually infiltrated with blood, and in prolonged cases there is evidence of pneumonia or pleurisy. These appearances are also those of enteric fever, *with* *this addition*, that there are in this disease signs of an attempt at elimination of the disease cause by the intestine.

Why this tract should be the seat of the special anatomical sign of this disease, and so frequently accompanied by pulmonary lesion, incipient or developed, has been explained by experiment. Whether we accept Schmidt's theory of coagulation, which is the most favoured in this country, or that of Denis, we find that the experiments of these physiologists show that the selection of the gastro-intestinal mucous membrane for the elimination of septic poison is not *specific*, for we may explain it by the difference between the arterial and venous pressures being less in the parts which send their blood to the portal vein than anywhere else, except in the lungs. It has been shown, clinically as well as experimentally, that in the intestine capillary embolism occurs with great facility, and the stoppage of a single artery is capable of producing a localized gastro-enteritis, with all its accompaniments of capillary congestion, stasis, exudation, and shedding of epithelium (see Sanderson's "Handbook for the Physiological Laboratory," p. 166; also his recent lectures on the "Intimate Action of a Septic Poison").

This view of enteric fever being, in the first place, due to hepatic insufficiency, arising in some cases (*originated*) from the absorption of putridinous matters, and in others (*propagated*) from absorption of matter in which the enteric fever process is already inaugurated, seems to me to explain at once the likeness and the difference between this disease and that other form of fever—the "typho-malarial" of Prof. Maclean.

The processes are of the same nature; but one is caused by putridity, and results in a localized suppuration, the anatomical sign of the enteric fever process; while in the other, malaria is, *in addition*, a prominent factor in causation, and it is accordingly characterized by the special lesions peculiar to that agency—as, for example, the hyperæmia and softening of the upper part of the small intestine.* The first is propagable, because its characteristic suppuration results in an elimination of bioplasm, in which its specific process is actually going on, and which is capable of communicating that process to the bioplasm of healthy blood; the latter is not propagable, because no such shedding of living germinal matter occurs.

* See note on page 300 of Appendix.

Now the hepatic insufficiency which, according to this view, we suppose to be the first stage of the enteric fever process, may be brought about by (*a*) idiopathic individual causes; (*b*) the absorption of putrid matters into the blood; and (*c*) the absorption into the blood of the fully developed virulent pus, which is the product of and potentially carries the enteric fever process from a foregoing case of the same.

Among the first may be classed climatic influences, such as excessive heat, sudden vicissitudes of temperature, malaria, &c., and probably other conditions of which we are in ignorance, which produce a *degradation*, or special modification of the vital processes in the bioplasm of the blood.

The third contains only the effects of contagion propagation, which we will consider in the following chapter.

The second includes cases of origination by the absorption of putrid matters in food and drink.

Milk and water seem to be the most frequent vehicles of the cause of enteric fever. In many cases in which the disease appears to have been caused by milk, it has been proved that contagion was the cause; but some recent experiments of Professor Lister on the butyric fermentation, suggest that, under certain circumstances, milk may be an originating cause of the inauguration of the enteric fever process; quite apart from its being, as no doubt it sometimes is, a carrier of contagion.

As to water, which is the immediate subject of this inquiry, we see from the view of the nature of the enteric fever process which I have here at some length elaborated, that theoretically, from the nature of the disease, it is probably a very common vehicle of the originating cause of enteric fever; because it is pre-eminently the source from which putridinous matters are, under ordinary circumstances, absorbed into the blood.

If we take this view of the nature of the disease, and assume that, when it arises independently of propagation from a foregoing case, it is originated by the ingestion of putrid matters principally through the medium of impure drinking water, we have at once a very fair and complete explanation of most, if not all, the phenomena of enteric fever; at least this theory seems to account for more of the phenomena than any other.

That the first step in the enteric fever process is an absorption of putrid matters, and their circulation in the venous blood; the partial and temporary elimination of this septic poison by the lungs, its subsequent presence in the arterial blood when its amount becomes too great for elimination by the lungs; then the strain on the lymphatic system to eliminate the poison, and its failure to do so (this failure being the inauguration or determiner of the specific enteric fever process); then its elimination by the glands of the intestinal tract in the form of a suppurative enteritis; and the ultimate elimination here of the septic poison in a virulent form, or as bioplasm endowed with a special modification of the vital process, and capable of communicating this specific process to healthy blood bioplasm, explains, I think, most satisfactorily (1) the occasional (spontaneous) origination of enteric fever; (2) its occasional pythogenetic origin, as insisted on by Murchison; (3) its origination as a result of increased temperature* (Innes); (4) its occurrenee chiefly during the age of functional activity of the intestinal glands; (5) its comparative rarity after the age at which these glands are known to atrophy; (6) the unmistakable symptoms of hepatic and splenic insufficiency which accompany it; (7) the true first stage of it, which is only an apparent period of incubation (see page 225); (8) the occasional observance of bacteria in enteric fever blood, this being a result of the septic transformation of nitrogenous matter (Burdon-Sanderson, Privy Council Reports for 1874); and (9) its occasional propagation by contagion, which is the original poison, or the patient's bioplasm, in which the septic transformation is developed, and which is capable of communicating the same process to a healthy organism.

Affirmative experimental evidence.

The evidence under this heading is of two kinds—namely, that in favour of the occasionai *de novo* origin or protogenesis of enteric fever, and that which goes to establish drinking water as an originating cause. The first is an essential part of the second, and in itself affords some probability of drinking water being an agent in the origination of the disease. A large mass of evidence exists in the form of observations of enteric fever, chiefly in the civil section of the community, in favour of its protogenesis which are to be found in the writings of Murchison, Gauthier, Britanneau, Gendron, Ragaine, and others. To these observations, however, I cannot at length refer in the limited space of such an Essay as the present. I shall confine my remarks to

* And hence its rarity in natives of tropical climates.

the military view of the subject, referring only to the observations on enteric fever as it has occurred in our army during the past four years, in the United Kingdom and in India, as recorded in official reports; and to my own experience of the disease.

The records of these four years (1873–76) furnish a very strong practical argument in favour of the protogenesis of enteric fever, in the unlocalized character of the outbreaks. The disease was widely scattered over all the stations of the army.

It appeared in nearly all the groups of stations in the United Kingdom; and many of the cases, particularly in India, were absolutely solitary.

And even when a considerable number of admissions occurred at one station, this apparently was not owing to propagation of the disease by intercourse—as, for example, in the epidemic (?) of Dublin in 1873. "Here eighteen admissions for enteric fever occurred in an average strength of 4,608 men; though more numerous in the first and last quarters of the year, in no quarter were admissions absent in the returns. The cases were received from ten different regiments, occupying five different barracks, placed widely apart, and dissimilar in the more apparent conditions likely to influence health." The same unlocalized character of the outbreaks was very remarkable in the United Kindom in 1874. "Admissions due to enteric fever are returned from twenty-six different stations; eight of which returned only one each, and eleven only two each."

In 1875 half the admissions and more than half the deaths due to enteric fever occurred in Ireland, and are to be attributed to an outbreak at Kinsale.

But even this outbreak was so scattered in its cases, and these were so manifestly independent of ordinary propagating influences, that it is described in the Blue Book Report for that year as a "short and well-defined house epidemic." The last Report, that for 1876, brings the same tidings. "No district was quite free from this form of continued fever;" and again, "No epidemic outbreak occurred at any station, though in a few instances two or three attacks followed in sequence."

As regards India, the same is to be observed. The Sanitary Report for 1873 says:—"Of ninety-three stations occupied by European troops, it (enteric fever) appeared in forty-four. In many instances the cases were solitary."

In 1874, "of fifty-nine stations in Bengal, twenty-eight furnished cases. In Madras and Bombay they are returned from fifteen out of thirty-five stations, and many of the cantonments show only one or two cases in each; but in others—especially at Barrackpore, Hazarebagh, Dinapore, Fysabad, Lucknow, Meerut, Saugor and Peshawur—they were more numerous."

The same is to be found from the Reports of 1875 and 1876. Now this practical evidence seems to do away with whatever objections may be urged against the theory of the water origin of enteric fever, on the grounds of its being only caused by propagation, and never by origination or protogenesis.

As regards *the influence of drinking water* in originating enteric fever, the evidence from experience is obscure. In proof of this it is sufficient to point to the discrepancy in the conclusions arrived at in the Army Medical Department Report for 1873, and those published in the Report for 1874.

In the former year "the limited character of the outbreaks" (it being presumed that the same water was generally used) "seems to point to some more restricted cause" (p. 135). In the Report for 1874 the following remark occurs:—"Overwhelming evidence exists to show that it has its origin in the vitiation of air or water by excremental pollution" (p. 123). If vitiated water be not a sufficiently restricted cause, surely vitiated air is much less restricted. It is to be regretted that these Reports lose so much of their value from want of accuracy in using the words *origin* and *propagation*. In some places origin is plainly used to mean propagation, as in the phrase "*origin by importation*," and the correctness of its use in the extract last quoted is certainly open to suspicion.

When we come to consider the individual reports of medical officers on the cause of enteric fever, we find that the most noticeable feature of them is their reticence with regard to the source of origin of this disease. This reserve would seem to show that, in many cases at least, conflicting evidence rendered it difficult to arrive at definite conclusions.

In some cases, however, an opinion founded on experience is deliberately stated in favour of the impure water origin of the disease; as in the following:—

The Army Medical Department Report for 1873 states (p. 135) with reference to Bengal: "The reports are generally silent as to the causes of the occurrence of enteric fever in the corps affected in 1873; but the medical officer of the 1st Battalion 11th Foot attributes the outbreak of the disease in that regiment at Subathoo to the use of impure water coming from the slopes and hollows of a neighbouring hill, collected and stored in tanks, and in them receiving polluting matters of a very offensive kind; but the Report does not contain evidence that the water was polluted by specific infecting matter—the discharges of persons suffering from enteric fever." This is, therefore, a statement referring to true origin.

Then, again, the Report of 1874, referring to enteric fever as it occurred during that year in the United Kingdom, states (page 10): "The third outbreak occurred in the military prison in Limerick in December, but the first admission of the series was in the previous month. The medical officer attributes the outbreak to the use of unwholesome water, which, when analysed in 1873, was found to contain 5·3 grains of organic matter and 2·4 grains of chlorides in each gallon; the pipe conveying it was corroded into holes where it passed beneath the wash-house surface drain. On the use of this water being discontinued there were no more admissions from enteric fever." Scattered up and down through medical literature we often find the same experience reported. For example, in the *Indian Medical Gazette* for March 1, 1876, Surgeon O'Brien reports the details of an outbreak of enteric fever in the 43rd Assam Light Infantry at Gowhatty, an unhealthy town in Lower Assam. He says: "The origin of the disease in the first two cases was distinctly traced to the use of the water of a tank in the middle of the Sepoys' lines. The water of this tank was so bad (I believe it has since been closed up) that a sentry had to be placed on its banks to forbid its use. Notwithstanding the prohibition, it was found that these two men, Jerwah recruits, had been in the habit of using the water." Of course, the value of such observations depends on what their authors mean by *distinctly traced*, and such expressions; but they seem to be conclusive to the minds of some, and are therefore worthy of being taken in evidence *quantum valeat.* Many cases of the same kind can be culled from various sources. There remains another argument from experience in favour of the water origin of enteric fever, to which importance is given by some—namely, *the endemicity of this disease*—its localization at certain stations, and the connection assumed to exist between this and water supply. I am aware that some accurate observers (and among them, I believe, is Dr. Bryden) deny the endemicity of enteric fever, saying that this is only apparent. I cannot now discuss this question; but those who believe in the true endemicity of this disease found thereon an argument in favour of its water origin, which it is right to consider here. I quote an example. As an illustration of endemicity of enteric fever, whether it be real or apparent, the station of Hazarebagh may be taken. Of late years this station has become notorious because of the frequent and fatal outbreaks of enteric fever which occur among the British troops cantoned there. But, curiously, this station may be said, speaking generally, to be a healthy station, so far as the more apparent influences of climate are concerned. It would seem to be healthily situated, being on the uplands of the Chota Nagpore plateau, at an elevation of 2014 feet above the sea-level, with an average temperature of 74° (F.), ranging between 61° in December and January and 86° in June; a rainfall of 52 inches, and a mean humidity of 42. There is abundant evidence, both general and statistical, of its being otherwise a healthy station.

A comparison is given in the following table of the health of the European army of the Bengal Presidency and of the troops living in Hazarebagh during the ten-year period 1860-69, showing the ratio per 1,000 of strength of the admissions, daily sick, and deaths:—

Admission Rate.		Daily Sick Rate.		Death Rate.	
Presidency.	Hazarebagh.	Presidency.	Hazarebagh.	Presidency.	Hazarebagh.
1754·9	1535·7	67·1	70·5	29·98	19·15

And the same averages for the three succeeding years are as follows:—

Years.	Admission Rate.		Daily Sick Rate.		Death Rate.	
	Presidency.	Hazarebagh.	Presidency.	Hazarebagh.	Presidency.	Hazarebagh.*
1870	1731·9	877·1	63·8	32·1	21·90	19·00
1871	1449·6	1384·4	57·9	60·9	17·53	16·91
1872	1514·5	1118·3	50·8	45·5	27·45	6·83

When Dr. Mouat recently endeavoured to show that the climatic risks to European life in India do not much exceed those which affect longevity in Europe, one of the strongest illustrations which he put forward in support of his argument was drawn from the health of the Hazarebagh community. Thus the more ordinary and apparent of the influences which affect health must be excluded in considering the cause of enteric fever here. The British regiments stationed at Hazarebagh suffer excessively from enteric fever, while the civilians residing there do not suffer much from this disease. Thus the topographical position of the place cannot be the cause of the outbreaks, nor vitiated air; and we thus go on excluding influence after influence until we come to the water.

Then the possibility suggests itself, that in the drinking water we have an agent acting on the military section of the community and not on the civil, as the source of the drinking water is in the two cases different. But again, examination fails to detect any serious impurity in the cantonment supply. The facts, at all events, point very strongly to a localized cause, and some would argue that, other influences acting equally on soldiers and civilians, the cause of this disease exclusively in the one must be due to the influence of the drinking water.

As regards *my own experience* of the origin of enteric fever, I have met with only two cases which were at all apparently due to drinking water. One of these occurred in the case of a gentleman on his return from shooting ducks on a bheel in India. He had been standing four or five hours in the swampy margin of the bheel, which was full of decaying organic matter. A little of the clearer looking water was drunk, which, however, was certain to contain a large amount of dissolved organic impurity. The day after, he was attacked with a fever, which proved fatal, and which I have no doubt, from the symptoms, was no other than true enteric, although the diagnosis was not verified by post-mortem examination. Seemingly there was hardly any period of incubation in the case, if this was the true cause, and no other could be discovered. The other case occurred in a soldier, and was the first of an outbreak. No quantitative analysis of the drinking water was made, but it evidently contained much organic impurity when examined with Fauré's test, alcoholic solution of gallic acid. On experience so slight as this I refrain from drawing any conclusion.

Negative theoretical evidence.

The first and most general theoretical argument against the origin of enteric fever by drinking water is that against the possibility of its origination by *any* cause founded on the fact of *its occasional undoubted propagation by contagion,* and the assumption that a specific process like that of enteric fever cannot be caused by more than one agent—that is, contagion. Dr. Budd quotes the opinion of Sir Thomas Watson in saying that if this disease be proved to be once due to propagation by contagion, we cannot help entertaining a doubt of its ever being originated *de novo.* Many distinguished scientific physicians at the present time hold this view. It is, however, not a little remarkable that many of these etiologists do not appear to experience any difficulty in accepting the doctrine of the protogenesis of other specific diseases, which are equally with enteric fever proved to be also sometimes propagated by contagion.

For example, some who on this ground deny the possibility of origin of enteric fever seem, as far as can be judged from their writings, to accept the *first conclusion* regarding cholera of the International Sanitary Conference at Vienna in 1874—namely, *cholera arises spontaneously only in India, and reaches other countries from without.*

Another argument which comes under this head, and which Dr. Budd refers to in his "eleventh conclusion" (quoted on page 201) "as the facts relating to the geographical distribution, past and present, of this and the other contagious fevers," is generally put forward as strong evidence against the protogenesis of enteric fever.

This argument appears hardly so strong as some seem to think; because *geographical distribution* must mean either the time in relation to locality at which the disease first appeared, or its present topographical distribution. Evidence as to the time at which enteric fever first appeared in any one country is not easily obtained with any accuracy, because it is only since the publication of Louis's treatise, in 1829, that this has been recognized as a distinct disease, and it is only about thirty years that this view has been generally adopted. As regards India, we know from statistics that, to found any opinion on the period at which enteric fever was first recognized there, would lead to a most obvious fallacy. "Dr. Bryden has discussed this question in his papers on the influence of age and length of service, and seems to have shown satisfactorily that it has been known for many years, although it is only of late that it has been generally recognized. Statistical returns of the total death rate ascribed to fevers now-a-days, when enteric fever forms so important an element in the mortality of the British soldier in India, compared with those of previous years, when all fevers were classed under the heads of intermittent, remittent, and continued, show that the death-rate for all fevers has not increased, and that in 1873 it was lower than it has been for ten years. Were enteric fever a new disease, we should naturally expect to see a considerable increase in the mortality from fevers, even after making allowance for any reduction in the loss occasioned by malarious fevers due to the milder forms of malarious disease now met with, and the more successful mode of treating them ("Report of Sanitary Commissioner for India," 1873, page 21).

This must be the meaning of *geographical distribution* as applied to enteric fever, because in the second meaning of the phrase—the topography of recent outbreaks—in this sense enteric fever has *no* geographical distribution peculiar to it. This is a well-established fact by observation both in the United Kingdom and India.

Dr. Bryden remarks in his "Report on Age and Length of Service as affecting the Sickness, Mortality, and Invaliding of the European Army in India" (page 53): "Let me repeat, in concluding this most important subject—important alike to the sanitary officer and to the student of the etiology of typhoid—that I know of no single circumstance that would suggest to me that the type of the fever of which I am speaking is determined by local causes.

"*Typhoid has no geography*, and is of universal occurrence; taking, for example, the deaths recorded in 1869 as they stand in our death-roll, the twenty-seven deaths are returned from twenty-one stations."

Dr. Maclagan and others attach much importance to there being a *period of incubation* in enteric fever, as an argument going to prove that this disease is brought about solely by contagion, and is never originated by drinking water or any other cause.

I think it is very questionable whether there is any incubation period in enteric fever, but it is very certain that there is no *definite* period of incubation.

The indefiniteness of such a period is admitted on all sides, and

Dr. Budd makes the contagion theory account for such indefiniteness in this manner :—

"There is reason to believe, however, that the duration of the period of incubation varies considerably, partly with the nature of the medium through which the specific poison finds admission to the living body, partly by reason of the conditions it meets with there, and still more so in virtue of the greater or less intensity of the state of change in which the poison itself may be at the moment of its reception."

Now, it appears to me that there can be no doubt that the pythogenetic theory of Murchison accounts for this in a much simpler way. Murchison's *sixth conclusion* (quoted on page 202) is, "an outbreak of enteric fever implies poisoning of air, drinking water, or other ingesta, with decomposing excrement." Now, Bernard's experiments on the results of injecting the volatile products of putrefaction into the systemic veins, prove that such an indefinite period between the entrance of the poison assumed by Murchison, and the exhibition of its results, is absolutely necessary. In other words, that the indefinite incubation period is in itself, so far as it goes, a strong piece of evidence in favour of the pythogenetic theory of enteric fever—of its being originated by drinking water containing putrescent organic matter.

Bernard has pointed out that the poisonous effects of such putridinous matters "are produced only when the toxic agent is brought into direct contact with the tissue elements by the arterial blood. Now, when a very volatile gas, such as sulphuretted hydrogen, is introduced into the systemic veins, whether by injection or osmosis, it will never enter the arterial system so long as the rate of its introduction does not exceed that of its elimination by the lungs. It is only when the gas enters the blood faster than it can be got rid of in the expired air that poisonous symptoms ensue proportional to the amount of gas which penetrates into the left heart."*

In some cases this occurs sooner than in others, and (may we say *consequently?*) in some cases the specific process of enteric fever is set up sooner after the absorption of putrescence than in others; that is, the incubative (?) period is indefinite.

Another argument against the origin by putrescence of enteric

* *Brit. and For. Med. Chir. Review*, 1877, page 295.

fever, and therefore against its origin by drinking water, is supposed to follow from the fact that *this form of fever is peculiar to man.*

Animals having been fed for long periods (three months and more) on the alvine evacuations of enteric fever patients, and this strange addition to their usual food not producing any derangement of their health, it is supposed to follow that the agent producing the enteric fever process must be formed in and peculiar to the human organism. But it would seem that this is equally applicable to other diseases which are undoubtedly produced by agencies which act and are developed external to the human organism.

And if we accept this conclusion, I think we must also believe that, because a certain quantity of the ipecacuanha root will produce emesis in a man, and that the same amount of the same substance may be administered to one of the lower animals in its food every day for three months without producing an emetic effect, therefore ipecacuanha root is a substance developed in and peculiar to the tissues or fluids of the human organism. And the same in many other instances.

Another theoretical argument against the protogenesis of enteric fever by drinking water, or any other cause (and the last to which I can devote space), is based on the *exhaustion of susceptibility* to enteric fever. It must be admitted that true exhaustion of susceptibility to a disease yields a strong probability of such disease being truly specific—that is, specific as to its process, but not necessarily so as to its cause In the case, however, of enteric fever, there are grounds for doubting that exhaustion of susceptibility can be counted among the phenomena of the natural history of this disease. It would seem to be *apparent only*, as an incubation period in this disease is *apparent only*. And neither the one nor the other can be taken as evidence against the pythogenetic theory of the etiology of enteric fever.

The pathognomonic lesion of the intestine is a *sine quâ non* of enteric fever—in fact, it is this lesion of the intestinal glands which constitutes the disease. But the function of these glands on which the possibility of the occurrence of the disease (as a recognized definite entity) depends, is destroyed by this disease process. "The mucous membrane of the intestines having existed for several weeks in the state of irritation which has been described, and the catarrh being more or less excessive, *an atrophic condition of the intestine at last supervenes.* The mucous tubes become wasted, irregular in form and size, as seen on microscopic examination, sometimes separated by an interstitial growth of a granular nature, and their bulbous ends disappear. The whole substance of the gut then becomes so thin that it resembles a portion of thin paper rather than intestine" (Aitken).

Here, then, we have a very good reason why persons are not attacked by enteric fever a second time.

"The disease having once occurred, the patient is protected from a second attack" (Aitken). Not, perhaps, necessarily in the way usually understood by the word "*protected,*" but probably because the glands whose function is necessary for the inauguration of this special disease process have been destroyed by a first attack; and thus a first renders a second attack impossible, in a manner capable of a satisfactory practical explanation, without the hypothesis of exhaustion of susceptibility, using up of "*the second factor,*" &c. It is to be noticed that all the theoretical evidence against the origination of enteric fever through the influence of drinking water comes in the form of evidence against the possibility of its protogenesis *by any agent whatsoever.* But if the possibility of the protogenesis of this disease be admitted, then there exist no theoretical grounds on which the possibility of its being originated by drinking water can be denied.

Negative experimental evidence.

The great practical fact which seems to forbid a general acceptance of the doctrine that enteric fever is originated by the influence of drinking water is, that careful investigation has failed to find any connection between the occurrence of isolated cases of this disease, or the first cases of outbreak, and impurity of drinking water. I am still keeping to the medico-military view of the subject.

For some years past a special inquiry has been instituted in India into the causes of origin of enteric fever. In summing up the replies received from medical officers, Surgeon-General Currie stated in a letter submitted to the Indian Government, referring to the year 1874:—"Neither the water supply nor any articles of food or drink used by the men have been found, after fullest inquiry, to be at fault, with the exception of the tank-water at Barrackpore, which is said to give evidence of the presence of an unusually large amount of organic matter. As regards the water supply to troops on the line of march, it is only in one instance surmised that the disease may have been caused by it. With the exception of defective surface drainage at Umballa, the overcrowded state of the barracks occupied by the 70th Regiment at Rawul Pindi, and the leaky condition of the roofs of the barracks at Hazarebagh, no preventible sanitary defects are noticed in the reports." No more satisfactory information was obtained from the investigations of 1875; and in summarizing the replies for 1876, Surgeon-General Ker-Innes, C.B., in his letter submitted to the Indian Government, dated Simla, October 20, 1877 (Appendix, p. 233), has expressed some very decided opinions, founded on the failure to detect in drinking water and other supposed agencies any explanation of the causes of the outbreaks. He says:—"(12) Throughout all the reports, of which a brief resumé has above been given, an anxious effort is everywhere observable to bring home a definite source to the contagium* of enteric fever.

"It is shown either in the endeavour to trace its origin† to some previous case; or, when this has not been practicable, the faulty condition of the surroundings has been strained to the utmost in order that it might yield a colourable explanation of its genesis.

"To such lengths has this attempt been carried that in one case the use of unfiltered water on a railway journey is suggested as the only apparent cause of the disease, and in another the presence of some old tents and disused bamboo frames in the lower storey of one of the barracks is commented on—puerilities which cannot but tend undeservedly to discredit the science of preventive medicine. . . . . (15) At the same time, it is impossible to refuse due weight to the accumulated evidence, however negative this may be, of its well-defined and essentially idiopathic character, or not to acknowledge that, wholly irrespective of contagion, it may develop itself in the European as the direct result of a consensus of causes, the chief of which seems to be the prolonged exposure of an alien and unripe constitution to an intensified and unaccustomed temperature, to which certain other more recondite climatic conditions may probably be superadded. (16) Such a view derives support

* From the following sentence it is evident this word is used as including originating causes as well as propagating causes.

† This word is evidently used here in the sense of *propagation*. Origin traced to a previous case must be propagation.

from the testimony of witnesses, who, if not already engaged to opposing theories, show a natural prepossession in favour of opinions adverse to it, because these are sanctioned by the teaching of some of the advanced medical authorities, while it is found that typhia is a form of ailment almost special to one period of life amongst the class above indicated, and that it very exceptionally exists among the soldiers of the native Indian army or in native Indian jails." We will have to return to this summary by Mr. Innes when considering the subject of the propagation of enteric fever, in the next chapter.

But although this absence of evidence is very remarkable, there would seem to be some considerations which ought perhaps to be regarded as positive evidence under this heading. These may be summarized as—(*a*) practical facts which seem to deny the possibility of a protogenesis of enteric fever; (*b*) facts which go to prove that (allowing protogenesis) the origin of enteric fever is due to causes other than drinking water; and (*c*) facts which show that in certain outbreaks drinking water could not have been the cause of origin.

(*a*) Those who have followed me up to the present page will be prepared for my expression of doubt as to whether the so-called facts on which some persons deny the possibility of the *de novo* occurrence of enteric fever are really facts. I am inclined to include them under the epigrammatic paradox which Prof. Aitken has applied to another class of facts, and call them "FALSE FACTS." It is said that *because* enteric fever is sometimes propagated, it can never be originated. This argument is not based on *facts*. It is true that there is one fact, but an argument requires two. There is a *major* fact, that propagation sometimes occurs; but if there be a *minor* in the argument at all, it is a hypothetical one; therefore it is clearly erroneous to maintain that this idea is based on facts. It would seem also that there exists a prejudice in favour of this view which, perhaps, to some extent prompts the conclusion. Sir Thomas Watson says: "If this fever be really contagious, it is not only erroneous, but dangerous to hold the contrary opinion." Undoubtedly; but if this fever be ever originated by putrid impurity, other than contagion, in drinking water, it is not only erroneous, but dangerous to teach that it is due only to propagation, because persons may continue to drink any filthiness, believing themselves secure, so long as there is no communication between their water supply and previous cases of disease.

(*b*) The commencement of some outbreaks of enteric fever is obviously due to causes much more general than any of which drinking water is likely to be the vehicle. This is especially seen in India as a result of the very extensive field for the observation of this disease presented in that country.

"A phenomenon, which at first sight appears very extraordinary, shows itself in the statistics of enteric fever in 1872. In the last ten days of August almost every station over several enormous areas began to return cases of enteric fever. Secunderabad and Poona, in the Deccan; Nusseerabad, Neemuch and Mhow, in the north of the Bombay Presidency; Kurrachee and Hyderabad, in Scinde; and Meerut, Muttra, Jullundur and Umballa, in Northern India—all returned in the same week, and after an interval of months, during which no typhoid had appeared, fatal cases of enteric fever. It is of very great importance that the interpretation of this phenomenon should be accurately made. To me it appears an admirable demonstration of the truth that the typhoid of India, which I have described, is not primarily attributable to special and locally existing conditions. It was not enteric fever alone which came forward in this week, but purely climatic fevers of every variety; and while the exciting cause of all was the same, the type varied according to predisposition and special circumstances. . . . Many pages might be filled with extracts from the weekly returns of medical officers in all parts of India, showing that the cases which they record were unmistakably cases of true typhoid fever. Such extracts were indeed prepared for publication, but to produce them would be merely to reprint what appeared in last year's Report, and to burden this note with details of clinical observations substantially the same as those which were then so fully given" (Bryden, "Ninth Report San. Comm. to Gov. of India," 1872; Appendix B, p. lxxiv.).

The above extract may be taken as representing a vast accumulation of practical evidence continually increasing, which goes far to prove the protogenesis of enteric fever by climatic influences described (as before quoted) by Surgeon-General Innes as "a consensus of causes, the chief of which seems to be the prolonged exposure of an alien and unripe constitution to an intensified and unaccustomed temperature, to which certain other more recondite climatic conditions may probably be superadded."

Such evidence seems to me to go a long way in favour of the pythogenetic origin of enteric fever, and not only so, but also as favouring the connection between enteric fever and hepatic derangement dwelt on at the beginning of this chapter; and thus indirectly, I think, this evidence is in favour of the probability of the protogenesis of enteric fever by impurity of drinking water.*

But even if we give to these observed facts their full direct value, as proving that the causes of origin of this disease must be much more widely diffused than those of which drinking water can be the vehicle, the evidence only proves that some cases of enteric fever must be originated by causes acting through some other medium, and does not disprove that, in other cases and under other conditions, drinking water may be the medium of the originating cause of this disease.

(*b*) Outbreaks of enteric fever are on record in which not only was it impossible to trace any connection between the first cases and impurity of water supply, but where the conditions seem to deny absolutely the possibility of such a connection, and to exclude drinking water from possible causes.

Examples of this are constantly occurring, particularly in India, where the first cases of an outbreak cannot in any way be traced to propagation, and must therefore be assumed to be cases of origination, where an entire community makes use of drinking water from the same source or from similar sources, and where only a few comparatively are attacked by the disease.

But these observations, in the same manner as those of the foregoing group, refer only to the outbreaks in which they are made—that is, prove only that certain outbreaks have occurred independently of the influence of drinking water, and give no evidence whatever on the possibility or otherwise of other outbreaks, under other circumstances, being originated by this influence.

* When a disease is shown to be due to atmospheric causes, this in itself amounts to a probability that drinking water may also be a vehicle of the cause. The International Sanitary Congress of 1866 embodied this doctrine, with reference to cholera, in sect. xxxi. of Report of its Proceedings :—"The atmosphere is not the only vehicle for the conveyance of the choleraic principle. Facts observed in England seem to place it beyond doubt that water—either soiled, as we have said, by matter proceeding from choleraic dejections, *or contaminated by the morbific agent diffused in the atmosphere*—may be the means of introducing this agent into the human system."

There are many points of great interest which have been left out of consideration in the foregoing remarks on the influence of drinking water in originating enteric fever, either because sufficient evidence is not available to arrive at any satisfactory conclusion with reference to them, or because the evidence bearing on them appears to be for the present conflicting—as, for instance, the subject of the asserted connection between the level of the subsoil water and the prevalence of this disease. That no definite conclusion can be arrived at regarding this subject will be evident from the following extracts:—

"With regard to typhoid fever and ground water, Virchow has some interesting remarks (Report on the Sewerage of Berlin, 1873). The most striking evidence has been obtained of its dependence on the changing level of the ground water. 'The cases increase when the ground water sinks, and they lessen when the ground water rises. At the time of the *lowest* water-level we have every year a little epidemic'" (Prof. Parkes' Report on Hygiene, Army Medical Department Blue Book for 1872).

"The greatest prevalence of fevers during the period of observation (February, 1872, to August, 1874) occurred coincidently with the period of maximum carbonic acid and *highest* water-level" ("The Soil in its Relation to Disease," Appendix B of Report of Sanitary Commissioner for India for 1874).

"The quarterly returns received from the Bengal Command do not permit of a closer analysis than the above being made as to the interdependence of prevalence of enteric fever and season, nor do the reports of the medical officers notice in any instance the subject of the asserted connection between the level of the subsoil water and the prevalence of that disease. In a few reports there are incidental notices which give indications on the matter; but, taken together, the evidence, if not contradictory, is at least indecisive" (Army Medical Department, Report for 1873, page 134).

SUMMARY.

Theoretical.

With regard to the possibility of enteric fever ever being originated *de novo*, the theoretical evidence in favour of protogenesis seems to be as strong as the nature of the case admits. The theoretical evidence which is brought forward as opposing this etiological deduction seems to a large extent based on observations and arguments which may be questioned as to accuracy (such as that based on the apparent incubation period, and apparent immunity from a second attack of the disease), and this evidence seems very open to contradiction, or differeut explanation.

If the possible protogenesis of enteric fever be admitted, then, with reference to the question of the influence of drinking water in originating it, there is strong evidence from hypothetical considerations that this is a very likely vehicle by which the originating cause may be introduced into the body in cases in which the disease is originated by external agency. But it would seem that such cases are rare as compared with those in which the disease seems to occur in the idiopathic or true spontaneous form. If the possibility of protogenesis be admitted, there is no theoretical evidence whatever against the deduction that drinking water MAY be a vehicle of the originating cause.

Experimental.

Taking into consideration the large amount of evidence brought forward by Murchison and others, I think it is only fair to admit (and if we take military experience, particularly in India, we must admit) that many cases of enteric fever seem to, and may, occur independently of foregoing cases.

Some practical considerations, no doubt, seem opposed to this view, but absolutely no practical evidence is brought forward by Budd or others to disprove it.

Admitting protogenesis, then, what do facts say with regard to drinking water being an originator of this disease? Practical evidence on this point is conflicting and indecisive. The only indication at all clear would seem to be that enteric fever may sometimes be developed by the inhalation of putrid effluvia or miasmata. If this is taken as favouring the doctrine that the same may cause this disease by being ingested with drinking water, it should be placed in the theoretical evidence, although derived from facts; because it rests on the hypothesis that water may absorb from the air the putrescent matters which cause the disease, and thus carry them into the system. Professor Parkes, in his Report on Hygiene for 1872, sums up an able resumé of all this conflicting evidence thus:—" It seems to me an undecided position, but with the stricter evidence in favour rather of Budd than Murchison" (Army Medical Department Reports, vol. xiv. p. 217).

This indecision is added to by some, who would aver that, even when the disease is clearly traced to drinking water (as in Donnet's Report on Typhoid in the Channel Fleet in 1870, apparently traceable to impure drinking water taken in at Vigo and Lisbon), this origin is illusory, and that the disease is really due to propagation; maintaining that such water, if the disease be in truth caused by it, must have contained contagium from some foregoing case.

Under these circumstances, if any conclusion can be arrived at, I consider we are justified in basing it for the present on the balance of the theoretical evidence; and it appears to me that from this the following is a fair deduction:—

It is questionable whether or not enteric fever is ever originated de novo, *but there is a slight probability in favour of this occasional protogenesis. If this be admitted, there is also a slight probability of the disease being originated through the influence of drinking water. But if any cases are so caused, they must be rare as compared with those in which the disease seems to arise from climatic or local causes, from the inhalation of putrid sewerage emanations, from idiopathic conditions, or from true propagation by specific contagion.*

CHAPTER VI.

THE INFLUENCE OF DRINKING WATER IN PROPAGATING ENTERIC FEVER.

General observations.

As in the foregoing chapter we had to consider the question of the protogenesis of enteric fever, so here we have to inquire whether or not this disease is capable of being propagated by contagion, before we can form any correct idea of the influence of drinking water in propagating it.

We have seen that the various views taken of the cause of this disease may be arranged in two groups—namely, the doctrine *that enteric fever is never originated, but always propagated ;* and the doctrine *that it is sometimes originated, and sometimes propagated.*

But when we come to consider here the views held on the propagation of enteric fever, we find that a third doctrine, if not absolutely and formally stated as an etiological deduction, is to a large extent floating in an indefinite form in the mind of the medical community, particularly in India, and is in that country rapidly assuming the shape of a popular belief—namely, *that enteric fever cannot be considered as a contagious disease in the usual sense of the term.*

However improbable this latter extreme view may appear to be, there certainly seems to be some evidence in favour of it from observations of the disease in India.

"As regards evidence of the disease having been imported either from England or from one station in India to another, no facts have been adduced which can be held to give any valid support to the belief that the disease originated in this manner. *The same may be said with respect to its spread by contagion*" (Letter to Government of India from Surgeon-General Currie, 1874).

This discrepancy of opinion arises from the fundamental idea that this specific process can only arise from one specific cause. Some persons refuse to credit that more than one cause can bring about this enteric fever process; and thus some, unable to reconcile the evidence favouring occasional protogenesis with that favouring occasional propagation, take refuge in the belief that the enteric fever of Europe (which chiefly affords evidence of propagation), and the enteric fever of India (which yields principally examples of the independent origin of the disease), are distinct and different diseases.

Then the question arises, Is it easier or more rational to admit different sources of causation for one disease, or to believe that the enteric fever of Europe and the enteric fever of India are distinct diseases, because they seem to occur under different conditions, although both are characterized by the same symptoms and the same post-mortem appearances—these similar symptoms and lesions being admittedly specific and pathognomonic, the result of a specific process, and not occurring in other diseases? It appears to me much more rational to believe that the disease is *the same*, as it occurs in Europe and in India, and to explain any differences it may present in different parts of the world as being different phases of the one disease, due to a varying etiology.

I think most unprejudiced persons must agree with Surgeon-General Innes that, with the data of which we are in possession, "it is difficult to admit the force of that generalization, however respectable its source, which would wholly assimilate the etiology of enteric fever as observed in the north with the allied variety of it existing in equatorial and semi-tropical latitudes, which, while presenting the same symptoms during life, and revealing identical necrological lesions after death, seem nevertheless to become developed under the well-defined phases which have above been insisted on" (Letter to Government of India, of October 20, 1877; see Appendix, p. 298). And with Dr. Bryden—"I would have it kept clearly in view, that it does not follow that, because typhoid puts on the aspect in which I here represent it, this is the only aspect under which enteric fever shows itself.

"When I point to the very large number of bodies of men affected by a certain aggregate, giving an exceedingly limited average of each, and the distribution of the disease by individual cases, I do not forget that there is a typhoid which affects communities as well as individuals. I do not doubt that the etiology of these varieties is to be studied separately; and from the combined study we may in time come to understand what the physiological significance of the typhoid deposit is, and under what condition it takes place in individuals, and, as well, how, through one common agency, a community may become infected with enteric fever" (Report of Commissioner, Government of India, Appendix B, p. lxxvii.).

As this concord of opinion is far from existing as regards the propagation of enteric fever, as well as its origin, it becomes necessary to consider here the different points of evidence and arguments brought forward in support of the different doctrines above indicated, both referring to the propagation of the disease, and the possibility of its propagation through the influence of drinking water.

Affirmative theoretical evidence.

The first argument justifying the deduction that enteric fever is propagable by contagion, and that contagion can propagate the disease through the medium of drinking water, is derived from the fact that in this disease process a characteristic elimination of a morbid product occurs — that this material is cast off in the intestinal dejections, and may thus find its way into other organisms by being ingested with drinking water. The hypothetical part of the doctrine is that this eliminated material is or contains the specific germ or virus of the disease, and that this can find its way into the blood from the stomach, *still preserving in integrity its specific particulate and virulent form.* The latter point I have referred to in a general way in the fourth chapter of the first part of this Essay, and will not discuss here. The doctrine that the eliminated material contains the specific germ of the disease process is supported by an observation seemingly well established—namely, that the "*typhoid deposit*" which constitutes the intestinal lesion in this disease "is a special growth, which in cases of recovery follows first a progressive or developmental course, and afterwards retrogrades, just as in variola we first observe the development and maturation of the pustule, and subsequently its disappearance" (Aitken).

If circumstances admitted of our experimenting with this eliminated product—if we could inoculate a healthy organism with it in the fresh state, and thus attempt an artificial propagation of enteric fever, as we do of variola in vaccination—such an experiment would perhaps throw some light on the question. Enteric fever is said to be peculiar to the human organism, and not propagable to the lower animals; but this observation, I believe, depends only on the fact that dejections of the disease, when taken into the alimentary canal of lower animals with food, do not affect their health. But it may be possible to arrive at a different result if the typhoid deposit, immediately after death—that is, in as fresh a state as possible—were introduced directly into the circulation. If there were no result, this would add nothing to our knowledge, because it may be said that, death having occurred before the matter was taken, the experiment is thus vitiated. But if any phenomena similar to this disease followed such inoculation, it would go far to prove that this eliminated product, the so-called "typhoid deposit," is or contains the specific germ or virus of the disease.

So far as I am aware, this method of investigation has not been adopted in its entirety—that is, excluding the possibility of any result being due to putrefaction of the inoculated matter. The obvious objection to the view of the eliminated product being, or containing, the specific virus of the disease, is that so fully elaborated by Murchison and others, that, if true, the result—namely, propagation—should be constant.

To this it is replied that the same objection may be stated against the pythogenetic theory, and against every other theory of the causation of this disease.

If a specific eliminated contagion be not a cause of it, it must be granted that either putridity or *something else* must be the cause. Then, whatever materials or conditions are assumed to be its cause, it can be shown that these may, and often do, exist without being absolutely and necessarily associated or followed with or by the appearance of enteric fever.

Another line of argument, still theoretical, is that of excluding all other theories except that of propagation by contagion. Budd maintains that neither the pythogenetic hypothesis of Murchison, nor any other except contagion, can explain the phenomena of the epidemics of this disease. In this he is supported by many well-known English and continental authorities.

This opinion is again replied to by saying that the contagion hypothesis is equally excluded by the apparently spontaneous cases, and by other phenomena of the disease, particularly as it is seen in India.

Then again, in reply, others, without attempting to unravel it, cut this Gordian knot with the sweeping assertion that these isolated cases are only apparently so, and are really due to propagation, and further by asserting that the disease in India may be a phase of a typhoid disease, but is not the true enteric fever of Europe. This is again replied to, and the reply in return objected to—and so on, with but little hope of arriving at any satisfactory conclusion.

But there is one point in Budd's attempt to overthrow the pythogenetic doctrine to which I would specially direct attention here, because it seems to me that it contains a fallacy which has not been sufficiently exposed by his opponents, and, as I think, has a very important bearing on the question of the propagation of this disease by drinking water, being at the same time specially interesting from a military point of view as going some way to explain the peculiarities of this disease as observed in the section of our army serving in India. He says that this fever cannot be "born of putrescence," because it sometimes happens that when "the putrefactive compounds are rife" enteric fever does not become prevalent, and it is asserted that workers in drains and sewers enjoy an immunity from enteric fever.

Now if this latter observation be a fact, what is the explanation of it? It is certainly very remarkable that in a community those only who are habitually accustomed to the inhalation of putrid sewerage should enjoy immunity from a certain disease. Does the contagion theory explain it? I fail to see how. But if this be a fact, I think it points as unequivocally as possible to some connection between sewer emanations and the disease with regard to which this immunity is said to exist. I think it tells directly in favour of the doctrine against which it has been advanced, and seems to be perfectly explicable by that doctrine, and by it alone. If enteric fever be a form of septic toxæmia, throwing, for the elimination of the poison, additional work on the lymphatic system, and especially on the hepatic function, would it not be *à priori* expected that a state of chronic functional activity of this system would produce an immunity from this or any other disease arising from a sudden call on or increase of the function of that system? And is this immunity of workers in sewers to be considered as analogous to the infrequency of enteric fever in soldiers of long tropical service, or who have already suffered from a morbid hepatic activity, as compared with the prevalence of it among young men fresh from temperate climates, whose lymphatic system, and especially whose livers, have not undergone any preparatory tolerating morbid process ?*

It seems probable that in both cases a *hepatic sufficiency* is after some time established, which is able to cope with or tolerate the demand made on the lymphatic system for the elimination of the septic poison.

The arguments in favour of propagation by contagion may, for all practical purposes, be taken as in favour of propagation by drinking water—it being assumed that contagion can be introduced into the stomach by this means, and thus propagate the disease. I have elsewhere referred to the question of whether or not it really does so.

Affirmative experimental evidence.

The chief evidence in favour of the contagiousness of enteric fever, and its propagation by drinking water is, of course, derived from actual experience of epidemics. Are these real epidemics, or are they aggregations of synchronously occurring endemic cases ? I shall first consider the evidence in favour of their being real epidemics due to true propagation by contagion; and then consider the evidence in favour of this propagation being due to the influence of drinking water. A very complete case of propagation is quoted by Dr. Parkes in his Hygiene Report for 1872, which I give, with his remarks, here, verbatim, as it is a good example of the first kind of evidence:—"In enforcing his view, Dr. Budd describes with most graphic force, from his own experience, the spread of typhoid fever in villages where the previous non-existence of the disease, its introduction at a given date by a person arriving who had the disease, and its subsequent spread, were all perfectly proved. Nothing can be stronger than his facts, but it would have added to his arguments had he given short references to the other very numerous cases of this kind, beginning with those collected by Bretonneau more than fifty years ago. Those cases are very numerous both in France and Germany and in England; and even since Dr. Budd wrote, another instance has been given by Dr. Ballard ("On Enteric Fever in the village of Combroke, in February, 1873"), the main facts of which may be cited. A row of three

* See remark on page 213, referring to medical history sheets of men attacked by enteric fever.

cottages, made out of an old farmhouse and under one roof, was inhabited by three families, midway in the little street of the village of Combroke, near Stratford-on-Avon. In these three cottages an outbreak of typhoid fever occurred in February, 1873, while all other houses in the village on either side entirely escaped. What was the reason of this attack, and of this escape? For many years prior to this outbreak there had been no case of enteric fever in the village, says Dr. Ballard. There must, therefore, have been something very special to cause such a localized outbreak in these three cottages. On November 14, a girl, aged seventeen, from one of these cottages, went to service in Leamington, to a house where a young lady had just recovered from typhoid fever. On November 25 the girl began to have diarrhœa, and on December 3 shivering, and went to bed. On December 8 she was removed to her own home at Combroke, with the diagnosis of typhoid fever (confirmed by four medical men), and became convalescent in about three weeks after her return home. All her evacuations were thrown into an ash-pit privy common to two of the cottages; her soiled linen was washed by her mother, and the water was emptied into a grating in the yard. The family in this cottage consisted (in addition to this girl) of nine persons, of whom four were at home at the time; three came home in March, and two others were occasionally at home. There was also a nurse in the house in March, who was not attacked. Now, of these nine persons, no less than seven were attacked with typhoid fever in February and March, and the two who were not attacked were those who were there for a short time. In the next cottage, which used the same privy, there was a family of nine, of whom three got fever. In the next cottage (where there was a separate privy) there were two cases, and both were mild. Dr. Ballard enters minutely into the question of whether the air or the water was the medium of communication, but this is foreign to my purpose. No other cases occurred in the village. Now, can any one shut his eyes to the force, positive and negative, of this evidence? Is there any link wanting, or any shade of doubt resting on any part of this history? Then multiply this instance by scores of similar cases, and we form an idea of the position taken up by those who adopt, like Dr. Budd, the view of continuous succession."

It is remarkable that these examples of the undoubted propagation by contagion of enteric fever are almost exclusively furnished by the civil section of the community. During the four-year period which I have selected for illustration of the occurrence of this disease in the army—namely, 1873–4–5–6—but one outbreak (as it is termed in the Report*) is described as being with any clearness due to propagation—namely, that which occurred at the Dartmoor camp of exercise in 1873; and which principally affected the 19th Hussars, the details of which are given in vol. vi. of the Army Medical Department Reports. Even in this case propagation by contagion is far from being indubitable. Indeed, our general military experience of this disease may be described in almost the same words as those used in reporting the occurrence of it in the army for the year 1876:—"No epidemic outbreak occurred at any station, though in a few instances two or three attacks followed in sequence;" because, the short and very circumscribed epidemics described as "house epidemics," can hardly be taken as identical in causation, and wholly assimilable in nature with those severe and widely spread visitations of pestilence usually indicated by the term "epidemics." In the four-year period referred to, enteric fever was as prevalent in the army as in any other period. If such evidence as this be considered sufficient to establish the deduction that enteric fever is sometimes propagated—and it must be allowed that, at least as derived from observation of this disease in the civil population in European countries, this form of evidence appears to be most satisfactory and very abundant—it remains for us to inquire what part the influence of drinking water plays in such propagation.

As a preliminary to this inquiry, we must form an opinion as to whether the agent of propagation is a definite particulate contagium, or a miasm or emanation.

I have dwelt in a former part of this Essay on the differences of *contagium* and *miasm* with reference to the influence of drinking water in acting as a carrier or propagator of them, showing that, while it is generally admitted that water may be a vehicle of contagion, it is doubtful whether miasmic poisons can be introduced into the system by this means. For the purposes of practical sanitation, which require only that the dejections, raiment, and habitations of enteric fever patients should be disinfected, and any

* This word *outbreak* is a source of much confusion. It should not be used except when the first case of a series is proved to be originated or spontaneous.

poisonous or disease-carrying agency (whether this exist in the form of *contagium* or *miasm*) be thus destroyed, it is a matter of indifference in what form the poison exists. But the question is full of interest to the scientific etiologist, and is practically important in considering the influence of drinking water in propagating this disease.

It must be admitted that the evidence brought forward in support of Pettenkofer's view, that the propagating influence of enteric fever is not a *contagium* (that is, a substance multiplying only in the human body) but a *miasm* (that is, an agency which may adhere to the persons, clothes and houses of those affected, but which multiplies independent of and outside the human body)—the view that (as Dr. Parkes puts it) *man carries, but the locality brings forth* the propagating influence—seems to be clear and well founded on practical experience. Observation of the disease as it occurs in India gives very strong support to this view. If it be correct, serious doubt is thrown on the possibility of enteric fever being propagated by drinking water.

But again, the position taken up by Professor Ranke (see Dr. Parkes' Hygiene Report for 1872), that drinking water must be considered one medium of the propagation of this disease (ignoring this hypercritical refinement in the definition of the terms contagium and miasm), is not to be lightly passed over.

The majority of European etiologists believe not only the propagation by contagion of this disease, but also its spread through the influence of drinking water, and support this view by an appeal to extensive experience.

"Biermer, from an analysis of more than 1,300 cases, cites evidence of water carriage; and Geissler quotes from Haglir an extremely strong case of the spread through drinking water" (Parkes).

A good deal of experimental evidence in favour of the propagation of enteric fever by contagion through the influence of drinking water has accumulated in England within the past few years. A clear and judicial summary of this evidence is to be found in the last Report on Hygiene written by the late Professor Parkes (Army Medical Department Reports, vol. xv.). Although so many cases have been brought forward to support the doctrine of continuous succession, it must even still be admitted that this deduction is as yet only a probability. True, perhaps (in the words of Mr. Netten Radcliffe), "a probability amounting for all practical purposes to a certainty ;" still, it must be remembered, not a certainty. My own experience of enteric fever has been very slight, but, such as it is, cannot be brought forward under this heading, as, if any deduction can be drawn from it, it is against the propagation of this disease. The evidence from the experience of a few individuals on one side or the other of such a question, cannot be worth much as compared with many and constant observations made on large communities; and for this reason I am inclined to attach more importance to our army experience of this disease, particularly as seen in India—presenting, as that country does, so extensive a field of observation—than to evidence derived from experience of the disease among the civil population of European countries.

Negative theoretical evidence.

The first division of this evidence is that which seems to prove enteric fever ought not to be grouped among contagious diseases; contradicting the evidence on which the deduction of contagiousness is based. This opinion is chiefly founded on a *definite incubation period* and an *exhaustion of susceptibility*. "Ces deux caractères fondamentaux, incubation et immunité ultérieure, appartiennent à un groupe bien défini de maladies—c'est celui des fièvres contagieuses, et plus particulièrement celui des fièvres éruptives, avec lequel la fièvre typhoide a de frappantes affinités, déjà signalées par Bretonneau et après lui, par un grand nombre de médecins français. Ainsi l'induction tirée des lois de la pathologie et des caractères constatés dans les maladies contagieuses s'unit à l'observation directe pour affirmer la contagiosité de la fièvre typhoide."*

In the chapter on the origin of this disease I pointed out how and why it is doubtful that an incubation period (as usually understood) and a subsequent immunity are to be included among the phenomena of enteric fever ; and I have shown how the experiments of Bernard do away with the objection which an incubation period is supposed to yield to the pythogenetic origin of this disease.

"L'existence d'une period d'incubation, observée dans toutes les maladies spécifiques, me parait difficile à concilier avec la theorie pythogénique. Les substances toxiques produisent des effets immédiats. Qui ce soit un poison végétal ou une poison minéral, c'est peu de temps après leur introduction dans l'écono-

* Dr. Gueneau de Mussy, "Recherches sur l'Etiologie de la Fièvre Typhoide," page 50.

mie que leur action se manifeste. Comprend-on des phénomènes d'empoisonnement se déclarant une ou plusieurs semaines après que l'agent, que les cause a pénétré dans les voies d'absorption?"* Bernard's experiments, as I have pointed out (page 225), proving that a period elapses, varying in different persons (as does the so-called incubation period of enteric fever) between the introduction of putrid gases into the venous circulation and its entrance into the arterial blood, appear to cut the ground from under this objection.

In fact, on the pythogenetic theory, this indefinite delay in the expression of symptoms is, *à priori*, to be anticipated. I have also in the same place dealt with the question of exhaustion of susceptibility.

There are facts on which the deduction is based, that no substance is eliminated in enteric fever containing a specific virus at all analogous to the carrier of contagion in vaccinia and syphilis.

The experiments of Thiersch, Pettenkofer and others, who fed animals on the dejections, seem to prove that, *when fresh, these matters are harmless*, and that any poison-carrying properties they may possess are developed only after they have been broken up by the putrefactive process. This argument is quite useless unless it be also shown that fresh vaccine matter and fresh syphilitic virus, *when swallowed*, produce their characteristic processes.

The second part of this evidence is directed against the possibility of drinking water being a medium of enteric fever propagation. There can be no doubt that a striking analogy exists between the mode of prevalence of enteric fever and climatic diseases—those caused by increased temperature and malarial influences, particularly as seen in India. I have before quoted from the Indian Sanitary Reports in support of this view; referring to the last ten days of August, 1872, when almost every station over several enormous areas began to return cases of enteric fever. And it was not only enteric fever which came forward at this period, "but purely climatic fevers of every variety, and while the exciting cause of all was the same, the type varied according to predisposition and special circumstances."

So close an analogy, indeed, exists, as to justify the belief that the causes of prevalence and influences of transmission (if there be

* Dr. Gueneau de Mussy, "Recherches sur l'Etiologie de la Fièvre Typhoide," page 50.

such) are similar in both. This deduction would indicate that the medium of prevalence, and, presumably, the medium of transmission from one organism to another, is much less restricted in its operation than drinking water.

If a specific virus, or particle of contagious bioplasm, eliminated from the body of an enteric fever patient, capable of being carried *in its integrity* from one organism to another by drinking water, had any real existence, then we would expect to find that the certainty of propagation *increased directly with the freshness* of the dejections at the time of their introduction to the healthy organism. All experience, however, proves that this is not the case, but that the direct reverse obtains; the dejections of enteric fever patients being, moreover, particularly prone to early decomposition.

Then, if drinking water can carry propagation, it must do so by carrying certain of the products of this decomposition. There are no grounds whatever for assuming that the products of the putrefaction of the stools of enteric fever patients differ in any respect from those of ordinary fæcal matter when decomposed.

"Polli, Tigri, and many others, have attributed typhoid fever and allied diseases to bacteria in the blood acting after the manner of ferments; but the objections raised to the fermentation theory have not yet been disposed of by those who advocate this doctrine. It appears that bacteria germs grow and multiply whenever a change takes place in the solids and fluids of the organism which develops compounds suitable for the pabulum of these living bodies. From the fact that bacteria grow and multiply not only in a few special fevers, but in a great variety of different morbid conditions, it is evident that they have nothing to do with any particular form of disease. All attempts to demonstrate various constant species of bacteria, representing different contagious diseases—and many attempts have been made—have completely failed" (Professor Beale, "Disease Germs," p. 71).

Negative experimental evidence.

The experimental evidence against the spread or propagation of enteric fever by drinking water may be grouped as that contradicting the evidence of propagation by contagion, and that against the possibility of drinking water being a carrier of contagion, if such be admitted.

I. To the argument that the contagion theory must be correct, because it alone serves to explain epidemics of this disease, it is replied that enteric fever is not an epidemic or contagious disease in the usual sense of the term—that such apparent epidemics are merely masked endemics. And this view is supported by the very numerous cases of local outbreaks recurring again and again in the same places; the very circumscribed limits of all outbreaks of this disease; and, especially, by our experience of enteric fever as it occurs in India.

If the theory of enteric fever propagation by contagion were correct, every case and every outbreak should be capable of being traced to a foregoing case. This is not so. Numerous cases and outbreaks are on record in which the previous existence of *contagium*, as in any way connected therewith, has remained undiscovered after the most searching inquiry—in which its existence would on the evidence seem to be absolutely impossible.

There are other cases in which a source of contagion has been asserted to exist, but which break down on more careful investigation. If Budd's reply to this, that such contagion must always exist, but that we are unable to trace it, be admitted, then it seems useless to take any evidence on this or any other scientific question. His objection is one of those hypothetical scientific generalizations, so plentiful in the present day, which it is difficult for those persons who do not accord to imagination a prominent place in scientific inquiry, to accept as true.

Again, cases are constantly occurring in which an enormous quantity of this suspected material is eliminated and allowed to find its way into the ground, into sewers, &c., and where no attempt at disinfection has been made, and the result which would be expected to follow on the contagion theory—namely, spread of the disease—does not ensue. This may not be of much value as a positive argument, because we cannot say under what conditions such a poison would be operative, what liberates it, what renders it virulent, what absorbs it, &c.; but it seems to be a complete answer to Budd's great position, that he has "always arrested the disease from spreading in houses, schools, communities, &c., by thorough disinfection of the enteric fever discharges." If he had not disinfected, the outbreaks would have been equally circumscribed; or, at least, it is his place to prove the contrary. It may have been remarked that this argument—one of Budd's strongest points—I did not put in the experimental evidence in favour of propagation, because it seems to be as clearly as possible opposed to propagation. Budd's experience quite coincides with other experience on the limited extent of enteric fever outbreaks. This experience points unequivocally to the non-contagious deduction; but Budd fancies this is explained by a disinfection which was adopted only in his own cases (a very small minority of the entire experience), without an attempt to prove that if such disinfection had not been adopted the result would have been different. But the mass of the evidence proves that, as regards limitation of the outbreaks, disinfection does not to so great an extent influence the result. Of course it is advisable to disinfect, but all experience shows that, whether disinfection be adopted or not, the outbreaks of enteric fever are circumscribed.

Now, how can it be explained that, although in many cases and outbreaks, after most careful search, no trace of contagion can be discovered as in any way related to them, *putridity* can always be found to be in some way related to all cases. This goes a long way to prove that even in those cases which can be satisfactorily traced to enteric fever dejections, it is the putridity of these dejections—and, from their proneness to early decomposition, the dejections of enteric fever may be said, practically, to be always putrid before they are ingested—which causes the disease, and not any hypothetical germ or particle of bioplasm eliminated with them from the affected organism.

Louis observed only three cases of 117 which he could in any way connect with propagation; and Chomel has related, from his experience, only five which were developed under circumstances in which he believed propagation by contagion was possible.

At Guy's Hospital, Dr. Wilks, at St. Thomas's, Dr. Peacock, have never seen the hospital attendants attacked by this disease. The official inquiry made in the hospitals of London in 1873 brought to light only two cases of this disease in hospital attendants. This was in the Royal Free Hospital. In the space of twenty-three years 5,988 cases of enteric fever were admitted to the London Fever Hospital; during that period only seventeen of the hospital attendants were attacked by this disease, and many of these seventeen had no direct communnication with the hospital cases of enteric fever. Experience of enterie fever as it occurs in the army coincides more with this evidence, derived from its occurrence in the civil population, than with that derived from the same source in favour of the propagation of the disease by contagion.

It would be useless to take up space here by a detailed reference to the many isolated cases and very circumscribed epidemics (if they are to be so termed) occurring among soldiers both on home service and in India. A perusal of this experience, as it is recorded in the Army Medical Department Reports, must convince even the most firm adherents to the contagion theory that there exists much practical evidence to be adduced in support of the opposite view, that enteric fever is not, or at least is not always, due to propagation by contagion.

My own experience of the disease has not been extensive. In a a period of five years' tenure of an appointment held under the Local Government Board in England, before entering the Army Medical Department, in a district of 14,489 acres, and a population of 12,968, I have seen but four cases of this disease. All were isolated, and occurred at the height of summer. All were also in the close proximity of a river, the flood of which varies much in summer and winter. In summer a broad margin of marshy bank is exposed, and many stagnant pools are left. But this water was not drunk, and a pure supply was used for culinary purposes. No two of these cases occurred in the same year, and in none of them could contagion be discovered as a cause. Indeed, from later experience of the disease, I do not fail to observe that, in causation, these seemed to bear a close resemblance to those isolated cases supervenient on long existing suspicious endemic circumstances so frequently met with in India. A combination, or coincidence, of at least two sets of conditions (and perhaps many more) seems essential. The disease does not arise from endemic influences, except when certain climatic conditions co-exist with them; and the same climatic conditions will not cause the disease when unaccompanied by further conditions, furnished, perhaps, sometimes by endemic influences, at other times by epidemic influences. In the four cases above mentioned the dejections were buried, and no spread of the disease occurred.

My experience of the disease in India is closely similar. It so happens that I have never seen it in that country either in an epidemic form.

I once saw three cases follow each other in close sequence, but there was no apparent connection between them which would warrant the inference of propagation, and these seemed to me to be merely three originated (as distinguished from propagated) cases, occurring nearly synchronously, due either to climatic, idiopathic, or endemic conditions, but not affording any of the phenomena of a true epidemic—meaning by that term propagation by contagion.

I am inclined to deduce from this experience, such as it is, that synchronicity is often the only phenomenon which is presented in many so-called epidemics of enteric fever, which at all justifies the use of this term, and that many aggregations of originated cases of the disease are often thus erroneously described, each of which would be recorded as a spontaneous case but for their accidental coincidence in time and locality.

II. The second division of the evidence under this heading is that in favour of the deduction, that, allowing the possibility of the propagation of enteric fever, the influence of this propagation is not of a kind likely to be carried by drinking water.

It is founded on the fact that, in nearly all instances of propagation being likely, there is more probability of the influence of infection having been conveyed as air effluvia than of its being due to carriage by drinking water.

As an example, we may take the case quoted from Dr. Ballard by Dr. Parkes, before given (page 241), as one in which the evidence of propagation is very strong; and inquire whether the balance of probability is in favour of propagation being in this case carried by air or water.

There seems to be no doubt whatever that the disease was imported into the village of Combroke by the girl who came from Leamington.

In the cottage where this case occurred seven out of nine persons were attacked by the disease.

In the next cottage, where the same privy was used, three individuals out of nine were attacked.

Now, how is this disproportion in the number of attacks in the two cottages to be explained?

The inhabitants of both were under similar conditions as regards the privy used. Then, in the third cottage there were two cases. The incidence of the disease here was nearly the same as in the second cottage; although the second cottage used the privy into which the enteric fever dejections were thrown, and the third cottage had a separate privy.

Here, although we have undoubted evidence of the propagation of the disease, the circumstances seem to disfavour the hypothesis that this propagation was effected by the bowel discharges; and this goes some way against the probability of the disease being propagated by drinking water. A better explanation of the facts is found in the hypothesis, that the disease was transmitted by means of the atmosphere—that is to say, through infection, by a miasm or emanation leaving the diseased organism by means other than the bowel discharges.

This infection influence being, of course, more concentrated in the first cottage than in the second, and in the second than in the third, would seem to account for the disproportion of attacks in each of the cottages—seven in the first, three in the second, and two in the third. No other case of enteric fever occurred in the village. Why was the disease localized to these three cottages? If the disease was propagated by drinking water, it is reasonable to suppose the disease would have attacked the inmates of some of the other houses. But these three cottages were connected in a way favourable to the hypothesis of miasmic infection: they were "made out of an old farm-house, *and were under one roof.*"

Then the question arises, If enteric fever can be propagated in this way by miasm, can this be carried by drinking water as well as by air? We have no proof whatever of this. There is only a slight probability of it, resting on the very hypothetical assumption, that water exposed to an atmosphere containing a miasm of this kind may absorb it.

I will notice but one more fact which seems to go against the theory of the propagation of enteric fever by means of drinking water—namely, that *the propagation of the disease is not found to follow the course of rivers.* The value of this observation can hardly be rightly estimated by persons who have not studied the phenomena of epidemics in tropical climates. In civilized communities, where the water supply is chiefly artificial, there is, of course, no correspondence between the spread of even the most undoubtedly contagious bowel diseases and the direction of the water flow.

But those who have observed such in countries where the population use streams largely for drinking purposes (as in the outer Himalaya), can appreciate the value of the fact, that no such phenomenon can be traced in the spread of enteric fever.

I have carefully examined many reported epidemics of this disease with reference to the topographical configuration of the districts in which they have occurred in India, and find that in no case can any such correspondence between the direction of its spread and the direction of water-courses be detected.

SUMMARY.

Theoretical.

We have no theoretical grounds on which to found a belief in the specific nature of the *eliminated product* of enteric fever.

The proposition, that its power of causing disease may arise from its peculiar proneness of decomposition, is not easily answered.

The argument, that enteric fever must be contagious *because* it possesses certain characters found only in the specific contagious diseases, such as an incubation period and exhaustion of susceptibility, is, I think, quite negatived by the high degree of probability which exists of these latter phenomena being only apparent in enteric fever.

It would appear that the phase of enteric fever with which we are familiar in tropical regions teaches that the disease is to be considered as being in its nature more analogous, and in its natural history more closely allied to diseases of climatic origin (using the term in a wide sense), than to diseases of a specific contagious nature.

Thus the balance of theoretical evidence seems to indicate, *quantum valeat*, that enteric fever is not likely to be propagated by contagion; and that, if it be propagated or transmitted from person to person, it is more probable that it is transmitted by infection than propagated by contagion.

Thus it would seem, from this part of the evidence, that there exists but a small probability that drinking water exerts any influence in propagating enteric fever.

Experimental.

When we find so many distinguished observers bringing forward such conflicting practical evidence and holding such opposite views on a question of practical experience, I think we are justified in concluding here (as we are so often obliged to do in the daily concerns of our lives) that all are partially right, and all are partially wrong; that each is right in entertaining the view he does *with regard to his own experience*, but wrong if he give to that view or to that experience so extensive an application as to cover the experience of other observers.

From evidence on one side we must believe that enteric fever is sometimes truly propagated by contagion, and that this propagation is *sometimes* effected through the influence of drinking water. But, in consequence of evidence brought by others against this affirmative deduction, proving (so far as it goes) that this deduction must be of limited application, we have further to modify this conclusion by substituting *may* for *is;* and the proposition then stands thus—the propagation of enteric fever *may* be *sometimes* effected by drinking water.

Then, if we combine the deduction from the theoretical evidence, that "there exists but a small degree of probability that drinking water exerts an influence in propagating enteric fever;" and the deduction from the practical evidence, that "the propagation of enteric fever *may* be *sometimes* effected by drinking water;" the following conclusion may perhaps be taken as a fair result:—*Drinking water may sometimes exert an influence in propagating enteric fever; but such influence must be infrequent and of very limited extent.*

CHAPTER VII.

THE INFLUENCE OF DRINKING WATER IN ORIGINATING CHOLERA.

General observations.

A COMPILATION of evidence bearing on the etiology of cholera is a work from which the most energetic may well shrink.

During recent years so vast an accumulation of evidence has been submitted to the profession on the nature and cause of this disease, that the mastery of its details requires a considerable amount of time and industry.

Unfortunately, this evidence is in many points conflicting, and it is a matter of no small difficulty to judge impartially between such apparent contradictions. This difficulty is increased by the fact, that, if a judgment of them is to be impartial, it must be conducted without any central starting-point or prepossession as to the nature of the disease. If we would arrive at a just conclusion, we must start without a theory. Thus we are thrown back on facts. But the facts of cholera are characterized first by their FEWNESS, and second by their FALSENESS. The facts observed of cholera are so few, and not only so unconnected, but apparently so absolutely contradictory, that it is well nigh hopeless to attempt to explain or embrace all of them in a general system.

In endeavouring to interpret the laws of *weather*, "we are," Herschell remarks, "in the position of a man who hears at intervals a few fragments of a long history related in a prosy unmethodical manner. A host of circumstances omitted or forgotten, and the want of connection between the parts, prevents the hearer from obtaining possession of the entire history. Were he allowed to interrupt the narrator, and ask him to explain the apparent contradictions, or to clear up doubts at obscure points, he might hope to arrive at a general view. The questions that we would address to Nature are the very experiments of which we are deprived in the science of meteorology."

Our position is closely similar in studying the etiology of cholera. Then not only are our facts few, but we are not sure that they are *true*. "On looking into the distractingly confusing history of this disease, it will be found that there are quite as many *false facts* (SO-CALLED *facts*, which are not facts) as there are false theories" (Professor Aitken).

Thus the prosecution of an inquiry on any point connected with the nature and cause of cholera resembles entering an intricate labyrinth, in which many paths are presented to the wanderer; who, after spending much time and trouble in the attempt to ascertain the true path, finds that his labours have resulted in bringing him round again to the point from which he started.

He must hold the silken cord to guide him; and this can be done only by taking up some single point, to which our observation must be absolutely restricted, to the exclusion of all other considerations, however enticing or facile of interpretation they may seem. The one point to be considered here is the influence by which drinking water may act in originating cholera; and we must watchfully guard against wandering into the trackless and desolate wastes in which we shall soon find ourselves by inquiring into the nature and cause of the disease.

Of course the influence of drinking water in originating cholera depends on, and is bound up in, the nature and cause of cholera; and to consider the latter question first may be one way of arriving at correct views with reference to the first. This would be to form a theory of the nature of the disease, and then endeavour to explain the influence of drinking water on this theory. It is no doubt a plausible short cut, but experience teaches us to avoid this path. I shall not consider this subject, as I have treated the same question in the foregoing chapters with reference to diarrhœa, dysentery, and enteric fever. We must proceed on this inquiry altogether independently of theory, *because there is no theory of the nature and cause of cholera*, if a legitimate theory be a hypothesis which affords an adequate explanation of all the phenomena to which it relates.

No doubt very many speculations have been promulgated on the nature and cause of cholera, and some of these have secured many able advocates, but it is to be observed that while every one of these theories "is apparently supported by a large amount of evidence, direct and circumstantial, each is also opposed, to some extent, by a 'considerable number of obstinate facts' which the particular theory does not explain. In opposition to the view that a specific fungus is the cause of cholera, there seems now to be positive evidence to show that there is no special cholera-fungus in India. In opposition to the theory that cholera springs from some special condition of ground water, there is evidence that cholera has assumed great intensity where there is no ground water, and that Pettenkofer's theory does not find confirmatory evidence in India. In opposition to the view that cholera excreta are the special *foci* of cholera, the disease has spread where the excreta have been carefully dealt with in conformity with the indications of this theory, and ceasing to spread in the hospitals of Calcutta where the excreta had not been so dealt with. In opposition to the belief that cholera spreads by its specific poison getting into water, it has been known to spread where no such occurrence could possibly have taken place" (Professor Aitken).

Neither do I purpose to present here, as I have done in treating of the other diseases, evidence which would seem to go against the deduction that drinking water may exert an influence in originating cholera. Such evidence would be chiefly derived from theoretical considerations, and we will here avoid any such. Practical evidence against this deduction, or, indeed, any other deduction, is of little value as regards the etiology of cholera, because of the contradictory nature of *all* the observations which have been made regarding it. Even the best established conclusion can be challenged by some point of evidence. This is well illustrated by a passage in Dr. Parkes' Report on Hygiene for 1872. Speaking of the Ninth Report of the Sanitary Commissioner with the Government of India, he says, granting "that in none of the 108 outbreaks (of cholera) in 1872 in Bengal was there carriage by human intercourse, this could not upset the many and indisputable cases of such carriage in other countries and at other times; it might show the carriage by persons to be less common than we supposed, but it could not go beyond this." Thus, if we have one fact in favour of the deduction that drinking water may influence the origin of cholera, it is useless to bring forward evidence against it, as the latter can only affect its frequency; and we do not want to inquire into the frequency or infrequency with which drinking water influences the origin of cholera, so much as whether or not such influence ever is exerted, and what the nature of this influence may be. Thus all the evidence brought forward here will be, according to the distinction used in the foregoing parts of this Essay, affirmative.

Affirmative experimental evidence.

Before going into the question of the *direct* influence of drinking water in originating cholera, it will be well to refer briefly to what we know of the question of its possible *indirect* influence—that is, if it may act as a "predisposing cause" of this disease.

This question of the predisposition to cholera may be, or may not be, different from that of its direct origination, when we deal with definitely expressed predisposition and definitely expressed origination. But there is ground for believing that the two actions merge imperceptibly into each other; that is, that a predisposing cause existing in great excess, or prolonged in its action over a considerable period of time, may under those circumstances act as an originator of the disease. And as there is no definite point at which we can say the predisposing action ceases and the originating action begins, so in like manner it would seem, no fixed line can be drawn between the two actions or influences as regards their nature.

Therefore, whether the influences of predisposition and origination be etiologically the same or distinct, it seems allowable in practice to consider predisposition under the head of origination, as the two influences may possibly be identical in nature.

Our knowledge of the conditions predisposing to cholera is chiefly derived from experiments on the physiological effects of cholera poison on animals, and the results of microscropic examination of the intestines of human subjects who have died of cholera.

The experiments made on animals with cholera dejections are well known. Those of Thiersch, Pettenkofer, and Sanderson were made principally on mice, with filtering paper saturated with cholera discharges. The results of these experiments were open to question, because Ranke subsequently pointed out that filtering paper itself, unsoiled, produces injurious effects on mice. Professor Högges, of Pesth, seems to have eliminated this source of fallacy (and, indeed, as would appear, all other) by causing animals to inhale a stream of air carried off from non-disinfected cholera discharges.

Six definite conclusions follow from his experiments; they are quoted by Professor Parkes in the Hygiene Report for 1872. We have to do only with two of them in this place—namely, the second and third. "The principal or the invariable appearance of the injurious action after every mode of introduction of the cholera discharges is a more or less strong inflammatory change in the stomach and intestinal tract. An artificially produced stomach and intestinal catarrh makes animals more susceptible to the injurious action." Now, bearing in mind the lesions which result from gastric and enteric catarrh (they are very fully described by Professor Aitken, "Science and Practice of Medicine," vol. ii. pp. 916–18), a comparison of the result of these experiments with the result of microscopic examination of the cholera intestine is full of interest, and (in my opinion) unsurpassed in practical importance by any observation yet made regarding the nature and cause of cholera.

Professor Lionel Beale gives in carefully elaborated detail the results of his examination of the cholera intestine under high microscopic powers, in his book on the "Nature and Origin of Disease Germs." It is difficult to condense his text consistently with clearness; and the high opinion I entertain of the practical importance of this matter justifies, I think, the reproduction of some of his remarks verbatim in this place. Speaking of the permanent damage done to vessels and tissues by disease, he says (page 182):—"Wasting of tissue, usually circumscribed, which not unfrequently follows a bad attack of contagious fever, is a direct consequence of the vascular changes which have been referred to. . . . . If such changes have affected an extensive tract of small intestine, it will be seen that the proper functions of this important surface can never again be properly discharged. Although, no doubt, in the healthy state there is a much greater extent of absorbing surface than is really required, it must be obvious that if this be much reduced, as in a severe attack of cholera, the absorbing area will be too limited to take up the quantity of nutriment required to maintain a state of health." And again—"It may be confidently affirmed that such changes as those described could not have taken place in a few days. There is sometimes evidence of alterations which must have been going on even for weeks before death. The kind of degeneration which has been observed obviously requires some time for its completion, although I have not the data to enable me to fix the precise period.

The time requisite for the changes which occur in blood-clots can be ascertained accurately in some cases, and we have no reason for inferring that the red blood corpuscles could be much more quickly disintegrated in the tissue of the villi, or hæmatoidin crystals formed in a shorter time, than in other situations.

"In many of the specimens of small intestine from cholera cases I have found villi in every stage of wasting—the villus in which the change has only just commenced, and villi of which all that remained were little stunted elevations projecting slightly from the surface of the mucous membrane. Had the patient recovered, I am of opinion that new villi would have been formed, and to some extent have replaced those which had been removed. As I have already remarked, in a given area of intestine in cholera cases there are fewer villi and fewer Lieberkuhn's follicles than in health; and I believe that many victims of this disease had been suffering from degeneration of their villi for a long while before the occurrence of the attack of cholera which proved fatal. The constant introduction of bad food and water, and in many instances terrible deficiency of food of all kinds, will sufficiently account for the marked changes which have been described. I think the evidence advanced in favour of the view that *healthy* persons die of cholera is defective and inconclusive, and believe, if this scourge is ever to be prevented, it will be by constant and unremitting attention to the food and general habits of life of the poor, not merely while we are appalled by the actual presence of the scourge, but at all times. There seems reason for thinking that it is possible by good management to prevent people from being attacked by cholera. Have we not reason to conclude that much may be done to prevent people from *becoming subjects for the cholera poison?* We know, alas! that we can do little, sadly little, to cure those attacked—though, perhaps, very much to improve the health of those liable to attack.

"We might thus mitigate to some extent the severity of the disease, and improve the patient's chances of recovery. I should waver in these views if I should obtain but a single specimen of small intestine in which I could not demonstrate diseased and altered villi. So far, I have found them, without exception, in every case I have examined; and hence I have been led to form the above opinion, which becomes stronger as I work on."

Now, these experiments of Högges, which show that the principal or invariable effect of the cholera poison is an inflammatory change in the stomach and intestine, and that a pre-existing stomach and intestinal catarrh renders animals more susceptible to the action of this poison ; and Beale's microscopic examinations of cholera intestine, which led him to believe that healthy persons do not die of cholera, but that the degeneration of the intestinal villi (which is usually the result of inflammatory action) observed after death from this disease, must have existed for some time previous to exposure to the cholera poison, appear to confirm each other in a striking manner. And their comparison cannot fail to suggest that the remote and proximate causes of cholera may, and probably often do, co-exist under such circumstances as to render their separate recognition difficult, if not impossible. And further, taken together, they seem to afford a plausible explanation of the discrepancy observed among the opinions of those who have had large practical experience of cholera, as regards the influence exerted by drinking water in causing this disease.

We know that many substances likely to be ingested with drinking water will cause those conditions which Hőgges found to predispose to the action of cholera poison, when produced artificially by him. And it seems justifiable to conclude from these experiments, confirmed as they are in so remarkable a manner by Beale's observations, that an enteritis caused by the ingestion of certain substances with drinking water predisposes to, or is one of the remote causes of cholera. We will therefore briefly consider here what substances likely to be ingested with drinking water will produce this predisposing condition. Professor Aitken states (*opus cit.*) that chronic catarrh of the intestines is the result of the following conditions :—" (1) Obstruction to the hepatic circulation, and especially to the escape of the blood from the portal vein. (2) Lesions of the respiratory and circulatory organs, which cause obstruction to the emptying of the *vena cava.* (3) Disturbance to the external circulation, such as accompanies severe inflammations of the skin, as from burns or from sudden exposure of the skin to low temperatures; as by travelling in cold weather. (4) As a result of peritonitis. (5) As a result of mental excitement. (6) As the result of local irritations from the use of purgatives; of some kinds of vegetable food; the passage of undigested decomposing substances from the stomach into the intestines ; or the retention of fæcal masses. (7) A symptom of such lesion as lardaceous disease of the intestine, of septicæmia and of cholera (simple and malignant)." Now, a remarkable similarity cannot fail to be observed between these conditions, which result in what we see to be a remote cause or predisposing condition of cholera, and the conditions—climatic, endemic and idiopathic or personal—under which cholera is observed to occur, the variations in which give rise to so much etiological dispute.

And this similarity presents, I think, a most interesting and hopeful view of the problem of the nature and cause of cholera, if etiologists will but only accept the doctrine that *specificity of a disease process does not necessarily imply specificity of the cause which inaugurates it.*

Very probably, as in the case of enteric fever, all the theories on the nature and cause of cholera are *right* as regards the facts on which they are based, but *wrong* in their profession of comprehensiveness, of being a complete explanation of all the phenomena of the disease, and in their denial that any phenomena occur in it other than those which they explain. Do not the *obstruction to hepatic circulation*, the *vicissitudes of external temperature*, the *mental excitement* above referred to, as causes of a known predisposing condition, explain many otherwise mysterious phenomena of epidemics of cholera? But, be this as it may, we have in this place only to deal with the conditions marked (1) and (6) as quoted above from Professor Aitken. I have before, when treating of enteric fever (page 213), pointed out how putrid organic matter ingested with drinking water may interfere with the integrity of the hepatic function, producing obstruction to the circulation in the liver, and sometimes intestinal and pulmonary lesion—the proneness of these two tracts to suffer thus being experimentally proved, and shown to be due to the difference between the arterial and venous pressures being in them less than in any other situation. Thus, drinking water containing putridity will produce the first condition—namely, hepatic obstruction; and perhaps, under favouring circumstances, the pulmonary lesions referred to in the second condition, and perhaps, also, the septicæmia referred to in Dr. Aitken's seventh condition. Then a multitude of ordinary water impurities may be set down as likely to produce Dr. Aitken's sixth condition—putridity here again, also, as well as the various substances noticed in the second chapter of the first part of this Essay, grouped under Excess of Normal Constituents and Presence of Foreign Matters, being likely to produce such an intestinal irritation.

It is unnecessary to describe here again in detail what these impurities are; it will suffice to say that most of them, when existing in large quantities, or when their action is continued for a considerable period of time, will, under favouring conditions, produce that inflammatory change in the intestinal tract, which appears on good authority to be a condition predisposing to cholera. Thus, as exerting this influence, drinking water must be in this way considered as being one remote cause of this disease.

I have said there are grounds for believing that this remote cause may merge into a proximate cause. What facts, then, can be adduced in support of the view that drinking water exerts an influence as a proximate or direct originating cause of cholera? There is a good deal of such evidence on record, but I shall quote here only a few of the more remarkable cases.

I am aware that if drinking water have any influence in originating cholera, every fact regarding the disease must more or less bear on and elucidate this influence. But I elect to deal with the question of water origin in this restricted manner, noting only the points which bear directly on it, and only the most salient of these, because I do not fail to perceive that one of the causes of our ignorance of cholera is, that persons who contribute evidence generally attempt to do too much. They endeavour to present a comprehensive view of the nature of the disease, forgetting that the worker in the mint is dependent on the raw material furnished by the worker in the mine. This is a tendency inherent to the mind, and must be carefully guarded against. "The bondage under which all true science lies to fact—the necessity of groping among the details of little and common things—this is a hard lesson for the human intellect to learn, conscious as that intellect is of its own great powers, of its own high aims, of its own large capacities of intuitive understanding. But it is a lesson which must be learned. There are no short cuts in Nature. Her results are always attained by method. Her purposes are always worked out by law. So must ours be" ("The Reign of Law").

I shall not, therefore, in this place attempt to explain or even connect the facts here adduced.

We shall consider what experience says in reply to the following five questions :—

(1) Excluding what seem to be epidemics of cholera (that is, those aggregations of cases in which contagion can be clearly traced between primary and succeeding cases), do we find that in aggregations of sporadic and endemic cases, the portion of a community attacked by cholera, and the portion of the same community which escapes the disease, correspond to the portions of the same community which exist under dissimilar conditions as regards their drinking water supply ?

Here all possibility of contagion being contained in the drinking water must be excluded; and it seems a matter of no small difficulty to obtain evidence on this point in which this source of fallacy is absolutely eliminated.

The mass of evidence relating to the (erroneously so-called) *water origin* of cholera cannot be used here, because it really applies to propagation by contagion, and not to the origin of the disease, such as the outbreaks now so well known as those of "Horselydown," "the Broad Street Pump," "Wandsworth," "West Ham," &c. At first sight, the evidence furnished by the Registrar-General on the London outbreak of 1853 would seem to be a case in point.

It was shown that the districts supplied by the Lambeth Water Company and the Southwark Water Company suffered much less from cholera than the districts supplied by the latter company alone.

And Dr. Snow, in his work on cholera published in 1855, showed by an elaborate inquiry that in the districts supplied partly by the Lambeth Company and partly by the Southwark Company, the attacks were chiefly in the houses supplied with the latter water. The mortality was 37 per 10,000 in the former, and 130 per 10,000 in the latter.

But when it is remembered that the Lambeth supply came from Teddington Lock, and the Southwark from the Thames at Vauxhall Bridge, and that, therefore, this last was liable to pollution by the sewage of the metropolis, it is seen that, while this evidence may perhaps be applicable to the predisposition to or propagation of cholera, it is altogether vitiated as regards the *origination* of the disease.

In the same way many other pieces of evidence, even more apparently promising than this, break down on a closer examination. The best evidence I can find on this point is a case which has come under my own notice, but was not under my care.

Some time ago, at a flower-show in India, two ladies partook of refreshment from the public stall, in common with many others present. These two ladies then sat in conversation apart, and one of them told a khitmutgar to bring her a glass of water. There happened to be at the time no water at the refreshment stall. The man went, and brought water, as was subsequently discovered, from a tub which had contained white paint, but into which clean water had been put just before, for watering flowers.

The water when brought looked clear. This lady, Mrs. G., drank the water; and the other, Mrs. B., told the khitmutgar to bring some for her.

He did so, bringing it from the same source, and she also drank of it. No other persons drank this water. In about half an hour both ladies began to experience uneasy sensations, and reminded each other that the water they drank had a peculiar taste, which they had not taken much notice of at the time. Both had to retire; and in the evening each sent for her medical attendant, who on inquiry discovered the source of the water, as above given

In a few days Mrs. G. was suffering from malignant cholera, diagnosed as such by several experienced practitioners; and Mrs. B. from severe dysentery.

Mrs. G. was attacked with all the symptoms of the malignant Asiatic cholera—vomiting, coldness, rice-water stools, &c.—but happily recovered. The dysentery in the other case was quite unmanageable, drifted into the chronic form of the disease, and proved fatal in eight months. Now, this case of cholera, although very nearly so, is not satisfactory either. Its weak point is, of course, the possibility of an error in diagnosis, unconfirmed as this was by the absence of a fatal issue and post-mortem examination.

And, even admitting that the diagnosis of cholera was correct, it may be said that the drinking water was only a predisposing cause, bringing into play some pre-existing absolute cause of origin—climatic, epidemic, or other. But this is again replied to by the fact that the disease was not epidemic in the locality at the time, and no other persons were attacked.

Also the rapidity with which disease followed the ingestion of this suspected water in both cases seems to go against the possibility of its influence being a predisposing one.

These cases seem also to confirm that doctrine which I have so often stated, but which I cannot too often repeat or too forcibly impress—that *specificity of a disease process does not necessarily imply specificity of the cause which inaugurates it.*

On the whole, I think it is not possible to answer from present experience the question which heads this section. The reason of this is that, in outbreaks of cholera, it is nearly impossible to be sure that propagation by contagion is not the influence by which drinking water may cause the disease; and in isolated cases the evidence is replied to by saying that the influence exerted by drinking water may be a predisposing, not an originating influence, and that in those cases the absolute originating cause was some other condition, which produced its effect only when the predisposing influence began to co-exist with it, and brought it into play.

In making this statement I believe I interpret correctly the views entertained on this point by the majority of those who have had much experience of malignant cholera.

Another phase of this question is the different habits of the section of a community attacked by cholera, and that which escapes the disease.

It is true that the poorer classes in all countries suffer most from this scourge. Can this difference in susceptibility be connected with their less cleanly habits, and inattention to the purity of their drinking water? It may perhaps appear from Indian experience that this may have something to do with it. But we often find that the higher castes, Brahmins and others, suffer severely in cholera epidemics; and in European cities, where the water supply of both classes is nearly identical, we find that the same difference of susceptibility exists.

(2) Can any disparity of conditions as regards drinking water be ascertained to be constant between localities in which cholera is endemic and localities which are cholera free?

Some diseases are proved to be originated by drinking water on such evidence—as, for instance, goitre, some diseases of the skin and of the bones.

In these cases a very marked difference is found to exist between either the composition of the water or the geological formation of the endemic zones of these diseases, and localities free from them.

I think it must be admitted that attempts to connect any constant peculiarity in the composition of drinking water with cholera endemicity have failed, except as regards general organic impurity, and this may act as a predisposing and not as an originating cause. As regards the geological formation of endemic areas of cholera (which is included in the above question) I may sum up what is known on the subject by referring to the last Report of Messrs. Lewis and Cunningham, which they call "Cholera in Relation to certain Physical Phenomena," published as an appendix to the Thirteenth Annual Report of the Sanitary Commissioner with the Government of India. That Report states (page 168): "It may be observed at starting that the history of cholera all over India presents one common feature, and that is, that it can only be fairly regarded as endemic in such localities as manifest a close resemblance in the more superficial layers of their geological formation. This feature, it will be found, is more conspicuous than any of the other physical characters to which we shall have occasion to refer." The Report goes on to point out the undoubted fact, which has often before been demonstrated—namely, the influence of an alluvial soil on the prevalence of cholera. At page 169 we read:—"Taken generally, it may be stated that the more the soil of a district in India approaches to the character here described, the more likely it is to be one whose inhabitants are more or less constantly liable to be affected with cholera."

This Report professes to deal with this question only with reference to Pettenkofer's subsoil theory, which relates to the driving out of miasmata from the soil, but does not refer to any influence of drinking water. Nevertheless, the facts here given may be used in their indirect reference to our present question.

It is remarkable that no geological formation which can affect the composition of drinking water in any definite way is found to correspond uniformly with endemic cholera areas. Alluvial deposits do not produce any constant definite impurity of water. The composition of one water passing through one alluvial bed may be very different from that of another water passing through some other bed of an identical deposit. But even if all alluvial deposits produced a constant definite impurity in water passing through them, this would be of little importance, because, as a reviewer of this Report remarks (*Indian Medical Gazette*, May 1, 1878), "it would be easy to prove that the state of such a soil, and of every discovered condition associated with it, is the same at times when and in places where cholera prevails, and when and where it does not prevail; and it is impossible in times of prevalence to distinguish between the soil and other conditions affecting certain areas, or parts of areas, where the disease prevails, and others where it does not. Whatever influence an alluvial soil exercises must, therefore, be secondary or adjuvant, and certainly not direct or essential. Indeed, all general influences fail as applied to cholera facts, because of its partial incidence—a partial incidence which is true both as regards time and space. This partial incidence, though concealed by averages of years or communities, is a fundamental fact of cholera, and must be held well to the front in all investigations concerning it." Thus, we see any evidence under this heading must be checked by the question next following.

(3) In those localities in which cholera is not persistently endemic—that is, in which endemicity of the disease alternates with freedom from it—are these periods of endemicity and freedom characterized by any constant difference in the character of the drinking water supply?

I cannot find any evidence on this point which refers solely to origination. It would seem that most inquirers restrict the meaning of the *water theory* of cholera altogether to its propagation. Yet the fact of cholera outbreaks occurring, sometimes severely, in certain localities, and at other times these localities being comparatively free from the disease, whilst the supply of drinking water remains the same, must, I think, show that drinking water influences very little the origin of the disease, as well as its propagation by contagion. The Report of the Sanitary Commissioner with the Government of India for 1872 states*—"There were nearly 60,000 deaths from cholera in 1869; in 1870 there were 107; in 1871 there were only 19; in 1872, 1,592. Is this extraordinary difference to be explained by supposing that in the first of these years the water supply over this great area was generally poisoned by cholera evacuations, which it escaped in other years." Now, the same question may be put regarding origination which is here asked regarding propagation—Is it to be supposed that in 1869 the drinking water supply of the central provinces of India (the locality referred to) contained some constituent capable of originating cholera, which was absent in the years 1870–72; although in all the supply was apparently the same as regards source,

* With reference to the "Central Provinces."

storage, and means of distribution? The Report referred to goes on to say—"The same difficulty presents itself in the history of individual stations. Last year, for example, after a long period of exemption, both Dugshai and Kussowlie suffered severely from cholera; while the adjacent hill station of Subathoo, though at a much lower elevation than the other two, suffered only to a slight extent—a result the more unexpected, because it has suffered several times when both Kussowlie and Dugshai escaped, yet the water supply of the one is just as liable to contamination as that of the others. Similar facts can be adduced regarding stations in the plains. At Allahabad, Cawnpore, and Lucknow the wells are just the same construction as those at Sealkote, Nowshera, or Mooltan. The first-named stations are subject to frequent and severe attacks; the others have, in comparison, rarely been attacked, and never with any severity."

I go on to quote the next paragraph of the Report, which applies perhaps more to our second question than to the present:—"Or let us examine individual outbreaks in relation to this question. The water theory was founded on the fact that among masses of people living in London, side by side, and apparently subject to the same influence, except in this one matter of water supply, those supplied from one source suffered far more than those drinking from another. Is there any evidence of this nature? On the contrary, there is no instance in the whole course of last year's epidemic in which any special severity of the disease among any particular section of the community can be associated with the use of a particular water used by that section. In Lahore, for example, there are 1,400 wells; in Anarkullee (a suburb) they are also very numerous; but there is no suspicion that persons using any one of these wells suffered more than others, much less that the severity of the outbreak was confined to those drinking from particular wells. Both here and in the city of Peshawur, where also the wells may be numbered by hundreds, the disease was spread generally among the people.

"In these places, and in many other instances which might be cited, we find numbers of people drawing from many different sources, and yet all suffering to much the same extent—a result which is hardly reconcilable with the idea that the attacks were caused by any *specific poison* accidentally introduced into the water supply." I think the same conclusion may be arrived at from these facts with reference also to a *non-specific* originating cause.

This may be taken as a sample of the recorded evidence relating to this point. It is seen, from the last sentence of the quotation, that propagation by contagion is not excluded; therefore, as far as origination only is concerned, we must receive such evidence with a reservation.

But, as I have said, the facts here recorded must to some extent bear also on the origination of the disease.

However, the most absolute proof that drinking water had no influence in the origin of one or more endemies, does not deny that other endemics, at other times, in other places and under other circumstances, *may have been* influenced by this cause. I am not able to find any absolute affirmative evidence on this question.

(4) Do the past cholera endemicity of any localities, and the present freedom from cholera of the same, or the reverse, correspond in time and other circumstances with an improvement or deterioration of their drinking water supply?

This is a difficult point to determine, inasmuch as improvement or deterioration of drinking water supply generally, indeed practically always, proceeds *pari passu* with improvement or deterioration of other sanitary conditions. One illustration will suffice to show how real this difficulty is.

As is well known, Calcutta, which may be taken as a typical endemic locality, has within recent years been much improved as regards sanitation. Before 1870 there was no wholesome water generally available to the inhabitants.

In the first quarter of that year 800 hydrants were erected, at intervals of 200 yards, along its public roads and lanes, liberally supplied with pure filtered water.

The following year was marked by a wonderful diminution in the mortality from cholera:—

1864	4,000	1868	4,186
1865	5,078	1869	3,592
1866	6,826	1870	1,563
1867	2,270	1871	800

It is seen from these figures that a diminution had commenced prior to this supply of pure water. And further, that the town was liable to great and sudden variations in cholera mortality, arising perhaps from some widely-spread cosmical influences; as in a comparison of the mortality in the years 1866 and 1867, which could not have been caused by water supply.

However, the year 1871 was not a year of general immunity in the province; and the diminution in that year may perhaps be fairly attributed to improved local sanitation. Now, if no other sanitary improvement had been effected in the town but this pure water supply, it would be very reasonable to assign this as the cause of the decrease in the cholera mortality in 1871. But many other sanitary improvements took place about the same period.

For instance, in 1866–8, underground main drainage was completed in the southern part of the town, and in 1869 it was extended to the northern division.

Now, it is impossible to overlook that this improved drainage materially weakens the position of one who would impute to the pure drinking water supply the sole causation of the comparative cholera immunity of 1871.

And this is so in all the evidence available on the above question. But I instance Calcutta here for a further reason. Almost by accident this source of fallacy was to a great extent removed, and the evidence furnished by this case becomes thus of some real practical value. The supply of pure water had to be curtailed, and the cholera mortality increased.

The Report of the Health Officer of Calcutta for 1876 says :— "Soon after the use of pure water began, the insufficiency of the supply was felt, and in April, 1872, it was necessary to stop the flow between 6 P.M. and 5 A.M., a very scanty and uncertain quantity being obtainable at the hydrants north of Muchoa Bazaar between those hours. There has been also a diminished flow in the street pipes between 11 A.M. and 3 P.M. from reduction of pressure. The demand for pure water has rapidly outgrown expectation and provision, and the desire to spread its benefits more widely has led to their being curtailed in quarters where at first there was no restriction." In the next and succeeding years the cholera mortality increased :—

1871	800	1874. . . .	1,329
1872	1,142	1875	1,726
1873	1,155		

But then, again, as regards the *origin* of cholera, we have in this part of the evidence, as in other parts, the same liability to confound propagation with origination.

Dr. Payne, in the Report just quoted from, devotes a section to "Cholera produced by Bad Water;" but he here, of set purpose and for obvious reasons, includes origination and propagation under the term *production*, ignoring the distinction observed in the theme of this Essay—thus rendering the evidence given in this part of his otherwise very valuable Report useless for my present purpose. He says:—"I desire to say nothing of the instrument by which filth operates in the production of cholera. Whether there be poison taking particulate form as living germs, or what not, is a question which is to my mind yet unanswered; while the intimate connection of cholera with filth is open to no more doubt than an axiom in geometry. I am anxious to do no more than set forth this connection, and the abundant consequences of it in Calcutta, with the preventive efficacy of a good water supply." And again: "I am not concerned to advocate or refute the doctrine known as the 'water theory' of cholera, under which it is contended that the spread of the disease is to be explained by special contamination of drinking water with germs or other particles passed from the bodies of infected persons, but merely to claim for these ponds the power which no one denies to excremental filth in this form everywhere. To show, on the evidence of chemical science, that the tanks are loaded with such potent filth; and, on the evidence of facts, that their effect on the public health is in strict accord with the forebodings of science concerning them, and with universal experience in other places."

Thus, while evidence replying to this fourth question renders the connection between drinking water and the *production* of cholera as indubitable "as an axiom in geometry," it throws little light on the connection between drinking water and the *origin* of this disease. Perhaps the comparison between the cholera proposition and an axiom in geometry is more apt than Dr. Payne intended, when one reflects on the difficulties experienced in endeavouring to prove such an axiom.

There are not a few etiological axioms which experience forces us to accept as true, but which are absolutely unprovable.

(5) Do the beginning and decline of endemic outbreaks of

cholera correspond with any meteorological conditions likely to influence the character of the drinking water supply?

I am not disposed to place much faith in conclusions derived from meteorological observations as at present conducted, when these require the use of delicate instruments and elaborate calculations; but we sometimes observe very plain meteorological conditions, as excessive drought or heavy rainfall, which appear to influence in some way the beginning and ending of cholera outbreaks. I witnessed a very remarkable instance of this at Darjeeling (British Sikim) in the summer of 1876. Happening to be intimate with the managers of many of the tea plantations in the district, I availed myself of the opportunity of collecting details of the outbreak as it appeared in their gardens, some of which bear very forcibly on the present question. This outbreak furnished the subject of an exhaustive Report by Dr. Purves, then Civil Surgeon of the station; extracts from which are given in the Report of the Sanitary Commissioner of Bengal for 1876. Little, however, is there recorded of the meteorological condition to which I refer.

"The mortuary returns show that 1734 deaths occurred during this outbreak, but Dr. Purves is of opinion that only about one half the actual mortality was recorded."

Cholera may be said to be endemic in the belt of terai, at the foot of these outer hills of the Himalaya. In January the disease extended from this endemic locality to two gardens close to each other on the southern side of the Balasun river, near Punkabaree. For some time the disease was circumscribed to the immediate vicinity of these plantations. About the 15th of March cholera appeared at Punkabaree. In April and May the disease spread rapidly up the hills. "Early in June," says the Commissioner's Report, "there was a regular cholera wave over the whole district, lessening in the plains, but rapidly increasing in the hills. In this month the disease attained its maximum intensity and most fatal type. Most of the gardens in Darjeeling were attacked, and coolies were getting panic-stricken, and flying in all directions, roaming about the forests, carrying their sick and dying with them. Of the 910 deaths recorded this month, 509 occurred in Kurseong, 199 in Darjeeling, 82 in Kalimpong, and 120 in the terai. With the heavy rains of July a great and general improvement took place."

Referring to Dr. Purves' Report, the Commissioner observes :—"With regard to water supply, which had a special bearing on this epidemic, he says that some of the planters informed him that, in their belief, water had never been so scarce before. He ascertained that the water supply was not only inadequate, but was also objectionable, the reservoirs being frequently used for both drinking and washing purposes, ravines, used as latrines, intervening between the springs and reservoirs. He noticed that the habitations of the labouring classes, particularly of the coolies in the gardens, were crowded together; that too many lived in one house; that the houses were surrounded with filth; that conservancy in the coolie lines was limited to sweeping, the refuse and filth being left undisturbed; that the discharges of those who were attacked by cholera were allowed to saturate the floor of the houses, or were thrown outside near the huts."

From personal observation at the time I am able to bear testimony to the correctness of this description. But I must question the correctness of the phrase in the Commissioner's Report, "*with regard to the water supply, which had a special bearing on this epidemic.*"

Was this an epidemic? If so, the water supply had certainly no bearing on it; because the disease spread up the hills, contrary to the direction of the water-courses, and there was no other supply available for the plantations in the valleys between those hills. If the outbreak was an epidemic due to propagation by a specific disease-product carried by the water, the movement of the disease should have been the reverse of what it was. Under these circumstances, I think we must believe (and from personal observations on the spot I do believe) that this outbreak was an extension of endemic localization radiating from the permanent endemic centre in the terai. All the factors of origination were ready to originate an outbreak in the hills when some other condition, of the nature of which we have no knowledge, spread up from the endemic terai and brought these agencies into full play. I believe that this was an endemic, consisting for the most part of originated cases; and I would point out the connection between its commencement and ending, and the drinking water supply as affected by meteorological conditions.

It is observed above, that prior to the beginning of the outbreak there was a remarkable scarcity of water in this district as compared with other years, when there was no cholera in Darjeeling. In this year, in consequence of little rain, there was unusual heat, and, of course, condensation of the water impurities always to be found where a number of coolies are located. It can hardly be doubted that the cholera outbreak had some connection with these conditions. I would now draw attention to a fact not dwelt on in the Commissioner's Report—namely, the remarkable suddenness in the cessation of the outbreak (it can hardly be called "a decline"), and the coincidence of this sudden cessation with a heavy and equally sudden rainfall. This was quite conclusive to me and other medical men at the time, and must have been equally so to any one who witnessed the occurrence, that the outbreak ceased as a result of the thorough washing out of the water-courses and the substitution of a comparatively pure water supply.

It is seen from this slight consideration of these five questions that at present but little trustworthy evidence can be adduced in reply to them, in consequence of the nearly hopeless entanglement of the origin and propagation of cholera in most Reports. I have stated these questions more as indicating a method to be pursued in obtaining information regarding the influence of drinking water in originating cholera, than for the present practical value of replies to them.

Yet, difficult as it is to isolate facts bearing solely on the absolute origin of cholera, there seems to be some dimly indicated probability that drinking water does in some way influence the origin of this disease.

There is a very general impression among medical men that this influence is more than a probability. Some indeed affirm with certitude that drinking water *is the originating cause* of cholera, although actual proof seems so deficient to persons not already prepossessed in favour of this doctrine. Thus, in the Report of the Sanitary Commissioner for Bengal for the year 1876 we read (page 18):—"From the reports of local and special outbreaks which follow it will be seen that there is a large and remarkable concensus of opinion on the point that impure water is the chief of the exciting causes of cholera." And again: "The most ready entrance of the disease into the system is by water impregnated with decaying organic matters. This water soon loses this power when the decaying changes are completed, and reacquires it when fresh decaying matters find their way into it. I do not know any of the phenomena associated with the origin and spread, the seasonal and local prevalence of, and the persons affected with the disease, with which the above conditions are not connected." Most persons will consider this opinion too dogmatically expressed on examination of the evidence supposed to warrant it.

It seems to me an apt illustration of a passage in one of Lord Bacon's Essays—"When the mind is once pleased with certain things, it draws all others to consent and go along with them, and though the power and number of instances that make for the contrary are greater, yet it attends not to them, or despises them, or else removes them by a distinction, with a strong and pernicious prejudice to maintain the authority of the first choice inviolated."

SUMMARY.

To sum up the evidence on this point of the influence of drinking water in originating cholera :—

It cannot, I think, be fairly contradicted that abundant practical evidence proves beyond doubt that drinking water in some way influences the *production* of cholera. But when we come to distribute this influence between the two factors of production—namely, *origin* and *propagation*—we tread on very uncertain ground.

For it is far from generally agreed that the disease can be originated; some maintaining that it is always propagated by a specific contagium. I am forced, from personal experience, to believe most implicitly in its occasional spontaneous or *de novo* origination, as opposed to constant propagation by a specific virus.

Many instances have come under my observation (not before alluded to, because they afford no evidence regarding the influence of drinking water) of solitary cases of cholera occurring in large and fully occupied barracks, untraceable to any cause—the diagnosis confirmed absolutely by life symptoms and autopsy—not a trace of evidence of contagion propagation, either as a cause of these solitary cases or as an effect of them, being perceptible. I am confident that one hot weather on duty at Fort William, Calcutta—the sanitation of which garrison is as near perfection as can be—would shake the faith of its firmest adherent in the exclusive contagion theory of cholera.

If, then, origin be admitted as one factor in the production of cholera, it must be allowed that there is no evidence against this origin being influenced by drinking water—to put the probability in its very lowest terms.

But then we must eliminate predisposition as a further factor in origination. The evidence of drinking water influence in predisposition to cholera seems to me of more weight than that favouring the same influence on absolute origination.

The opinion at which I have arrived on this point (which opinion results from careful study of much more extensive evidence than could possibly be put forward in the limited space of the present Essay) is as follows:—The evidence on the whole seems to indicate that drinking water may be one occasional cause of origin. But we have no reason to think that any agent of which drinking water can be the vehicle has power to act as a cause of cholera origin, absolutely alone—that is, without the co-operation of other, perhaps many other, conditions.

Hence I conclude—

There is evidence favouring the probability of drinking water being a vehicle of agency which may influence cholera origination. But this agency must be regarded as far from being a constant cause of the disease, and as exerting an influence which is only one of a series of factors seemingly necessary for the production of cholera.

[Perhaps I could express the meaning of this opinion more clearly by putting it in a numerical form.

The production (origin and propagation) of cholera may equal 500. The origin of cholera may equal 100. The cases of origin (predisposition and absolute origination) influenced by drinking water may equal 10. Of this, the influence of drinking water in predisposing to cholera may be represented by 7; and the influence of drinking water in originating cholera may equal 3.

It is superfluous to observe that these figures are of no intrinsic value as an estimate of the absolute probabilities. They are merely intended as a convenient expression of my own individual ideas regarding the question.]

CHAPTER VIII.

THE INFLUENCE OF DRINKING WATER IN PROPAGATING CHOLERA.

Is cholera capable of being propagated by contagion or not? Happily, the theme of the present Essay does not require the discussion of this definite question. We here are only concerned to ascertain if there are any facts in favour of the probability that drinking water *may sometimes* propagate the disease; and if so, what the nature of such a hypothetical influence may be.

It must, however, be stated at the outset that the propagability of the disease by contagion is contradicted by many facts of experience; and these are by no means confined to Indian experience of the disease. I shall relate but one such case.

Prof. Dr. V. Pettenkofer relates—"A cholera patient had so soaked his bed that he really swam in rice-water evacuation, and in order to dry him, an attendant raised him, all saturated as he was, in his arms, while a second rapidly withdrew the bedclothes, and replaced them with fresh linen. In the short time which this required, the patient discharged, whilst in the arms of the attendant, some pints of fluid over his arms, trousers, stockings, and slippers. A few hours later the attendant was seen in the same dress, the dejections having dried on his clothes, and these must necessarily have been converted into dust by his movements, and have been inspired by himself and those around him, and yet no cholera case occurred. Such instances, though perhaps not quite so marked, unquestionably occur daily and hourly in every cholera hospital" (*Indian Medical Gazette*, April, 1877, p. 108).

Hence, if the disease be propagated by contagion, it would seem to be so *only sometimes, and under peculiar circumstances*. We pass on to consider the nature of the evidence before us in favour of its being so. "The discharges being liquid, the great bulk of them find their way into the ground, from which the poison may be propagated in three ways—(*a*) by rising into the air as a product of evaporation; (*b*) by percolating into the drinking water; (*c*) by atmospheric dispersion in the form of impalpable dust, after it has passed into the dry state. It is, of course, difficult to establish these modes of propagation by direct proof, but circumstantial evidence and evidence from analogy is so cogent and weighty, that no reasonable doubt can now be entertained regarding these modes of propagation. . . . . The experience of 1866 and 1867 confirms all previous experience as to the propagation of cholera, so well summed up by Mr. Simon in his official memorandum of July, 1866." Although considering "that cholera is little contagious, in the senses in which small-pox and typhus fever are commonly called contagious, he admits it is not less true that all matters which the patient discharges from his stomach and bowels are infective. That the patient's power of infecting other persons is due entirely, or almost entirely, to these discharges; that these, however, are comparatively non-effective when first discharged, but afterwards, while undergoing decomposition, acquire their maximum of infecting power; that if cast away without previous disinfection, they impart their own infective power to other excremental matters; that if they get access, even in the smallest quantity, to wells or other sources of drinking water, they may infect very large volumes of water."* This being the state of the matter—the probability of drinking water carrying the propagating influence of cholera not resting "*on direct proof, but circumstantial evidence and evidence from analogy*"—it is obviously undesirable to devote space here to the discussion of the question. I shall content myself with stating briefly some of the experience from which it is concluded that drinking water MAY influence the propagation of cholera, if other circumstances co-operate. First, we have the *à priori* evidence that cholera discharges may pass into drinking water, and by this means be brought into contact with another intestine; and that cholera discharges administered to animals in food, or injected to the circulation, produce injurious results. The objection to this evidence used as a basis for conclusion regarding *absolute propagation* of cholera (the generation of *like by like*) is, that the same "*definite*" result is produced whether the agent introduced is "*specific*" or not.

"It appears from these results that the dejections of persons suffering from cholera, and also those of persons in good health,

* Professor Aitken, "Science and Practice of Medicine," vol. i. p. 671.

when injected into the veins, act in some cases as a poison—have the power of producing a definite effect on the intestinal mucous membrane, resulting in a disorganization of its substance. The symptoms and pathological changes induced by both varieties of material, the choleraic and non-choleraic, present no differences" (Lewis and Cunningham, "Researches on the Nature of the Agent or Agents producing Cholera," 1874).

However, *quantum valeat,* these experiments do yield some probability that drinking water may influence the propagation of cholera by acting as a carrier of hypothetical *contagium*.

Secondly, we have evidence resulting from actual experience of epidemics. It is unnecessary to refer here in detail to the many facts observed during cholera epidemics which seem to warrant the conclusion that drinking water influences the propagation of the disease. They have been frequently published and fully discussed.

A very complete resumé of this part of the evidence is to be found in the recently published "Notes on the Hygiene of Cholera for Ready Reference," by Surgeon-General C. A. Gordon, M.D., C.B., A.M.D., chap. xcix., 'Water in relation to Cholera.'

Added to this we have the varied experiences related in the Annual Reports of the Sanitary Commissioners with the Government of India, the general conclusions drawn from which usually seem to coincide in some remarkable way with the previously entertained views of whoever the Commissioner may happen to be. Indeed the majority of the evidence relating to the propagation of any disease may be taken as favouring at the same time very opposite views, "according to the particular view entertained *as to what the tests of vitality are*" (Lewis and Cunningham).

I shall here refer to but one recent Indian epidemic of cholera which has come under my own observation.

Bhagsu, the military cantonment of Dharmsala, a station situated in the outer Western Himalaya, 7,000 feet above sea-level, is garrisoned by two companies of the European infantry regiment stationed at Jallundar. In August, 1875, I was ordered to proceed to Bhagsu, as there was but one medical officer there, and cholera was in the neighbourhood. The 1st Goorkha Regiment, whose lines are situated at the foot of the Dharmsala hill, were suffering from the disease. One case had occurred at Fort Kangra (five miles off), July 16, which recovered. A strict quarantine was at first kept up as regards these two places and Bhagsu.

During the latter half of July and in August, before cholera appeared here, there were twenty-four admissions for diarrhœa, in a strength of 121 men—that is, about 20 per cent. of the entire garrison had diarrhœa. One of these cases was accompanied by vomiting. Hitherto diarrhœa was unusual in this garrison. On the 8th of August quarantine was relaxed, so as to allow the men to walk about the vicinity of the upper station. On the 10th the quarantine was broken by a boy bringing milk: he was seized with cholera on the road. To this the outbreak which commenced on the 12th was attributed.

"The first man attacked" (I quote from the Sanitary Commissioner's Report for 1875) "had eaten pork, and also, it is said, drank milk brought up by a boy who was taken ill of cholera on the road when bringing it. Two men of the detachment are reported to have drunk of this milk; one was attacked, and the other felt no ill effects (details are not given as to where the milk came from, in what sort of vessel it was brought, when it was drunk, &c.)." With reference to the latter points I can say that not only had the boy who brought the milk himself cholera, but it was brought from an infected village (either Mundi or Jawala Muki, I forget which, but it does not matter, as there was cholera in both) and he brought it through the Goorkha lines. It was carried in an ordinary bamboo chonga, and was drunk on the evening of the 10th. On the 13th of August the first case of cholera was admitted, and it proved fatal that evening. On the next day, the 14th, there were four admissions, three of which died during the day. There were no more cases, and the diarrhœa from which the detachment suffered at once declined on going into camp on the 15th. Now, are the phenomena of this epidemic for or against the doctrine of contagion propagation by drinking water? I believe they are in favour of it, although no less an authority than Professor Pettenkofer instances this case as against propagation by contagion. But Pettenkofer was many miles away, and I was on the spot, and suffering there severely in my own person from diarrhœa and vomiting. The other medical officer had no vomiting, but had diarrhœa, which was not checked for some months. Pettenkofer in his "Nine Propositions" brings forward this Dharmsala epidemic (including the Goorkhas) to prove that because the attendants on the sick were not attacked in greater proportion than those not brought into contact with them, therefore cholera is not propagable by contagion; and the Sanitary Commissioner reasons in the same way. They give a *major* proposition and a *conclusion*, but I confess I fail to catch the *minor* proposition which connects them, although these writers must consider it very obvious, since they do not think it necessary to enunciate it.

If it is anything like this—*But attendants in the sick-room are more likely to drink water contaminated with the patients' discharges than persons outside*—I must distinctly contradict it; I believe the very contrary is the truth.

It is an argument perhaps against infection by emanations from the breath or person, but certainly not against the influence of drinking water in propagating the disease. There are other arguments brought forward against propagation in this outbreak, but such reasoning is merely waste of time. If we are to ascertain anything with scientific accuracy regarding these questions, it will be with the scalpel, microscope, or test-tube. The truth cannot be ascertained by reasoning on the phenomena of epidemics. Exactly similar in kind is the surgical question—Is gonorrhœal conjunctivitis a constitutional affection, or is it a local lesion propagated from the urethra? In every case of gonorrhœal conjunctivitis the factor of propagation is present and ready for action, but this does not prove that the disease is always propagated. Nay, further, if in a dozen cases of gonorrhœal conjunctivitis we could actually prove contact, this would not prove that in none of these cases the eye affection may have been constitutional. In every epidemic of cholera the factor of propagation is, in like manner, present, and mere pointing out this presence, and showing that it may have been swallowed, does not in the least affect the ultimate question. Although I have no proof, in the scientific sense of the word, I am convinced of the propagability of cholera, and that drinking water does influence its propagation by being one of the vehicles of contagion. With reference to Professor Aitken's remarks (*op. cit.*, vol. i. p. 669), "it is curious that in India, the birth-place and head-quarters of the disease, the doctrine of contagion is almost universally disbelieved in by our professional brethren," I would say that, although I have seen many cases of what I believe to be originated or spontaneous cases of cholera, my Indian experience is very far from causing any doubt in my mind as to the contagiousness of the disease. I believe in *occasional* origination and *occasional* propagation. It may be, and is, a fashionable scientific generalization to say that the two beliefs are incompatible; but it is most unphilosophical to deny the possibility of both causes of production.

Any one who, on his experience of spontaneous cases, founds a doubt of propagation, should remember that facts of origination do not bear on the possibility of propagation, and that facts referring to propagation have nothing whatever to say to the possibility of origination.

Nor do the undoubted facts of *immunity* deny propagation by contagion. "Those medical men who thought that they saw in this immunity the proof that cholera was not transmissible, forgot that the same immunity, the same resistance, more or less, is found in all diseases, without exception, reputed to be the most transmissible or contagious. It is found to exist with regard to the plague, the yellow fever, small-pox, scarlatina, &c. If it were otherwise, if all these diseases were transmitted by the mere fact of their being transmissible and the subjection of persons to their influence, the human species would long since have disappeared from off the face of the globe. Happily, it is not so; and a well-balanced frame* in the majority of cases opposes an efficacious resistance to all these diseases" (Proc. Intern. Sanitary Conf., p. 122).

There is one other point to which I would refer before leaving this subject—namely, a form of pseudo-propagation of cholera by drinking water; not propagation by carriage of contagion, but true propagation of a predisposing cause—to wit, *entozoa*. I stated in a former chapter that the entozoal form of diarrhœa was almost the only form of this affection which could be satisfactorily traced to true propagation. It only remains, therefore, to show that this is a predisposing cause of cholera. In the outbreak of cholera at Darjeeling in 1876, to which I have before referred, this was very apparent. My friend Dr. Purves has noticed it in his Report on that outbreak, and the Sanitary Commissioner quotes thus from his Report:—

Alluding to the widely spread diarrhœa which ushered in the graver disease, he says: "At this time also, and when diarrhœa began to assume choleraic symptoms, cases which were reported as cholera, but which were not cholera, and yet appeared to be like it in certain particulars, prevailed very commonly. These proved to be cases of intestinal worms (Ascaris lumbricoides). An

* Can this "*well-balanced frame*" be the condition hinted at in note on page 127?

interesting account of the symptoms which this disease presented was submitted by Dr. Purves, but it would be out of place to reproduce it here. With regard to the cause he says: 'It is no doubt due to the drinking supply. Water taken from the ravines, which may at the same time be used as latrines, easily accounts for their wide distribution. When the tenacity of life of the ova is considered, and it is borne in mind that one female round worm is capable of producing some sixty millions of ova, there seems a grand field for their propagation in these hills.'"

He may have put it much stronger that these parasites must be due to drinking water, because in this very outbreak I have seen Brahmins, Rajpoots, the Bazaar Kyahs, and others who have never eaten meat, suffer severely from them. They were both passed by stool and vomited. I am inclined to think this point deserves more attention than it has hitherto received.

SUMMARY.

It can hardly be doubted that cholera is in some way *transmitted* from one individual to another. There are grounds for believing that this transmission may be effected both by *infection* and *propagation*. If miasma or emanations be the agents of transmission, there is some slight probability that drinking water may be one carrier of these, founded on the presumption that these agencies may be absorbed by water, deduced from such facts as, that, if rain be collected directly falling over densely populated districts, it is found to contain organic impurity. If actual propagation by contagion be the means of transmission, it would seem that drinking water may carry it, by washing specific material out of a soil saturated therewith; as it is an established fact that, in India at least, "the soil on which an assemblage of men (soldiers or others) suffering from cholera has encamped may transmit the disease." (Prof. Maclean, M.D., C.B., "Report of the Royal Commission on the Sanitary State of the Army in India," vol. i. p. 114, Lond. 1863).

Further, if contagion be admitted, there is a high degree of probability that the carrier of contagion may pass directly into drinking water, and be thus introduced into the organism.

The International Sanitary Conference of Constantinople in 1866, regarding the question, "Independently of the atmosphere, what are the other vehicles of the choleraic principle?" concluded that "facts observed in England seem to place it beyond doubt that water—either soiled by matter proceeding from choleraic dejections, or contaminated by the morbific agent diffused in the atmosphere—may be the means of introducing this agent into the human system." And the Conference of Vienna in 1874 replied unanimously in the affirmative to the question, "Can cholera be propagated by drinks, and especially by water?"

I therefore conclude that—

Without accepting any hypothesis as to what the nature of the propagating agent of cholera may be, the balance of evidence seems to favour the view that such an agent may exist; and further, that there is probability of its being a substance capable of carriage by water.

PART III.

GENERAL SUMMARY AND CONCLUSION.

CHAPTER I.

GENERAL SUMMARY.

In the chapters of the foregoing part of the Essay we considered the questions—which are secondary to the immediate subject of the theme—of whether or not, or to what extent, drinking water influences the origin or propagation of diarrhœa, dysentery, enteric fever, and cholera. Having arrived at some definite conclusions regarding this series of questions, we now return to the primary subject of the theme—namely, the INFLUENCE of drinking water in originating or propagating these four diseases—that is, in producing the effects we have just been considering; applying here to each individual case the general principles which were reviewed in the first part of the Essay.

The influence of drinking water in originating diarrhœa.

Regarding this question the following conclusion was arrived at (page 164) :—That "drinking water originates diarrhœa by disturbing the innervation or circulation of the intestinal canal, producing muscular contraction or increased afflux of blood. This irritation and congestion, either simple or passing on to more complex results, is often merely a mechanical or non-vital process; but must also be frequently referred to chemico-physiological action —a combination or interaction of physical and vital forces, of the nature of which we have no accurate knowledge." Thus it appears that we make very little progress in our investigations regarding the part played by drinking water in the etiology of these diseases, before we find ourselves beyond the pale of scientific verification.

Whether beyond or not the *possibility* of such verification it would be presumptuous to say. We may maintain (but, indeed, I doubt that even this can be scientifically proved) that the simplest process of origination of the simplest form of diarrhœa is absolutely understood, being due to a non-vital contraction brought about by a mechanical stimulus.

Thus far our vaunted physics may illumine us, but surely at present no farther. The influence which originates diarrhœa, dependent on the presence of parasites in the intestinal canal, is probably to be referred in many instances, but not in all, to mechanical irritation. As regards the more complex forms of diarrhœa caused by the ingestion of organic matters, either fresh or putridinous, with drinking water, we are not in a position to describe the influence of their origin more definitely than to refer it generally to chemico-physiological action.

The influence of drinking water in propagating diarrhœa.

Regarding this question the following conclusion was arrived at (page 170) :—That "the only form of diarrhœa regarding which we can be certain that its propagation is due to an agent which is carried by drinking water is that dependent on the presence of parasltes in the intestinal canal. Although there are some slight grounds for believing that the pythogenic forms of diarrhœa may be transmitted in some manner from one individual to another, we have no good reason to believe that such transmission is due to propagation by the influence of drinking water." In the propagation of entozoal diarrhœa we have a *type* of true propagation. The passage of an ovum from one intestine into another, and there developing into a replica of the disease cause which existed in the first, and setting up disease absolutely identical.

It is not difficult to understand what the influence of drinking water is under these circumstances. It acts simply as a vehicle for the carriage of LIFE. As regards the propagation of this disease, most persons, I suppose, would rest satisfied with this explanation; but in some other diseases, under circumstances exactly similar, they insist on inquiring what this life or ovum or germ is. Doubtless, contagion differs in many respects from a parasitic ovum; but as regards the point I am at present considering they are absolutely analogous, both being the carriers of a specific modification of vitality. If in the one instance we are satisfied with the explanation that a specific modification of vitality is carried from one organism to another, we should be equally satisfied in other instances, without requiring an explanation of what the ultimate nature of that carrier is, and of the ultimate nature of the modification of vitality which it carries.

The objection of one who would refuse to credit the propagation of entozoa by the passage of ova from one intestine to another, until the *nature of the life* with which these ova are endowed is clearly explained to him, is not of very much practical utility, however modishly scientific his objection may be. And is not the question of the life thus carried closely analogous to the question of the nature of contagium?

The probability of the pythogenie forms of diarrhœa being propagated through the influence of drinking water is so slight that it may be practically disregarded.

The influence of drinking water in originating dysentery.

Regarding this question the following conclusion was arrived at (page 181):—That "our knowledge of dysentery warrants the belief that this disease may sometimes be caused by impurity of drinking water. But it would seem that drinking water produces this disease chiefly by furnishing an exciting cause when other predisposing causes co-exist. At the same time the possibility is recognized that drinking water under peculiar circumstances may furnish both a predisposing and exciting cause, and thus become a true originator of the disease; but such a combination of circumstances is probably rare."

The nature of the influence by which drinking water acts as an excitant of dysentery would seem to be exactly similar to that by which the same agent produces diarrhœa—the difference in results being due to the coexisting conditions which predispose to dysentery. Thus such excitation may be referred to mechanical irritation, to irritation the result of chemico-physiological action, and (regarding the connection between dysentery and malarial disease) very probably excitation by some obscure influence of which we are at present ignorant. As regards the ultimate nature of the influence by which drinking water may furnish predisposing causes of dysentery, we are, I think, wholly ignorant. We may, indeed, say that the constant ingestion of putrid organic matter results in a cachexia by producing a lesion of nutrition; but this is merely a combination of words without any distinct meaning.

The influence of drinking water in propagating dysentery.

Regarding this question the following conclusion was arrived at (page 197):—That "drinking water has no influence in propagating dysentery, as far as our present knowledge affords us means of judging." This conclusion, it must be remembered, by no means denies the *transmissibility* of this disease. It refers only to the *propagation* of dysentery (as strictly understood), and such propagation *through the influence of drinking water.*

The influence of drinking water in originating enteric fever.

Regarding this question the following conclusion was arrived at (page 234):—That "it is questionable whether or not enteric fever is ever originated *de novo*; but there is a slight probability in favour of this occasional protogenesis. If this be admitted, there is also a slight probability of the disease being originated through the influence of drinking water. But if any cases are so caused, they must be rare as compared with those in which the disease seems to arise from climatic or local causes, from the inhalation of putrid sewerage emanations, from idiopathic conditions, or from true propagation by specific contagion."

I have here expressed the probability of the protogenesis of enteric fever in the lowest possible terms, in deference to the opinions held by persons whose judgment we are bound to revere. In the fifth chapter of the second part of this Essay I endeavoured to express my own conclusions on this subject; but however complacently I may view these, and however strong my conviction of their probable correctness, I am unwilling that they should appear in the general summary of conclusions, these being based rigorously on established facts.

However, as regards the possible origination of enteric fever by putrid non-specific fæcal matter ingested with drinking water, as maintained by Dr. Murchison and others, we may say that the influence can hardly be true origination, but may be an excitation—that a certain combination of conditions is necessary for the inauguration of this specific process by any excitant likely to be contained in drinking water.

The ultimate nature of such influences (both the predisposing and the exciting) are unknown; or at least the convictions entertained regarding their nature are not sufficiently based on proved facts to entitle them to a formal consideration in this place.

The influence of drinking water in propagating enteric fever.

Regarding this question, the following conclusion was arrived at (page 253):—That "drinking water may sometimes exert an influence in propagating enteric fever, but such influence must be infrequent and of very limited extent."

Here, again, I have chosen to err on the side of saying too little, rather than risk assuming more than is warranted by the facts. As regards the nature of this influence—if drinking water does exert any influence in propagating enteric fever, or any other disease, this influence must be precisely identical in all cases. It must influence the propagation of enteric fever exactly as it influences the propagation of entozoal diarrhœa—that is, by being a carrier of life or a specific modification of life.

This is the one central truth. Whether or not there be any intermediate receptacle of that life; whether it is contained in a particle or is capable of being carried in absolute solution in fluid media; what the ultimate nature of this specific modification of life may be; and, if there be an intermediate receptacle, whether this should be called a germ or a microzyme, a bacterium or a fungus, a particle of degraded bioplasm or what not, are questions utterly irrelative to the main issue, and at present of no practical value, so far as I can see. The influence, then, by which drinking water may act in propagating this as well as any other propagable disease, is by acting as a carrier of a specific modification of vital force; or, for all practical purposes, we may leave out the intermediate receptacle of life, and say by being a carrier of diseased vitality.

The influence of drinking water in originating cholera.

Regarding this question the following conclusion was arrived at (page 276):—"There is evidence favouring the probability of drinking water being a vehicle of agency which may influence cholera origination. But this influence must be regarded as far from being a constant cause of the disease, and as exerting an influence which is only one of a series of factors seemingly necessary for the production of cholera."

We have seen that some persons believe, not without reason, that bad drinking water, as well as unwholesome food, acts as a predisposing cause of cholera—that is, renders persons susceptible to the influence of the cholera poison. We are unable to define what the nature of such an influence is, and can only say, as we did of the causes predisposing to dysentery, that the long-continued ingestion of putrid organic matter results in a cachexia by a lesion of nutrition which produces a constitutional state prone to succumb under the influence of any disease cause; remembering that this is here also merely a roundabout expression of the facts, and no explanation of them.

We have further seen that the idea of cholera being absolutely originated by drinking water is not without supporters, who adduce in favour of this view an array of theoretical and practical evidence which varies much in value according to preconceived conceptions of the nature of the disease. But I have endeavoured to embody in the above conclusion the belief, that to say that cholera is absolutely originated by drinking water, is going beyond what is warranted by the facts. It would seem to be only one of a series of factors in the production of the disease. Other conditions being favourable, drinking water may furnish another factor; thus completing the necessary series of conditions. Under these circumstances, we can only speak of drinking water as a possible *excitant* of cholera. The nature of this exciting influence is obscure, but I do not know that we are in possession of any facts which would warrant the belief that it differs materially from the influences which act as exciting causes of the other diseases we have here dwelt on, the chief of which we have seen to be chemico-physiological irritation, or irritation resulting from some more recondite vital action produced by fresh or putrid organic matter.

It being proved by experiment that the action of the cholera poison is more severe when a gastro-intestinal irritation exists, it is possible that all the factors of the disease may exist and yet not be developed until a source of irritation is furnished by drinking water. Under these circumstances, drinking water would *seem* to be the originating cause of the disease, while in reality merely an excitor or inaugurator of the disease process, and not its cause—that which sets it going as distinct from its cause; just as heat may be the form of force which inaugurates or sets going a chemical process, while the chemical affinities of the materials acted on represent the force which is the absolute cause of the special re-action in which the process results.

The influence of drinking water in propagating cholera.

Regarding this question, the following conclusion was arrived at (page 284)—" Without accepting any hypothesis as to what the nature of the propagating agent of cholera may be, the balance of evidence seems to favour the view that such an agent may exist; and further, that there is probability of its being a substance capable of carriage by water." Here, as in enteric fever, we only assume that the propagating influence of drinking water is its carriage of a specific modification of vitality or vital force. There may be a constant special receptacle of this vitality, or there may not. My readers will apprehend from the foregoing parts of this Essay that I rather doubt that a constant special receptacle or carrier of this modified vitality is *necessary*. M. Robin admits that bacteria, germs, degraded bioplasm, &c., may serve as carriers of contagion, but only in the sense in which any solid particle, impregnated with the virulent liquid in which it happens to float, may act as a carrier of contagion. This view I endorse, if it be freed from the trammels of humoralism with which he restricts it; and prefer to say that, if bacteria, germs, bioplasm, &c., possess any potentiality as carriers of disease, they do so in the sense that any particle of an organism—*solid or fluid*—carries in itself the special kind of vitality peculiar to the parent organism from which it has been separated. I have before explained how this view accounts for multiplication of disease product (p. 137), and I may here describe Robin's application of these principles to the propagation of cholera. The choleraic principle he assumes to be an albuminoid substance in a peculiar state of molecular change, or endowed with a specific modification of vitality. "This enters the blood" (I quote from a review of his "Leçons sur les Humeurs Normales et Morbides du Corps de l'Homme"), "where it gradually communicates its own molecular condition to the albumenoid elements of the plasma. The chief effect thus wrought in them is a loss of their power to hold water in combination—to fix it; their dehydration entails an abundant flux of the superfluous and uncombined water, holding salts, &c., in solution, from the capillaries of the alimentary mucous membrane. The choice of these capillaries is determined by their comparative nearness to a free surface—is therefore purely mechanical. The exosmosis of liquid into the bowel entails a corresponding cessation of absorption from its mucous surface. The volume of the plasma being enormously reduced by loss of water, the pulse grows thready, the patient is tormented by thirst, and the nutritive interchange between the blood on the one hand, and the tissue-elements and secreting cells on the other, is arrested

Hence the muscular cramps, the diminution and ultimate suppression of the biliary and renal secretions, &c. Further, the blood corpuscles preponderate unduly in the lessened volume of plasma. They tend to stick together; they are arrested, and accumulate in the capillaries, where they part with their oxygen and become loaded with carbonic acid; unable to return to the air-cells of the lungs for renewal, the deoxygenated blood assumes a violet hue; hence cyanosis of the skin and tissues. The exosmotic current from the capillary network of the intestinal mucous membrane softens and strips off its epithelial coat, leaving the sub-epithelial surface naked and without protection." A view extreme in *humoralism* no doubt, but not more so than certain other theories now in vogue are extreme in their *particulateness*. I have endeavoured to adopt a middle course.

But whatever be the nature of the carrier of cholera propagating power which is itself carried by drinking water—or if there be any such intermediate carrier, solid, liquid, or gaseous—we are only authorized in concluding that drinking water influences the propagation of cholera by being a vehicle or carrier of some peculiar and specific modification of vitality or molecular force.

I subjoin a tabular statement of the principal conclusions at which we have here arrived.

TABULAR STATEMENT OF THE CONCLUSIONS ARRIVED AT IN THIS ESSAY.

DISEASE.	Whether or not its Origin or Propagation is influenced by Drinking Water.	The nature of the influence of Drinking Water in Originating or Propagating it.
DIARRHŒA ...	Drinking water may be one of the agents of origination of nearly all forms of Diarrhœa.	(1) Mechanical irritation; (2) chemico-physiological irritation; and (3) probably other more obscure influences, of the nature of which we are ignorant.
	Drinking water influences the propagation of Entozoal Diarrhœa. (It is doubtful that such an influence is exerted with reference to any other form of this disease.)	By being a carrier of a specific modification of vitality.
DYSENTERY ...	Drinking may be one of the agents of Dysentery origin, but seemingly only as an excitant when predisposing conditions exist.	The same as in the origination of Diarrhœa.
	Probably drinking water does not influence the propagation of Dysentery.	
ENTERIC FEVER	Probably drinking water sometimes influences Enteric Fever origination as an excitant.	The same as in the origination of Diarrhœa.
	Drinking water does sometimes influence the propagation of Enteric Fever.	The same as in the propagation of Diarrhœa.
CHOLERA	Probably drinking water sometimes influences Cholera origination as an excitant.	The same as in the origination of Diarrhœa.
	Drinking water does sometimes influence the propagation of Cholera.	The same as in the propagation of Diarrhœa.

CHAPTER II.

CONCLUSION.

In bringing to a close this very inperfect sketch of this vast subject, it may be noticed that the chief consideration forced on us by a review of the entire question of the influence of drinking water in originating or propagating these diseases, is the very empirical nature of our knowledge and our practice regarding it. The facts, even, are as yet but imperfectly established. We have seen that still the question of whether or not such an influence exists affords material for discussion, and that the opinion of the medical community is far from determined regarding the bare facts on which conclusions respecting it are to be based.

And if our knowledge of the existence of this influence is imperfect, our knowledge of the nature of the agency by which such influence may be exerted is still more so.

Possibly the nature of this influence is a question with reference to which we cannot arrive at accurate knowledge. It may be a question regarding which ultimate truth cannot be attained. In a former part of the Essay I noticed that questions regarding the origin of disease—the beginnings of death—partake largely of the characters of a metaphysical discussion; and that conclusions regarding them are, from their inherent nature, to a great extent insusceptible of proof. In his work on "The Philosophy of Aristotle" Mr. Lewes maintains that the knowledge of *measure*—the "verifiable element," as he calls it, is that which determines whether any theory belongs strictly to metaphysics or to science.

It is not likely that in this inquiry conclusions are to be arrived at capable of this kind of proof, requisite in the exact sciences. Inasmuch as such verification is here apparently impossible (although some hold that the possibilities of our means of physical research are illimitable), all we believe of such influences of disease cause must be greatly speculations; for we cannot affirm that they are absolutely true—that such beliefs are not founded in delusions of sense. Hence we must expect that there will always be difference of opinion regarding the exercise of the influences here treated of, and regarding their intimate nature.

But as practical hygienists we may very well afford to ignore discussion on the ultimate nature of such influences. As long as we have any evidence that they *may exist* the indications for practice are clear. If this practice be as yet empirical, it is not the less obligatory. In the words of him who has been called "the founder of hygiene," and whose revered memory is still so fresh among us—"In the midst of all our weakness, and all our many errors, we are certainly gaining knowledge, and that knowledge tells us, in no doubtful terms, that the fate of man is in his own hands."

Foremost of the teachings of hygiene is the necessity of purity in water supply; but even still that teaching has to wrestle with carelessness, cupidity, and sloth.

Many places in the vicinity of our centres of civilization and in our rural districts still answer to the eloquent description of one by the Professor of Art at Oxford:—"I have never seen anything so ghastly in its inner tragic meaning—not in Pisan Maremma, not by the Campagna tomb, not in the sand isles of the Torcellan shore—as the slow stealing of aspects of reckless indolent animal neglect over the delicate sweetness of that English scene; nor is there any blasphemy or impiety—any frantic saying or godless thought—more appalling to me, using the best power of judgment I have to discern its sense and scope, than the insolent defiling of those springs by the human herds that drink of them. Just where the swelling of stainless water, trembling and pure like a body of light, enters the pool of Carshalton, cutting itself a radiant channel down to the gravel through warps of feathery weeds all waving, which it traverses with its deep threads of clearness like the chalcedony in moss agate, starred here and there with the white grenouillette; just in the very rush and murmur of the first spreading currents, the human wretches of the place cast their street and house foulness—heaps of dust and slime, and broken shreds of old metal, and rags of putrid clothes—they, having neither the energy to cart it away nor decency enough to dig it into the ground, thus shed into the stream to diffuse what venom of it will float and melt away in all places where God meant those waters to bring joy and gladness. Half a dozen men with one day's work could cleanse those pools and trim the flowers about their banks, and make every breath of summer air above them rich with cool balm, and every glittering wave medicinal, as if it ran, troubled of angels, from the porch of Bethesda. But that day's work is never given, nor will be; nor will joy be possible to heart of man for evermore about those wells of English waters" (Introduction to "The Crown of Wild Olives").

Let us hope this last thought may not come true. May our people soon learn the *usefulness* of purity—the utility of it as well as the goodness of it and the beauty of it. May they learn and practise it as a necessity of existence—a necessity taught alike by Religion, by Science, and by Art.

DEFINITIONS.

Cause of Disease.—The term *cause*, when not qualified by the words *predisposing*, *originating*, or *propagating*, is used in a comprehensive sense, as including these three varieties of cause.

Endemic Disease.—That class of diseases which can be originated by causes peculiar to a locality or community.

Endemic Locality.—An area in which the originating causes of certain diseases are rife.

Epidemic (a substantive).—Used in this Essay in a more restricted sense than its etymology warrants, as a series of cases of disease in each of which transmission from a foregoing case of the same disease *by propagation* (excluding infection) can be traced.

Infection.—Transmission of disease from one organism to another through the influence of an agency which is invisible and intangible.

Origin.—The production of a disease by any causes other than propagating cause, its production independently of a foregoing case of *the same* disease.

Propagation.—Transmission of disease from one organism to another through the influence of a definite *materies morbi*, which latter has at one time formed part of the diseased organism from which the disease is transmitted.

Specific Disease.—A morbid process which consists of definite stages characterized by a uniformly definite sequence of phenomena.

Specific Modification of Vitality.—A mode of vital action which, when once inaugurated, cannot be again changed to normal vital action, but continues a morbid vitality so long as any vitality continues in the matter in which it resides, and which is capable of communicating its own morbid vitality—its own peculiar mode of organic life—to healthy bioplasm.

APPENDIX.

Note to page 159.—In connection with Surgeon-Major Massy's researches on "The Presence of Fungi at Jaffna" the observations of Professor Lionel Beale regarding certain disease germs, given in the following extract, are interesting:—"It is not impossible that the germinal matter of some of the lower simpler plants and animals, when exposed to altered conditions, may give rise to morbid forms bearing a relation to their normal healthy living germinal matter, similar to that which pus bears to the germinal matter of healthy tissues; and it is possible that in our observations upon the lower forms of life we may be sometimes examining morbid instead of normal healthy organisms. It may be that the matter of the malarial poison may thus result, in which case it must be regarded as a morbid bioplasm of some low organism—not as a species of any kind whatever, but as a deteriorated form of living matter, freely multiplying, but incapable of producing healthy matter, or of returning to its primitive healthy state" ("Disease Germs," page 117). The bearings of this view on the influence of drinking water in originating the malarial forms of diarrhœa are obvious.

Extract from Letter No. $\frac{\text{D}}{500\frac{1}{2}}$ *Statistical* $\frac{\text{Sanitary}}{\text{Typhoid Fever}}$ *from the Surgeon-General, P.M.O., Her Majesty's British Forces in India, to the Quartermaster-General in India, dated October* 20, 1877.

"13. In some of the cases the possibility has been suggested that either water, milk, or other supplies have acted as contagious media; this is notably the case in the outbreak at Fyzabad amongst the newly-arrived 1–25 Regiment, in F–11 R.A. at Saugor, in the various detachments of that district, and at Cherat; but in all these instances the first link in the chain is ever wanting, and no case is shown in which the previous contamination of the sources by persons already suffering from the disease is substantiated.

"14. But the reports do also include instances in which the disease was presumably disseminated by personal communication with those affected; and but few will now be found disposed to deny to it a certain degree of communicability—a quality which it seems to possess in common with other lesions in which the mucous surface is chiefly involved, although this tendency is essentially limited by the dry earth system of conservancy.

"15. At the same time, it is impossible to refuse due weight to the accumulated evidence, however negative, of its well-defined and essentially idiopathic character, or to acknowledge that, wholly irrespective of contagion, it may develop itself in the European as the direct result of a consensus of causes, the chief of which seems to be the prolonged exposure of an alien and unripe constitution to an intensified and unaccustomed temperature, to which certain other more recondite climatic conditions may probably be added.

"16. Such a view derives support from the testimony of witnesses who, if not already engaged to opposing theories, show a natural prepossession in favour of opinions adverse to it, because these are sanctioned by the teaching of some of the advanced medical authorities, while it is found that typhoid is a form of ailment almost special to one period of life amongst the class above indicated, and that it very exceptionally exists among the soldiers of the native Indian army or in native jails.

"17. It is not desired to originate or illustrate a theory, but with the data yielded by the returns before us, tallying minutely as they do in their general indications with those drawn from similar sources in other years, it is difficult to admit the force of that generalization, however respectable its source, which would wholly assimilate the etiology of enteric fever as observed in the north with the allied variety of it existing in equatorial and semi-tropical latitudes, which, while presenting the same symptoms during life, and revealing identical necrological lesions after death, seems nevertheless to become developed under the well-defined phases which have above been insisted on.* In our Mediterranean stations it is equally found to occur under circumstances which admit of no suspicion of contagion, and an ample detail of such cases will be found in the report of the proceedings of a mixed Naval, Military, and Medical Commission, presided over by the writer, and published in Malta under authority (1874), to inquire into the condition of the Corradino Military Prison in that command.

"18. The various sanitary defects noted by medical officers are important, *quantum valeat*, and have from time to time been remedied by the local authorities, so far as remedy may have been practicable; but

* This is a very plain statement of the doctrine that specificity of a disease does not necessarily imply specificity of its cause.

by none of the observers has the relation of cause and effect between these and enteric fever been established with even an approach to exactitude."

Note to page 215.—It was intended to append a note on "the relationship of typho-malarial fever and enteric fever with reference to the influence of drinking water in originating the latter," but want of space obliges the writer to suppress it.

Note to the Appendix.—It was intended that this Appendix should contain notes on "a detailed explanation of the meaning of the phrase *specific modification of vitality* as used in this Essay," and on "some of the more obvious causes of difference of opinion between etiological observers in temperate and tropical climates;" but it is found that the Essay has already exceeded its allotted limits.

INDEX.

THE END.

PRINTED BY BALLANTYNE, HANSON AND CO.
LONDON AND EDINBURGH

Catalogue B]

London, New Burlington Street
March, 1881

SELECTION

FROM

J. & A. CHURCHILL'S GENERAL CATALOGUE

COMPRISING

ALL RECENT WORKS PUBLISHED BY THEM

ON THE

ART AND SCIENCE OF MEDICINE

A SELECTION

FROM

J. & A. CHURCHILL'S GENERAL CATALOGUE,

COMPRISING

ALL RECENT WORKS PUBLISHED BY THEM ON THE ART AND SCIENCE OF MEDICINE.

N.B.—*J. & A. Churchill's Descriptive List of Works on Chemistry, Materia Medica, Pharmacy, Botany, Photography, Zoology, the Microscope, and other Branches of Science, can be had on application.*

The Practice of Surgery:
A Manual. By THOMAS BRYANT, Surgeon to Guy's Hospital. Third Edition. 2 vols. Crown 8vo, with 672 Engravings (many being coloured), 28s.

"Much compactness. . . . . Full of matter well employed and of experience well used. . . . Illustrations good."—*British Medical Journal.*

"It is upon the excellence of much of the clinical teaching to be found in its pages that the success which the book has met with in great measure depends."—*Medical Press and Circular.*

Illustrations of Clinical Surgery.
By JONATHAN HUTCHINSON, Senior Surgeon to the London Hospital. In Quarterly Fasciculi. I. to XIII., 6s. 6d. each. Fasciculi I. to X. bound, with Appendix and Index, £3 10s.

The Principles and Practice of Surgery. By WILLIAM PIRRIE, F.R.S.E., Professor of Surgery in the University of Aberdeen. Third Edition. 8vo, with 490 Engravings, 28s.

A System of Practical Surgery.
By Sir WILLIAM FERGUSSON, Bart., F.R.S. Fifth Edition. 8vo, with 463 Engravings, 21s.

Surgical Emergencies:
Together with the Emergencies Attendant on Parturition and the Treatment of Poisoning. By PAUL SWAIN, F.R.C.S., Surgeon to the Royal Albert Hospital, Devonport. Third Edition. Crown 8vo, with 117 Engravings, 5s.

A Course of Operative Surgery.
By CHRISTOPHER HEATH, Surgeon to University College Hospital. With 20 Plates drawn from Nature by M. LÉVEILLÉ, and coloured by hand under his direction. Large 8vo, 40s.

By the same Author.

The Student's Guide to Surgical Diagnosis. Fcap. 8vo, 6s. 6d.

Also.

Manual of Minor Surgery and Bandaging. For the use of House Surgeons, Dressers, and Junior Practitioners. Sixth Edition. Fcap. 8vo, with 115 Engravings, 5s. 6d.

Also.

Injuries and Diseases of the Jaws. Second Edition. 8vo, with 164 Engravings, 12s.

The Surgeon's Vade-Mecum:
A Manual of Modern Surgery. By ROBERT DRUITT, F.R.C.S. Eleventh Edition. Fcap. 8vo, with 369 Engravings, 14s.

Treatment of Wounds:
Clinical Lectures. By SAMPSON GAMGEE, F.R.S.E., Surgeon to the Queen's Hospital, Birmingham. Crown 8vo, with Engravings, 5s.

By the same Author.

Fractures of the Limbs,
And their Treatment. 8vo, with Plates, 10s. 6d.

Outlines of Surgery and Surgical Pathology. By F. LE GROS CLARK, F.R.S., assisted by W. W. WAGSTAFFE, F.R.C.S. Second Edition. 8vo, 10s. 6d.

Surgical Enquiries: Including the Hastings Essay on Shock, the Treatment of Inflammations, and numerous Clinical Lectures. By FURNEAUX JORDAN, F.R.C.S., Professor of Surgery, Queen's College, Birmingham. Second Edition, with numerous Plates. Royal 8vo, 12s. 6d.

Clinical and Pathological Observations in India. By Sir J. FAYRER, K.C.S.I., M.D. 8vo, with Engravings, 20s.

The Female Pelvic Organs, Their Surgery, Surgical Pathology, and Surgical Anatomy, in a Series of Coloured Plates taken from Nature: with Commentaries, Notes, and Cases. By HENRY SAVAGE, Consulting Officer of the Samaritan Free Hospital. Third Edition. 4to, £1 15s.

Transfusion of Human Blood: With Table of 50 cases. By Dr. ROUSSEL, of Geneva. With a Preface by Sir JAMES PAGET, Bart. Crown 8vo, 2s. 6d.

Hare-Lip and Cleft Palate. By FRANCIS MASON, F.R.C.S., Surgeon and Lecturer on Anatomy at St. Thomas's Hospital. 8vo, with 66 Engravings, 6s.

By the same Author.

The Surgery of the Face. 8vo, with 100 Engravings, 7s. 6d.

A Practical Treatise on Aural Surgery. By H. MACNAUGHTON JONES, M.D., Professor of the Queen's University in Ireland, Surgeon to the Cork Ophthalmic and Aural Hospital. Crown 8vo, with 46 Engravings, 5s.

By the same Author.

Atlas of Diseases of the Membrana Tympani. In Coloured Plates, containing 62 Figures, with Text. Crown 4to, 21s.

Diseases and Injuries of the Ear. By WILLIAM B. DALBY, F.R.C.S., Aural Surgeon to, and Lecturer on Surgery at, St. George's Hospital. Second Edition. Fcap. 8vo, with Engravings, 6s. 6d.

The Ear: Its Anatomy, Physiology, and Diseases. By CHARLES H. BURNETT, A.M., M.D., Aural Surgeon to the Presbyterian Hospital, Philadelphia. 8vo, with 87 Engravings, 18s.

Diseases of the Throat and Nose. A Manual. By MORELL MACKENZIE, M.D. Lond., Senior Physician to the Hospital for Diseases of the Throat and Chest. Vol. I. Diseases of the Pharynx, Larynx, and Trachea. Post 8vo, with 112 Engravings, 12s. 6d.

"There can be but one verdict of the profession on this manual. It stands without any competitor in British medical literature, as a standard work on the organs it professes to treat of."—*Dublin Medical Journal.*

Ear and Throat Diseases. Essays by LLEWELLYN THOMAS, M.D., Surgeon to the Central London Throat and Ear Hospital. Post 8vo, 2s. 6d.

Clubfoot: Its Causes, Pathology, and Treatment. By WM. ADAMS, F.R.C.S., Surgeon to the Great Northern Hospital. Second Edition. 8vo, with 106 Engravings and 6 Lithographic Plates, 15s.

By the same Author.

On Contraction of the Fingers, and its Treatment by Subcutaneous Operation; and on Obliteration of Depressed Cicatrices, by the same Method. 8vo, with 30 Engravings, 4s. 6d.

Lectures on Orthopædic Surgery. By BERNARD E. BRODHURST, F.R.C.S., Surgeon to the Royal Orthopædic Hospital. Second Edition. 8vo, with Engravings, 12s. 6d.

Osteotomy: With an Enquiry into the Etiology and Pathology of Knock-knee, Bow-leg, and other Osseous Deformities of the Lower Limbs. By WILLIAM MACEWEN, M.D., Surgeon and Lecturer on Clinical Surgery to the Glasgow Royal Infirmary. 8vo, with 51 Engravings, 7s. 6d.

Orthopraxy: The Mechanical Treatment of Deformities, Debilities, and Deficiencies of the Human Frame. By H. HEATHER BIGG, Assoc. Inst. C.E. Third Edition. 8vo, with 319 Engravings, 15s.

The Orthopragms of the Spine: An Essay on the Curative Mechanisms applicable to Spinal Curvature, etc. By ROBERT HEATHER BIGG, Assoc. Inst. C.E. 8vo, with Engravings, 5s.

Orthopædic Surgery, And Diseases of the Joints. By LEWIS A. SAYRE, M.D., Professor of Orthopædic Surgery in Bellevue Hospital Medical College. 8vo, with 274 Engravings, 20s.

Diseases of the Testis, Spermatic Cord, and Scrotum. By THOMAS B. CURLING, F.R.S., Consulting Surgeon to the London Hospital. Fourth Edition. 8vo, with Engravings, 16s.

Fistula, Hæmorrhoids, Painful Ulcer, Stricture, Prolapsus, and other Diseases of the Rectum. By WILLIAM ALLINGHAM, Surgeon to St. Mark's Hospital for Fistula. Third Edition. 8vo, with Engravings, 10s.

Cancer of the Rectum: Its Pathology, Diagnosis, and Treatment. By W. HARRISON CRIPPS, F.R.C.S., Surgeon to the Great Northern Hospital, etc. Crown 8vo, with Lithographic Plates, 6s.

Hydrocele: Its several Varieties and their Treatment. By SAMUEL OSBORN, late Surgical Registrar to St. Thomas's Hospital. Fcap. 8vo, with Engravings, 3s.

By the same Author.

Diseases of the Testis. Fcap. 8vo, with Engravings. 3s. 6d.

Diseases of the Urinary Organs: Clinical Lectures. By Sir HENRY THOMPSON, F.R.C.S., Emeritus Professor of Clinical Surgery in University College. Fifth Edition. 8vo, with 2 Plates and 71 Engravings, 10s. 6d.

"Catholic in his investigation of the fruit of the labour of others, cautious in all his deductions, rejecting all specious theories in the effort to obtain practically useful results, as clever with his pen as he is with the sound or lithotrite, one can scarcely wonder that he is esteemed the master that he is."—*American Journ. of Med. Sci.*

By the same Author.

Diseases of the Prostate: Their Pathology and Treatment. Fourth Edition. 8vo, with numerous Plates, 10s.

Also.

Practical Lithotomy and Lithotrity; or, an Inquiry into the best Modes of Removing Stone from the Bladder. Third Edition. 8vo, with 87 Engravings, 10s.

Also.

The Preventive Treatment of Calculous Disease, and the Use of Solvent Remedies. Second Edition. Fcap. 8vo, 2s. 6d.

Lectures on the Surgical Disorders of the Urinary Organs. By REGINALD HARRISON, F.R.C.S., Surgeon to the Liverpool Royal Infirmary. Second Edition, with 48 Engravings. 8vo. 12s. 6d.

Lithotomy and Extraction of Stone. By W. POULETT HARRIS, M.D., Surgeon-Major H.M. Bengal Medical Service. 8vo, with Engravings, 10s. 6d.

The Surgery of the Rectum. By HENRY SMITH, Professor of Surgery in King's College, Surgeon to King's College Hospital. Fourth Edition. Fcap. 8vo, 5s.

Diseases of the Bladder, Prostate Gland, and Urethra, with a practical view of Urinary Diseases, Deposits, and Calculi. By F. J. GANT, Senior Surgeon to the Royal Free Hospital. Fourth Edition. Crown 8vo, 10s. 6d.

Renal and Urinary Diseases: Clinical Reports. By WILLIAM CARTER, M.B., Physician to the Liverpool Southern Hospital. Crown 8vo, 7s. 6d.

The Marriage of Near Kin, Considered with respect to the Laws of Nations, Results of Experience, and the Teachings of Biology. By ALFRED H. HUTH. 8vo, 14s.

The Reproductive Organs In Childhood, Youth, Adult Age, and Advanced Life, considered in their Physiological, Social, and Moral Relations. By WILLIAM ACTON, M.R.C.S. Sixth Edition. 8vo, 12s.

Student's Primer on the Urine. By J. TRAVIS WHITTAKER, M.D., Clinical Demonstrator at the Royal Infirmary, Glasgow. With 16 Plates etched on Copper. Post 8vo, 4s. 6d.

Urinary and Reproductive Organs: their Functional Diseases. By D. CAMPBELL BLACK, M.D. Second Edition. 8vo, 10s.

Lectures on Syphilis. By HENRY LEE, Consulting Surgeon to St. George's Hospital. 8vo, 10s.

Harveian Lectures on Syphilis. By JAMES R. LANE, F.R.C.S., Surgeon to St. Mary's Hospital. Second Edition. Fcap. 8vo, 3s. 6d.

A Treatise on Syphilis. By WALTER J. COULSON, Surgeon to the Lock Hospital and to St. Peter's Hospital for Stone. 8vo, 10s.

By the same Author.

Stone in the Bladder: Its Prevention, early Symptoms, and Treatment by Lithotrity. 8vo, 6s.

Also.

Coulson on Diseases of the Bladder and Prostate Gland. Sixth Edition. 8vo, 16s.

Syphilitic Nervous Affections: Their Clinical Aspects. By THOMAS BUZZARD, M.D., F.R.C.P. Physician to the National Hospital for Paralysis and Epilepsy. Post 8vo, 5s.

Pathology of the Urine, Including a Complete Guide to its Analysis. By J. L. W. THUDICHUM, M.D., F.R.C.P. Second Edition, rewritten and enlarged. 8vo, with Engravings, 15s.

Genito-Urinary Organs, including Syphilis: a Practical Treatise on their Surgical Diseases, designed as a Manual for Students and Practitioners. By W. H. VAN BUREN, M.D., and E. L. KEYES, M.D. Royal 8vo, with 140 Engravings, 21s.

Histology and Histo-Chemistry of Man. By HEINRICH FREY, Professor of Medicine in Zurich. Translated by ARTHUR E. J. BARKER, Assistant-Surgeon to University College Hospital. 8vo, with 608 Engravings, 21s.

Principles of Human Physiology. By W. B. CARPENTER, C.B., M.D., F.R.S. Ninth Edition. By HENRY POWER, M.B., F.R.C.S. 8vo, with 3 Steel Plates and 377 Wood Engravings, 31s. 6d.

A Treatise on Human Physiology. By JOHN C. DALTON, M.D., Professor in the College of Physicians and Surgeons, New York. Sixth Edition. Royal 8vo, with 316 Engravings, 20s.

The Spectroscope in Medicine. By CHARLES A. MACMUNN, B.A., M.D. 8vo, with 3 Chromo-lithographic Plates of Physiological and Pathological Spectra, and 13 Engravings, 9s.

"It is painstaking throughout; it aims not at brilliancy but at accuracy; and the result is that we feel we may safely rely on the conclusions of the author."—*Medical Times and Gazette.*

Manual of Anthropometry: A Guide to the Measurement of the Human Body, containing an Anthropometrical Chart and Register, a Systematic Table of Measurements, &c. By CHARLES ROBERTS, F.R.C.S. 8vo, with numerous Illustrations and Tables, 6s. 6d.

The Student's Guide to Human Osteology. By WILLIAM WARWICK WAGSTAFFE, Assistant Surgeon to St. Thomas's Hospital. Fcap. 8vo, with 23 Plates and 66 Engravings, 10s. 6d.

Practical Histology. By WILLIAM RUTHERFORD, M.D., F.R.S. Second Edition. Crown 8vo, with 63 Engravings, 6s.

Manual of the Dissection of the Human Body. By LUTHER HOLDEN, Senior Surgeon to St. Bartholomew's and the Foundling Hospitals. Fourth Edition. 8vo, with 170 Engravings, 16s.

By the same Author.

Human Osteology: Sixth Edition. 8vo, with 61 Lithographic Plates and 89 Engravings. (*In the Press.*)

Also.

Landmarks, Medical and Surgical. Second Edition. 8vo, 3s. 6d.

Sanderson's Handbook for the Physiological Laboratory. By E. KLEIN, M.D., F.R.S.; J. BURDON-SANDERSON, M.D., F.R.S.; MICHAEL FOSTER, M.D., F.R.S.; and T. LAUDER BRUNTON, M.D., F.R.S. 8vo, with 123 Plates, 24s.

Text-Book of Physiology. By J. FULTON, M.D., Professor of Physiology, &c., in Trinity Medical College, Toronto. Second Edition. 8vo, with 152 Engravings, 15s.

Lectures on Pathological Anatomy. By SAMUEL WILKS, M.D., F.R.S., Physician to Guy's Hospital; and WALTER MOXON, M.D., Physician to Guy's Hospital. Second Edition. 8vo, with Plates, 18s.

A Manual of Pathological Anatomy. By C. HANDFIELD JONES, M.B., F.R.S.; and EDWARD H. SIEVEKING, M.D., F.R.C.P. Edited (with considerable enlargement) by J. F. PAYNE, M.D., F.R.C.P., Lecturer on General Pathology at St. Thomas's Hospital. Second Edition. Crown 8vo, with 195 Engravings, 16s.

The Student's Guide to Surgical Anatomy: An Introduction to Operative Surgery. By EDWARD BELLAMY, F.R.C.S. and Member of the Board of Examiners. Fcap. 8vo, with 76 Engravings, 7s.

"A text-book which breathes meaning and relational value into the study of human anatomy from the surgical side."—*British Medical Journal.*

"We do not know any book of the kind which will be found so useful to students and to surgeons preparing for the higher examinations."—*Dublin Medical Journal.*

Atlas of Pathological Anatomy. By Dr. LANCEREAUX. Translated by W. S. GREENFIELD, M.D., Assistant Physician to St. Thomas's Hospital, with 70 Coloured Plates. Imp. 8vo, £5 5s.

Practical Anatomy: A Manual of Dissections. By CHRISTOPHER HEATH, Surgeon to University College Hospital. Fourth Edition. Crown 8vo, with 16 Coloured Plates and 264 Engravings, 14s.

Wilson's Anatomist's Vade-Mecum. Tenth Edition. By GEORGE BUCHANAN, Professor of Clinical Surgery in the University of Glasgow; and HENRY E. CLARK, M.R.C.S., Lecturer on Anatomy at the Glasgow Royal Infirmary School of Medicine. Crown 8vo, with 450 Engravings (including 26 Coloured Plates), 18s.

Braune's Atlas of Topographical Anatomy, after Plane Sections of Frozen Bodies. Translated by EDWARD BELLAMY, Surgeon to, and Lecturer on Anatomy, &c., at, Charing Cross Hospital. Large Imp. 8vo, with 34 Photo-lithographic Plates and 46 Woodcuts, 40s.

An Atlas of Human Anatomy: By RICKMAN J. GODLEE, M.S., F.R.C.S., Assistant Surgeon and Senior Demonstrator of Anatomy, University College Hospital. With 48 Imp. 4to Plates (112 figures), and a volume of Explanatory Text, 8vo, £4 14s. 6d.

Post-Mortem Examinations: A Description and Explanation of the Method of performing them, with especial reference to Medico-Legal Practice. By Prof. VIRCHOW. Translated by Dr. T. P. SMITH. Second Edition. Fcap. 8vo, with 4 Plates, 3s. 6d.

Surgical Anatomy: A Series of Dissections, illustrating the Principal Regions of the Human Body. By JOSEPH MACLISE. Second Edition. 52 folio Plates and Text. Cloth, £3 12s.

By the same Author.

On Dislocations and Fractures. Uniform with "Surgical Anatomy." 36 folio Plates and Text. Cloth, £2 10s.

Medical Anatomy. By FRANCIS SIBSON, M.D., F.R.C.P., F.R.S. Imp. folio, with 21 Coloured Plates, cloth, 42s., half-morocco, 50s.

Diagrams of the Nerves of the Human Body, exhibiting their Origin, Divisions, and Connections, with their Distribution to the Various Regions of the Cutaneous Surface, and to all the Muscles. By WILLIAM H. FLOWER, F.R.C.S., F.R.S., Hunterian Professor of Comparative Anatomy, Royal College of Surgeons. Third Edition, with 6 Plates. Royal 4to, 12s.

Anatomy of the Joints of Man. By HENRY MORRIS, Surgeon to, and Lecturer on Anatomy and Practical Surgery at, the Middlesex Hospital. 8vo, with 44 Lithographic Plates (several being coloured) and 13 Wood Engravings, 16s.

The Anatomical Remembrancer; or, Complete Pocket Anatomist. Eighth Edition. 32mo, 3s. 6d.

The Student's Guide to the Practice of Medicine. By MATTHEW CHARTERIS, M.D., Professor of Materia Medica in the University of Glasgow. Second Edition. Fcap. 8vo, with Engravings on Copper and Wood, 6s. 6d.

The Student's Guide to Medical Diagnosis. By SAMUEL FENWICK, M.D., F.R.C.P., Physician to the London Hospital. Fourth Edition. Fcap. 8vo, with 106 Engravings, 6s. 6d.

By the same Author.

The Student's Outlines of Medical Treatment. Fcap. 8vo, 7s.

Also.

On Chronic Atrophy of the Stomach, and on the Nervous Affections of the Digestive Organs. 8vo, 8s.

The Microscope in Medicine. By LIONEL S. BEALE, M.B., F.R.S., Physician to King's College Hospital. Fourth Edition. 8vo, with 86 Plates, 21s.

Also.

On Slight Ailments: Their Nature and Treatment. 8vo, 5s.

A Manual of Medical Diagnosis. By A. W. BARCLAY, M.D., F.R.C.P., Physician to St. George's Hospital. Third Edition. Fcap. 8vo, 10s. 6d.

Clinical Medicine: Lectures and Essays. By BALTHAZAR FOSTER, M.D., F.R.C.P. Lond., Professor of Medicine in Queen's College, Birmingham. 8vo, 10s. 6d.

Clinical Lectures and Cases, with Commentaries. By HENRY THOMPSON, M.D., F.R.C.P., Consulting Physician to Middlesex Hospital. With Temperature Charts. 8vo, 7s. 6d.

Clinical Studies: Illustrated by Cases observed in Hospital and Private Practice. By Sir J. ROSE CORMACK, M.D., F.R.S.E., Physician to the Hertford British Hospital of Paris. 2 vols. Post 8vo, 20s.

Relapse of Typhoid Fever, especially with reference to the Temperature. By J. PEARSON IRVINE, M.D., F.R.C.P., late Assistant Physician to Charing Cross Hospital. 8vo, with Engravings, 6s.

Croonian Lectures on Some Points in the Pathology and Treatment of Typhoid Fever. By WILLIAM CAYLEY, M.D., F.R.C.P., Physician to the Middlesex and the London Fever Hospitals. Crown 8vo, 4s. 6d.

A New System of Medicine; Entitled Recognisant Medicine, or the State of the Sick. By BHOLANOTH BOSE, M.D., Indian Medical Service. 8vo, 10s. 6d.

By the same Author.

Principles of Rational Therapeutics. Commenced as an Inquiry into the Relative Value of Quinine and Arsenic in Ague. Crown 8vo, 4s. 6d.

Medicinal Plants: Being descriptions, with original figures, of the Principal Plants employed in Medicine, and an account of their Properties and Uses. By Professor BENTLEY and Dr. H. TRIMEN. In 4 volumes, large 8vo, with 306 Coloured Plates, bound in Half Morocco, Gilt Edges. £11 11s.

Royle's Manual of Materia Medica and Therapeutics. Sixth Edition, by JOHN HARLEY, M.D., Physician to St. Thomas's Hospital. Crown 8vo, with 139 Engravings, 15s.

A Manual of Practical Therapeutics. By E. J. WARING, M.D., F.R.C.P. Lond. Third Edition. Fcap. 8vo, 12s. 6d.

The Student's Guide to Materia Medica. By JOHN C. THOROWGOOD, M.D., F.R.C.P. Lond. Fcap. 8vo, 6s. 6d.

Materia Medica and Therapeutics: Vegetable Kingdom. By CHARLES D. F. PHILLIPS, M.D., Lecturer on Materia Medica and Therapeutics to Westminster Hospital. 8vo, 15s.

The National Dispensatory: Containing the Natural History, Chemistry, Pharmacy, Actions and Uses of Medicines, including those recognised in the Pharmacopœias of the United States and Great Britain and Germany, with numerous references to the French Codex. By ALFRED STILLÉ, M.D., LL.D., and JOHN M. MAISCH, Ph.D. Second Edition. 8vo, with 239 Engravings, 34s.

Binz's Elements of Therapeutics: A Clinical Guide to the Action of Drugs. Translated by EDWARD I. SPARKS, M.B., F.R.C.P. Crown 8vo, 8s. 6d.

The Pharmacopœias of the London Hospitals, arranged in Groups for Easy Reference and Comparison. By PETER SQUIRE, F.L.S., assisted by his sons, P. W. SQUIRE and A. H. SQUIRE. Fourth Edition. 18mo, 6s.

Also.

A Companion to the British Pharmacopœia. Twelfth Edition. 8vo, 10s. 6d.

Dental Materia Medica and Therapeutics. By JAMES STOCKEN, L.D.S.R.C.S., late Lecturer to the National Dental Hospital. Second Edition. Fcap. 8vo, 6s. 6d.

The Wasting Diseases of Children. By EUSTACE SMITH, M.D., Physician to the King of the Belgians, Physician to the East London Hospital for Children. Third Edition. Post 8vo, 8s. 6d.

By the same Author.

Clinical Studies of Disease in Children. Post 8vo, 7s. 6d.

Infant Feeding and its Influence on Life; or, the Causes and Prevention of Infant Mortality. By C. H. F. ROUTH, M.D., Senior Physician to the Samaritan Hospital. Third Edition. Fcap. 8vo, 7s. 6d.

A Manual of what every Mother should know. By EDWARD ELLIS, M.D., Author of "A Practical Manual of the Diseases of Children." Fcap. 8vo, 1s. 6d.

The Student's Guide to the Practice of Midwifery. By D. LLOYD ROBERTS, M.D., F.R.C.P., Physician to St. Mary's Hospital, Manchester. Second Edition. Fcap. 8vo, with 111 Engravings, 7s.

Handbook of Midwifery for Midwives: from the Official Handbook for Prussian Midwives. By J. E. BURTON, L.R.C.P. Lond., Senior Assistant Medical Officer, Ladies' Charity, &c., Liverpool. With Engravings. Fcap. 8vo, 6s.

Obstetric Medicine and Surgery: Their Principles and Practice. By F. H. RAMSBOTHAM, M.D., F.R.C.P. Fifth Edition. 8vo, with 120 Plates, 22s.

Lectures on Obstetric Operations: Including the Treatment of Hæmorrhage, and forming a Guide to the Management of Difficult Labour. By ROBERT BARNES, M.D., F.R.C.P., Obstetric Physician to St. George's Hospital. Third Edition. 8vo, with 124 Engravings, 18s.

By the same Author.

A Clinical History of Medical and Surgical Diseases of Women. Second Edition. 8vo, with 181 Engravings, 28s.

West on the Diseases of Women. Fourth Edition, revised and in part re-written by the Author, with numerous Additions by J. MATTHEWS DUNCAN, M.D., Obstetric Physician to St. Bartholomew's Hospital. 8vo, 16s.

Observations on the Cæsarian Section, Craniotomy, and on other Obstetric Operations, with Cases. By THOMAS RADFORD, M.D., Honorary Consulting Physician, St. Mary's Hospital, Manchester. Second Edition, with Plates. 8vo, 10s.

Clinical Lectures on Diseases of Women: Delivered in St. Bartholomew's Hospital, by J. MATTHEWS DUNCAN, M.D., F.R.S.E. 8vo, 8s.

By the same Author.

Papers on the Female Perineum, &c. 8vo, 6s.

The Principles and Practice of Gynæcology. By THOMAS ADDIS EMMET, M.D., Surgeon to the Woman's Hospital, New York. Second Edition. Royal 8vo, with 133 Engravings, 24s.

"The work teems with original ideas, fresh and valuable methods of practice, and is written in a clear and elegant style."—*British Medical Journal.*

The Student's Guide to the Diseases of Women. By ALFRED L. GALABIN, M.D., F.R.C.P., Assistant Obstetric Physician to Guy's Hospital. Second Edition. Fcap. 8vo, with many Engravings, 7s. 6d.

Notes on the Diseases of Women. Specially designed for Students Preparing for Examination. By J. J. REYNOLDS, M.R.C.S. Fcap. 8vo, 2s. 6d.

Practical Gynæcology: A Handbook of the Diseases of Women. By HEYWOOD SMITH, M.D. Oxon., Physician to the Hospital for Women and to the British Lying-in Hospital. Crown 8vo, with Engravings, 5s. 6d.

Obstetric Aphorisms: For the Use of Students commencing Midwifery Practice. By JOSEPH G. SWAYNE, M.D. Seventh Edition. Fcap. 8vo, with Engravings, 3s. 6d.

A Complete Handbook of Obstetric Surgery. Giving Short Rules of Practice in every Emergency. By CHARLES CLAY, late Senior Surgeon to St. Mary's Hospital, Manchester. Third Edition. Fcap. 8vo, with 91 Engravings, 6s. 6d.

Schroeder's Manual of Midwifery, including the Pathology of Pregnancy and the Puerperal State. Translated by CHARLES H. CARTER, B.A., M.D. 8vo, with Engravings, 12s. 6d.

Diseases of the Ovaries: Their Diagnosis and Treatment. By T. SPENCER WELLS, F.R.C.S., Consulting Surgeon to the Samaritan Hospital. 8vo, with about 150 Engravings, 21s.

A Handbook of Uterine Therapeutics, and of Diseases of Women. By E. J. TILT, M.D., M.R.C.P. Fourth Edition. Post 8vo, 10s.

By the same Author.

The Change of Life In Health and Disease: a Practical Treatise on the Nervous and other Affections incidental to Women at the Decline of Life. Third Edition. 8vo, 10s. 6d.

Rupture of the Female Perineum: Its Treatment, immediate and remote. By GEORGE G. BANTOCK, M.D., Surgeon to the Samaritan Hospital. 8vo, with 2 Plates, 3s. 6d.

Influence of Posture on Women in Gynecic and Obstetric Practice. By J. H. AVELING, M.D., Physician to the Chelsea Hospital for Women. 8vo, 6s.

A Manual for Hospital Nurses and others engaged in Attending on the Sick. By EDWARD J. DOMVILLE, Surgeon to the Exeter Lying-in Charity. Third Edition. Crown 8vo, 2s. 6d.

The Nurse's Companion: A Manual of General and Monthly Nursing. By CHARLES J. CULLINGWORTH, Surgeon to St. Mary's Hospital, Manchester. Fcap. 8vo, 2s. 6d.

Lectures on Nursing. By W. R. SMITH, M.B., Physician to the Cheltenham Dispensary. Second Edition. Post 8vo, with 26 Engravings, 6s.

Handbook for Nurses for the Sick. By ZEPHERINA P. VEITCH. Second Edition. Crown 8vo, 3s. 6d.

Notes on Fever Nursing.
By J. W. ALLAN, M.B., Superintendent and Physician, City of Glasgow Fever Hospital. Crown 8vo, with Engravings, 2s. 6d.

Chronic Disease of the Heart:
Its Bearings upon Pregnancy, Parturition, and Childbed. By ANGUS MACDONALD, M.D., F.R.S.E., Physician to the Edinburgh Royal Infirmary. 8vo, with Engravings, 8s. 6d.

Asthma:
Its Pathology and Treatment. By J. B. BERKART, M.D., Assistant-Physician to the City of London Hospital for Diseases of the Chest. 8vo, 7s. 6d.

Pay Hospitals and Paying Wards throughout the World.
By HENRY C. BURDETT, Secretary to the Seamen's Hospital Society. 8vo, 7s.

"Clearly and pleasantly written; full of facts, and contains many valuable suggestions."—*British Medical Journal.*

By the same Author.

Cottage Hospitals — General, Fever, and Convalescent: Their Progress, Management, and Work. Second Edition, with many Plans and Illustrations. Crown 8vo, 14s.

Notes on Asthma:
Its Forms and Treatment. By JOHN C. THOROWGOOD, M.D., Physician to the Hospital for Diseases of the Chest, Victoria Park. Third Edition. Crown 8vo, 4s. 6d.

Observations on the Result of Treatment of nearly One Hundred Cases of Asthma. By T. L. PRIDHAM, M.R.C.S. Third Edition. 8vo, 2s. 6d.

Diseases of the Chest:
Contributions to their Clinical History, Pathology, and Treatment. By A. T. HOUGHTON WATERS, M.D., Physician to the Liverpool Royal Infirmary. Second Edition. 8vo, with Plates, 15s.

Winter Cough:
(Catarrh, Bronchitis, Emphysema, Asthma). By HORACE DOBELL, M.D., Consulting Physician to the Royal Hospital for Diseases of the Chest. Third Edition. 8vo, with Coloured Plates, 10s. 6d.

By the same Author.

Loss of Weight, Blood-Spitting, and Lung Disease. Second Edition, to which is added Part VI., "On the Functions and Diseases of the Liver." 8vo, with Chromo-lithograph, 10s. 6d.

The Ocean as a Health Resort:
A Handbook of Practical Information as to Sea Voyages, for the use of Tourists and Invalids. By WILLIAM S. WILSON, L.R.C.P., with Chart of Ocean Routes, &c. Crown 8vo, 7s. 6d.

Prognosis in Cases of Valvular Diseases of the Heart. By THOMAS B. PEACOCK, M.D., F.R.C.P., Honorary Consulting Physician to St. Thomas's Hospital. 8vo, 3s. 6d.

Clinical Medicine:
A Systematic Treatise on the Diagnosis and Treatment of Disease. By AUSTIN FLINT, M.D., Professor of Medicine in the Bellevue Hospital Medical College. 8vo, 20s.

"We strongly recommend his text-book as not only the most recent but the most complete hitherto published on clinical medicine."—*Dublin Medical Journal.*

By the same Author.

Phthisis:
In a series of Clinical Studies. 8vo, 16s.

Diphtheria:
Its Nature and Treatment, Varieties, and Local Expressions. By MORELL MACKENZIE, M.D., Physician to the Hospital for Diseases of the Throat. 8vo, 5s.

Manual of the Physical Diagnosis of Diseases of the Heart, including the use of the Sphygmograph and Cardiograph. By ARTHUR E. SANSOM, M.D., F.R.C.P., Assistant-Physician to the London Hospital. Third Edition. Fcap. 8vo, with 48 Engravings, 7s. 6d.

By the same Author.

The Antiseptic System in Medicine and Surgery: A Treatise on Carbolic Acid and its Compounds, etc. With 9 Plates (42 Figures), 8vo, 10s. 6d.

Tracheotomy,
Especially in Relation to Diseases of the Larynx and Trachea. By PUGIN THORNTON, late Surgeon to the Hospital for Diseases of the Throat. 8vo, with Photographic Plates and Woodcuts, 5s. 6d.

Lectures on Syphilis of the Larynx (Lesions of the Secondary and Intermediate Stages). By W. MACNEILL WHISTLER, M.D., Physician to the Hospital for Diseases of the Throat and Chest. Post 8vo, 4s.

Sore Throat:
Its Nature, Varieties, and Treatment. By PROSSER JAMES, M.D., Physician to the Hospital for Diseases of the Throat. Fourth Edition. Post 8vo, with Coloured Plates and Engravings, 6s. 6d.

A Treatise on Vocal Physiology and Hygiene, with especial reference to the Cultivation and Preservation of the Voice. By GORDON HOLMES, L.R.C.P. Edin., Physician to the Municipal Throat and Ear Infirmary. Second Edition. With Engravings. Crown 8vo, 6s. 6d.

Principal Health Resorts
Of Europe and Africa, and their Use in the Treatment of Chronic Diseases. By THOMAS MORE MADDEN, M.D., M.R.I.A. 8vo, 10s.

The Riviera:
Sketches of the Health Resorts of the North Mediterranean Coast of France and Italy, from Hyères to Spezia; with Chapters on the General Meteorology of the District, its Medical Aspect and Value, &c. By EDWARD I. SPARKS, M.B., F.R.C.P. Crown 8vo, 8s. 6d.

"He has spared no pains in making his book as comprehensive and accurate as possible."—*Edinburgh Medical Journal.*

"We do not know any work on the subject which more completely supplies all that varied information which it is so important for any one to have who for the first time visits the Riviera as a patient. It often happens that a physician suddenly orders an invalid to be sent there without loss of time. The patient and his friends alike know nothing of the place, and nothing of the best means of making the journey. In Dr. Sparks's volume they will find all the information they require."—*Pall Mall Gazette.*

Winter and Spring
On the Shores of the Mediterranean. By HENRY BENNET, M.D. Fifth Edition. Post 8vo, with numerous Plates, Maps, and Engravings, 12s. 6d.

By the same Author.

Treatment of Pulmonary Consumption by Hygiene, Climate, and Medicine. Third Edition. 8vo, 7s. 6d.

Family Medicine for India:
By WILLIAM J. MOORE, M.D. Published under the Authority of the Government of India. Third Edition. Post 8vo, with 66 Engravings, 12s.

The Elements of Indian Hygiene.
Intended to guide the Public in acquiring some knowledge in the all-important subject of the Preservation of Health and Prevention of Sickness. By JOHN C. LUCAS, F.R.C.S., H.M.'s Indian Medical Service. Crown 8vo, with Map of India, &c., 5s.

Bazaar Medicines of India,
And Common Medical Plants: With Full Index of Diseases, indicating their Treatment by these and other Agents procurable throughout India, &c. By E. J. WARING, M.D., F.R.C.P. Third Edition. Fcap. 8vo, 5s.

Indian Notes.
The Voyage out; Travelling in India; Upper India; Stations; The Hills; Mineral Waters; Herbs and Simples. By F. R. HOGG, M.D., Surgeon-Major. Crown 8vo, 5s.

Diseases of Tropical Climates,
And their Treatment: with Hints for the Preservation of Health in the Tropics. By JAMES A. HORTON, M.D., Surgeon-Major. Second Edition. Post 8vo, 12s. 6d.

Endemic Diseases of Tropical Climates, with their Treatment. By JOHN SULLIVAN, M.D. Post 8vo, 6s.

Diseases of the Stomach:
The Varieties of Dyspepsia, their Diagnosis and Treatment. By S. O. HABERSHON, M.D., F.R.C.P., late Senior Physician to Guy's Hospital. Third Edition. Crown 8vo, 5s.

By the same Author.

Pathology of the Pneumogastric Nerve, being the Lumleian Lectures for 1876. Post 8vo, 3s. 6d.

Also.

Diseases of the Abdomen,
Comprising those of the Stomach and other parts of the Alimentary Canal, Œsophagus, Cæcum, Intestines, and Peritoneum. Third Edition. 8vo, with 5 Plates, 21s.

Studies on Functional Nervous Disorders. By C. HANDFIELD JONES, M.B., F.R.S., Physician to St. Mary's Hospital. Second Edition. 8vo, 18s.

Headaches:
Their Nature, Causes, and Treatment. By WILLIAM H. DAY, M.D., Physician to the Samaritan Hospital for Women and Children. Third Edition. Crown 8vo, with Engravings, 6s. 6d.

Fits:
Diagnosis and Immediate Treatment of Cases of Insensibility and Convulsions. By JOHN H. WATERS, M.D., K.C., St.G.C., Surgeon to the C Division of Metropolitan Police. Crown 8vo, bound in leather, 4s.

Nervous Diseases:
Their Description and Treatment. By ALLEN McLANE HAMILTON, M.D., Physician at the Epileptic and Paralytic Hospital, Blackwell's Island, New York. Royal 8vo, with 53 Illustrations, 14s.

Lectures on Diseases of the Nervous System. By SAMUEL WILKS, M.D., F.R.S., Physician to, and Lecturer on Medicine at, Guy's Hospital. 8vo, 15s.

On Megrim, Sick Headache and some Allied Disorders: a Contribution to the Pathology of Nerve Storms. By EDWARD LIVEING, M.D., F.R.C.P. 8vo, 15s.

Nutrition in Health and Disease:
A Contribution to Hygiene and to Clinical Medicine. By HENRY BENNET, M.D. Third (Library) Edition. 8vo, 7s. Cheap Edition (Second Thousand). Fcap. 8vo, 2s. 6d.

Food and Dietetics,
Physiologically and Therapeutically Considered. By F. W. PAVY, M.D., F.R.S., Physician to Guy's Hospital. Second Edition. 8vo, 15s.

By the same Author.

Croonian Lectures on Certain Points connected with Diabetes. 8vo, 4s. 6d.

Imperfect Digestion:
Its Causes and Treatment. By ARTHUR LEARED, M.D., F.R.C.P. Sixth Edition. Fcap. 8vo, 4s. 6d.

The Sympathetic System of Nerves: Their Physiology and Pathology. By Professor EULENBURG and Dr. P. GUTTMANN. Translated by A. NAPIER, M.D., F.F.P.S. 8vo, 5s.

Gout, Rheumatism,
And the Allied Affections; with a Chapter on Longevity and the Causes Antagonistic to it. By PETER HOOD, M.D. Second Edition. Crown 8vo, 10s. 6d.

Notes on Rheumatism.
By JULIUS POLLOCK, M.D., F.R.C.P., Senior Physician to the Charing Cross Hospital. Second Edition. Fcap. 8vo, with Engravings, 3s. 6d.

Lectures on Dermatology:
Delivered at the Royal College of Surgeons, by ERASMUS WILSON, F.R.C.S., F.R.S. 1870, 6s.; 1871–73, 10s. 6d.; 1874–75, 10s. 6d.; 1876–78, 10s. 6d.

Parasites:
A Treatise on the Entozoa of Man and Animals, including some Account of the Ectozoa. By T. SPENCER COBBOLD, M.D., F.R.S. 8vo, with 85 Engravings, 15s.

"The work fixes the reader by the complete mastery of the subject displayed by the author, and by the ever-strange and increasing interest of its several details."—*Lancet.*

"The most complete and most important treatise on entozoa accessible to the English reader."—*Medical Press and Circular.*

Atlas of Skin Diseases:
By TILBURY FOX, M.D., F.R.C.P. With 72 Coloured Plates. Royal 4to, half morocco, £6 6s.

Eczema:
By McCALL ANDERSON, M.D., Professor of Clinical Medicine in the University of Glasgow. Third Edition. 8vo, with Engravings, 7s. 6d.

Psoriasis, or Lepra.
By GEORGE GASKOIN, M.R.C.S., Surgeon to the British Hospital for Diseases of the Skin. 8vo, 5s.

On Certain Rare Diseases of the Skin: being Vol. 1 of "Lectures on Clinical Surgery." By JONATHAN HUTCHINSON, Senior Surgeon to the London Hospital, and to the Hospital for Diseases of the Skin. 8vo, 10s. 6d.

Certain Forms of Cancer,
With a New and Successful Mode of treating it. By ALEX. MARSDEN, Senior Surgeon to the Cancer Hospital. Second Edition. 8vo, with Coloured Plates, 8s. 6d.

Cancer Life:
Its Causes, Progress, and Treatment. A General and Historical Treatise. By ROBERT MITCHELL, M.R.C.S. 8vo, 7s. 6d.

A Manual of the Laws affecting Medical Men. By ROBERT G. GLENN, LL.B., Barrister-at-Law. 8vo, 14s.

The Medical Adviser in Life Assurance. By EDWARD H. SIEVEKING, M.D., F.R.C.P., Physician to St. Mary's and Lock Hospitals, &c. Crown 8vo, 6s.

Madness:
In its Medical, Legal, and Social Aspects. Lectures by EDGAR SHEPPARD, M.D., M.R.C.P., Professor of Psychological Medicine in King's College. 8vo, 6s. 6d.

Medical Jurisprudence: Its Principles and Practice. By ALFRED S. TAYLOR, M.D., F.R.C.P., F.R.S. Second Edition. 2 vols. 8vo, with 189 Engravings, £1 11s. 6d.

By the same Author.

A Manual of Medical Jurisprudence. Tenth Edition. Crown 8vo, with 55 Engravings, 14s.

Also.

Poisons, In Relation to Medical Jurisprudence and Medicine. Third Edition. Crown 8vo, with 104 Engravings, 16s.

Lectures on Medical Jurisprudence. By FRANCIS OGSTON, M.D., Professor in the University of Aberdeen. Edited by FRANCIS OGSTON, JUN., M.D. 8vo, with 12 Copper Plates, 18s.

A Manual of Psychological Medicine: With an Appendix of Cases. By JOHN C. BUCKNILL, M.D., F.R.S., and D. HACK TUKE, M.D., F.R.C.P. Fourth Edition. 8vo, with 12 Plates (30 Figures) and Engravings, 25s.

Medical Ophthalmoscopy: A Manual and Atlas. By WILLIAM R. GOWERS, M.D., F.R.C.P., Assistant Professor of Clinical Medicine and Physician to the National Hospital for the Paralysed and Epileptic. With 16 Coloured Autotype and Lithographic Plates, and Woodcuts, comprising 112 Original Illustrations of the Changes in the Eye in Diseases of the Brain, Kidneys, &c. 8vo, 18s.

By the same Author.

Pseudo-Hypertrophic Muscular Paralysis: A Clinical Lecture. 8vo, with Engravings and Plate, 3s. 6d.

Also.

The Diagnosis of Diseases of the Spinal Cord. 8vo, with Coloured Plate and Engravings, 4s. 6d.

Idiocy and Imbecility. By WILLIAM W. IRELAND, M.D., Medical Superintendent of the Scottish National Institution for the Education of Imbecile Children at Larbert, Stirlingshire. 8vo, with Engravings, 14s.

A Handy-Book of Forensic Medicine and Toxicology. By W. BATHURST WOODMAN, M.D., F.R.C.P., and C. MEYMOTT TIDY, M.D., F.C.S. 8vo, with 8 Lithographic Plates and 116 Engravings, 31s. 6d.

A Manual of Practical Hygiene. By E. A. PARKES, M.D., F.R.S. Fifth Edition. By F. DE CHAUMONT, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School. 8vo, with 9 Plates and 112 Engravings, 18s.

Sanitary Examinations Of Water, Air, and Food. A Vade-Mecum for the Medical Officer of Health. By CORNELIUS B. FOX, M.D., F.R.C.P. Crown 8vo, with 94 Engravings, 12s. 6d.

Dangers to Health: A Pictorial Guide to Domestic Sanitary Defects. By T. PRIDGIN TEALE, M.A., Surgeon to the Leeds General Infirmary. Second Edition. 8vo, with 55 Lithograph Plates (mostly coloured), 10s.

Microscopical Examination of Drinking Water. By JOHN D. MACDONALD, M.D., F.R.S., Assistant Professor in the Army Medical School. 8vo, with 24 Plates, 7s. 6d.

A Handbook of Hygiene and Sanitary Science. By GEO. WILSON, M.A., M.D., Medical Officer of Health for Mid-Warwickshire. Fourth Edition. Post 8vo, with Engravings, 10s. 6d.

Also.

Healthy Life and Healthy Dwellings: A Guide to Personal and Domestic Hygiene. Fcap. 8vo, 5s.

CHAP. 1, Introductory; 2, The Human Body; 3, Causes of Disease; 4, Food and Diet; 5, Cleanliness and Clothing; 6, Exercise, Recreation, and Training; 7, The House and its Surroundings; 8, Infectious Diseases and their Prevention.

Dress: Its Sanitary Aspect. A Paper read before the Brighton Social Union, Jan. 30, 1880. By BERNARD ROTH, F.R.C.S. 8vo, with 8 Plates, 2s.

Handbook of Medical and Surgical Electricity. By HERBERT TIBBITS, M.D., F.R.C.P.E., Senior Physician to the West London Hospital for Paralysis and Epilepsy. Second Edition. 8vo, with 95 Engravings, 9s.

By the same Author.

A Map of Ziemssen's Motor Points of the Human Body: A Guide to Localised Electrisation. Mounted on Rollers, 35 × 21. With 20 Illustrations, 5s.

Lectures on the Clinical Uses of Electricity. By J. RUSSELL REYNOLDS, M.D., F.R.S., Physician to University College Hospital. Second Edition. Post 8vo, 3s. 6d.

The Student's Guide to Diseases of the Eye. By EDWARD NETTLESHIP, F.R.C.S., Ophthalmic Surgeon to, and Lecturer on Ophthalmic Surgery at, St. Thomas's Hospital. Fcap. 8vo, with 48 Engravings, 7s. 6d.

"It is clear, concise, and thoroughly scientific." —*British Medical Journal.*

"A thoroughly good manual. It is clear, accurate, and scientific, and will prove of great value." —*London Medical Record.*

A Manual of the Principles and Practice of Ophthalmic Medicine and Surgery. By T. WHARTON JONES, F.R.C.S., F.R.S. Third Edition. Fcap. 8vo, with 9 Coloured Plates and 173 Engravings, 12s. 6d.

A Manual of Diseases of the Eye. By C. MACNAMARA, F.R.C.S., Surgeon to Westminster Hospital. Third Edition. Fcap. 8vo, with Coloured Plates and Engravings, 12s. 6d.

Diseases of the Eye: By HAYNES WALTON, F.R.C.S., Surgeon to St. Mary's Hospital and in charge of its Ophthalmological Department. Third Edition. 8vo, with 3 Plates and nearly 300 Engravings, 25s.

Hints on Ophthalmic Out-Patient Practice. By CHARLES HIGGENS, Ophthalmic Assistant-Surgeon to, and Lecturer on Ophthalmology at, Guy's Hospital. Second Edition. Fcap. 8vo, 3s.

Glaucoma: Its Causes, Symptoms, Pathology, and Treatment. By PRIESTLEY SMITH, M.R.C.S., Ophthalmic Surgeon to the Queen's Hospital, Birmingham. 8vo, with Lithographic Plates (comprising 58 Figures), 10s. 6d.

"A brilliant contribution to ophthalmology, of which British surgery may be justly proud."—*Birmingham Medical Review.*

Essays in Ophthalmology. By GEORGE E. WALKER, F.R.C.S., Surgeon to St. Paul's Eye and Ear Hospital, &c., Liverpool. Post 8vo, 6s.

Liebreich's Atlas of Ophthalmoscopy: Composed of 12 Chromolithographic Plates (containing 59 Figures). The Text translated by H. ROSBOROUGH SWANZY, M.B. Second Edition. 4to, 30s.

A Manual of Ophthalmoscopy for the use of Students. By DR. DAGUENET. Translated by C. S. JEAFFRESON, Surgeon to the Newcastle-on-Tyne Eye Infirmary. With Engravings. Fcap. 8vo, 5s.

A System of Dental Surgery. By JOHN TOMES, F.R.S., and CHARLES S. TOMES, M.A., F.R.S. Second Edition. Fcap. 8vo, with 268 Engravings, 14s.

Dental Anatomy, Human and Comparative: a Manual. By CHARLES S. TOMES, M.A., F.R.S. Second Edition. Crown 8vo, with many Engravings.
[*In the Press.*

A Manual of Dental Mechanics. By OAKLEY COLES, L.D.S.R.C.S., Surgeon-Dentist to the Hospital for Diseases of the Throat. Second Edition. Crown 8vo, with 140 Engravings, 7s. 6d.

The Student's Guide to Dental Anatomy and Surgery. By HENRY SEWILL, M.R.C.S., L.D.S. Fcap. 8vo, with 77 Engravings, 5s. 6d.

A Practical Treatise on Operative Dentistry. By JONATHAN TAFT, D.D.S., Professor in the Ohio College of Dental Surgery. Third Edition. With 134 Engravings. 8vo, 18s.

A Dictionary of Medical Science: Containing a concise Explanation of the various Subjects and Terms of Medicine, &c.; Notices of Climate and Mineral Waters; Formulæ for Officinal, Empirical, and Dietetic Preparations; with the Accentuation and Etymology of the Terms, and the French and other Synonyms. By ROBLEY DUNGLISON, M.D., LL.D. New Edition. Royal 8vo, 28s.

A Medical Vocabulary: Being an Explanation of all Terms and Phrases used in the various Departments of Medical Science and Practice, giving their Derivation, Meaning, Application, and Pronunciation. By ROBERT G. MAYNE, M.D., LL.D. Fourth Edition. Fcap. 8vo, 10s.

Abridged Medical Account Books. The "Expedite" Method. By JAMES MACNAB, L.R.C.S.E. *Index Ledger.* Royal 4to. For one year, 5s. 6d.; for four years, 21s. *Visiting List.* Cloth, 2s. 6d.; Leather, 3s. 6d.

Medical Education And Practice in all parts of the World. By HERBERT JUNIUS HARDWICKE, M.D., M.R.C.P. 8vo, 10s.

"Dr. Hardwicke's book will prove a valuable source of information to those who may desire to know the conditions upon which medical practice is or may be pursued in any or every country of the world, even to the remotest corners of the earth. The work has been compiled with great care, and must have required a vast amount of labour and perseverance on the part of its author." —*Dublin Medical Journal.*

INDEX TO J. & A. CHURCHILL'S CATALOGUE.

[*Continued on the next page.*

51