

Typhoid fever : its nature, mode of spreading, and prevention / By William Budd, M.D., F.R.S.

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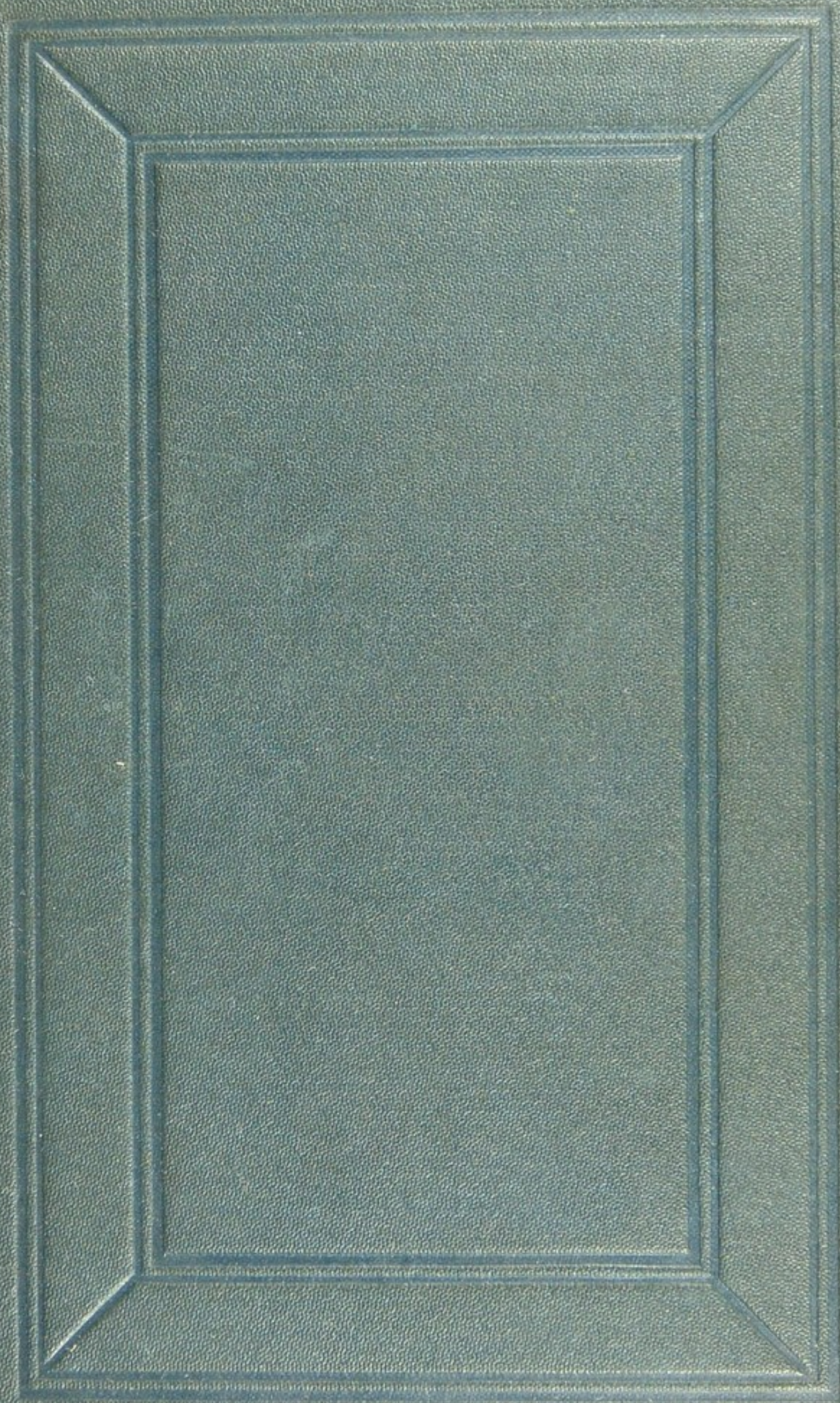
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TYPHOID FEVER.



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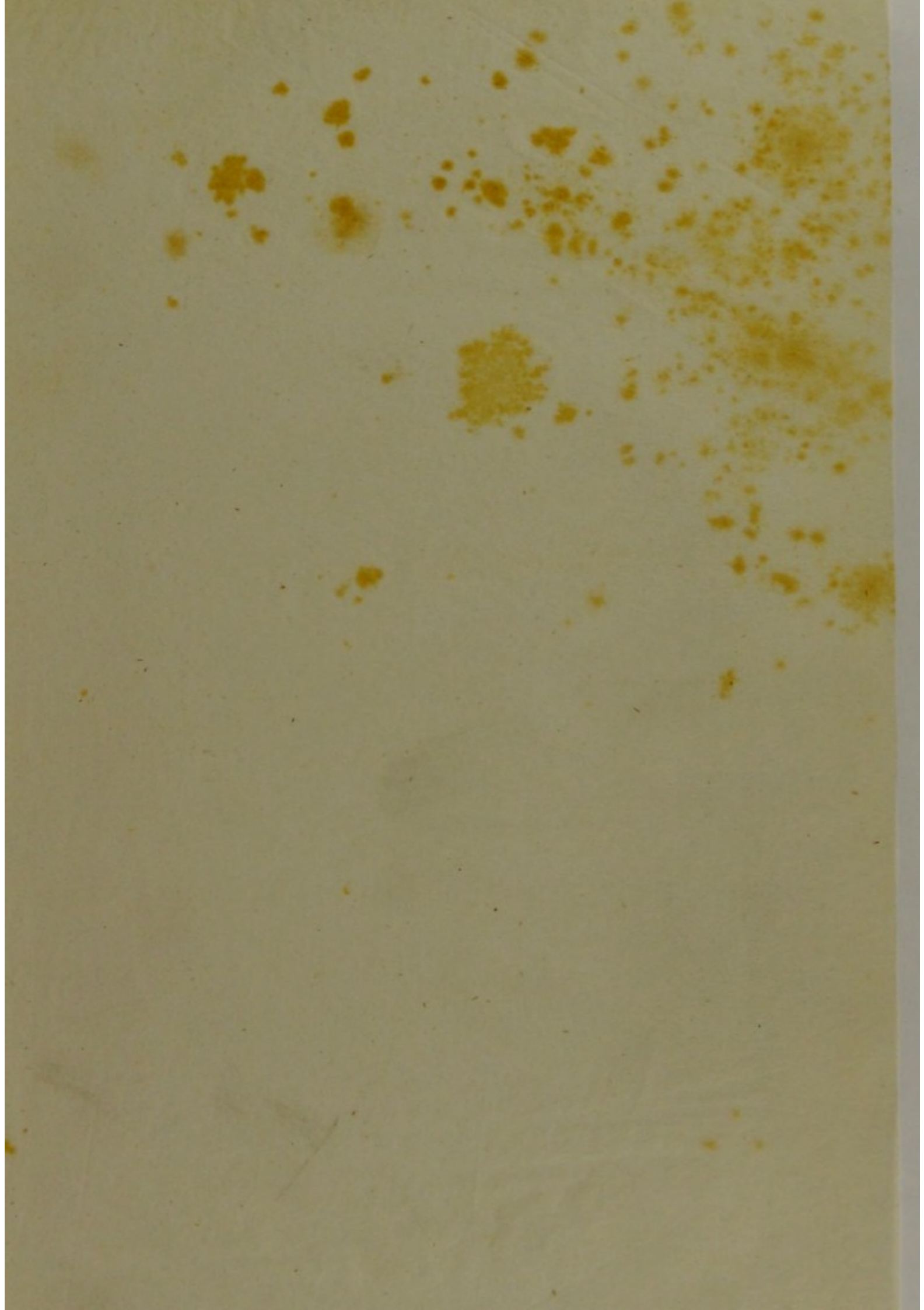


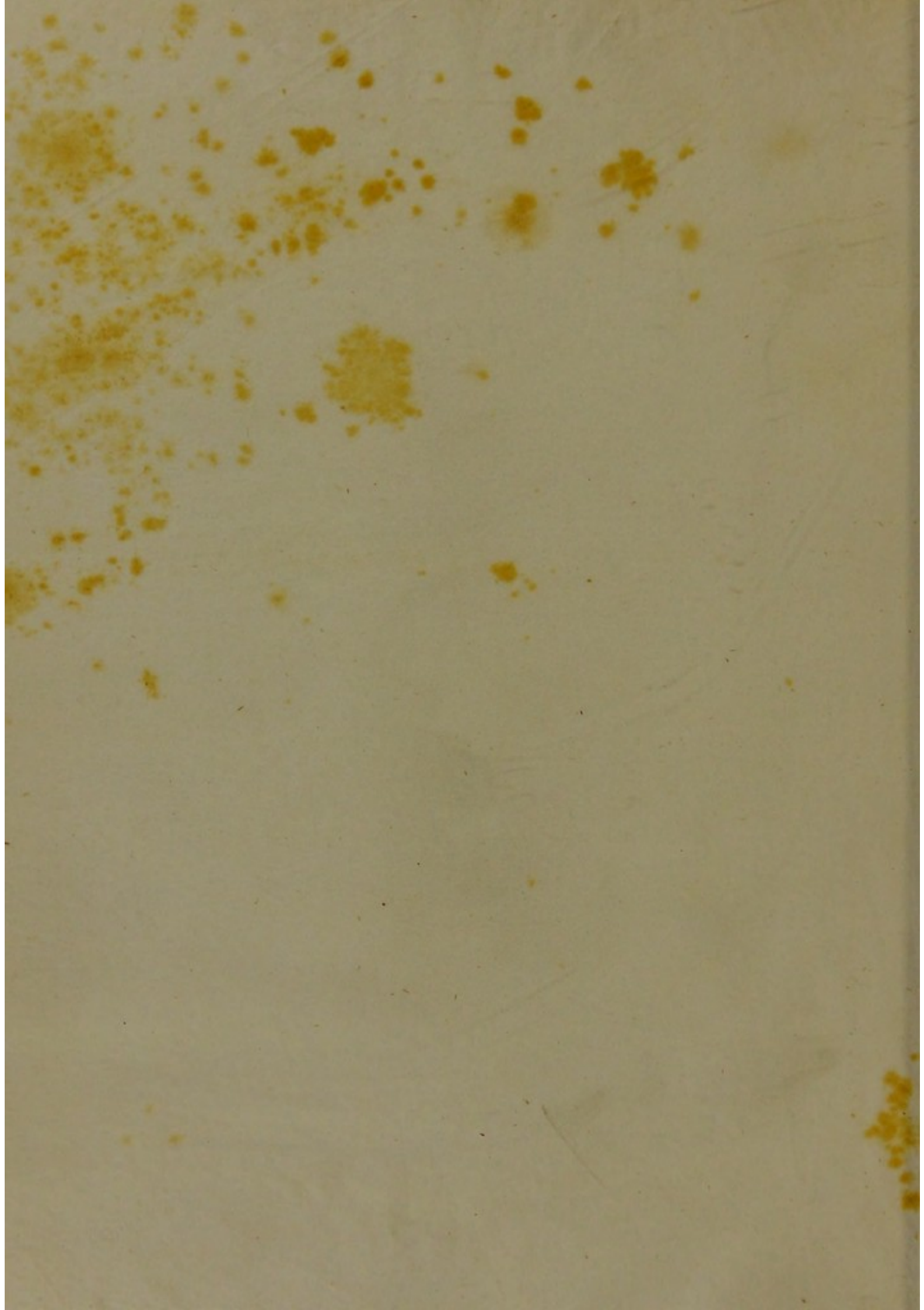


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Kell Bro^s Lith London.

Typhoid Fever 17th day.





TYPHOID FEVER :

ITS NATURE, MODE OF SPREADING, AND PREVENTION.

BY

WILLIAM BUDD, M.D., F.R.S.

LONDON :
LONGMANS, GREEN, AND CO.
1873.



TO

SIR T. WATSON, BART., M.D., F.R.S.

IN

GRATEFUL REMEMBRANCE OF MANY ENCOURAGING WORDS

THIS BOOK IS DEDICATED

BY HIS FORMER PUPIL

THE AUTHOR.

PREFACE.

IN 1857, on the occasion of a severe outbreak of Typhoid Fever in the Clergy Orphan School, St. John's Wood, London, I enunciated, briefly, in a Paper published in the 'Lancet,' the doctrines as to the nature and mode of spreading of this fever, of which the following pages only give a fuller development.¹ For many years prior to the date of that Paper, I had taught these doctrines in my Lectures at the Bristol Medical School, and had acted upon them in practice, in the prevention of the fever, but this was the first time that I had put them before the Profession, in a separate form, in print.

This Paper was followed by a series of others, a part of which appeared in the 'Lancet,' and a part in the 'British Medical Journal,' down to 1860—papers in which

¹ The Paper here referred to, together with the narrative in the *Lancet* by which it was suggested, are reprinted in the Appendix.

the whole subject of the nature and causation of Typhoid Fever was pretty fully treated.

The substance of these papers, which were founded on observations extending over a period of more than twenty years, is reproduced in the following pages ; and, together with some important evidence which has since come into my hands, constitutes the basis of the present work.

While it has been passing through the press, the Registrar General's Report for 1872 has been issued. The Report shows a marked and very encouraging reduction in the mortality from fever, upon the returns of most former years. The deaths from the principal forms of fever—typhus included—were, for the entire year, only 13,507. In the six preceding years, ending with 1871, which, as well as 1872, was a year of comparatively light fever mortality, the yearly average of deaths under the same head exceeded 18,000. In 1866, they were over 21,000: in 1868, they bordered closely on 20,000. At a somewhat earlier date they averaged, for a whole quinquenniad, more than 20,000 per annum.

It will be understood that these returns are for England only, and do not include Scotland and Ireland.

Concurrently with the decrease of deaths from fever in

1872, there was, in spite of the continued prevalence of small-pox, a corresponding decrease in the death rate from most other Zymotic diseases, especially in the last quarter of the year, when the rainfall was so heavy.

In that quarter, the seven principal Zymotic diseases caused only 16,794 deaths as against 25,907, and 26,977, in the corresponding quarters of 1870 and 1871.

‘Abundant rain,’ the Registrar General remarks, ‘not only mechanically cleanses the streets and sewers, but purifies the atmosphere, and carries off zymotic exhalations which generate disease.’

‘Although’—the same distinguished authority continues—‘the remarkably low death rate in town districts, last quarter, may be due to the somewhat unusual meteorological conditions which prevailed, it may be safe to assume that a portion of the improvement in their health is permanent, and is the result of the general awakening to the importance of sanitary measures which has been so conspicuous in the last few years.’

There are very special reasons for the hope that, as far as the fever treated of in the following pages is concerned, this assumption will prove to be true.

It is obvious that if the great abatement in its prevalence, observed in 1872, should continue, a considerable reduc-

tion will have to be made in some of the numerical statements which occur in subsequent pages, in order to adapt them to current experience.

P. S.—Since the beginning of the present year a series of Reports of local outbreaks of typhoid fever in various parts of the kingdom have appeared, which possess great interest and importance, in their bearing on the leading questions hereinafter discussed. Among these Reports I may mention two by Dr. Ballard, on typhoid at Nunney, near Frome, and at Hawkesbury Upton, in Gloucestershire, and a third by Dr. Thorne on fever at Whitchurch—all issued by the Medical Department of the Privy Council—as especially valuable in this respect. A Report by Dr. Russell on an epidemic of the same fever at Parkhead, originating in the use of fever-tainted milk, is scarcely less so. These Reports are admirably drawn up, and they all offer the most striking illustrations of the twofold position that typhoid fever is a strictly self-propagating fever, (mainly) disseminated by the specific discharges from the sick. I much regret that they have come to hand too late to enable me to take advantage of the materials they contain.

March, 1873.

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Erratum.

At page 30, second line, *for* 'last chapter' *read* 'last section.'

NOTE.



*This volume was printed in the spring of 1873;
but, owing to the illness of the Author, the publication
has been delayed.*

Errata.

Page 28, lines 13 and 14, omit the most.

„ 24, *line 16, omit for 1845.*

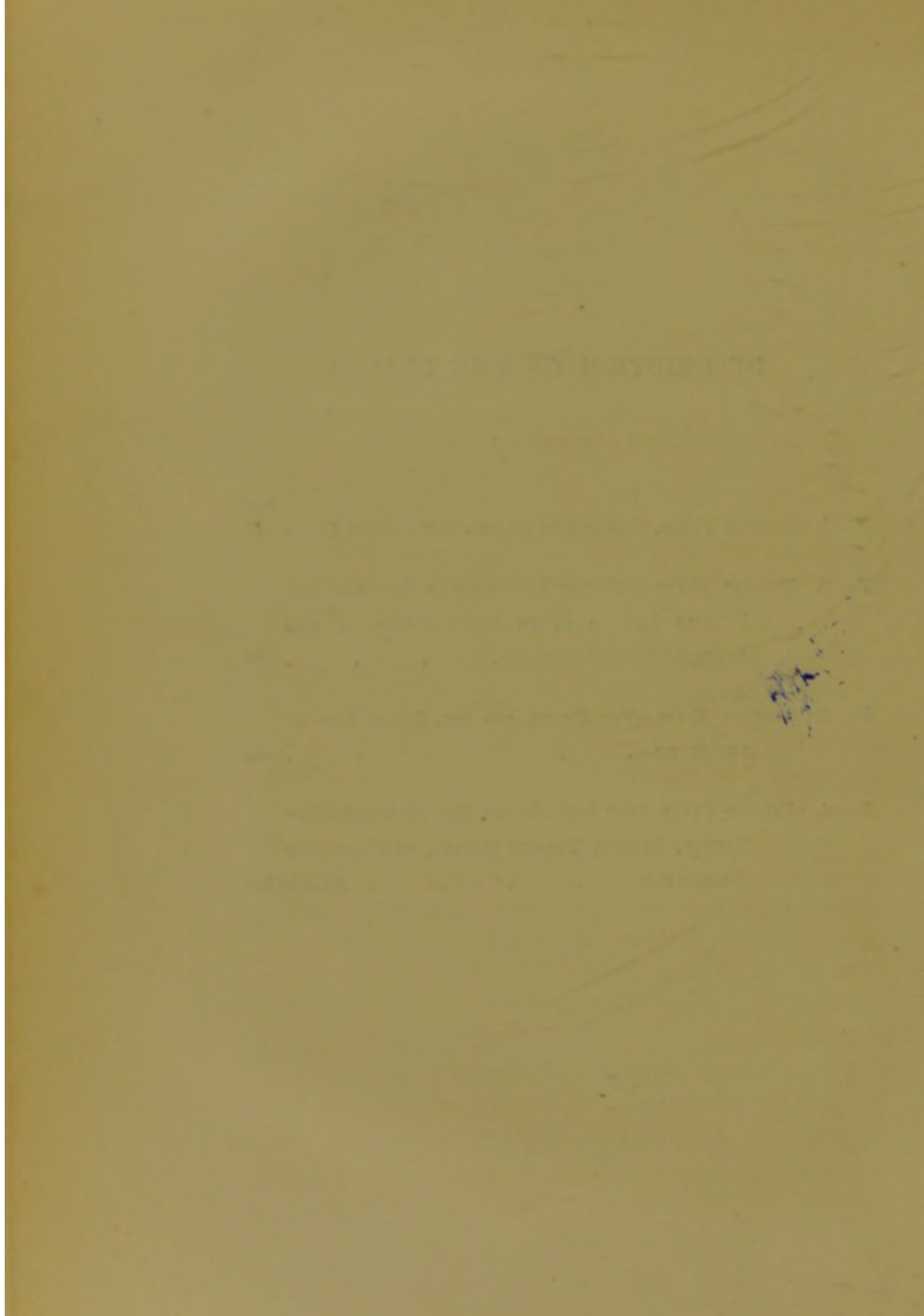
„ 24, „ 17, *for 1848 read 1828. See also 'Journal des Archives*

Médicales' for 1829, p. 361.

DESCRIPTION OF THE PLATES.



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TYPHOID FEVER.

CHAPTER I.

INTRODUCTORY.

‘Time wears off the fictions of opinion, and doth, by degrees, discover and unmask the fallacy of ungrounded persuasion.’—BP. WILKINS.

SYNONYMS—*English*: Gastric Fever—Enteric or Intestinal Fever—Low Fever—Common Continued Fever—Infantile Remittent—Endemic Fever—Pythogenic Fever—*German*: Darm-Fieber—Darm-Typhus—Ileo-Typhus—Typhus Abdominalis—*French*: La Fièvre typhoïde—La Dothinenterie—Fièvre entéro-mésentérique.

§ 1. THERE are few things which concern the people of this country more deeply than to know the exact truth touching the mode in which this fatal fever is disseminated amongst them. Every year, on an average—take the United Kingdom through—some fifteen thousand or more of their number perish prematurely by it: a population equal to that of a considerable city every year swept into the grave by a single, and, as I hope to show, a perfectly preventable plague.

As nine or ten recover for every one who dies, one hundred and forty thousand persons, or more, must every year pass through its protracted miseries. The real amount of suffering involved in this is, however, but feebly represented by these bald figures.

No one can know what they really imply who has not had experience of this fever in his own home. The dreary and painful night-watches—the great length of the period over which the anxiety is extended—the long suspense between hope and fear, and the large number of the cases in which hope is disappointed and the worst fear is at last realised, make up a sum of distress that is scarcely to be found in the history of any other acute disorder. Even in the highest class of society, the introduction of this fever into the household is an event that generally long stands prominently out in the record of family afflictions. But if this be true of the mansions of the rich, who have every means of alleviation which wealth can command, how much more true must it be of the cottages of the poor, who have scant provision even for the necessaries of life, and none for its great emergencies! Here, when Fever once enters, WANT soon follows, and CONTAGION is not slow to add its peculiar bitterness to the trial.

As the disease is, by far, most fatal to persons in middle life, the mother or father, or both, are often the first to succumb, and the young survivors being left without support, their home is broken up and their destitution becomes complete.

How often have I seen in past days, in the single narrow chamber of the day labourer's cottage, the father in the coffin, the mother in the sick bed in muttering delirium, and nothing to relieve the desolation of the children but the devotion of some poor neighbour who in too many cases paid the penalty of her kindness in becoming, herself, the victim of the same disorder!

In its ordinary course, human life has few such consummations of misery as this.

It is impossible to contemplate events such as these,

merely as objects of science. It is, indeed, a fundamental axiom in scientific investigation that our emotions should be rigidly excluded from it. But, although, by the nature of things they cannot help in the solution of a problem, they may, at least, be suffered to give a spur to inquiry. Where the interests concerned are the sacred interests of life and death, this is their proper function, and that in a degree of which none of the common alternatives that hang upon human duty can give any adequate measure. It were well with us all if they were more often allowed to have their true weight with the conscience.

Having been by accident thrown much in the way of this fever, I have long felt that it is impossible to bear a part in the calamities of which it is the source, without becoming possessed with a burning desire to devote the best powers of the mind to the discovery of means by which such calamities may be prevented.

From the fact, already referred to, of its being so much more deadly to grown-up persons, this disease has a relation to pauperism which is almost peculiar to itself.

‘From returns made in 1838 by the medical officers of twenty unions and parishes in the metropolis, it appeared that 13,972 cases of claims to relief on the ground of destitution were created during that year by attacks of fever’ alone, and that in 1,281 cases the attacks proved fatal. The general deaths from fever in the metropolis during that year appear, from the Summary of the Superintendent Registrar’s returns, to have been 5,634.’¹

¹ Letter from the Poor Law Commissioners to the Metropolitan Board of Guardians, November 1840. A certain proportion of these cases were probably cases of *typhus*. But what proportion cannot now be told, as, at that time, save by a few scientific men, *typhoid* and *typhus* were confounded with one another, and registered as one disease. Both, as being much more fatal to adults than to young persons, may be described as specially pauperising fevers.

It is humiliating to think that issues such as these should be contingent on the powers of an agent so low in the scale of being, that the mildew which springs up on decaying wood must be considered high in comparison.

To know how these powers take effect, in what way they grow to such a height, and to learn therefrom, perchance, by what means their operation may be defeated, are problems in which human happiness is deeply interested.

Perhaps there are few battles to be fought, in which a successful issue depends so closely as here on a real knowledge of the enemy.

If it be true of diseases in general, that all prevention must be based on an intimate knowledge of their causes, how much more true must it be of that great group of diseases which is the work of definite and specific agents, having not only the power of breeding within the body, but capable, for limited periods at least, of existing externally to it? For it is clear, that, in such a case, a thing against which we may be impotent so long as it infects the body itself, may present, on its issue from the body, the conditions of an easy conquest.

That Typhoid Fever is a true member of this group, or in other words, that it is, in its essence, a contagious or self-propagating fever, was proved long ago.

It is scarcely to the credit of the medical profession that this great truth should still be disputed.

Not to speak of the decisive evidence in its behalf recorded by Bretonneau and Gendron de l'Eure, I addressed to the *Lancet* in 1858-59, a series of papers containing observations, which still seem to me to suffice, of themselves, to establish it. The doctrines advanced in those papers as to the origin and mode of spreading of typhoid fever,—doctrines which it is the object of the

present work still further to enforce,—are, in fact, entirely founded on this view of its nature.

These doctrines have since received the adhesion of many scientific men of the highest eminence. Among these, it will be sufficient to mention Sir T. Watson, who, in his admirable work on the Practice of Medicine, has not only lent his powerful sanction to them, but has given an exposition of them in that terse and lucid style of which he is so great a master.

But, notwithstanding this, there is abundant reason to believe that, in respect of this species of fever, the great majority, not of the laity only, but of the profession also, still remain anti-contagionists. And this, moreover, is not only true of the rank and file, but distinguished men, who have gained great credit and wide acceptance as teachers of medical science, are to be found, who appear to lean to the same side.

In a Manual on the Practice of Medicine, by the late Dr. Tanner—a book which has reached its sixth edition—the doctrine that typhoid fever is a contagious fever, and is chiefly propagated by the typhoid excreta, is spoken of as ‘an illusive hypothesis ;’ while, in Hooper’s ‘Physician’s Vade Mecum,’ another standard work, the Editors, Drs. Guy and Harley, dismiss this doctrine with the following remark:—‘Observed facts and the few experiments which have been made tend, however, to disprove these views.’

Treating expressly of the question of contagion, the same writers proceed thus:—

‘Much doubt prevails whether enteric (typhoid) fever be infectious or not, and the question really turns upon the existence of a distinct, specific poison. Positive proof that it may be conveyed from one person to another is wanting, and certainly the majority of people affected with the disease derive it, upon the clearest evidence, from one

and the same source. Those in attendance upon persons suffering from enteric fever do sometimes fall ill of the disease, but the source of the disease may be present in any house.'

In the discussions on the cause of typhoid fever which filled so large a space in the public papers, both lay and medical, for many weeks together, on a recent memorable occasion, the idea of contagion in connection with the disease was almost universally either ignored or repudiated.

A medical friend of my own, who had the curiosity to read almost everything that fell from the press during that anxious time, tells me that he met with the word 'contagion' but seven times altogether, and that in four of the seven it was only mentioned to be discredited.

The profession in foreign countries, including the leaders of it, seem still to be equally opposed to the idea that the disease is contagious. A foreign Professor with whom I lately discussed the subject—a man second to none in Europe—seemed quite startled to find me contending for its contagious nature, and assured me that his own experience gave no countenance whatever to such a view.

Not long ago, one of the most eminent physicians in the United States held the same language to me.

The practice of both had been confined to large cities.

This direct opposition of opinion to fact in a matter of such vital importance, and so open to observation, is as perplexing as it is discouraging. And the more so, because the property of contagion, of which the proof is so clear, so far from being new to disease, is already familiarly known as the common property of a great family group, of which this fever repeats, in unmistakable traits, the family characteristics.

The great natural order of *contagious* fevers has long filled a conspicuous place in nosological systems. In one

of the number—small pox—the contagious faculty has, in past times, even been made use of for the artificial production of the disease on an enormous scale by direct inoculation. Analogy of the closest kind and of the greatest possible force, is, therefore, at hand to recommend what direct evidence teaches. In other branches of inquiry instances may, no doubt, be cited in which general belief has been slow to follow the advance of knowledge. But such instances, not common at any time, have latterly become more and more rare. Even in the few that have happened, there have commonly been sufficient grounds to palliate, if not to excuse, the popular hesitation and doubt. The problem to be solved has been abstruse; the methods for its solution have been difficult and recondite; the evidence hard to interpret, or the new truth, even when disclosed, has lain beyond the scope of common apprehension. In such a subtle matter as the undulatory theory of light, for instance, it was no great wonder that Young and Fresnel should, for some time, continue to be in advance of their age. But in the case before us, methods are not in question at all—the evidence lies on the very surface of common events, and the conclusion to which it tends, so far from transcending ordinary apprehension, is often so salient as involuntarily to force itself on the mind even of the vulgar, on the first view of the facts.

That this conclusion should still be resisted is, I repeat, very perplexing.

Three principal causes may be cited, as, in part, explaining this great anomaly.

The first is, that medical writers, and especially those among them who exercise the widest influence, pass the greater part of their lives in great metropolitan cities—amid conditions, that is to say, under which, for reasons

that will abundantly appear in the following pages, the operation of contagion in this particular fever is not only masked and obscured, but issues in a mode of distribution of the disease, which to the superficial observer would appear to exclude the idea of contagion altogether.

The second :—

The great zeal with which during the whole period of its existence, the General Board of Health, backed by an able and energetic staff and unlimited printing power, continued to urge anticontagionist doctrines.¹

The third :—

The continued prevalence of very limited views as to what constitutes evidence of contagion or self-propagation in the case of disease.

Formidable as these obstacles are to the reception of more accurate ideas in this matter, they are losing force every day, and must eventually cease to operate. With the growth of liberality in matters of opinion, there is reason to hope that, in course of time, both the public and the profession will allow their just weight to facts when well authenticated. The anticontagionist notions of Southwood Smith and his followers in their application to spreading diseases are already rapidly waning, and must soon finally give way before the exact investigations of a more scientific school; while, on the other hand, the extension of scientific culture cannot fail gra-

¹ It is but just to add, that by its untiring efforts to show how vast is the amount of preventable disease suffered to prevail among us, and how great the danger it presents to the future of our race, this Board did much to awaken the minds of the people and the Government to the national importance of Sanitary Reform, and to give to the question the prominence it has since assumed.

And there is no denying that in fixing attention on defective drainage, as the chief of sanitary evils, its members were guided by a true instinct, however widely they may have erred in their scientific interpretation of the facts.

dually to awaken physicians to wider views on the subject of contagion.

Perhaps it may not be too much to hope that the considerations advanced in the following pages may give some little help in this direction.

It is obvious that the formation of just opinions on the question how diseases spread may depend less on personal ability than on the opportunities for its determination which may fall to the lot of the observer. It is equally obvious that where the question at issue is that of the propagation of disease by human intercourse, rural districts, where the population is thin, and the lines of intercourse are few and always easily traced, offer opportunities for its settlement which are not to be met with in the crowded haunts of large towns.

This is one of the cases in which medical men practising in the country have for the acquirement of medical truths of the highest order advantages which are denied to their metropolitan brethren, and which constitute, on the whole, no mean set-off against the greater privileges of other kinds which the latter enjoy.

In the early part of my professional life, while engaged in country practice in Devonshire, outbreaks of typhoid fever continually fell under my eye, amid conditions singularly favourable to the study of its origin and mode of dissemination.

Of these outbreaks the most memorable occurred in the village of North Tawton, where I then lived.

In addition to the advantages enjoyed by country practitioners, generally, in the observation of such events, there were others peculiar to the position I then occupied.

Having been born and brought up in the village, I was personally acquainted with every inhabitant of it ; and being, as a medical practitioner, in almost exclusive

possession of the field, nearly every one who fell ill, not only in the village itself, but over a large area around it, came immediately under my care.

For tracing the part of personal intercourse in the propagation of disease, better outlook could not possibly be had.

At the date of the outbreak in question, the people of the place numbered some eleven or twelve hundred souls.

Of these, a small minority, consisting chiefly of women and children, worked in a serge factory. The rest were employed in agricultural pursuits.

The spot on which this community dwelt is richly endowed with all the natural conditions of health. Built on a dry soil, in the midst of an open and well drained country, and occupying the side of a hill sloping gently to the north-west, this village had long been justly noted in that part of Devon for the rare healthiness of its site.

What is more to the present purpose is, that it had for many years enjoyed an almost entire immunity from the fever to which it was so soon to pay so large a tribute.

This is the more to be remarked, because there were in the economy of the place, and in the habits of the people, many things which, according to modern views, are hard to reconcile with such a fact. In the first place, there was no general system of sewers. A few houses, occupied by the more opulent, were provided with covered drains, but all these might be counted on the fingers. In the cottages of the men who earned their bread with their hands, and who formed the great bulk of the inhabitants, there was nothing to separate from the open air the offensive matters which collect around human habitations. Each cottage, or group of three or four cottages, had its common privy, to which a simple excavation in the ground served as cesspool. Besides this, it was a part of the

economy of all who worked in the fields, as indeed of many more, to keep a pig, one of whose functions was to furnish manure for the little plot of potatoes which fed man and pig alike. Thus, often, hard by the cottage door there was not only an open privy, but a dungheap also.

Nevertheless, these conditions existed for many years without leading to any of the results which it is the fashion to ascribe to them.

Much there was, as I can myself testify, offensive to the nose, but fever there was none. It could not be said that the atmospheric conditions necessary to fever were wanting, because while this village remained exempt, many neighbouring villages suffered severely from the pest. It could not be said that there were no subjects, for these, as the sequel proved, but too much abounded.

Meanwhile privies, pigstyes and dungheaps continued, year after year, to exhale ill odours, without any specific effect on the public health.

Many generations of swine innocently yielded up their lives, but no fever of this or any other sort could be laid to their charge. I ascertained by an inquiry conducted with the most scrupulous care that for fifteen years there had been no severe outbreak of the disorder, and that for nearly ten there had been but a single case.

For the development of this fever a more specific element was needed than either the swine, the dungheaps, or the privies were, in the common course of things, able to furnish.

In the course of time, as was indeed pretty sure to happen, this element was added, and it was then found that the conditions which had been without power to *generate* fever, had but too great power in promoting its spread when once the germ of fever had been introduced.

On the 11th July, 1839, a first case of typhoid fever

occurred in a poor and crowded dwelling. Before the beginning of November, in the same year, more than eighty of the inhabitants had suffered from it under my care.

I kept an accurate record of all the principal events which marked this terrible outbreak ; and it is to certain of these events, in their bearing on the mode in which this fatal disorder spreads, that I now wish to draw attention.

As, however, the narrative will necessarily occupy some little space, it shall be made the subject of another chapter.

CHAPTER II.

TYPHOID FEVER A CONTAGIOUS OR SELF-PROPAGATING
FEVER.

‘L'affection typhoïde est-elle contagieuse? La réponse à cette question se trouve dans les faits que la science possède; et il me suffit d'en rappeler quelques-uns pour en convaincre le lecteur.’—LOUIS.

‘J'emploierai le mot *contagion* dans sa signification la plus étendue, désignant sous ce nom toute transmission de la maladie d'un individu malade à un individu sain, quel que soit le mode suivant lequel elle s'opère.’—PIEDVACHE.

§ 2. THE first thing to arrest attention after the disorder had become rife in North Tawton was the strong tendency it showed, when once introduced into a family, to spread through the household. Thus, in the family of Ann N——, a young woman who was taken ill in the second week in July, and who was the subject of the first case, the mother, a brother, and a sister—making four in all—were one after another laid up with the same fever; the father, who had already had the disease in former years, and the young infant, being the only inmates spared. In another house, four out of six persons were successively attacked; in another three, and so on. Without going into further details of these cases (of all of which I possess accurate notes), it will be sufficient to say that, before the disease finally died away, there were few houses in which, having once appeared, it did not further extend itself to one or more members of the family. This, which was through-

out its most striking character, was, in itself, sufficient to lead to a strong presumption of the contagious nature of the disorder.

But while these events were occurring in the village itself, there were others happening at a distance, which converted this presumption into a certainty. During the prevalence of the fever in North Tawton, it so happened that three persons left the place after they had become infected. By a fatality which is but too common under such circumstances, all three communicated the disease to one or more of the persons by whom they were surrounded in the new neighbourhood in which they fell ill. Two of these three persons were sawyers by trade, who had hired themselves for a few weeks to a timber merchant living in the village. While these men remained in North Tawton, they lodged in a court with a single and a common privy, and next door to a house in which the fever was. In the course of time both these men sickened for the disorder, and on the occurrence of the first decided symptoms, both returned to their own homes, in the parish of Morchard, about seven miles off.

The first was a married man, with two children. He left North Tawton on August 9, being already too ill to work. Two days after reaching Morchard he took to his bed, and at the end of five weeks he died. Ten days after his death his two children were laid up with the same fever, and had it severely; the widow escaped. The other sawyer, was a single man, and an aged couple who lived with him were the only other inmates of the house. Like his comrade, he was driven from North Tawton by indisposition, which rendered him unable to follow his employment, and cut off his means of support. He began to droop on July 26, but did not leave for Morchard until August 2. On the 3rd he finally took to

his bed. His attack was severe, but, after a long struggle, he recovered. When this man was at his worst, a friend who came to see him was called upon to assist in raising him in bed. While thus employed, the friend was quite overpowered by the smell from the sick man's body. He felt very unwell from that time, and continued to be harassed for days afterwards by a sense of the same pestilent smell, and by a fixed impression, which under the circumstances was natural enough, that he had caught the fever. On the tenth day from the date of this event, he was seized with a violent shiver, which was immediately followed by an attack of typhoid fever of long duration. Before he became convalescent, two of his children got the same fever, as well as a brother, who lived at some distance, but who had repeatedly visited him during his illness.¹

The houses occupied by these four men lay some way apart, and, unless underneath their roofs, there was no fever at the time in that part of the country.

Was this series of events, it may now be asked, the result of chance or the work of contagion? If any rational person should entertain doubts as to the true answer to be given to this question, the history of the next case may be safely left to resolve them. The subject who was the means of propagating the disorder in this instance was a widow named Lee, residing in North Tawton. She began to droop on August 20. On the following day, not knowing what was impending, she went to visit her brother, a farmer who occupied a large farm in the hamlet of Chaffcombe, about seven miles off. On the 23rd she was laid up. On the 24th I was sent for

¹ The most important of these particulars were kindly furnished to me by Mr. Brutton, surgeon at Morchard, who had the charge of this group of cases. By a very natural figure of speech, the fever from which all these persons suffered was known amongst the Morchard people as the 'North Tawton' fever.

to see her, and found her in bed in the first stage of fever. In the after progress of her case, which, though it presented no malignant features, was very prolonged, she exhibited, in turn, all the most characteristic marks of the disorder. Amongst these may be mentioned nose-bleeding, spontaneous and obstinate diarrhœa, tympanitis, dry tongue, low delirium, and other typhoid symptoms, together with (towards the end of the second week) the now well-known eruption of rose-coloured spots. After lying several weeks under my care at Chaffcombe, she slowly recovered.

It may not be amiss to observe, that the fever had become meanwhile so rife at North Tawton, that, while I was attending Mrs. Lee, I had no fewer than seventeen persons under my care in the village in various stages of it.

A few days after she had become convalescent, her sister-in-law (Mrs. Snell), who had nursed her, fell ill of the same fever. Her case was very severe, and, after a protracted struggle, terminated fatally on November 4. The husband (Mr. Snell), who had spent the chief part of his time in his wife's sick room, and had sat up many nights by her, in great anxiety and distress, was the next sufferer. He began to droop in the last week of October, but was not finally laid up until the day of his wife's death. After having lain for some time in a very precarious state, he recovered. While he was yet ill—at the end of three weeks, in fact, from the date of the seizure—one of the farm apprentices was attacked in the same way. Then followed a lad employed as day labourer on the farm; and then Miss S——, who had come to take charge of the house after the death of Mrs. Snell. Next in order came another apprentice; and again, as a last group, a servant man, a servant girl, and

another young person (a daughter of Mrs. Lec), who, until she was laid up, had acted the part of nurse.

As far as external conditions went, the sanitary state of the homestead which had become the seat of this terrible scourge differed in nothing from what it had been for many years before, during which the household had continued to enjoy perfect health. The only new incident in its history was the arrival of Mrs. Lee from the infected village, seven miles off, with the fever upon her. What, perhaps, is still more to the point is, that many other such homesteads lay near to this one, which were far worse off in respect of these same conditions, but in which no fever of this or any other kind existed. There was no single case of the sort, indeed, within miles of the place, or nearer than North Tawton, whence the taint had been imported.

The outbreak, severe as it already was, did not, however end here. In order to lighten the burden of so heavy a sick list, the servant girl, already referred to as one of the sufferers, was sent to her own home (a small cottage in the hamlet of Loosebeare, about four miles away) as soon as the first symptoms of illness appeared. Here she lay ill for several weeks under my care. Before she had recovered, her father, a farm labourer of the name of Gibbings, was likewise seized, and narrowly escaped with life. A farmer, named Kelland, who lived across the road, and who visited this man several times during his illness, was the next to take the disorder. His case was, in turn, followed by others under the same roof; and the fever, spreading from this to other houses, became the focus of a little epidemic, which gradually extended to the whole hamlet.

Scattered over the country side there were some twenty or thirty other hamlets, with the condition of which I had

long been intimately acquainted, and which in all things were the precise counterparts of this. Two or three farm-yards and a few labourers' cottages clustered round them, made up, in each case, the little community. In each of these were the usual manure-yard and the inevitable pigsty; in each there was the same primitive accommodation for human needs. The same sun shone upon all alike, through month after month of the same fine, dry, autumnal weather. From the soil of all, human and other exuviæ exhaled into the air the same putrescent compounds, in about equal abundance. In some amongst them, indeed, to speak the exact truth, these compounds, if the nose might be trusted—and in this matter there is no better witness—were much more rife. And yet, while at Loosebeare a large proportion of the inhabitants were lying prostrate with fever, in not one of the twenty or thirty exactly similar places was there a single case.

To explain a contrast so signal there was but one fact to appeal to—the arrival from Chaffcombe, where the fever was already raging, of Mary Gibbings with the disease actually upon her. Before that event, in spite of manure heaps, pig-styes, and the like, Loosebeare, too, was free from the malady. The diseased intestine of the infected girl had continued to deposit its morbid excreta upon the soil for a fortnight or more before the fever began to spread, and the first cases that succeeded to hers sprang up immediately around her person.

The Chaffcombe tragedy—if I may so call it—had yet another episode. One of the boys already mentioned in the infected list, was the means of widely disseminating the fever in quite another direction. This boy, who was employed as day labourer on the farm, lived, when at home, in one of a pair of cottages standing by the roadside, about midway between Bow and North Tawton.

The cottage in question was occupied by the boy's mother; the cottage next door by the husband and family of one of her married daughters. Of the ten persons who, one after another, contracted fever at Chaffcombe, this boy, Oliver Lang, was the fifth in order of attack. Like Gibbings, he was sent home to his friends as soon as he fell ill; and he took to his bed in the last week of December. I attended him for a long time at his mother's house, and his case was very severe. Before he had become fully convalescent, his mother, who had nursed him, sickened; and while she yet lay ill, his sister took the fever. In the last-named subject the course of the disease was unusually rapid, terminating fatally as early as the ninth day. On January 24 she had a severe shiver, on the 26th she was unable to leave her bed, and on February 2 she died.

The next to be attacked were two children of the family next door, every member of which ended by being laid up with the disorder. Another married daughter (a sister of Oliver Lang), who had come from a distance to take care of her sick relatives, being at length infected, became, on her return home, the means of propagating the fever in yet another quarter. This new group of sufferers also fell under my charge; but, as the history of the introduction and spread of the fever amongst them would only offer a repetition of incidents precisely similar to those that have gone before, I need not further pursue it. It is only important to add that, with one exception, all the cases included in the last narrative were either under my own care, or under that of one of my brothers, who was associated with me in their treatment, and that I kept, as I have already stated, an accurate record of them at the time of their occurrence, with the express view of illustrating the mode of propagation of this particular species of fever.

In the next case the disease did not spread so widely among the attendants on the sick as in the examples already given, but it brought some other relations into view, which render it worthy of being placed on record.

In the summer of 1855, Miss R——, a lady residing on St. Michael's Hill, Bristol, went to spend a few weeks in France, taking a party of five young ladies with her. After passing a month at Havre, one of the number was obliged to return home; the other five went to Paris. On their arrival there, they took an apartment in a 'hôtel garni,' near the Bourse, which they continued to occupy during the nine days of their stay in the French capital. Being limited as to time, and this being their first visit, they gave themselves up to sight-seeing with the ardour usual under such circumstances, and incurred great fatigue in consequence. Some days before quitting Paris, they discovered, from the frequent passing to and fro of Sisters of Mercy, and from other unmistakable signs, that some one was lying dangerously ill in their hotel, in the apartment next to that which they occupied. On the day preceding that of their departure for England, a priest made his appearance on their landing, and, on inquiry, they were told that he had come to administer the last sacrament to a lady who was dying of '*la fièvre.*'

On Thursday, July 20, they left Paris, and reached Sydenham in the course of the same day. The day following they devoted to the Crystal Palace, and, in the evening, they parted company. One of the young ladies went to Pembroke, and another to Tetbury; while Miss R——, with the other two, came to Bristol. On the day following, one of the two, who had already shown symptoms of illness at Sydenham, was laid up. She kept her bed some weeks, and her disease was pronounced to be 'gastric fever.'

In the middle of the next week, the other three young ladies, who had continued well up to that time, began to droop ; and on Saturday, July 29—exactly nine days after leaving Paris—they were all in bed with the same fever. The young lady from Tetbury died after a month's illness ; the other three recovered. Of these three, one, a Miss T——, was attended by myself throughout. Her case, which was a very severe one, presented in turn all the most characteristic marks of intestinal fever. Towards the latter end of the second week, there was a copious eruption of the well-known spots ; and, in the course of the third, she very nearly died of intestinal hæmorrhage. On September 14, Mary Y——, a servant who had nursed her, began to droop ; and on September 19, was admitted into the Bristol Royal Infirmary, suffering from the same fever. The case proved to be a mild one, but presented all the diagnostic marks of the disease, including the characteristic eruption of rose-coloured spots. There was at that time no other case of fever on St. Michael's Hill. Miss R——, who was an elderly person, and the young lady who did not accompany the party to Paris, escaped illness.

From this narrative it is clear that the four young persons who, in different parts of the kingdom, were thus attacked, within a few days of one another, by this specific fever, derived it from a common source. That this source was the sick lady who was their fellow-lodger in Paris, although not so certain, was in the highest degree probable. That Mary Y—— caught her fever from Miss T——, by attending on her, there could be no reasonable doubt.

In the next case the events offer, with some slight variations, a repetition of those which occurred at Chaff-combe.

The scene of the outbreak, in this instance, was a farmhouse, situated on the crest of a hill five miles west of Cardiff, and overlooking the village of Penhavod. Prior to this outbreak, fever had not occurred at the farm within the memory of man. The house itself was ill built, and the ventilation especially very defective. It was provided with a common privy, placed in one corner of the garden, about twenty yards from the house. That the place was not unhealthy, however, was proved by the fact that the owner of it had brought up there a family of seven children, who, up to the date of this visitation, were the very type of luxuriant health. Several members of two preceding generations had attained to great longevity on the same spot. In respect to sanitary conditions, the homestead was in precisely the same state in which it had been for many years past.

Such being the position of things, on December 16, 1858, an event occurred which proved to be of the most tragic moment to all concerned in it. On that day, one of the sons, William Phillips, a lad about twelve years old, was brought home from a boarding school at Cardiff in the first stage of typhoid fever. He was sent away from the school in consequence of having the fever upon him.

In this, as in all the cases which followed, the intestinal discharges were thrown, sometimes into the common privy, and sometimes into a large open pit, surrounded by a low wall, which served as an ash-pit. This pit was situated within a few yards of the back door of the house. The tainted linen was washed in the washhouse near the back kitchen, a place which all the household frequented. Before the arrival of the infected lad, the family, as I have

already said, were in the enjoyment of good health, and the neighbouring village and farms were entirely free from fever.

In the third week of his illness, Emma Phillips, one of the sisters, five years and a half old, was attacked with the same fever as that under which her brother was labouring, and died of it towards the end of the second week. A few days after Emma, Maria, another sister, aged eighteen years and a half, was seized with the same malady, which proved fatal on February 1, 1859.

While these two lay ill, a man-servant and a maid-servant also became infected. Both were sent to their own homes as soon as the first decided symptoms appeared, and both died of the fever after short illnesses. Within a day or two, Elizabeth, another sister, seventeen years old, was seized, and at the date of my visit had not yet perfectly recovered.

On January 31, John, a brother, seven years old, was laid up, and remained for several weeks in a very precarious state. A hired nurse, who had attended several members of the family in succession, afterwards took the infection, and had the fever severely.

On February 8, I saw William, Elizabeth, and John —, the three surviving sufferers, in consultation with Mr. Rees, of Cardiff. The condition of all three was characteristic of the respective stages of the disease under which they were labouring. Elizabeth had had intestinal hæmorrhage three days before I saw her, but appeared to be doing well. In John, who had been nine days in bed, all the diagnostic marks of fever were in full development. In addition to the typhoid symptoms, commonly so called —including prostration, wandering, deafness, subsultus

tendinum, and dry, encrusted mouth—there was diarrhœa, tympanitic belly, and one of the best marked eruptions of rose-coloured spots I have ever seen. William, who first brought the fever home, was still in that state of abject weakness and extreme emaciation which is one of the most characteristic results of this fever when severe and protracted; his shaven hair had only just begun to grow, and he bore on the nape of his neck a large, deep ulcer, which had formed there, in the course of the fever, by the sloughing of a blistered surface.¹

Three additional illustrations, borrowed from other observers, will suffice for the present.

The first is taken from the admirable series of Papers by Gendron de l'Eure, on the 'Epidemics of small Localities,' published in the 'Journal des Connaissances médicales,' for 1845.

On the 8th May 1848 a girl, named Lemonnier, was brought to the hamlet of Drouanderie. She came from Caumont, a town a league off, and on her arrival was already in the 12th day of typhoid fever, contracted in her master's house (fever, I may add, being already epidemic at Caumont).

The inhabitants of these two places had held hitherto no communication. They are separated by two communes, in which there were no fever cases.

The mother of this girl, a woman 60 years old, after nursing her daughter for some weeks, was attacked by the same fever. She was nursed in her turn by two neighbours, Guilliet and Bardet by name, who relieved

¹ It is worth noting, that after the fever showed such a strong tendency to spread, Elizabeth and John, with two other members of the family, were sent away to Stocklands, an outlying farm, about two miles off. Elizabeth was seized three days after her removal, but John remained well ten days longer.

one another, until the arrival of another of Lemonnier's daughters.

Guilliet was next laid up with typhoid, and kept her bed forty days.

The following persons living under the same roof next suffered:—

Madeline Guilliet, 25 years old; Guilliet, the father, 53 years old, who died in the fourth week; Guilliet, the son, aged 17 years.

Two servant girls, who, in fear and trembling, rarely visited the sick, but were, nevertheless, attacked.

The woman Bardet, who divided with Guilliet the care of Lemonnier, also took the fever, and was four weeks in bed.

The following inmates of her house were afterwards attacked:—

Julian Bardet, her son, eleven years old.

Constance Bardet, sixteen and a half years old: death in the fourth week.

Louis Bardet, aged eight years.

Françoise Bardet, not resident with her mother, but who visited her, also got fever.

The entire population of this hamlet numbered nineteen persons, of whom *one only, an infant at the breast*, escaped. It is probable from this and other circumstances that in this outbreak the fever was propagated through the drinking water.

The next case is by the illustrious Bretonneau, who was the teacher of M. Gendron, and one of the first to establish the contagious nature of the malady.

In one of his papers on typhoid fever he says:—

‘Dr. Potier has recently witnessed a case in which typhoid fever attacked all the young ladies of a school in the Faubourg St.-Germain, without even one escaping.

The first affected was a young lady recently come from the country.'

Now, at that time there was no typhoid fever anywhere in the neighbourhood of the school. With the history of the Chaffcombe and other outbreaks before us, we may well ask, with M. Bretonneau, 'to what can such a striking difference be due, save to the contagious nature of the malady?'

The third and last case is taken from an account, in one of the Reports of our own Privy Council, of a severe outbreak of typhoid which occurred in the village of Kingston-Deverill, in Wilts, a few years since.

'September 1st the *first* patient came under medical treatment; a man of the poorest class, an habitual tramp who for some time before his attack had been absent from home, and must, (it seemed), have contracted the disease while on his travels; the *second* case was in the person of this man's mother, who had come from her home on the other side of the village to stay with and nurse him during his illness, and herself required medical treatment on September 23rd. The *next* patients were two women (one of them living next door, west, to the first-infected cottage; the other living in another group of cottages), who, both of them, had frequented the first-infected cottage and had given help in nursing there; the *fifth* attacked (October 9th) was a child of other next-door neighbours (east) of the first patient. The disease eventually spread to more than twenty cottages, scattered, singly, or in little groups, about the large area of the village. In some cases, several inmates of the cottage were attacked — father and mother, and even three, four, or five children, and the infection of the disease was in at least one instance conveyed to a neighbouring village.'

(Third Report of the Medical Officer of the Privy Council, pp. 8, 9, 10.)

At the date of this Report, out of a total population of about 400 persons, 66 had already been attacked, and six deaths had occurred.

Truly, a touching, as well as most instructive history. And all this, not through foul smells, or the action of mere 'pythogenic' compounds which, no doubt, were as rife before the fever as during its course, but simply through direct importation of the fever itself, by a poor tramp.

§ 3. I shall not weaken, by any lengthened commentary, the force of the facts related in the last section.

Observed under conditions singularly favourable for tracing the order of events,—recorded by men whose good faith is beyond suspicion, and, in the most decisive of the instances, open to no ambiguity from any quarter—they fulfil every condition that can be required of evidence in such a case, and, in spite of all that has been asserted, and is still maintained to the contrary, furnish sufficient proof that this fever is an essentially contagious or self-propagating fever.

If need were, it would be easy to show, by the doctrine of probabilities, that to attempt to explain them on any other principle, would be absurd.

But it would be a waste of time and power to demonstrate by elaborate methods what common sense apprehends at once. The facts tell their own tale, and tell it in language so plain, that it cannot be misinterpreted.

Nor must it be supposed that the facts themselves are

in any sense exceptional. Instances of such wholesale infection as some of those adduced are, no doubt, only to be met with under particular circumstances of season, place, and habits of life. Instances equally decisive as to the propagation of the disease by personal intercourse abound. So true is this, that I could easily multiply to a large extent, from my own experience, cases in which this fever was imported into previously healthy districts, and there disseminated by persons who had contracted it in distant places. Indeed, I can safely affirm, that while I continued in country practice, it scarcely ever happened for three months to pass away without cases of this fever coming before me, under conditions that contained the most strong evidence of communication by contagion.

Now I need scarcely add, that of the various properties that can be shown to belong to any given malady, this one, of all others, is incomparably the most important. In the first place, it is clear that, in a far higher sense than can possibly attach to any other conceivable property, this mode of propagation sets upon a disease the stamp of a specific nature. In order to appreciate its full significance in this respect, we must not forget that, like the other contagious fevers, this fever, in particular, not only propagates itself, but, if common observation can be trusted in such a matter, propagates no other kind. In the numerous cases in which I have seen the disease palpably spreading by contagion, the offspring has always borne the same specific marks which distinguished the parent; and one case has followed another with the same constancy of specific type with which small-pox follows small-pox, or measles succeed to measles. It is well known, in fact, that there are many countries in which continued fever is not only common, but rife, and in which

this particular kind is the only kind that occurs. But *to propagate itself and no other*, and that in a series of indefinite progression, constitutes the very essence of the relation on which the idea of species is founded. How much this implies in the animal and in the plant we all know. It is strange that what it implies in the case of disease should be so seldom recognised.

‘That,’ saith Hooker, ‘*which doth assign to each thing the kind; that which determineth the force and power; that which doth appoint the form and measure of working, the same, we term, a LAW.*’ If these be the true titles to the designation of law, the law of self-propagation, as exemplified in the great group of fevers, not only possesses them all, but possesses them in an intense degree. This becomes more and more clear the more deeply we seek to penetrate into what is involved in the fact.

In the case before us there can be no ambiguity as to what the fact really implies.

The existence here, *as in the other contagious fevers*, of a latent period after the occurrence of infection; the exemption conferred by one attack against any future attack; and, lastly, the immunity of large numbers of persons, who, though freely exposed to the fever poison, yet remain proof against it,—are characteristics of which the significance cannot be doubtful. All three are characteristics of a very special order, and spring from a common root. Of the last-named of the three, as of a thing patent to all, nothing more need now be said. In offering some observations on the first and second, I am aware that I am laying myself open to the charge of illustrating what is already familiar, and of undertaking to prove what is already admitted. But if I transgress in doing so, I will, at any rate, endeavour to be brief.

Of the occurrence of a latent period, several well-

marked illustrations have already been incidentally recorded in the last chapter. To these I shall content myself with adding the three following:—

(a) In the autumn of 1854, typhoid fever broke out in a school for young ladies at Taunton, and spread so much that it became necessary for the time to 'break up.' Amongst those who were sent to their homes was a young lady named Oliver, whose family lived at a farm in the country, a few miles from Bridgwater, in an isolated spot. For more than a week after her return home, this young lady appeared to be in her usual health. On the tenth day after her arrival she was seized suddenly with fever, which laid her up for several weeks, and very nearly proved fatal to her. There was no other case of fever at the time in the neighbourhood of her home, and she was the only inmate of it who suffered.

(b) In the month of March 1853, I was called to attend a family in Park Street, Bristol, in which two children had been affected one after another with typhoid fever. At my suggestion, a third, a little girl eight years old, who had hitherto escaped, was sent into the country to a neighbourhood where no fever was. Here she remained for three weeks in entire separation from her friends, and with little or nothing amiss. At the end of the third week she began to droop, and in the middle of the fourth she was brought home with all the characteristics of typhoid fever upon her.

(c) The third and last example is taken from the outbreak of fever which occurred at the military school of La Flèche, in 1826. In this example, the peculiarity of the circumstances gave a scientific clearness and precision to the facts but rarely met with in medical evidence. The fever first broke out in the school in the month of July, and did not cease until 109 students had been

attacked by it. Amongst those who suffered were 26 who had been sent to their own homes in distant parts of France, in the vain hope that they might thus escape the disease which was spreading among their comrades. These 26 young men were, to all appearance perfectly well when they were sent away, and continued to be so for more than a week afterwards. In the second week they began to droop, and before the week had ended they were all laid up with typhoid fever. As it may be considered certain that these 26 subjects contracted the fever at the school, it is plain that the poison must have remained latent in their bodies at least a week or ten days.

Of the existence in this, as in all the other contagious fevers, of the remarkable immunity which one attack confers against any future attack of the same malady, the evidence, although requiring more pains to collect, is not less conclusive. M. Bretonneau, who, I believe, was the first to draw attention to this remarkable and all-important characteristic, avers that for a period of thirty years he had never seen an instance of this fever occurring twice to the same person.¹ In regard to the same point, Chomel² expresses himself in the following terms, which, precise and decided as they are, acquire additional weight from the well-known scrupulous accuracy of the writer:—

‘ We have already said that typhoid fever, in ordinary circumstances, only affects the same individual once. This appears from all the facts hitherto recorded. From the time when physicians began to make special and consecutive researches on this malady, no authentic instance to the contrary has been observed, although the number of cases of typhoid fever annually studied is so considerable that examples of recurrence must have been met with, had

¹ See ‘ Archives générales de la Médecine,’ 1st series, vol. xxi. p. 62.

² ‘ Leçons de Clinique médicale,’ p. 333.

the disease been susceptible of occurring more than once in the same subject. Although in interrogating our patients we have always taken care to call their attention to this point, they have never answered in a manner to lead us to believe that they had already had the disorder; and, after all, even were some opposite facts now and then found in a disease of such great frequency, a few exceptions would have nothing extraordinary in them, and would not destroy the kind of *law* which has just been enunciated; for small-pox, scarlet fever, and measles, which ordinarily attack the same individual once only, recur sometimes, especially in great epidemics of these disorders. It would not, therefore, be astonishing if examples of the same kind were now and then met with in the case of typhoid fever.'

Louis, who on all points relating to the natural history of this fever is the greatest of authorities, living or dead—whose monograph on it is unique in medicine as a model of elaborate research—and whose conscientious accuracy is only paralleled by his slowness of belief, declares himself to the same effect in language which is the more striking from the contrast it presents to the caution with which he expresses himself on most other subjects.¹

Among many illustrations of the fact which he cites from Gendron de l'Eure, especially remarkable is the case of the town of Caumont, which was swept twice by an epidemic of this fever, with an interval of eight years between, and in which all the persons who were attacked with the fever in the first visitation were spared in the second.²

I may add, that my own experience is in entire accord-

¹ See 'Recherches, &c., sur la Maladie connue sous les noms de Fièvre typhoïde, &c.,' vol. ii. pp. 371, 516. 1st Edition.

² See 'Archives générales de la Médecine.' 1st Series. Vol. xx. p. 372. The epidemics referred to occurred in 1820 and 1828.

ance with that of these distinguished writers. For seven years, I made careful inquiries as to the point in question in every case of this fever that fell under my charge; and during the whole of that period, although my range of observation included two great epidemics, in addition to a large average of fever patients, I only met with three subjects in whom there was reason to believe that the disease had ever occurred before. To these three I added a fourth in my own person shortly after.

During the same period, I was constantly meeting with persons who, having once had the fever, remained perfectly well under prolonged and intense exposure to its specific poison, while all around them were falling victims to it. Of such persons, I have many still vividly in my mind, who, by the very accident of having acquired in this way an exemption which none around them possessed, continued to perform for weeks, and sometimes for months together, the exhausting and dangerous office of nurse to the other members of an infected household, and who, nevertheless, came out harmless.

In reference to the four who were not so fortunate, it is only necessary to remark, that in no one of the contagious fevers is the protecting power of a first attack absolute. In the space of the same seven years, indeed, in which these cases came before me, I met with five others in which *small-pox* happened twice to the same person. And yet that, as a rule, small-pox occurs only once in life, is a fact established on a larger basis than any other fact in medicine. The occurrence of exceptions in the case of small-pox is, therefore, *the best possible proof* that the occurrence of similar exceptions in the case of typhoid fever does not invalidate the remarkable law in which both participate.

I shall have to recur to all these points in another place. I have already said that their real significance cannot be

doubtful. They define at once, indeed, the position and natural affinities, as well as the true pathology, of the disease to which they belong. For had we no other light than that which is afforded by them, we should see clearly enough that in the specific cause of this fever we have to deal with one of that remarkable group of poisons which, in order to produce their specific effects, require in the ^{*}human body not only a subject for their action, but conditions for their growth and development. This is a conclusion of immeasurable importance to the inquiry in which we are engaged. That the operation of all the poisons belonging to this group is entirely dependent on their own reproduction in the living body, may, I repeat, be inferred with great certainty from the relations on which we have just been dwelling. But the fact stands on even still surer ground. Demonstrable in all as a matter of inference, it has actually been demonstrated in one of the number as the result of experiment.

Of the diseases which the three very striking characteristics I have been endeavouring to illustrate separate into one great natural order, small-pox may be taken as the type. In very essence a contagious fever, it is a fever in which a period of incubation, on the one hand, and the protection conferred by an attack of the disease, on the other, have become experimental facts. For, with the introduction of inoculation, small-pox became the subject of an experiment (not the less instructive because instituted for a purely practical object) the most gigantic of any that has yet been applied to the phenomena of life. Adopted as a purely sanitary measure, and finally superseded, as such, by Jenner's admirable discovery, it has not the less left to us a legacy of the deepest scientific interest. On all the relations we are here considering it has thrown the clearest light.

Possibly we may never be able to understand *all* that is involved in what is called the 'latent period;' but it is, at the same time, as clear as day that its root lies in the infinitesimal minuteness of the dose, which inoculation *experimentally* shows to be sufficient to the specific effect of the morbid poison. In the same way, the intimate nature of the material conditions which protect, for the remainder of life, the body that has once gone through one of these diseases against any future attack from it, may, possibly, always transcend our means of research. But the practice of inoculation in the production of small-pox has shown, with a clearness and precision which are seldom exceeded, even in physical science, and with a certainty which cannot be surpassed, that these conditions, whatever their ultimate essence may be, *are, in fact, the conditions which attach to the reproduction of a specific poison in the most intimate recesses of the human body, by that most specific of processes which constitutes a contagious fever.* The disease named small-pox only occurs once in life, simply because the small-pox poison *cannot grow again* in a body in which it has once bred. In such a body, as experiment has often shown us, even the re-inoculation of the virus remains sterile and without effect.

On the other hand, it lies in the very nature of things that characteristics so cardinal as these—characteristics which are, at once, common to this group of diseases, and peculiar to it—which are perfect in their analogy one with another, but have no perfect analogy with anything we know of in nature besides—must have a common ground. The latent period in typhoid fever must be the same thing as the latent period in small-pox; the still more remarkable phenomenon of the protection conferred by one attack against any future attack must be in essence the same in the two diseases. So that if typhoid

fever happens only once in life, it is, as in small-pox, simply because the fever poison *cannot grow again* in a body in which it has once bred.

Here, if anywhere in our knowledge of disease, we are on sure ground. To appreciate the whole strength of the case, we must keep *this* constantly before us, that the leading fact of it, the great fact of all, has, in one instance, not only been experimentally revealed to us, but revealed by an experiment which for clearness of result is almost without parallel. In inoculated small-pox how striking is the way in which the great fact of the growth and multiplication of the specific poison in the living body is brought to light before us! The virus that is inserted in a speck so impalpable that the mind almost fails to figure its minuteness—so inappreciable that even the inoculated body takes at first, if I may so speak, no overt cognisance of its presence—issues before long in a new stock, which may not only poison the same body unto death, but is sufficient to impart the seeds of death to myriads of others. In most other provinces of medical inquiry we have cautiously to grope our way in the dark; but here some of the highest mysteries of disease are laid open to us in the form of visual phenomena that cannot possibly be misinterpreted. Germ and offspring, seed and crop, lie both before us, and the result, if not the nature, of the intervening process is as plain to the eye of the physician as that which the cornfield exhibits to the husbandman in the teeming increase of the scanty grain which his own hand had scattered.

What we actually *see* in small-pox—in the typical member of the group—is but a picture of what occurs in the rest. In typhoid fever, as in small-pox, it is the act of growth (with all that is incident to it) that kills: that constitutes the disease, in fact; and where the conditions

for this growth are wanting, the poison is powerless. Whether in this fever the scale of reproduction be as vast as in small-pox, we have not the same ocular means of judging; but that it is the same in kind, and immense in degree, the whole history and evolution of the disorder prove. *The living human body, therefore, is the soil in which this specific poison breeds and multiplies; and that most specific of processes which constitutes the fever itself is the process by which the multiplication is effected.*

This is what contagion in typhoid fever really implies, and it is thus that provision is made for the perpetuation of the malady

To many, these reflections will no doubt appear superfluous. I have thought it well, however, to introduce them here, because, even amongst those who admit the contagious nature of this fever, there is often a lurking disposition to ignore or evade the consequences which flow from the fact. Thus, by some, this quality, if mentioned at all, is passed lightly over as an incident of no importance; as a circumstance that may perhaps deserve a passing notice, or justify some precautions, but as in no wise touching the essence of the disease. Others, with an inconsistency that seems still more flagrant, assert that this fever is by nature non-contagious, but that it may become contagious under certain circumstances! If nothing more were meant by this than that its propagation by contagion requires conditions, there could be no objection to such a statement. But it is clear, from the terms used, that this is not what is meant. It is equally clear that if the views of what this mode of propagation implies here taken be true, the use of such language betrays an entire want of conception of the real import of the fact. To suppose that this fever is sometimes contagious and at other times not, by reason of

some intrinsic difference in the case itself, is just about as rational as to suppose that small-pox could continue to be small-pox and cease to reproduce and throw off the small-pox virus.

As in small-pox so in typhoid, to spread by this mode of reproduction is not only a characteristic, but the *master-fact* in its history.

The question of contagion settled, the next that arises is, in what form and from what surface or surfaces is the specific poison cast off by which the disease is propagated?

Now, I have no difficulty in at once giving my opinion that *all* the emanations from the sick are in a certain degree infectious. At the same time, it is one of the principal objects of this work to show that what is cast off from the intestine is incomparably more virulent than any thing else. The full consideration of the grounds on which this last conclusion is founded is reserved for the succeeding chapters. It may not, however, be amiss to observe, thus early, that striking evidence of its truth may be found in facts that are familiar to all.

I have said that events such as those just related are common. It should be added that, common as they are, they never occur except under one condition—that is to say, where no sufficient provisions have been made for preventing the discharges from the human intestine from contaminating the soil and air of the inhabited area. Where these provisions are wanting, the most spacious rooms, and the freest internal ventilation, afford no certain security against the spread of the fever. I could give the most striking instances of this, if need were. It was the almost entire absence of such provisions at North Tawton and at Chaffcombe, which gave to this scourge, when once it found its way there, such

deadly power. Where this one condition exists such events are of common occurrence ; where it fails, they never occur at all. So true is this, that I doubt not that those whose practice has only lain amongst such inhabitants of large towns as live in houses provided with good drains, and especially with good water-closets, will find it difficult to believe that the disorder which the foregoing narratives show to be possessed of such virulent powers of propagation by contagion, can really be identical with the fever which, in their own sphere of observation, has seldom appeared in more than single cases, or given other than doubtful evidence of being possessed of such powers at all. On the other hand, neither do I doubt that those who, like myself, have been much conversant with the malady as it appears in country places, will see in these narratives but the reflection of their own experience. In both cases, the nature of the disorder is one, and its power to propagate by contagion the same. But in the one case, the alvine discharges are no sooner passed from the diseased intestine than they are swept far away from the house where the sufferer lies ; while in the other, these discharges continue to accumulate, day by day, upon the soil on which the dwelling stands, and to exhale their poison into the air breathed by the inmates, or to distil it slowly into the water they drink. The extreme contrast in the result, in circumstances that differ only in this one condition, is of itself all but decisive of the question.

The power of the sanitary arrangements just referred to, in almost infallibly preventing the spread of a fever, which, in their absence, often strikes down several members of a family in succession, in spite of the presence of every other favourable sanitary condition, seems to show, with a force of evidence that is irresistible, that while this fever is an essentially contagious fever, the contagious element

by which it is mainly propagated is contained in the specific discharges from the diseased intestine.

Like malignant cholera, dysentery, yellow fever, and others that might be named, this is one of the great group of diseases *which infect the ground*. Hence the quasi-miasmatic character attaching to them all, which has misled so many observers as to their true mode of spreading. In another chapter I shall offer still more specific proofs of the truth of this statement. Meanwhile, it may still further advance the argument, to inquire what is the real significance of that peculiar disease of the intestine which throws off the noxious matter.

The doctrine that typhoid fever is contagious has to contend against another class of objectors, whose reasoning it may be well shortly to consider before we proceed further. To set aside this doctrine, one of two things is clearly necessary: either to show that the facts on which it rests are not true, or that, being true, they may bear another interpretation. But many persons of credit are to be found, both in this country and abroad, who, although confessedly unprepared either to deny the evidence or to dispute the logic, yet repudiate the doctrine, simply because it does not seem to tally with their own experience. These reasoners appear to think it a sufficient answer to the whole case to allege that, in their observation, this fever has seldom spread to the attendants on the sick, and that the cases in which this has happened have been the exception and not the rule. The answer to these persons is, that in the decision of this question, the observations on which they rely are really of no weight. One would have thought it would have been obvious that when once a disease has been proved by positive evidence

to be contagious, no amount of negative evidence can prove the same disease not to be so. The same thing cannot be by its nature at once barren and prolific. On the most superficial view, it is clear that the utmost which is implied by the fact on which these non-contagionists rely is, not that intestinal fever is not communicable, but that its communication requires some special conditions. But this may be said of the whole class of contagious diseases.

As might have been expected, the objection in question proceeds almost exclusively from those whose practice lies amongst the rich inhabitants of large towns, in whose families, for reasons already given, this fever very seldom spreads. But the fact itself is not even good for what it is supposed to be worth. Because the fever does not extend to the immediate attendants on the sick is no proof whatever that it does not extend itself elsewhere.

It is scarcely necessary to observe, that the specific agents by which contagious fevers are propagated are cast off in a material form by the infected body of the fever patient. Some are eliminated from one surface, and some from another. But, in regard to this point, there is a rule which, so far as I know, has no exception. It is, that the most characteristic of the *ejecta* or *excreta* in each disease are, in the same disease, the principal vehicle of the morbid poison. This truth is so familiar that it needs no particular illustration.

Now, it will be shown in the next chapter that of all the morbid products thrown off by the (intestinal) fever patient, the discharges from the diseased intestine are, in every sense, the most characteristic. These discharges contain matters on which the fever poison has set its seal in the most consummate fashion. Wherever they travel — wherever exhalations from them penetrate — there, at least, the most specific of all the exuviae from

the sick body are in operation. The sewer, which is their common receptacle, is, so to speak, the direct continuation of the diseased intestine.

To prove that any particular case of this fever has remained without progeny, it is, above all, necessary, therefore, to prove that the intestinal discharges from it have not, after their entrance into the sewer, been the cause of fever in any second person. To teach, in the absence of such proof, that the disease is not contagious, because the immediate attendants on the sick escape it, is simply to show that those who use such language have not realised the most fundamental conditions of the question they undertake to decide. It is much the same as to argue that, because the next successors of the tuft of rushes that overhangs yonder river do not spring up immediately around their parent, the spores it has committed to the stream are sterile, and that it is not in the nature of rushes to multiply at all.

The contrast which is observed between the contagiousness of intestinal fever in the hamlet and the farm, on the one hand, and the city mansion, on the other, is, I need scarcely add, in appearance only. In the country and in the city, the fever is in all things one and the same. It does not change its nature with change of place. It is as really contagious in Belgravia as it was at Chaffcombe or Penhavod. Wherever it may occur, to multiply and throw off the specific poison from which it springs, is, as we have seen, its very essence, or, to speak more strictly, is the fever itself. The scale on which the poison is reproduced by this process must be at least as great in the one condition as in the other. In urban populations, the disease is even more fatal to those who become infected with it. And the bulk of new virus cast loose upon society by each individual case is, no doubt, large in

the same proportion. The tribute which the sewers receive from the diseased intestine is not less profuse. Also, we have only to refer to the returns of the Registrar-General to see that the *materies morbi* of which the sewers thus become the channels, however the scene of its action may be shifted, does not the less bear its natural fruit.

Cities are not so subject as country places to violent epidemics of this fever; but, taking one year with another, they furnish a larger contingent to the mortality from it. And if the anticontagionist could but extend his field of view, he might often see in the fever-stricken tenants of some court or alley reeking with sewer exhalations, the first victims of a poison which had found its way there through subterranean passages from the diseased intestine of his wealthy patient, and against the deadly power of which the rich man's household had been preserved by arrangements which his poorer neighbours had not the means to purchase.

It should not be lost sight of that in this discussion the case has hitherto been put on its lowest ground. Even were typhoid fever often really, as well as apparently, without issue, that would prove nothing as against the cases in which it is proved to be self-propagated. For if from this we were to infer that the disease is not contagious, by precisely similar evidence we must infer that *small-pox* is not so.

I do not allude here to the cases in which small-pox remains sterile, because those who come in contact with it have had the disease before, and are now proof against it, or have earned a similar immunity by the still more extraordinary condition of having been vaccinated. These cases, as showing that the propagation of contagious diseases is not only not absolute, but requires conditions of the most special kind, would be strictly in point. But

speaking now of the numerous recorded instances in which small-pox has failed, or has ceased, to spread, where neither of these protecting causes were in operation, where a large prey still seemed to invite its attack, and where every condition was present, in its highest degree, that might be supposed to give effect to the poison.

The annals of the British Navy abound in examples of this kind. In evidence of this, the following characteristic extract from Dr. Lind's admirable work, 'On the Diseases of Seamen,' will suffice:—

'What is still more wonderful, not only the small-pox, the plague, but other contagions which I have known to rage in ships and in prisons, after exerting their utmost violence, will sometimes abate in their malignity, and at length stop. Have they exhausted themselves, or their subject? That they do not always exhaust their subject is plain from facts and our experience of the thing. Thus, for example, although the infection of the small-pox was pent up in the *Royal George* amongst 880 men, yet this contagion disappeared altogether at sea, and some months before she put into any harbour, after having destroyed four or five persons, and left near a hundred unattacked.'—Vol. ii. p. 112.

Sir G. Blane, in relating a still more remarkable instance of the same kind, adds, that he had seen many like it.

The argument used in regard to typhoid fever, if it were worth anything at all, would, therefore, prove small-pox to be non-contagious: a conclusion the absurdity of which is rendered palpable by the tangible form in which the small-pox poison is eliminated. Nay, if pushed to its limits, it would prove that because every seed which the thistle commits to the wind does not spring up into a new thistle—and not one in ten thousand does so spring up—thistles do not propagate by seed at all.

CHAPTER III.

NATURE OF THE INTESTINAL AFFECTION.

‘Il faut considérer cette lésion non-seulement comme propre à l'affection typhoïde, mais comme en formant le caractère anatomique, ainsi que les tubercules forment celui de la phthisie.’—LOUIS.

§ 4. IN his elaborate and masterly account of the morbid anatomy of typhoid fever, Louis divides the alterations found in the dead body into three groups, in accordance with the more or less specific relation they bear to the malady.

To the first are allotted morbid changes which have the twofold distinction of being *always* present in this disease, and *never* present in any other ; morbid changes, that is to say, which are specific in the highest conceivable degree.

The second group is devoted to alterations, which, although not constant in this fever, nevertheless occur so frequently in it, and so rarely in other diseases, as to be entitled, in a certain sense, to the rank of specific characters.

In the third and last group, this distinguished physician places all those morbid appearances which are met with as often in other disorders as they are in this one, and which possess, therefore, only a general importance.

Louis was, himself, the first to show that the well-known affection of the gut, the true interpretation of

which it is the object of this chapter to discuss, not only entirely fulfils the two conditions which define the first group, but is the only overt anatomical change disclosed by the dead body which does so.

Take the diseased intestine away, and it becomes impossible, in a common outward survey, at least, to distinguish the body of a man dead of typhoid fever, from that of a man killed by many another septic poison ; take away the body, but leave the intestine, and by the marks upon it, death from this fever is, at once, distinguished from death from every other cause.

By every title, therefore, this disease of the intestine is as much a specific character of the fever as a peculiar pustular eruption on the skin is a specific character of small-pox, or as tubercle—to borrow Louis' own illustration—is of phthisis.

It must be obvious to all who have had more than common opportunities for the anatomical study of it, that the advanced stage at which it usually falls under observation has been a great obstacle to the formation of a true conception of its nature. Generally speaking, we see, not so much the specific disease, as the havoc it has made. It is only by tracing the morbid changes through their early phases that we are enabled to recognise their true character. At the end of the first week of the fever, these changes appear under a very different aspect from that which they afterwards present. Although death at so early a period is comparatively rare, I have seen, in the course of a pretty long experience, some ten or twelve instances in which it occurred within the first nine days, and in which I had an opportunity of examining the diseased parts.

Judging from these cases, the following are the appearances which the intestine exhibits at this stage:—

A certain number of Peyer's patches, or of the isolated follicles, as the case may be, have acquired a great increase of thickness, and stand out in relief on the internal surface of the gut. In the site of these patches—to use the words of Chomel, whose description I here purposely adopt—the intestine feels as if a solid and elastic substance had been inserted between its coats. In cutting through a patch in this state, its texture is seen to be occupied by a yellowish-white cheese-like matter, of brittle consistence, about the tenth of an inch in thickness, and offering a smooth surface where divided by the knife. This yellow matter is the peculiar 'typhoid matter' whose presence is typical of the disease, and whose formation and elimination constitute the essence of the intestinal process.

In cases in which death occurs as early as the seventh day, the mucous membrane overlying the diseased patches, as well as that occupying the intermediate spaces, is sometimes found in a perfectly natural state, having its proper colour, thickness, and consistence. This is a fact of some importance, because it shows that this affection is not a disease which begins, as many suppose, in the mucous membrane properly so called, but in structures that lie beneath it; that it is not, in fine, an affection produced by agencies from without operating on the surface, but one which proceeds from a specific cause working from within. It would be easy to prove this by paramount considerations of another order, but it may not be amiss to show that we come to the same conclusion on purely anatomical grounds.

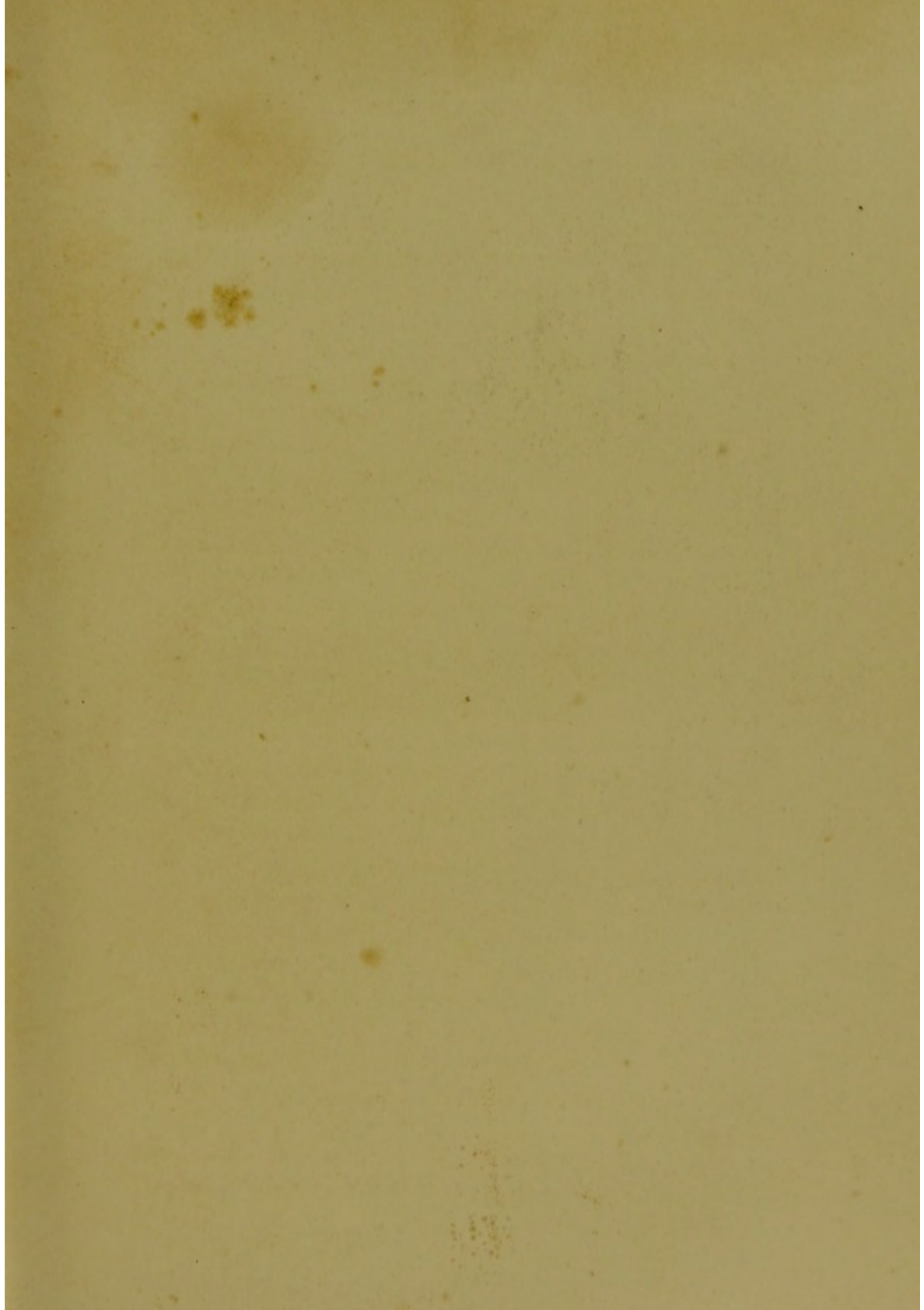
This important truth once clearly apprehended, the true significance of this morbid process seems to be no longer doubtful. When we reflect that it occupies part of a structure which, physiologically speaking, is as much the

surface of the body as the skin itself; that the morbid changes in which it consists are scattered widely over this surface, with spaces of healthy structure between; that, in their origin, these changes are confined to a single anatomical element; that they are attended *by the formation of a special product, the maturation and casting forth of which appear to be their natural climax*; and, finally, that they are peculiar to the disease before us, *and that disease a contagious fever*,—it is impossible not to see that the analogy already hinted at, as subsisting between this affection and the eruption of small-pox applies to more essential points than any yet mentioned, and that this disease of the intestinal follicles is, in fact, a true *exanthema* of the bowel.

In some cases, indeed, so salient are all these points of analogy, and so striking is the family likeness between the cutaneous eruption and the intestinal disease, that the conclusion just stated involuntarily starts to the mind on the first view of the morbid appearances.

In a young woman, who died under my care in St. Peter's Hospital in 1845, and in whose intestines, as now and then happens, the small circular (Brunner's) follicles were greatly predominant, and all diseased, the actual resemblance of the parts to the eruption of *variola* was so close that the student who had charge of the examination asked, in all simplicity, whether the case were not one of small-pox which had fallen on the bowels instead of on the skin. Such a fact as this shows, by the most striking of all testimony, that the analogy here sought to be established is, at any rate, not a far-fetched one.

The four accompanying illustrations will show how readily such an idea may arise in the mind of an unbiassed observer:—



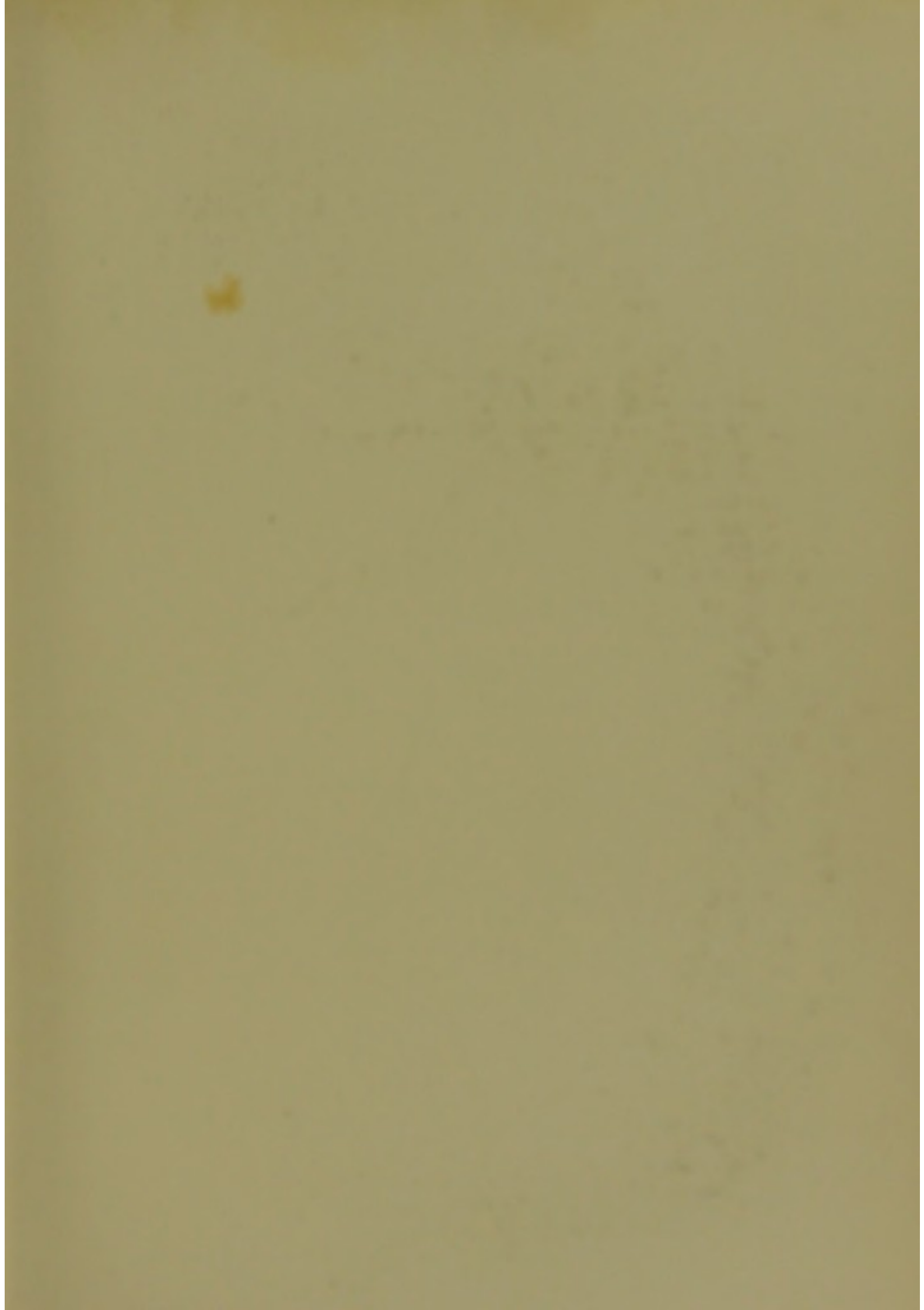


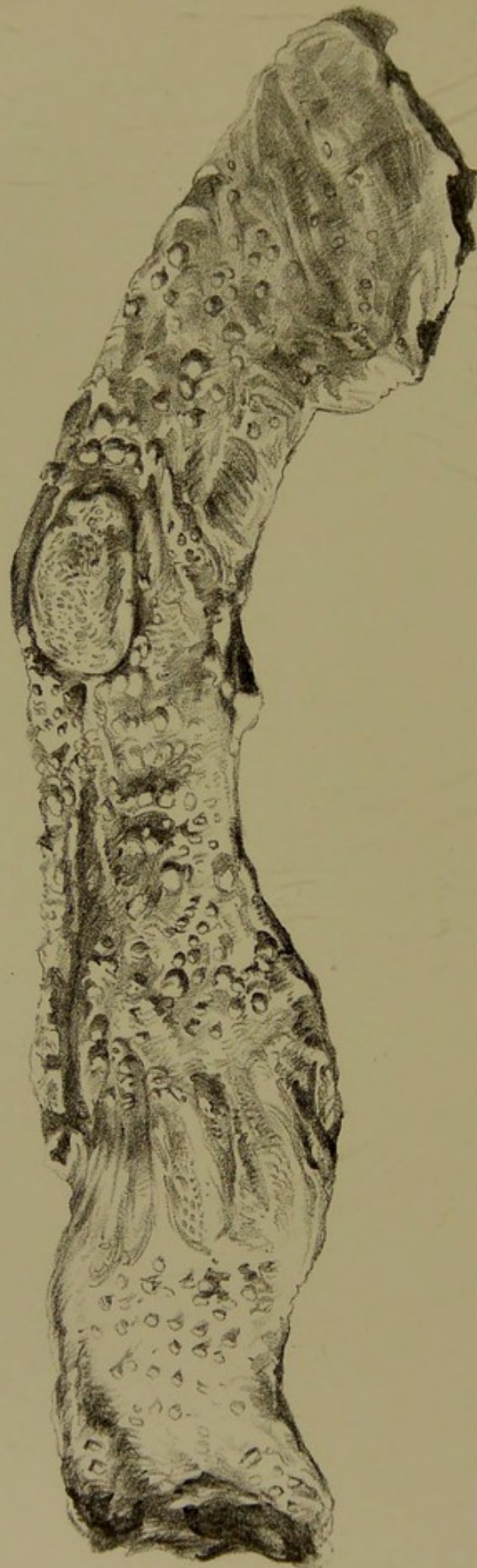
Kell Bro's Lith. London

Typhoid Fever 12th day.

TYPICAL ULCERATIONS IN THE LOWER END OF SMALL INTESTINE.

Photographed by W. Budd from a drawing from nature by J. Swayne

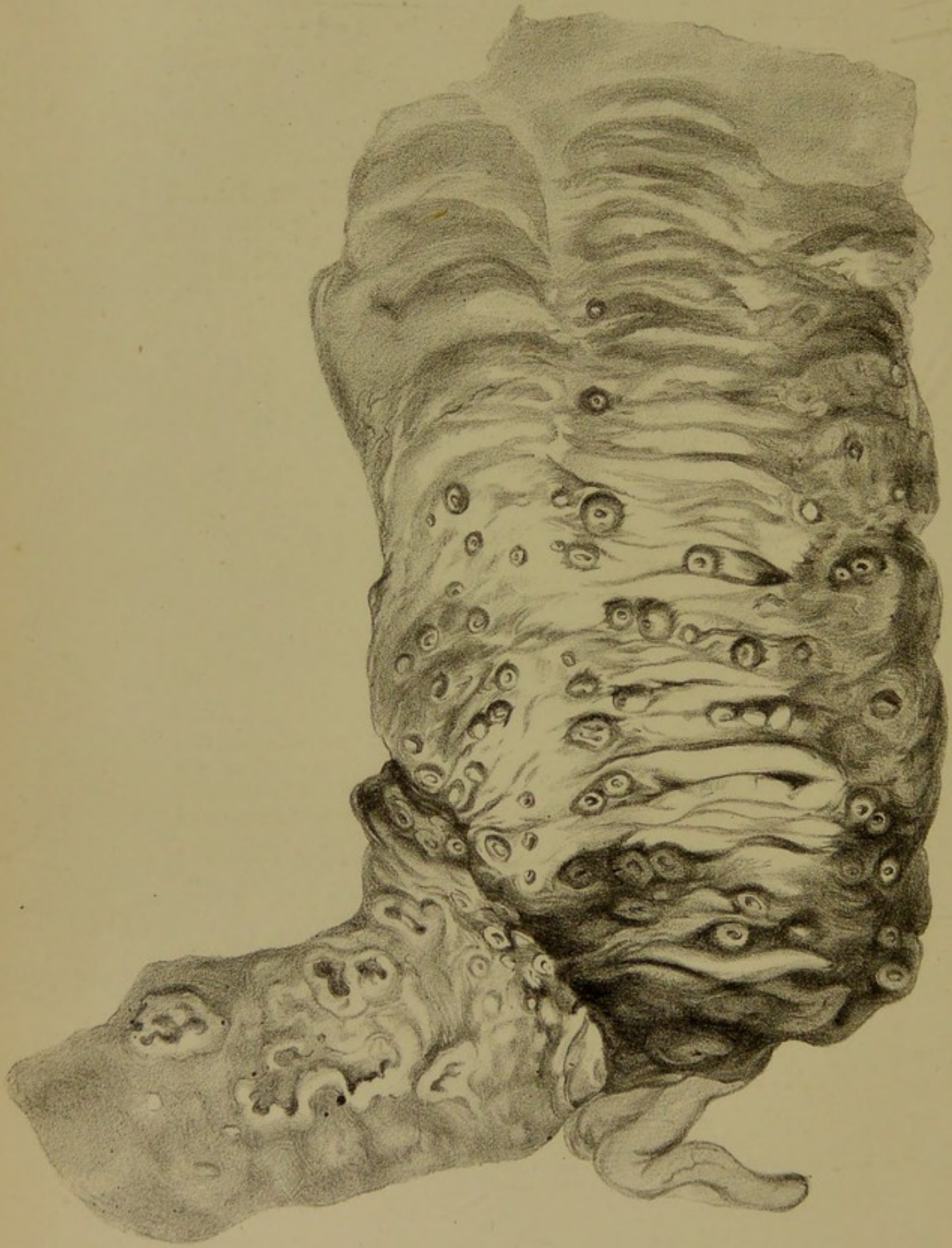




Hell Bro's Lab. London.

TYPHOID FEVER. LOWER END OF SMALL INTESTINE.
THE CHARACTERISTIC FOLLICULAR DISEASE IN AN EARLY STAGE.





Kell Bro^s Lith. London

Typhoid Fever — Advanced Stage.

TYPICAL ULCERATIONS IN THE COLON AND IN THE LOWER END OF THE SMALL INTESTINE.

No. 1 is from a drawing by my friend Dr. J. G. Swayne of the intestine of a young woman, who died in the Bristol Infirmary in 1850, on the twelfth day of the fever.

No. 2, from another drawing, also presented to me by Dr. Swayne, represents the lower end of the ileum of a patient who died in the Bristol Royal Infirmary, in a very early stage of the fever. The principal interest of this specimen lies in the fact that it exhibits the morbid change in a phase in which nothing has yet intervened to mar or obscure its interpretation. It is a striking and characteristic example of the form described by Petit and Serres, under the significant name of 'la forme boutonneuse.' Unless in the larger follicles, which have begun to fret into minute ulcers, the mucous membrane is entire, and the disease is in the first stage, or the stage of development.

No. 3 represents the cæcum and lower end of the ileum of a patient who died of typhoid fever, in St. Peter's Hospital, now nearly thirty years ago.

No. 4, as exhibiting the follicular disease in more various stages, and as being photographed directly from nature, is still more valuable than the other two. For these reasons, I have chosen it for the frontispiece of this work.

In cases like this, which address themselves to the eye, the sight of a single specimen is often more convincing than the most elaborate train of reasoning.

I do not know what impression these illustrations may make on others, but to me it seems impossible to look at them without the idea of an eruption at once arising in the mind.

When we remember that this affection—to repeat the essential points once more—is characteristic of this fever:

that it stands in the same relation to it, as a diagnostic mark, at least, as a peculiar pustular eruption does to small-pox:—that it is an affection which, proceeding from within, breaks out on the surface; that it results in the elimination of the morbid product, and lastly, that the product itself is the one known specific product of a contagious fever, the evidence becomes irresistible that we have here the essence of an eruptive process, whatever the name by which we may choose to call it.¹

The history of the yellow, typhoid, matter itself, from its first appearance to its final expulsion from the body, is entirely consistent with this view of the nature of the follicular disease.

It is now well known that this matter is made up almost entirely of well-defined nucleated cells in various stages of development.

From the unique and constant presence of this matter at all points where the specific changes wrought by this

¹ For the ten years during which I lectured on the Practice of Physic at the Bristol Medical School, I always taught this doctrine on the grounds advanced in the text. It was not until the summer of 1857 that I first became aware that M. Bretonneau had put forward the same view in a paper entitled, 'Notice sur la Contagion de la Dothinerie,' and which was read to the French Academy of Medicine so long ago as July 7, 1829. In that paper M. Bretonneau contents himself with a bare enunciation of the doctrine, and does not enter into any considerations in support of it. Having been unable to obtain a sight of his monograph on Fever, I do not know whether or not he has given more development to the subject in that work. It is obvious enough, however, that the idea that the intestinal affection is a true eruption had been very clearly apprehended by him, so that whatever credit may be supposed to attach to it belongs to him. At the same time he does not appear to have perceived its all-important bearing on the mode in which this fever is disseminated. Neither in his own papers nor in those of Gendron de l'Eure, who in the matter of typhoid fever may be looked upon as his disciple, is there the slightest hint either of the part which the discharges play in the work of propagation, or of the need of measures to disarm these discharges of their contagious power. Cruveillier speaks of the intestinal affection having been compared to an eruption, but refers to the notion as being altogether fanciful and unworthy of serious notice. The reader will find M. Bretonneau's paper in the 'Archives générales de Médecine,' tome xxi. 1829.

fever are going on, Rokitansky has been led to the conclusion that it constitutes the actual 'materies morbi' of the fever.¹ When I first wrote on this subject, I was inclined to look upon this view as, in great degree, fanciful. Pending the more ample discussion to which I hope on some future occasion to subject it, I may state that a more mature consideration of the facts has induced me to reverse my former opinion, and to think that this distinguished anatomist will prove to be right.

In that event the demonstration would be absolute.

Assuming, for the sake of argument, the case to be already established, it may be well, before we break new ground, to see what would be the extent of the infection thus arising, and through what channels, and under what character, it might be expected to appear.

To enable us to judge of the extent of the infection, there are two elements to be taken into account: first, the amount and duration of the intestinal discharge in each case; and, second, the number of cases annually occurring.

Louis made it his business to determine the former of these two points, with his usual accuracy, by the application of the numerical method. Fifteen days in mild and twenty-six in severe cases, he found to be the average duration of the alvine flux.² Although some

¹ The ulcers which sometimes occur in the stomach, in the gullet, and about the epiglottis, are also preceded by, and originate in, a deposit of the same yellow stuff. I believe the same to be the case with the ulcerations which now and then occur in the bladder, and of which I have seen several examples. When pneumonia comes on in the course of the fever, Rokitansky states that the same deposit is found in considerable bulk in the parenchyma of the lung, and also in the bronchial glands. Rokitansky's general statement is, that wherever the chief stress of the specific agent which causes fever falls, there the yellow matter also most abounds. Assuming the fact to be so, it would only establish, on a still wider basis, the close relation which subsists between the yellow matter and the specific poison.

² See 'Recherches sur la Fièvre typhoïde,' tome i. pp. 438-39.

little deduction from these figures must probably be made to meet our English experience, yet I should say that they are not, even here, far in excess of the truth.¹

The number of cases, on the other hand, is approximately known. Judging from the Reports of the Registrar-General, it would appear that, at the lowest computation, taking one year with another, 100,000 cases of intestinal fever occur annually in the United Kingdom.

Whether the word *eruption* be accepted or not, we come, then, to this :—that every year, in this kingdom, at least 100,000 *human intestines, diseased in the way which I have here attempted to characterise, continue, each for the space of a fortnight or thereabouts, to discharge upon the ground, floods of liquid charged with matters on which the specific poison of a contagious fever has set its most specific mark.* This is not a theory, but the bare statement of a fact. It is the fact of facts, in its bearing on the present investigation. To obtain an adequate conception of the magnitude of the provision thus made for the work of dissemination, we must remember how infinitesimally small a dose of the poison thus deposited is sufficient to reproduce the fever, and how vast is the multiplication which, according to all reasonable calculation, this poison undergoes in every individual case. If the main conclusion we have come to in this chapter be just, the great bulk of this multiplication is represented by the intestinal discharges.

What the pustular eruption in small-pox is to the atom of inoculated virus, that, in its own degree, and most

¹ It is hardly necessary to observe that in some cases the diarrhoea is slight, and in some few others is absent throughout. In the cases which formed the basis of Louis's great monograph, this last fact was observed in three out of ninety-four cases, or, on an average, about once in thirty times.

probably in no very inferior measure, is the poisonous matter thrown off by the intestinal follicles in this fever to the unit of fever poison from which it sprang. And as in small-pox the new stock engendered in a single case, is often sufficient in amount to inoculate, with small-pox, myriads of other subjects, so in this fever, in severe cases, there is reason to believe that the new poison thrown off by a single diseased intestine would be sufficient in amount, *were it all to take effect*, to impart the same fever to a large community. Multiply this by 100,000 or thereabouts, and we obtain a pretty fair approximation to the annual product.

If these calculations be well founded, one thing, at least, is clear—that a disease which is endowed with such vast provisions for the continuation of its species is not likely to die out for lack of heirs.

If we now inquire into the mode, as to season and place, in which these 100,000 diseased intestines annually taint the soil of England with the fever-product, some relations of great importance come at once into view.

As to the first of these two conditions, the principal point to keep before us now is, that although some seasons, and autumn especially, are apparently more favourable to the disease than others, there is no season in which it does not prevail.

In regard to place, there is a distinction of the first consequence to be taken between cities and large towns, on the one hand, and villages and small communities generally, on the other. In villages, whatever their sanitary state, typhoid fever is often absent for many years together ; in towns, above a certain size, it is never absent for a day. In the former, the infection is casual, and occurs only at intervals more or less remote ; in the latter, it is perennial, and constantly going on.

The necessary consequence is, that, in every large city, the sewers are constantly exhaling, at some point or other, and generally at a great number of points at once, *effluvia directly proceeding from the most specific of all the exuviae thrown off by the fever patient.*

In some seasons and in some city districts these effluvia are more rife than in others; but there are few or none from which they are altogether absent. In some localities they must necessarily be often present in a highly concentrated form.

To inhale sewer emanations in a great city, is, therefore, under conditions of the most frequent occurrence, *actually to inhale the very quintessence, so to speak, of a pre-existing fever.* This, whatever it may be worth, is a fact of the most real kind. It is a fact that has been singularly lost sight of in all current speculations on this subject.

Assuming the intestinal discharges to have the principal hand in the dissemination of the fever, we come at once, then, to the following deductions:—

1st. That, as a rule, this fever will spread the more, the less perfect the provisions for preventing the discharges from the human intestine from contaminating the soil and air of the inhabited area.

2nd. That where these provisions fulfil this end, the disease will show little or no contagious power.

3rd. That its tendency to run through families will oftenest take effect where there is only a common privy; least often where there is a well-appointed watercloset. That this tendency will be observed very commonly, therefore, in country places, and comparatively rarely amongst the wealthy inhabitants of large towns.

4th. That, generally speaking, the distribution of the disease will be different in country and in town; that in

the country, where there are few or no sewers, and where, consequently, the intestinal discharges accumulate around the infected dwelling, the disease will occur in a thickly clustered manner; that in the town, where these discharges are conveyed, often for long distances, by sewers, the ramifications of which extend through large communities, it will appear in a more scattered form.

5th. That, as what the sewer receives from the fever patient is incomparably more virulent than anything else thrown off by him, the infection (until the true interpretation of the events be known) will appear, for the most part, *as if it had its source in the sewer, and not in the already infected man.*

6th. That in the country, the contagious nature of the fever will be obvious and unmistakable; but that in the town, it will most commonly be masked and obscure.

7th. That in the former, the fever will be epidemic and thickly clustered; in the latter, as a rule, endemic and scattered.

8th. That separation of the healthy from the infected will be of no avail to prevent the spread of the fever, unless it include separation from the intestinal discharges also.

9th. That, for this reason, the severest outbreaks will be seen in schools, barracks, and other large establishments, where a single common privy is often, alike, the receptacle of the discharges from the sick, and the daily resort of large numbers of healthy persons.

To appreciate the full strength of the case, we must bear in mind that, with the exception of what relates to season and place, all that is here enunciated is elicited, not from observation of the events as they really occur, but as the result of *pure deduction* from the twofold assumption—that intestinal fever is contagious, and that the intestinal

discharges contain the most virulent part of the poison by which the contagion takes effect.

These nine propositions embody, not the results of experience, but the anticipations of theory. If experience and theory happen in this case to offer an exact coincidence, is it not because the one is in reality the true expression of the other ?

§ 5. IN the last section I endeavoured to show that the theory which regards the follicular disease of the intestine as the specific eruption of the fever, is in perfect harmony with many of the most characteristic facts which observation has taught us as to its mode of dissemination.

But it is not sufficient to prove a general correspondence of this kind, however close. It is important, also, to show that this correspondence is sustained when this theory is brought, face to face, with an individual case.

In 1858, a very fatal and widespread epidemic of typhoid fever, which subsequent events have rendered ever memorable, occurred in the town of Windsor.

The leading circumstances of the outbreak were investigated at the time with great thoroughness by Mr. Simon and Dr. Murchison. For the full particulars of it, I must refer the reader to the accounts of it given to the public by these two writers.¹

¹ See 'Second Report of the Medical Officer of the Privy Council,' article 'Windsor,' and a Paper 'On the Causes of Continued Fevers, with special reference to the Windsor Epidemic,' by Dr. Murchison, read before the Epidemiological Society, on February 9, 1859.

I subjoin in its entirety the abridged account which Dr. Murchison afterwards published in his book on Continued Fevers.

'In the autumn of 1858, an epidemic of enteric fever occurred at Windsor which was made the object of special inquiry by the medical officer of the Privy Council, and an account of which, founded on my own investigations, was communicated to the Epidemiological Society. It was calculated that during the last four months of the year, 440 persons, or about one-twentieth of the entire

It will be sufficient, here, to say that the net result of their investigations was to establish, not on general grounds merely, but by data of a perfectly scientific order, a fundamental connection between the outbreak and the state of the Windsor sewers at the time.

The considerable scale on which the phenomena occurred,—the accuracy with which they were recorded,

population, were attacked, of whom thirty-nine died. The characters of the fever were well marked: such as a red, fissured tongue, abnormal pain, tympanitis, diarrhœa, hæmorrhages, an eruption of lenticular rose spots, and a duration of three or four weeks. That the fever was due to the emanations from the sewers was the undisputed opinion of all who investigated the circumstances. Most of the cases, and all but one of the fatal cases, were confined to two of the three districts of the town, the low level and high level districts. Both of these districts had a complete system of drainage, with water-closets within the houses, and sinks in the basements and kitchens. The drains in these two districts were flushed, partly by a continuous flow of water through them from the Thames, and partly from artificial tanks. But, in consequence of a long-continued drought, the Thames had greatly fallen in its level, while the tanks had from neglect been allowed to get dry. The result was that the sewage accumulated in the sewers, and in consequence of their ventilation being very imperfect, the sewer gases escaped directly into the houses. In the two districts mentioned, the fever attacked the rich and poor indiscriminately; but the cases were most numerous and severe in that part of the low level district where all the drains of the town converged, and where they had the least inclination, that is, at the foot of Sheet Street, near the Barracks.

The inhabitants in these districts complained of the offensive smells from the drains in their houses, and particularly in the houses where the fever occurred. The district of the town which remained almost exempt from the fever was the worst and poorest, where cholera had raged with the greatest severity in 1849. Although the drains of this district also suffered from want of water, the water-closets were almost invariably outside the houses, and there was no communication by sinks or otherwise, between the drains and the interior of the houses. With few exceptions, bad smells were not complained of in this district. One woman, however, complained bitterly of the offensive smell from the gully opposite her door: her daughter had died of fever. No case of fever occurred in Windsor Castle; which, as may be seen from the annexed woodcut (not reproduced here) had a drain of its own, unconnected with the town drainage. This drain was well ventilated, and was flushed every morning by a special supply of water. A few of the houses in the Royal Mews participated in this exemption; but in the remainder of the mews, only separated by a roadway from the more favoured portion, but connected with the town drainage, there were thirty cases and three deaths. Lastly, a few cases of fever occurred in the collegiate residences of the Castle, which were also connected with the town drainage.' Dr. Murchison on 'Continued Fever, Pythogenic or Enteric Fever,' p. 446.

and the remarkable precision of the facts, all give to this case a special fitness as a standard, whereby to test the truth of theoretical views.

But, in the selection of this case, for this particular purpose, I have been actuated by a still stronger motive.

I have chosen it not so much for the exactness of the data, as for the striking way in which these data were considered, at the time, to confirm the modern doctrine as to the cause of typhoid fever, in opposition to the doctrine of self-propagation.

In the first place, they were supposed to put, on the authority of facts which could not be misinterpreted, a final and absolute negative on the notion that the transmission of a specific poison from the sick to the healthy has any considerable share in the dissemination of this malady. The indications of contagion, if existing at all, were reported to be insignificant, and by some were held to be altogether doubtful. In seven instances in which the operation of this cause was at first accused, the evidence was said to have entirely broken down on a closer scrutiny.¹

It seemed, indeed, to be generally felt that, in the events of this epidemic, the doctrine of contagion, as applied to this fever, had received its *coup de grâce*. On the other

¹ In the 'Medical Times and Gazette' for July 2, 1859, this epidemic is made, by the editor, the subject of the following remarks:—

'The epidemic of typhoid fever at Windsor is subsiding. It should be known that the disease is the true *endemic* typhoid, not the contagious epidemic *typhus*. Mr. Simon made out very satisfactorily that the cases supposed to prove the importation of the disease and its contagious nature, were really cases of scarlatina, which prevailed to some extent during the prevalence of typhoid. So much confusion is kept up by the similarity of the terms 'typhus' and 'typhoid' that it might be well, in default of a better term, to adopt Dr. Murchison's suggestion, and call typhoid 'pythogenetic' fever, especially as its dependence upon the poison generated by putrescent animal matter is becoming so generally acknowledged. At Windsor, its dependence upon the poisonous gases formed in the town sewers was most evident.'

hand, the pythogenic or 'putrescence' theory, as directly opposed to that of contagion, was thought to have found in the same events an equally final and decisive proof. Here, at any rate, sanitarians argued, was evidence, which none could gainsay, of the production of typhoid fever, on a large scale, by the compounds which mere putrescence breeds in sewage. The facts were as simple and clear to apprehend as they appeared to be severe and binding. By a large and influential school the case is still triumphantly cited in vindication of both these positions. And, that the Windsor epidemic proved, with a clearness and precision not often witnessed before, that typhoid fever may be widely disseminated by a poison issuing from sewers, there could be no doubt. This it was that constituted the special value of the case. But that the morbid agent was bred of mere chemical change occurring in the ordinary contents of sewers, and not in the living and already infected body, this, of all cases, was surely the least fitted to prove. In forming the conclusion that it was so bred—a whole order of facts had been left entirely out of view.

It has already been observed that this epidemic was a considerable thing. The total number of attacks was supposed at the time to exceed four hundred. Mr. Simon, apparently not without reason, believes this to have been an over-estimate. As, however, the deaths were thirty-nine, and as we know, from very large averages, that the proportion of deaths to recoveries is about one to nine and a half, the popular figure was probably not far in excess of the truth. At any rate we shall not err if we assume that at least three hundred and fifty genuine cases of this fever happened in the town.

Now, it is well known that the specific disease of the intestinal follicles of which it is the object of this chapter

to ascertain the true type, is an inseparable accompaniment of the fever. In the course of the epidemic, therefore, three hundred and fifty intestines, affected, more or less severely, with the typical disease figured in the foregoing plates, discharged their specific exuvia into the Windsor sewers.

For a period varying from a fortnight to a month, each one of these three hundred and fifty intestines was pouring—often, no doubt, in copious floods—the most specific of all the fever products *into the very channels which were proved to be giving issue to the fever poison*. From the mouths of all these channels, emanations directly proceeding from the fever-discharges must have been constantly exhaling into the houses in connection with them. This, whatever it might be worth, was as real a fact as any in the history of the epidemic. If the views I have ventured to propound in these pages be true, it was the most important of all. Under any view, it was obviously a fact that had the most imperative claims to be considered, at least, in any adequate discussion of the causes of the phenomena. And yet, strange to say, in the two otherwise very elaborate essays which have been published on this epidemic, it does not once appear as an element of the problem. Perhaps it is still more strange, as reflecting the state of medical opinion on this subject at that time, that at the meeting of the Epidemiological Society, at which one of these papers was read and discussed, there was no one present who undertook to repair the omission.

If the evidence already here advanced be worth anything, there can be no doubt as to what was the part played in this memorable outbreak by the incessant flooding of the sewers which were disseminating the fever with the specific fever-discharges.

In the first place, in seeking to determine this point, we

must never lose sight of the fact, already established, that the fever in question is a self-propagating fever.

For proof of the reality of this property, we need not go beyond this very case. Singularly enough, it is admitted, even by some of those who most repudiated the idea that contagion or self-propagation had any important hand in the events, that in four instances, if not more, there was decisive proof of the communication of the disease from the sick to the healthy. But this is, surely, a very important admission. It might not unreasonably have occurred to any one, that here, perhaps, was the one fact by whose light all the others should have been interpreted.

As regards this fever, I endeavoured to show, in the last chapter, what propagation by contagion really implies. If there be one thing more certain than another in the history of the disease, it is, that this property is not an accident or epi-phenomenon—not a thing that may be put off or on, but an attribute which springs out of conditions that touch its most intimate essence. To reproduce and cast off a specific morbid poison is, as I have already observed, inherent in its very nature, or, in other terms, is the disease itself.

But, if this be so, the only other element needed to explain, in the most simple way, everything that occurred at Windsor is, that the affection of the intestines which accompanied the fever should be what the engraved illustrations attached to this chapter represent it to be, and that the excreta from it should be charged (as under this view they necessarily would be) with the specific agent by which the contagion takes effect.

If these excreta contain the fever-virus, it is certain that the sewers were being incessantly impregnated with it, and, being so impregnated, it is equally certain that

they could not fail to be the principal media for the propagation of the fever. So that the events, as they actually occurred, are not only fully explained on this theory, but might have safely been predicted.

To hold, under these circumstances, that the fever which the sewers were communicating was not the effect of the specific fever-poison with which they were so largely supplied, but of some perfectly undefined and purely hypothetical compound which putrefaction is supposed to extricate from common sewage, seems to me to be nothing less than to invert all the rules which philosophy and experience have united in showing to be essential to a true induction.

To place on unassailable ground the theory of the Windsor fever which I here venture to substitute for that originally propounded, it is, however, necessary to show not only its own fitness, but the untenability of the rival, and only other possible, view. Without anticipating the more complete discussion to which I purpose to subject this branch of the question, I may remark that, under that view, two great difficulties at once suggest themselves.

Assuming, for the sake of argument, that in the great majority of cases the fever was caused by the common pythogenic or putrescent compounds which exist in all sewers, what are we to say to the cases which sprang from contagion? Had the poison two entirely distinct sources? Bred in the most intimate recesses of the living body, by that most specific of processes which constitutes the fever itself, was it also bred by the common chemical changes which occur in mere 'filth'? To establish a supposition so improbable, on its very face, as this supposition is, it is essential that the evidence should be not only open to no ambiguity, but of a nature to preclude

every other possible explanation. How far this was from being the case we have already seen. We know, in fact, that all the while other conditions were in operation, which might possibly, at any rate, explain, in the most natural way, all that happened, and render the supposition in question altogether gratuitous.

The nature of the other difficulty referred to can only be seen by enlarging the field of view.

One of the great characteristics of the Windsor outbreak was the intense power with which, wherever it took effect, the fever-producing cause was acting. Even in the comparatively well-appointed houses of the middle and upper classes it was no uncommon thing for four or five inmates to be struck down at once, or in pretty close succession. But if this were the effect of common sewer emanations, how can it be reconciled with the fact that at the very same time this fever was entirely absent from other places without number, where, nevertheless, there was the evidence of a sense which cannot deceive us, that such emanations were in the highest degree rife?

During the very period when the Windsor epidemic was at its height, I was in the habit of visiting several thickly-peopled courts in Bristol, where the atmosphere of the houses was charged with sewer effluvia to a degree that would not be endured, for a day, except by persons who were bound by the iron chain of poverty to a fixed spot. And yet, all the while, in not one of these courts did a single case of fever arise. Hundreds of other medical practitioners could, I doubt not, bear the like testimony.

The intense contrast between the results in these two cases showed by evidence of peculiar cogency and force that some difference must have existed in the nature of the effluvia. The miasm that, on every hand, was laying whole families prostrate with fever, and the miasm that,

at the very same time, was for months together causing no fever at all, must have differed in some essential thing.

Under the supposition that in the one case it was the vehicle of a contagious virus, while in the other it consisted only of putrescent compounds, the two orders of events were at once explained. On the supposition that putrescent compounds are the actual cause of fever, they are absolutely inexplicable. Adopt the one view, and all is consistent and in harmony with what we know; adopt the other, and everything is inconsistent and at variance with what we know. The events no longer cohere: we have to take up one theory for one set of facts, and another theory for another set; to explain how it was that the fever sprang sometimes from the sewer and sometimes from contagion; to show why, while it was so deadly at Windsor, it was entirely absent from places where the sanitary condition was fully as bad. In one word, we have to reconcile irreconcilable things.

Before I leave this topic, I have one more remark to make. If the version of the Windsor fever here given be the true one, we need go no farther for evidence to show that the intestinal discharges are not only contagious, but incomparably more so than anything else cast off by the sick. We have seen that out of the whole 350 or 400 cases, there were only four that could be traced to direct *personal* communication. Even in these four it does not appear that the disease might not have been propagated by what was thrown off by the bowel. In the remaining 340 or 350 the poison was transmitted through the sewer; or—to state the fact in other words, for now I feel we are entitled to do so—the fever was the work of the intestinal virus.

The whole history is, in short, summed up in the two orders of facts which are here contrasted. The cases of

proved contagion showed by decisive evidence what was the nature of the law by which the fever was being multiplied; the propagation of the fever through the sewers showed, by evidence equally decisive, from what particular surface of the infected body the material issued by which the law was taking effect. Owing to a peculiar combination of circumstances, both these points were brought out with the force and clearness which are generally only met with in the results of experiment. Indeed, had it been possible to furnish experimental proof of the two leading positions which it is one of the chief objects of this work to establish, the evidence in their favour could scarcely have been stronger than that which was here furnished by the natural order of events.

The second great point to which, before I conclude this chapter, I wish for a moment to draw attention, is, that the excreta to which all these fatal prerogatives are assigned are, on their issue from the body, entirely within our power. I shall show, in the course of these pages, that by placing a sufficient measure of a caustic solution of chloride of zinc, or an equivalent quantity of any other powerful disinfectant in the nightpan before it is used by the fever patient, the intestinal discharges may be entirely deprived of their contagious powers. I shall give in future chapters the evidence on which this statement is founded, but I believe the inference to be sure, that if the simple measure here referred to were universally carried into effect, typhoid fever, in so far as it is propagated by sewers, would at no very distant time become extinct. To insure the universal adoption of a measure even so simple as this would, however, require a degree of co-operation amongst medical men, and a degree of zeal and intelligence on the part of the attendants on the sick,

which, at present, at least, I fear we have no title to expect. But, although we may not attain to so desirable a consummation as this, we may come indefinitely near it. At present, the great bulk of what escapes from the intestines of fever patients in this and other countries, is, too often, let loose upon society without the slightest precaution being taken, and we see with what results. I trust the time is not far distant when to allow these matters to pass into the cesspool or sewer in full possession of their deadly powers will be looked upon, not merely as a careless, but as a highly culpable act.

CHAPTER IV.

NATURE OF THE RELATION OF TYPHOID FEVER TO
DEFECTIVE SEWERAGE.

‘In the Appendix to the Fourth Report of the Poor Law Commissioners, it is stated by Drs. Arnott, Kaye, and Southwood Smith, that the malaria arising from putrefying animal and vegetable matters produces typhoid fever. Although I highly respect all these gentlemen, and approve of the practical inferences which they draw from that opinion, so far as it goes, because I have no doubt that vitiated air, like all other causes that weaken the constitution, favours the diffusion of fever, yet I cannot subscribe to their opinion that this cause is adequate, of itself, to produce a contagious fever.’—ALISON.

§ 6. IF the argument in the last chapter as to the nature of the typhoid intestinal affection be sound, it supersedes, in great degree, at least, the need of further evidence, as regards the fundamental part of the problem I am endeavouring to solve.

For, once admit that this affection is the specific eruption of a contagious fever, and all the main positions taken in this work, as to the connection of this fever with defective drainage—would seem to follow as a matter of course.¹

Thus stated, the question is purely one of pathology, and what the physician has to ask is, whether this pathology be true or not.

If it can be proved not to be true, it falls to the

¹ Although, for reasons already given in the preface, I was, myself, slow to perceive this, the position appears to me, now, to be quite unassailable.

ground, and all the inferences that hang upon it, fall with it.

In the present chapter I shall endeavour to show, *as a matter of fact*, that, whether we accept this theory of the intestinal affection or not—that, whatever the view we take of its nature—the intestinal discharges constitute the material by which the fever is mainly propagated,—and that this, and this alone, gives the key to the relation which the spread of this particular species of fever bears to sewerage.

That typhoid fever is actually caused by a poison which sewers and other cloacæ often contain or carry, is a proposition which no English physician will dispute.

Were there no other evidence, the history of the Windsor Fever referred to in the last chapter, would, of itself, be sufficient to establish the fact.

As this fact, however, is quite fundamental to the present inquiry, it must not be allowed to stand on the footing of a mere article of popular belief, but must be made the subject of scientific demonstration. At the risk of being redundant, therefore, I will venture to cite another outbreak in illustration of it, in which the events, regarded in the light of scientific data, leave absolutely nothing to desire.

In the autumn of 1847 an outbreak of fever occurred in Richmond Terrace, Clifton, which acquired great notoriety, at the time, on account of the suddenness, extent, and severity of the visitation.

The terrace in question is built somewhat in the form of a horse-shoe, and consists of thirty-four houses of a good class, occupied by persons in a genteel rank of life. At the end of the terrace there is a pump, from which, at that date, the inhabitants of thirteen houses drew their drinking water. In the latter end of September it became

evident that this water was tainted with sewage. The fact first made itself known by a characteristic taste and smell in the water, and was afterwards further verified by an examination of the well and discovery of the actual leakage. Early in October, typhoid fever broke out nearly at once *in all the thirteen houses in which the tainted water had been drunk*. In almost every house of the thirteen, two or three persons were laid up, and in some a much larger number. Amongst others, the case of a school for young ladies was very striking. The first to suffer in the school was the lady of the house. She was taken ill on the first Monday in the month. Four of her pupils were seized on the following day; and before the end of the week, the mistress, six school girls, and two maid-servants, were all in bed with the same fever. In the beginning of the week following, two more were added to the list. Three children who were sent home on the first outbreak of the disorder, and three others who remained at the school, were the only persons who escaped.

The houses in which the same specific fever thus simultaneously broke out on so large a scale were many of them far apart in the length of the terrace, and their inmates were, for the most part, not in the habit of personal intercourse. The other families on the terrace, *living side by side with these*, continued all the while to be perfectly free from fever. The only important circumstance in which those who suffered so severely, differed from those who did not suffer at all, was that the former had drunk of the tainted well, and the latter had not.

Only a few doors from the school already mentioned there was another girls' school, with about the same number of pupils. In all that related to their internal

economy the two schools were exactly alike ; but while, in the one, eleven persons out of seventeen were struck down with fever, in the other there was not a single case. The one was supplied with drinking water from the poisoned well, and the other from an entirely different source. The circumstances gave to the contrast, here, all the force of an experiment. In complex questions it is not often, indeed, that even experiment yields results so clear and precise.

Amongst the sufferers at Richmond Terrace were nine servants, who were removed to the Bristol Infirmary soon after being attacked. To make the case complete, I have only to add, that all nine presented, in full development, the diagnostic marks of this species of fever. Two of the number, who were my own patients, offered perfectly typical specimens of the disorder. In two others, who died, the small intestine was crowded with the ulcerations which are characteristic of the disease.

The next case will take us a step farther, and is besides so important, in all ways, that it shall have a section to itself.

OUTBREAK OF TYPHOID FEVER AT COWBRIDGE IN 1853—
PROPAGATION OF THE FEVER BY FEVER-TAINTED
DRINKING WATER.

§ 7. IN the month of November 1853, it being Cowbridge Race week, two balls were held on two alternate nights at the 'Bear,' the principal hostel of that little Welsh town.

These balls were attended by about 140 persons, the greater number from different parts of Wales, but some from Gloucester, Somerset, and other distant counties.

Almost immediately afterwards, a number of these persons were seized with typhoid fever, and as many as eight are said to have died of it. Among the sufferers there was a considerable proportion who had never been in one another's company, except in the Cowbridge ball-room. It is clear, therefore, that they had in some way contracted the fever there; and that the typhoid poison was present in no common degree of force and intensity at the 'Bear.' It is not recorded that fever was specially prevalent at the time in the neighbourhood; and, with the exception of one or two persons who lived in the house, those who attended the balls appear to have been alone attacked.

An occurrence so painfully striking, and in all ways so remarkable as this, naturally attracted great attention at the time, and an inquiry was held, with a view to discover the cause of the calamity. The only sanitary defect elicited by this inquiry, in explanation of so terrible an outbreak, consisted in the fact that 'the supper-room was merely a temporary transformation of a loft over a seven-stalled stable,' and that the passage between it and the ball-room was built over a large tank which collected the water from the roof of the house.

From our present point of view, in regard to these matters, it must be obvious that the circumstances here mentioned, were inadequate to account for what had happened, and that to produce so startling a result, some more specific and more powerful elements must have been in operation, which had not been brought to light. The one striking fact in this history was the large proportion in which the guests were infected. Of the 140 persons or thereabouts who attended the balls, as many as forty—nearly a third, that is to say, of the entire

company suffered,¹ a figure which, large as it may seem, was, if we may judge by the number of deaths, probably by no means exaggerated.

By some, indeed, still larger numbers were named, as more nearly representing the truth.

That so many persons coming casually together into a single room, from various and widely distant parts, should be thus stricken down by one and the same specific fever, under so brief an exposure to the action of the morbid cause, is a fact, which, as far as I know, is unique in the records of medical science. It is this fact which gives to this Cowbridge case its great and abiding interest. Taken alone, and without any other evidence, it seems to be all but decisive as to the nature of the medium through which the poison had passed. From our present knowledge we may consider it almost certain that, when forty out of 140 persons, spending merely a few hours together in a room in which there was nothing more than ordinarily offensive to sense, receive there so large a dose of the specific poison of typhoid fever as to be infected with it, contaminated water was the agent by which the disease was produced. I have been informed by persons who were present at the time, that many drinks—lemonade among others—were largely supplied on the occasion and freely partaken of. Air charged with typhoid effluvia is, no doubt, as I shall hereafter show, a very efficient cause of fever; but under the circumstances, and in the absence of bad smells—for nothing of this kind was complained of—it could scarcely have pro-

¹ Of the inmates of the hotel itself, the landlady had fever severely, and some other members of the household had, I believe, slighter attacks. But I do not possess any evidence to enable me to give, with precision, the exact number of local sufferers.

duced so dense a crop as this. So far, the case appears to carry its own evidence with it. On the other hand, the sanitary arrangements of the hotel at the time—especially as regards drainage and water supply, were such as to render it highly probable on other grounds that this was the true view of the facts.¹ The only other element needed, was the occurrence of a prior case in the hotel, and this element also, it appears, was not wanting. Some 17 years ago, or thereabouts, when making a professional visit to Cowbridge, it was stated to me—and the fact seems twice to have become one of public notoriety—that almost immediately before the fever outbreak there had been a fever patient in the house, and that he had only recently quitted it when the balls were held.

One of the collateral points which this case appears to illustrate, in a very striking way, is a point to which I have already referred in a former page, namely: the very prolific nature of the typhoid poison. Here, from a passing exposure to a source of infection limited to a single house, and derived, there is reason to believe, from only one person, we see nearly one-third of a large assembly struck down by fever. Had the company been still more numerous, there is every probability that the number of sufferers would have been more numerous also, in something like the same proportion. But when we consider, in addition, that each new case must have been, in virtue of its very essence, endowed with the

¹ I have been given to understand that some time after the occurrence of this great calamity, the sanitary defects of the hotel were investigated and remedied by order of the noble landlady who at that time owned the property, and I have reason to believe that as regards these important conditions, the 'Bear' is now a well-appointed hostel.

power, under fitting conditions, of throwing up other fresh crops, it is clear, at least, that this fever, whatever other source it may be supposed to have, is liberally endowed by nature with the power of maintaining its species by self-reproduction.

Striking illustrations of this great truth have already been recorded in former pages ; others will occur as we proceed, and in the current Sanitary Reports examples of it are, of late especially, almost constantly appearing ; but among them there is perhaps scarcely one so telling as that which is afforded by this Cowbridge case.

In the interval which has elapsed since the date of these events one other fact has come to my knowledge which possesses great general interest. In the course of the nineteen years which separate us from them I have been consulted by several persons who contracted fever in the Cowbridge ball-room. From these persons I learnt that the one peculiarity which distinguished their illness was the unusual shortness of the time which intervened between the reception of the poison, and the development of the fever. Some of those persons had time to return to distant homes before falling ill, but all these were laid up within a week, and one or two even sooner. I have observed a like shortness of incubation in many other cases of typhoid, presumably communicated by infected water.

The fact is all-important to the true interpretation of such cases.

One consequence of it is, that in communities living in a single establishment, such as schools, workhouses, and other places of the same kind, the first case of a series, propagated in this way, is separated from those which are the actual offspring of it by so short an interval that they

*in culture
d in
sterile*

are looked upon as if they were simultaneous, and are often erroneously considered as the work of a common cause.

As regards this point, however, everything must depend on the nature of the communication between the cesspool or drain, and the well. Where this communication is large and direct, the succession of events is rapid, but where it is by gradual percolation through strata of some depth, a considerable time may elapse after the poison is discharged before it can again reach the human stomach. A heavy fall of rain sometimes appears to be the determining incident.

It seems probable, also, that a suddenly-increased draught upon the water of a well, by causing a correlative inflow from surrounding tainted strata, may also hasten the result.

OUTBREAK OF TYPHOID FEVER AT KINGSWOOD IN 1866—
PROPAGATION OF THE FEVER BY A FEVER-TAINTED
BROOK.

§ 8. ON October the 24th, 1866, my friend Dr. H. Grace, whom I had met on other business, told me that, if I had half an hour to spare, he would show me a striking illustration of my views on the spread of typhoid fever.

The temptation was too great to be resisted; so, jumping into his dog-cart, we presently pulled up in front of two labourers' cottages built in a single block, by the roadside. These cottages may be called, for convenience' sake, Nos. 1 and 2.

In the form of a lean-to against the gable end of No. 2

was a privy, which served in common for the inmates of both dwellings.

Through this privy there flowed, with very feeble current at that time, a small stream, named the Waynebrook, which formed a natural drain for it. Having already performed the same office for some twenty or thirty other houses higher up its course, this stream had acquired, as was patent to more than one sense, all the characters of a common sewer, before reaching the cottages in question. From this point, after skirting the high road for about forty or fifty yards, it passed into a field, and crossing, now as a covered drain, now as an uncovered ditch, some three or four meadows, the stream came into the open, again, in a large court occupied by two other labourers' cottages and some farm buildings. These two cottages may be conveniently called Nos. 3 and 4.

The sanitary relations which the stream held to their inmates was an exact repetition of that which obtained in regard to Nos. 1 and 2, already described.

Passing through the court uncovered, it acted as a drain to a small privy, common, as before, to both cottages.

I did not measure the distance which separated these two little homesteads, but I judge it to be somewhere about a quarter of a mile, as the crow flies.

The four cottages thus situated were the scene of the series of events which Dr. Grace was anxious to bring before me.

The outbreak began in the person of the father of the family living in No. 1. There were two circumstances attaching to this man which made his case different from that of any other member of his own or his neighbour's household.

1st. He was the only one of the group whose way of

life took him away to the neighbouring city ; and 2nd, he was the only one who was known to have been exposed to the infection of typhoid fever.

Having a horse and cart, he plied a small trade with Bristol, partly as hawker and partly as huckster. His chief business in the city lay in the filthy back-slums of St. Philip's, where, for some time immediately before his illness, typhoid fever—as I can affirm from my own observation—was epidemic. Whether he got his fever here, it is, of course, impossible to say with absolute certainty, but that in the course of his business he must have been largely exposed to its specific infection there was no doubt.

That his disease was contracted away from home was further indicated by the fact that when he was stricken all the other inmates of the two cottages were, and, indeed, continued for some time after, to be, in their usual health.

His attack proved to be severe and protracted, and for a considerable time was attended by profuse diarrhœa.

As a matter of course, all the discharges were thrown into the common privy. In this way, for more than a fortnight, the stream which passed through it continued to be daily and largely fed with the specific excreta from the diseased intestine of the patient.

Some weeks passed away thus, without any fresh incident ; but, in the latter end of the third, or beginning of the fourth week—which, as M. Piedvache has justly observed, is about the time when the contagion of this fever generally begins to show itself in fresh crops of the disease—a new order of events occurred.¹

¹ 'M. Gendron,' M. Piedvache observes, 'and all those who have written on the contagion of typhoid fever, have remarked that it is at an advanced period that it principally manifests its contagious properties. In all the cases I have seen, and notably in those I have cited, the first patient had reached an advanced period of the fever when it attacked those about him. In some cases

Several persons were simultaneously attacked with the same fever in all the four cottages.

Not, be it observed, in Nos. 1 and 2 merely, whose inmates might be described as living in more or less contiguity to the already infected man, but in Nos. 3 and 4 also, nearly a quarter of a mile away.

Within the space of a few days Dr. Grace was attending quite a cluster of cases in each of the four, and before long the majority of the persons living in them were in bed with the fever.

One fact more must be recorded to render the history complete.

From first to last, the outbreak was confined to these four cottages, and there was no other case of typhoid fever at the time in that neighbourhood.

These events speak for themselves.

If we look at them by the light of what has gone before—if, especially, we bear in mind the established fact that, in some way or other, this fever has the power to propagate itself, there can be no reasonable doubt that the second crop of cases was the offspring of seed cast off by the first sufferer.

But if this be so, the circumstances of the outbreak in the two lower cottages, Nos. 3 and 4, show by the most striking evidence what was the particular form under which this seed was liberated.

The significance of these circumstances will be appre-

he was entering on convalescence or had been already five or six weeks ill. Most frequently, of all, he was in the fourth week of the disorder; rarely in the third.' This corresponds very closely with my own experience. But as the latent period of typhoid fever lasts on an average some twelve or fourteen days, we must go back by that much from the actual onset of the fever to find the true date of infection. These observations apply, however, only to those cases in which the fever germ is received through the air. When it is introduced through the drinking water, the latent period is, as I have shown, often much shorter and the events succeed one another with greater rapidity in the same proportion.

ciated at once when it is added, that those who were attacked in this particular outbreak had not only held no intercourse of any kind with the inmates of Nos. 1 and 2, but had not the remotest suspicion of the origin of the deadly pest which had appeared thus silently in their midst.

The little stream laden with the fever-poison cast off by the intestinal disease of the man who had been stricken with the same fever some weeks before, was the only bond between them.

We have already learnt to see in this disease of the intestine the specific eruption of a contagious fever; we here see, as in small-pox, and other contagious fevers, the poison shed by this eruption producing fresh fruit.

But if the remarkable history here related shows with the utmost clearness that sewage when charged with the specific excreta of typhoid fever is all-potent in the propagation of that disease, it appears to me to show with equal clearness that sewage not so charged has no power of the kind.

While Dr. Grace was seeing his patients in Nos. 1 and 2, and I was standing outside, a gentleman on horse-back drew up, and addressed me in these words:—

‘Ah, I see what you are upon. The only wonder is, that all these poor people have not died of fever long ago. For, any time these last six years, but in summer especially, to anyone coming down this lane, the stink has been enough to knock a man down.’

But although—so to speak—strong enough to knock a man down, it had failed all these long years to cause a single case of fever.

How, if sewage emanations be as potent to cause typhoid fever as many teach, can this possibly be explained?

This failure, to recur to an argument used once before,

could not have been because the seasons had not been favourable to the development of the pest, for within this period this fever had more than once committed great havoc in the same parish. It could not have been that the little community who were now suffering so severely from it were proof against it, for as the event proved they were only too susceptible.

The very magnitude of the contrast between these many years of past entire immunity from fever, and present great prevalence of it is, surely, in itself, decisive of the question.

But there was no need of travelling back in time to give point to this antithesis. At the very date when the events were occurring, all the elements of this contrast were present on the spot.

I have already stated that before reaching cottages Nos. 1 and 2, the stream had served the office of common sewer to some twenty or thirty houses higher up. But while in Nos. 1 and 2, and in the two cottages *below* them, nearly every inmate was stricken with fever, in not one of the thirty houses above these, was there, from first to last, a single case.

It was down the stream that the seeds of the plague flowed.

Higher up, the stream was common sewage only; lower down, it was sewage *plus* the specific excreta of the fever-patient.

Hence the cardinal difference in the fate of those who were exposed to its emanations in the two situations.¹

¹ There was, it is true, one important difference between the two cases. Where the stream came into contact with the fever-stricken houses it was an open ditch: where it came into contact with the higher group of houses, it was a covered drain. But, on the other hand, almost immediately below these last, it passed uncovered through two long fields. In one of these two, a field used as a market garden, a large open catch-pit had existed for many years for

The only inference that it seems possible to draw from these facts is, that while sewage charged with the specific fever-poison is all-potent in breeding fever, sewage not so charged has no power to breed it at all.

About two years later, Dr. Grace took me rapidly over the ground of another outbreak, occurring in the same neighbourhood, in which there appeared to have been an almost exact repetition, on a larger scale, of the incidents just related.

In this case, also, a small rivulet was the carrier of the infection. Rising, like the Waynebrook, near the summit, but on another slope, of Kingswood Hill, and passing thence through the small village of Hanham, this rivulet winds its way through a valley about a mile long, until it discharges itself into the Avon.

All the way down the valley, below the village, its banks are studded with cottages, which are to the stream as so many beads strung on a string.

For some of these cottages the stream is the actual sewer; others, which are a little farther off from it, still, for the most part, deliver the excreta of their inmates, and other refuse, more or less directly into it.

Along the whole line of watershed, the stream is, in fact, the natural drain of the valley.

The first case of the series occurred in a cottage, which, for convenience sake, we may call No. 1, and which stood at some little distance—perhaps a hundred yards, or more—from its bank. The next cottage in which the disease appeared, and which we may call No. 2, almost immediately overhung the stream. That the infection was actually conveyed from the one to the other

storing the sewage of the stream, and from this fetid pool, at the very time the fever was prevailing, numbers of persons were continually dipping, with entire impunity, the liquid sewage in order to water the land with it.

there was no absolute proof. But while the circumstances were such as fully to justify this view, the improbability of any other was extreme.

The numerous class who confess their belief in the powers of sewer gas can have no difficulty about the origin of the first case.

The subject of it had been employed, for the two months immediately preceding his attack, *as a workman in the Bristol sewers.*

His case was severe and protracted, and was attended with much diarrhœa.

It is important to observe that at the date of his seizure there was not only no other case of typhoid fever in the neighbourhood, but that, for many years before, the whole of this valley had been entirely free from it. Specially, I may mention that at the very time when on the Waynebrook, about a mile away, this same fever was rife, in the outbreak just described, there was no trace of it here.

In the Hanham Valley, as in that outbreak, some weeks passed away after the seizure of the first subject, without any fresh incident.

At the end of that time, almost simultaneously, new cases of fever sprang up, not only in No. 1, but in No. 2 also, the inmates of which appear to have held no personal intercourse with it. From this date the fever may be said to have been fairly planted on the stream, and, for weeks together, the current was fed with large and increasing doses of the fever excreta.

I do not know the events which followed with sufficient accuracy of detail to speak of them in other than general terms. It is sufficient to say, that before the tragedy ended, this same specific fever, descending step by step, broke out in some twenty or thirty cottages, down the banks of the stream which bore the specific exuvia from it. So long as this stream conveyed common sewage only,

it had been powerless to cause fever; but when it became charged with the fever-poison, a large community were speedily infected by it.

I may observe in passing, that these two outbreaks, not only show fever to be self-propagating, but, like the events which occurred at Cowbridge, prove it to be very prolific.

They not only give the key to its wide dissemination, but offer a perfectly adequate explanation of it.

Give to any disease this faculty of self-multiplication—the power to cast off seeds and breed new crops, and you give it the power to perpetuate itself, and, where the climatic conditions allow, to spread widely over the earth.

It is by this power, and by this alone, that the other contagions keep up their kind, and have come, in course of time, to infest large portions of the globe.

Nay, it is by the very same power, that of self-multiplication—that the living species, which people it, have come down to us, and maintain their succession among us.

TYPHOID FEVER IN SCHOOLS AND OTHER PUBLIC ESTABLISHMENTS.

§ 9. The conclusions to which the four remarkable outbreaks just related so clearly point, are entirely borne out, as I have already more than once shown, by the general history of typhoid fever.

But the illustrations of these conclusions furnished from time to time by schools and other large public establishments are so striking, and the lessons they teach are, practically, so important, that I may be pardoned for citing one or two of which I possess records.

Some twenty years ago or more, I had occasion to visit

a large public school in the south of England, where, a short time before, typhoid fever had made great havoc.

As nearly always happens under such circumstances, the fever had begun by one or two straggling cases, followed, after some little time, by the seizure of a large number of inmates at once. On the strength of the general experience in such matters, the state of the common latrine, which was very defective and highly offensive, was at once fixed upon as a sufficient explanation of the calamity. As the boys who took the fever *were separated from the rest the moment they fell ill*, the operation of contagion was supposed to be entirely excluded, and the case was accordingly quoted, at the time, not only as opposed to contagionist views, but as a flagrant illustration of fever on a large scale caused by miasmata actually generated in a common sewer. But a closer scrutiny was fatal to this view of the case. In the first place, the cloaca, although sufficiently offensive, was in no worse state than it had been in for many months before, during the whole of which time no single case of fever had occurred. That what had been so long harmless should have become suddenly so deadly, of itself implied the introduction of some new element. On the other hand, in considering events of this kind, we cannot lose sight of the great fact that this fever is an essentially contagious fever. Those who came to the conclusion, that in this particular instance it had not spread by contagion, had overlooked the all-important circumstance that, although the *persons* of the sick were secluded, the locality which by common consent had been fixed upon as the very focus of the mischief had continued to be flooded daily by the most specific of all the emanations from them. I ascertained on inquiry that *the intestinal discharges from the fever patients still found their way to the common receptacle*; so that, on the very principle of

contagion, the boys who continued to use this receptacle ran a far greater risk of catching the fever than if, without resorting to it, they had actually passed their time in the sick chamber. Looking at the facts as a whole, therefore, and interpreting them by the light of other evidence, the conclusion seemed to be irresistible that the tainted latrine, to which everything pointed as the chief agent in spreading the fever, gave the disorder, not because it was exhaling *pythogenic* or putrescent compounds, but because it had become impregnated with the actual fever-poison.

A precisely similar order of events occurred, in the autumn of 1842, at the Female Orphan Asylum, Ashley-hill, near Bristol. This asylum is spacious and well built, and stands on a healthy site. At the date of the outbreak it held exactly fifty inmates. The fever began in the first week in August, with a single case, in the person of one of the girls. About twelve days before her attack, the orphans had spent a whole day out, and it was the matron's opinion that this girl had, by some chance, contracted the fever on that occasion. There were many things, indeed, to favour the conjecture. Amongst others may be mentioned the fact that, being in the enjoyment of her usual health up to the day of the holiday, she began to droop almost immediately afterwards. As soon as she was laid up she was placed in the sick-room, away from the rest, except one or two invalids, who, at first, shared the apartment with her. At a later period, when cases began to multiply, a special ward was set aside where all fever patients were kept, in strict seclusion from the moment of their attack, in the hope of staying the spread of the disorder. It was not until early in September, when the first patient was already in the fourth week of illness, that the second case occurred. This was soon followed by others, which came in, one by

one, in pretty quick succession, until the beginning of November, when the disease ceased to extend. By that time, twenty-three of the fifty inmates had been attacked by it. It may be well to add, that in the greater number diarrhœa was a very troublesome symptom, and that in the only fatal case the characteristic ulcerations of the intestine were found by my friend, Dr. Swayne, by whose kindness I was enabled to take part in the post-mortem examination.

There was nothing in the sanitary condition of the establishment to account for so severe an epidemic of fever under the popular view of its causation. The house was scrupulously clean, the drinking-water pure, and, with the qualification to be presently made, there were no bad smells about. There were in the construction of the latrine two points to which, no doubt, serious exception might be taken. In the first place, it was a common privy, and not a watercloset; in the next, the little room in which the eight or nine *sedilia* were, was close and ill ventilated. A dead wall stood almost immediately in front of the door, and the window did not admit of being opened. Whoever entered there, necessarily breathed for the time an atmosphere highly charged with exhalations from the foul excreta which it is the office of such places to receive.

But that this state of things was of itself powerless to cause the fever, was proved in the most striking way by the history of the place. For nearly twenty years this latrine, such as it then was, had served the needs of the whole establishment. Nothing had happened to derange either its structure or its functions. It was not more offensive now than it had ever been. But up to the date of this outbreak, no single case of fever had occurred to the long succession of orphans who had resorted to it. To

suppose that an agency which for so many years had not caused one case, should now cause more than twenty at once, would be on its face absurd. The extreme contrast between present great prevalence and past total immunity, was, in this instance, also, clear proof of the introduction of a new element.

We need not seek far to know what that element was. Had not the conclusion been forced upon us by the absence of any other rational explanation, as well as by the whole order of events, it would have been plain, from other considerations, that this fever was being propagated here by the same law as that by which it was propagated at Loosebeare and at Chaffcombe—that is to say, by contagion.

But if propagated by contagion, there could be no doubt as to the way in which the contagious germ passed from the sick to the healthy. *Kept in strict separation from one another, as far as their persons were concerned, the common privy was almost the only connecting link left between them.* A new event had lately occurred in the history of this cloaca. For the first time since its construction it had become impregnated with discharges from the diseased intestines of fever patients. The fifty persons who daily entered there, now breathed exhalations from these discharges in a high degree of concentration. What wonder if twenty-three of the fifty should pay the penalty in an attack of the disorder!

The history, of which two examples have here been given, is, in its main features, a very common one. In the records of the schools, workhouses, barracks, and prisons of this and other countries, a great number of strictly analogous cases are to be found. Outbreaks of Asiatic cholera, which, in all essential conditions, are the precise

counterpart of these outbreaks of fever, are also of common occurrence in the same establishments.

The conditions under which these outbreaks occur have a peculiar interest for two reasons: first, because, in many of them, the early separation of the sick has been, on principle, rigorously carried out; and, secondly, because, *in spite of such separation*, no other communities have ever offered such instances, as these, of rapid and wholesale infection.

I have already endeavoured to show, elsewhere, that in cholera—and the principle applies with the same strictness to typhoid fever—the solution of this enigma is to be found (in many instances at least) in the defective condition of some latrine, which is at once the receptacle of the discharges from the sick, and the daily resort of the healthy.

CHAPTER V.

CONDITIONS ATTACHING TO THE CONTAGIOUS AGENT AS IT
EXISTS IN MEDIA EXTERNAL TO THE BODY.

‘The positions of science must be tried in the jeweller’s scales, not, like the mixed commodities of the market, on the weighbridge of common opinion and vulgar usage.’—COLERIDGE.

It is plain, from what has gone before, that the typhoid poison has two separate modes of existence; one within the infected body, which constitutes the soil wherein it breeds and multiplies, and the other in media external to the body.

In its external phase the poison is exposed to a great variety of conditions, which, whether in the way of hindrance or promotion, materially affect the work of fever-propagation.

The more important of these conditions we must now proceed to consider.

THE FULL CONTAGIOUS POWER OF THE INTESTINAL DIS-
CHARGES NOT DEVELOPED AT FIRST—VARIOUS THEORIES
TO ACCOUNT FOR THE FACT.

§ 10. IN the ‘British Medical Journal’ for March the 16th, 1867, in the course of an abstract of a clinical lecture, by Dr. Murchison, on the eliminative plan of

treatment in typhoid fever, there occurs the following very remarkable statement.¹

‘The advocates of the eliminative plan of treatment ground their views on the assumption that the peculiar poison which gives rise to typhoid fever is contained in the evacuations ; but Dr. Murchison maintains that there is no proof, whatever, that the fresh stools passed by a typhoid patient are in any way deleterious. It is very probable, it is true, that the fever is propagated by the stools, but only after they have undergone decomposition. He rests his position on the fact, that, during the last five years, cases of enteric fever and cases which have not been fever at all, have been treated in the same wards of the London Fever Hospital, that 1,739 cases of the former and 2,123 of the latter have been interspersed together, and yet that not one of the patients in these wards has contracted enteric fever, although all the circumstances were most favourable to the propagation of the disease by the stools.

‘Night-chairs are placed between the beds, which are often indiscriminately used by the patients on each side ; the pans are emptied only once a day, and no means are taken for disinfecting the stools (a practice, I may parenthetically interpose, more honoured in the breach than the observance).

‘The attendants in these wards have enjoyed a like immunity.’

Observations to the same effect have been made by many other physicians.

Although I could give very cogent reasons for not accepting the dictum, that the intestinal discharges of

¹ Case of typhoid fever, with unusually severe cerebral symptoms in the beginning. Recovery. Clinical remarks on the case, and on elimination in the course of this disease, and on the propriety of checking or favouring the diarrhoea.

typhoid fever are not in any way deleterious when first voided, nothing is better ascertained than that, with common cleanliness, they are not very dangerous to the inmates of the sick-room.

As it is equally sure, and for proof we need not go beyond these pages, that this fever is mainly, if not wholly, propagated by them, it is clear that they must, after leaving the body, acquire in some way or other, a development of infective power.

The explanation of this seeming paradox has been made, in Germany, especially, the subject of many speculations, more fanciful than sound.

Conspicuous among the theories which have been invented to account for the phenomena, is that which is associated with the name of Pettenkofer.

This theory is couched by its author in terms so vague and mysterious that I never myself feel quite sure of exactly understanding what it involves.

His fundamental position, however, appears to be this:—

That the poison of typhoid fever is not, like the poison of small-pox and the other contagious fevers, cast forth from the body in a finished state; that, in direct opposition, as regards this, to the other contagious poisons, it has, when first discharged, no power to propagate the fever at all, but can only acquire this power by going through putrefactive decomposition first.¹

¹ This hypothesis is founded on the well-known experiments by Thiersch and Pettenkofer on the effects produced on mice by feeding them with cholera discharges. When these discharges were given in the fresh state, the animals suffered no harm; but, allowed to putrefy first, the mice fell into a state of collapse and speedily died. These experiments would be quite decisive, if it were proved that what the mice died of was real Asiatic cholera. To complete the proof, it would have been necessary to show that the mice thus poisoned had the power to communicate cholera to other mice in the same way in which cholera-stricken men communicate cholera to other men. But this fundamental piece of evidence is precisely what is wanting. On the other hand,

In later developments of the theory it is made to appear that the *soil* is an essential factor in the result—that in virtue of some mysterious reaction between the typhoid excreta and the soil, there springs up a ‘*tertium quid*’ different from either, and which constitutes the real agent by which the fever is propagated.

Now, if by this, it be meant that the typhoid excreta acquire in media external to the body powers of new nature and birth, and not inherent in them from the first; yet more, if it be meant that these powers are the offspring of a state of decay or putrefaction in the contagious germ itself; or, yet again (to illustrate the point in another way), if it be held under this theory that anything happens here essentially different from what happens in the case of the other contagions, the view is one from which I venture altogether to dissent.

Such a view is, as it appears to me, at once gratuitous and unnecessary, at complete variance with analogy, and unsupported by fact.

Exactly as in small-pox, so in typhoid fever, the contagious agent which issues in the specific excreta is the fruit of its own prior reproduction within the already infected body.

To carry on the line of succession a step farther, all that is needed is, in either case, that this agent should retain in its transmission to the next recipient the reproductive powers of which it is, itself, the offspring.

there are very strong reasons for believing that mice are not susceptible of cholera at all. Certainly the rat, the congener of the mouse, is proof against it. In Paris in 1849, the sewers were flooded with rice-water discharges to an enormous extent for months together. The rats, which people the sewers of that city so thickly, must have lived during the whole of that time immersed, so to speak, in these excreta. Yet, I believe, no dead rats were seen at the mouths of the sewers, which surely could not have been the case had these animals been subject to the pestilence. At the annual ‘*Chasse aux Rats*’ which followed, the ‘*bag*’ was said to be quite as big as usual.

That these powers may be quickened, after the discharges leave the body, in ways hereafter to be discussed, seems not improbable.

But that anything beyond this occurs there is, I believe, no reason to suppose.

I am the more confident in the soundness of this conclusion, because other conditions hitherto, for the most part, entirely overlooked, may be shown, of necessity, to intervene, which go far to explain, even if they do not altogether explain, the apparent enigma.

The practice of inoculation has proved by experiment that the contagious unit necessary to the propagation of a contagious fever is a thing of extreme minuteness. But common observation puts the same truth in perhaps a still stronger light.

Of such impalpable minuteness, indeed, is the contagious germ, that its reception into the body, when these fevers are taken in the natural way, is an act that is cognisable to no sense. Unless in the very rarest instances, in typhoid fever—the disease we are considering—the victim never knows the moment of his infection.

To inhale, for an instant, the exhalations of a sewer; to walk down an infected lane or alley, where there may even be no offensive smell to warn the visitor of his danger; to drink a draught of water which perhaps may only differ, to the sense, from any other drinking water, in being more sparkling, or in having a brisker smack, is often to be stricken with this fever unto death.

But if, in the act of implanting the disease, the poison is impalpable, its condition on issuing from the body, is the very opposite of this. Although in its final dispersion it is resolved into mere molecules, it is first cast off in bulk.

Much of it, even when first voided, is, no doubt, already in a state of fine division, but much, also, is present in

the form of clots or pellets of yellow matter, which are to the contagious germs which float impalpable in air or water, much as the block of granite is to the dust into which it may be ground.¹

The application of these considerations to the case before us must be obvious to everyone.

If they be true, it necessarily follows that, before the poison contained in the typhoid stool can exert, to its full extent, the contagious power inherent in it, and take its full part in the work of typhoid propagation, it must be liberated, by drying, fermentation, or some other mode of disintegration, from the clots, pellets, or other organic husk or entanglement, in which it is embedded, and resolved into particles, which, suspended in the media that surround us, represent the condition under which it can alone convey widespread infection.²

The case may be likened to that of the poppy or many another plant.

Poppies, like contagious fevers, propagate themselves. When the seed-capsule is ripe it drops off, but the capsule itself, has to be broken up—often travelling long distances the while—before the numberless seeds it encloses are cast out upon the soil to spring up as fresh poppies.

And so, in a measure, with the fever-seed also.³

It will be seen, further, that these considerations apply,

¹ The state of division in which the essential matter issues varies very much in different cases, and in different stages of the same case. In some cases the yellow matter is gradually fretted away from the ulcers, in a finely divided state, in others it is cast off in large sloughs.

² Having taught this doctrine here, in Bristol, for many years, it was a great gratification to me to find that Professor Viermer of Zurich, a physician of the highest eminence, had been led to the same view, in its application to the case of cholera, on entirely independent evidence. His ideas on the subject will be found in a pamphlet, published by him at Zurich, in 1867, under the following title: 'Ueber die Ursachen der Volkskrankheiten, insbesondere der Cholera.'

³ The illustration applies with still greater exactness to the excreta of cholera and tubercle.

more or less, whatever the medium through which the contagion passes from one subject to another. Less absolutely, however, to transmission by water than to transmission by air.

For, as the discharges are fluid, the poison they contain is more or less diffusible in water from the first.

But, in transmission by air, before this poison can take part, at all, in the work of propagation, it must not only be resolved into the molecular state, but the infective molecules must have escaped from the liquid medium, in which they were first eliminated, into the air we breathe.

When it is added that, in spite of all that has been said and written to the contrary of late, there is reason to believe that air is the chief vehicle of this infection, the importance of this principle, in its application to the dissemination of typhoid fever, is at once understood.

In water infection the succession of events is more rapid, and as a rule the resulting disease is more fatal; infection by air, although more slow, operates on a wider scale, and in the aggregate affects a still larger number of persons.

Thus it is, that without having recourse to hypothesis at all, and confining ourselves within the limits of conditions which are known to operate, we see why the intestinal discharges must, of necessity, become, in course of time, incomparably more effective in spreading the fever than when first cast off.

It is not that any new powers are called into being, but only that the powers, already existing, are brought into play.

In proportion as the surrounding conditions further or prevent the changes necessary to this, on the one hand, and in proportion as they lay open, or shut up, ways for the transmission to other subjects, of the liberated infec-

tious swarm, on the other, in the same proportion do these conditions hinder or promote the spread of the fever.

Subject, possibly, to a qualification hereafter to be mentioned, it is in this way, and in this way alone, as I believe, that external physical agencies operate.

This is particularly true of the important agencies of soil and water, about which, with some few points of real interest, so much that is questionable has been written and said.

In following out these same relations we may see the real explanation of the part which fermentation or putrefaction takes in the process.

I have already expressed my opinion that fermentation does not act, at all, by communicating any new property to the essential agent.

It creates no new power or gift.

But, as the great instrument of the softening and disintegration of organic matters, it probably has the principal hand in hastening the extrication and liberation of the germs in which the infective power resides.

It is even probable that the gases which are so abundantly evolved, in result of this process, and which often rise into the air with considerable mechanical force, still further help, by carrying these germs with them, the atmospheric diffusion of the contagious matter.

By the light of these considerations the very striking facts related at the beginning of this chapter on the authority of Dr. Murchison lose all their apparent mystery.

No facts in the history of this fever have more perplexed observers, have given rise to so much groundless speculation, or have proved so great an obstacle to the reception of the doctrine of contagion.

Does the explanation here offered of them apply to all

cases? Does it cover the whole field? Are there any outlying phenomena which do not fall within it?

Various considerations have led me to think that it may be worth inquiring whether exposure to the open air, and specially to the oxygen it contains, may not give increased energy to the contagious agent.

Dr. Calvert has lately made the important observation that infusoria multiply much more rapidly in oxygen than in common atmospheric air.

As oxygen is the great quickener of all vital power, this increased fertility is a result that is intelligible enough.

But this multiplication of the living organism is something so near akin to the multiplication of the contagious agent, that it would not be surprising if this also were affected in a similar way by similar exposure.

Although, however, such considerations as these may be interesting as suggesting inquiry, they must not be considered, at present, as having gone beyond the speculative stage, or as standing at all in the same category with what has been laid down before as regards the atmospheric dispersion of the contagious particles.

Other doctrines have been put forward which may be dealt with more summarily. Such, for instance, is the doctrine which some hold, that the specific poison of typhoid fever, multiplied as it is known to be within the living body, in virtue of the fever process itself, continues to multiply after its exit from the body, in the media into which it passes.

There is, in reality, no plea whatever for this assumption.

On pathological grounds there are the strongest reasons for believing that as in small-pox, so in typhoid fever, the specific poison is only reproduced in the living body infected with it.

The same general reply may be made to another hypothesis, which, on abstract grounds, is not otherwise untenable.

Many examples are now known of very noxious things which propagate themselves by multiplying within the living body, but which cast off germs totally incapable of carrying on the succession until they have passed through another phase, in media external to the body. Whether these media be the living bodies of other creatures, or not, makes no difference.

The case of the tapeworm is a familiar instance. It is well known that the ova of this parasite, after being shed by man, have to pass through an intermediate form in the body of the pig or some other domestic animal, before they can again produce the tapeworm in the human subject.

It has often occurred to me to ask whether something of what is matter of actual demonstration as regards the palpable entozoon, may not happen in regard to the impalpable entities which cause contagious diseases.

That some of these will be eventually found to come under this description seems not improbable.

But that the material cause of typhoid fever should be counted among them there is no reason to believe.

Before leaving this part of my subject, there are one or two minor questions, of some importance, on which it may be well to say a word.

Typhoid fever is a contagious fever propagated by a specific poison.

How long after its discharge from the body does this poison retain its contagious power?

Unhappily, there are no data which enable us to give an exact answer to this question, and, for obvious reasons, such data must be very hard to get.

If, in default of these, it may be permitted to draw from analogy, we come to a very decided conclusion. There is a growing belief that the specific germs which cause contagious fevers are, in reality, so many living species.

Now it is a well-known fact that, under certain conditions, infusoria and other minute organisms may retain their reproductive powers in a dormant state for indefinite periods of time.

There is abundant evidence to show that the same thing holds goods of the specific properties of many of the contagions. Vaccine, when protected from air, or when in the dried state, keeps good for a long time.

It has been all but proved that articles of dress tainted with the poison of scarlet fever retain the power of communicating that disease for years.

Many facts have come under the notice of observers which seem to show that the poison of typhoid fever offers no exception to the rule.

I once knew a labourer's cottage which remained vacant nearly two years, in consequence of nearly every inmate of it having contracted typhoid fever. At the end of that time it was re-tenanted, and three weeks had scarcely elapsed before several of the new inmates were simultaneously seized with the same fever. The cottage stood alone in a secluded spot, and there was no fever in the neighbourhood at the time of the second outbreak.

Some years ago there was a very heavy epidemic of typhoid fever in the parish of Lapford, in North Devon. One farm, in particular, suffered terribly, as many as seven of the farmer's family having taken the infection. At that time the farmer's wife escaped the fever. But, about fifteen months afterwards, when the disease had long disappeared from Lapford, she was seized with it, and

*infection of the
dormant*

narrowly escaped with life. She had not left the place for a day since the former outbreak.

In his work on 'The Practice of Medicine,' Trousseau relates a similar case, in which the second attack occurred at the end of a year, and adds the curious remark that, in his experience, the fever has shown great proneness to return in the same house at the twelvemonth's end.

Under what guise the dormant poison lurks in such cases it would not be easy to say.

But in seeking to interpret the conditions under which this species of fever breaks out, facts and considerations of this order must not be lost sight of.

THE CESSPOOL AND THE SEWER.

§ 11. As the great bulk of the typhoid poison, from the form in which it is cast off, necessarily finds its way to the soil, the nature of the conditions it meets with there must affect in an important way its distribution.

I should have much to say on the effect of these conditions had I time for the task.

As, however, the object of this work is mainly practical, and, by bringing to light the cause of typhoid fever, to teach us how to destroy it; in other words, as its principal aim is, as far as this fever is concerned, to render us independent of soil and everything else, my treatment of this branch of the subject will be purposely brief.

In the study of it, two principal cases have to be distinguished: the cesspool and the sewer.

It is in the first that variations in soil chiefly come into play.

The nature and constitution of the soil itself, its degree of porosity, its elevation, slope, line of watershed, greater or less saturation with air and water, its temperature, and

many other conditions that might be named, all affect more or less the power and distribution of the poison.

On many of these points, Hirsch, Pettenkofer, and, more recently, Dr. Haviland, have made observations which, although they must not be taken in too absolute a sense, are not without interest.

Those who desire further information on these topics I must refer to the writings of these observers.

As regards the sewer, the error commonly committed is to look at it too much in the abstract. By most people it is conceived of, simply, as an artificial and more or less perfectly closed channel, in which human and other excreta, in a state of decomposition, and extricating foul gases, are carried away from the inhabited area.

Any one who has read Parent Duchatelet's remarkable book on the sewers of Paris, will see that this is a very imperfect idea of the great diversity of conditions the sewer presents.

The greater or less degree of incline, the amount of water carried and the rapidity of its flow, the variations of temperature in accordance with variations in climate or season, or depth below the surface, the manner in which its temperature is influenced by frost, and especially by the sudden melting of large masses of snow or ice, the existence, sometimes in fixed directions, of strong aerial currents, are all elements that must not be lost sight of.

Another element remains which, as far as I have seen, has hitherto been entirely overlooked, but which there is reason to believe may become a very important factor in the case. I speak of the admixture with the sewage proper, of various strong chemicals, often in very large quantities, under the form of manufacturing refuse. Some of these chemicals are known to have strong disinfecting powers, and it has often occurred to me that it

their immediate support, and the unfortunate family, though in comfortable circumstances previous to the attack of this calamity, is soon reduced to a level with those in great indigence.

‘But the evil does not terminate here. The clothes and linen, *especially of those who die*, are impregnated with contagion as well as the room, and servants who visit their friends or acquaintances during the fever, and more particularly those who buy articles of linen or apparel from pawnbrokers, introduce the infection without suspicion into the families of the affluent. Such unsuspected modes of introducing contagion can seldom be traced, but that they frequently operate powerfully cannot be doubted.’

Dr. Clark adds, by way of illustration, that the most malignant cases of fever he ever attended in Newcastle were in the families of three pawnbrokers.

Whether these were cases of typhus or typhoid, there is no means, now, of knowing. But, in the course of a long experience, many instances have come to my knowledge in which the receipt of a parcel of typhoid tainted linen has become the means of imparting the fever to its new possessors.

It is no doubt chiefly in virtue of a taint communicated to the *clothes* that nurses and other persons in attendance on the sick acquire, sometimes, the power of communicating the disease to others, although not themselves infected.

Piedvache and some other writers have attempted to throw discredit on this mode of communication, but I have seen too many instances of it, to doubt its reality.

When I was in practice at North Tawton, I attended the wife of a butcher there, in a severe attack of typhoid fever.

There was no fever at North Tawton at that time, but a short time before this woman was attacked, her mother, who lived with her, had returned from a small lone cottage, six miles off, where she had been engaged for several weeks in nursing a whole family laid up with typhoid.

In December, 1867, two young ladies, living in country houses about two miles apart, having been invited to a ball, were measured for new dresses, on the same day, by the same dressmaker, who went for this purpose from one of these houses to the other, staying a considerable time at each. In the course of a fortnight both sickened for typhoid, and in both the attack proved to be very severe. They had not been away from home for several weeks before, and there was no fever in the immediate neighbourhood. But at the time when they were measured, the dressmaker, herself, had been nursing a child of her own, for several weeks, in a very bad attack of typhoid fever. For a fortnight or more this woman had passed a great part of every day with her sick child on her lap, and as there was severe diarrhœa, it is more than probable that her clothes had become more or less soiled with the specific excreta. As I was consulted in one of these cases, and heard from the medical man in attendance all the particulars of the illness of the dressmaker's child, I can, as far as these two are concerned, vouch for the facts.

I have brought forward these illustrations of the conveyance of typhoid fever by infected clothing, and I could add largely to their number, not only because this particular mode of infection has been allowed of late years to drop entirely out of sight, but because it at once strips from the contagion of this particular species of fever numerous fantastic notions, which, without a shred of

*mediate infection
at 2 miles*

scientific evidence to support them, have been suffered to gather round it.

The minute and impalpable agent which gave the fever in the cases just referred to, had had no commerce with drains, and was perfectly innocent of sewer gas. It had entirely escaped that mysterious concoction which is supposed to occur only in the drain, and which is held to be essential to the production of this particular type of contagion.

It had had no contact with the soil or with drinking water.

Cast off, in each case, by an already infected subject, and caught and retained by the woven fabrics in use about the sick, it gave the fever to the next sufferers, just in the same way in which the small-pox virus carried in the tainted clothes of a small-pox nurse has been known in numberless instances to give small-pox.

It is the act of infection stripped of all extraneous and adventitious conditions, and shown in its naked simplicity.

Between the reproduction of the fever poison in the intimate recesses of the already infected body, by the action of the fever process, and its effective implantation in the bodies of the next sufferers, a few fibres of cotton or of wool were, in many instances, all that intervened.

In the small-pox and in the fever the two facts are of exactly the same order. Much stress has, no doubt, been laid upon the inoculability of the one and the failure of all attempts, *hitherto made*, to inoculate the other. By some this has even been considered warrant sufficient to place these two self-propagating fevers in two distinct categories. As if the point of a lancet made any difference!

It would be just as rational to argue that two kinds of seed cannot both be of the nature of seed, because one

has to be planted by dibbling, while the other may be sown broadcast.

That the germinal unit, no matter how implanted, which is the offspring of a former crop, should be capable of producing another crop like it, that, and that alone, is the essence of the matter.

Of the efficacy of infected beds as agents for the propagation of this fever I shall speak in a future page. It will be sufficient here to observe that in the course of my experience I have seen many decisive examples of it.

In connection with this part of my subject I may state that two cases have come under my own observation which have made a great impression on my mind, as showing how unconsciously, in the ordinary course of life, we may become exposed to some of the sources of contagion here passed in review.

Many years ago I attended, in consultation with another practitioner, the landlord of a large hotel, who was suffering from a fatal attack of typhoid fever. During his illness he lay, at first, in a room looking into a street in which there was much noise and incessant traffic. Towards the latter end of the third week he became violently delirious, and as the noise appeared to excite him, he was moved into a back room. I had gone to the front room so often that on my visit the next day mechanically, so to speak, I went there again. What was my horror, when, on opening the door, I saw a strange gentleman there just beginning to dress. Arriving the night before, he had been placed in the very same bed which the fever-patient had vacated in the morning.

This frightful step had been taken more in ignorance than anything else. The hotel people had been assured that the fever was not in any way contagious, but the

result of bad drainage only, and so they supposed that they had exposed their guest to no sort of risk.

Some years later, I was consulted in a case in which a large proportion of the inmates of another hotel had been successively stricken down with typhoid. The fact had been kept strictly secret from the visitors, and no steps whatever had been taken to repair the drainage, which was in very bad order, or to disinfect the typhoid discharges.

On my expostulating on account of the possible consequences of such neglect in a house which strangers were constantly passing through, the answer made to me was, that to be using disinfectants, or to be pulling about the drains, would most probably betray the presence of the fever, and that to betray the presence of the fever would be to half ruin the business!

Truly, this is, in its very inmost soul, a commercial age.

Whether any of the persons exposed to infection, in these two cases, took the fever or not I cannot say; but supposing some of them to have done so, it is easy to see how perplexed they might have been to trace their illness to its source, and with what plausibility the fever thus arising might be set down to spontaneous origin.

MEDIA OF TRANSMISSION CONTINUED—AIR AND DRINKING-WATER.

§ 13. OF the part which tainted hands, linen, bedding, and wearing apparel, take in the propagation of this fever, sufficient was said in the last section.

The proportion of cases that originate in these sources is, no doubt, comparatively small, but unless they are kept in view, many incidents in the general history of this fever will remain unexplained.

But the propagation of typhoid fever on a large scale, and down through the ages, is effected through other media.

We have already seen that the bulk of the poison by which the succession is kept up, is cast off by the intestine in a liquid form. The first effect is, therefore, as I have before said, to infect the ground.

Obviously, there are two *principal* ways, and two only, in which a poison cast out upon the ground can find its way back again into the living organism. Either through the drinking-water, or by emanations borne upon the air.

The outbreaks of typhoid fever at Richmond Terrace, Clifton; at Cowbridge, in Wales — and many others equally striking might be added to these—show better than any general statement, what a potent means of propagation infected drinking-water may become.

In order to have a just estimate of the share it takes in the propagation of this fever, we must include the cases in which fever-tainted water is drunk as a diluent of milk.

How widespread and fatal the infection from this source may become is strikingly illustrated by the memorable outbreak of fever in Islington, related by Dr. Ballard some two or three years ago, and which that gentleman traced with admirable skill to the use of milk supplied from a dairy where typhoid fever was at the same time prevailing.

Two very important instances of the same kind have lately come under my own observation, and I have no doubt that this mode of infection is much more common than it is generally supposed to be.

The following very graphic narrative, which, under the head of 'A Milk Walk,' appeared in the 'Bristol Times' for June the 30th, 1855, will show how completely, in this

vital matter, the people who have the misfortune to live in towns are at the mercy of men of the very lowest class:—

‘ In one of those flat outlying districts of Bristol, where moist meadows, dry cinder-heaps, tall smoky chimneys, and the oozy bed of a flat low-lying river contend for the mastery in the landscape—I, a few mornings since, found myself. Unpromising as was the scenery, my walk nevertheless led me through a pleasant green field, on which a number of cows were grazing, and where some milkmen were already filling their cans. Along the edge of the meadow a stream ran, not the most pellucid in the world, and a short lane led thence out into the road, which was skirted by the dwellings of those whom I would best designate by the title of the humbler of the middle class. Having often heard the popular opinion as to the questionable quality of the article sold to whiten our morning Souchong, I indignantly exclaimed to myself, as I saw one milkman shoulder his cans and start on his journey, “Now, surely, that milk can’t but be genuine.” I was a little too soon, however. The owner of the cans went not quite straight to the short lane aforesaid, but deviated on his way to the unpellucid brook. Uncovering his two cans at its edge, he caused one thereof to hang over the water, and the other the land, and then gently stooped his respectable person, until one vessel rested on the earth, and the other received over its edge a bountiful supply of the running stream.

‘ When he had apparently satisfied himself with this process, he placed both his vessels on the earth, poured some of the contents of one into the other, then back again—in fact, he brewed the beverage.

‘ Shouldering his cans then, and passing up the short lane and out into the street, he cried out—

“MILK!”

This sketch is the more important because, when he wrote it, Mr. Leech, whose lively and facile pen the readers of the 'Bristol Times' will at once recognise, had clearly no sanitary after-thought in his mind. The only plea he offers for its insertion in his journal is that what it describes is actual fact.¹

The extreme gravity of the incident may be conceived when I add that in the course of little more than a mile above the point where this reckless, but, it is to be feared, too typical, milkman² replenished his cans, the unpellucid stream, which supplied the water, received a large proportion of the sewage of the village of Stapleton, and the entire sewage of the workhouses of Fishponds and of Stapleton—establishments counting, between them, some twelve hundred inmates, *and two large infirmaries!*³

Large, various, and terrible are the possibilities here.

In ordinary times the addition to milk of water even thus polluted might, it is true, lead to no very immediate or flagrant consequences to the public health; but when, as must often happen under such conditions, the pollution largely consists of the excreta of infectious diarrhoeal disease, the use of milk so watered must be full of danger to those who drink it.

¹ I have ascertained from the writer that this narrative is literally true.

² Even as I write, the following newspaper cutting is put into my hands:—

'According to the "Inverness Advertiser," an examination of all the milk-carts coming into that town has led to the discovery that only two out of a dozen on the east side of the river carried milk entirely unmixed.'

³ The sewage connection between these workhouses and the brook was severed some years ago.

As this story, from its perfect authenticity, may possibly cause some panic, it may be a satisfaction to know that, as far as the propagation of specific disease goes, all risk may be abolished by *boiling* the milk, however deeply it may be polluted.

Drinking unboiled milk is like eating raw meat, and is open to consequences of the same pathological order.

I have no doubt that, if all milk were boiled before being used, a marked diminution in the prevalence of more than one very serious type of disease would soon follow.

One point to be particularly noted in connection with the propagation of fever by milk diluted with infected water is that, in towns especially, the real source of the disease is generally quite unsuspected, and the events take a form that seems to baffle speculation.

Many cases of so-called spontaneous origin have, no doubt, sprung up in this way.

From the prominence which a few signal instances of it have given to the agency of drinking-water in propagating this form of fever, some writers have jumped to the conclusion that this medium is the only vehicle of the fever-poison.

This, as we shall see, is a great mistake.

It will be sufficient to cite one or two instances in proof.

Take, as an example, the outbreak at Chaffcombe (described in a former chapter). At the farm there, which suffered so severely that nearly every inmate was attacked, the drinking-water was beyond the reach of possible contamination.

In the third report of the Medical Officer of the Privy Council, an epidemic of typhoid fever is described¹ which occurred at Kingston Deverill, in Wilts, in 1859-60. At the date of the report, out of a population of about 400, 66 had already been attacked, and six deaths had occurred.

A like portion of attacks and deaths occurring in London, estimating its population in round numbers at 3,000,000, would give to typhoid fever alone nearly 500,000 attacks and more than 45,000 deaths. A truly enormous proportion.

Now, in this terrible outbreak, the drinking-water, which came from deep wells in the chalk, was not only

¹ The Privy Council account of this epidemic has been already given at p. 26.

bright and clear, but was reported by the Government officer 'to be beyond even the suspicion of foulness.'

The same relations were observed at Festiniog in Wales, in 1863. The exact number of cases of fever could not be ascertained, but among a population of six or seven thousand, in the affected districts, it was computed that there had been not fewer than six or seven hundred cases of fever. In the villages of Bethania, Tanygrision, and Glanypool, scarcely a house escaped, and in many five or six persons were attacked in a single house; and yet, with the exception of Bethania, the drinking-water for these villages was got from wells in the mountain, 'presumably situate beyond any possible source of pollution.'

But if it be suggested that, in these examples, the typhoid infection might, after all, have got into the water in some subtle and unsuspected way, the three yet to be related shut up the last opening to such a view.

Of these three, two—namely, an outbreak of typhoid fever in a convent at Arno's Court, near Bristol, and an epidemic which occurred some years ago in the parish of St. James, Bristol—have already been cited by Dr. Tyndall in his interesting and deeply suggestive paper on 'Dust and Disease.' The history of the outbreak at Arno's Court will be given, at length, in the next chapter. It will be sufficient for my present purpose to say that the establishment, which was the scene of it, was supplied by water from wells beyond the reach of sewage contamination, and to mention the following additional facts as proofs that this water had no hand in spreading the fever.

1st. The water was proved, by examination of the well and by accurate chemical analysis, to be entirely free from sewage.

2nd. The fever was confined to one division of the inmates. Another large division, drinking the same water

with the first, escaped; or, in other words, those who were decimated by fever drank the same water with those who had no fever at all.¹

3rd. From the very time when disinfection was brought to bear on the typhoid excreta, the fever began to cease, although the same water was drunk as before.

Lastly, since the outbreak nothing has been done to the well—the water remains what it was; but, with the exception of one or two imported cases, which have been dealt with by disinfectants, fever has not recurred in the establishment.

The evidence furnished by the Bristol epidemic is to the same effect. In the course of an hour, Dr. Pring, who, as Poor Law Medical Officer, had charge of the sick, and who, a few weeks later, died of typhoid fever contracted in his attendance on them, showed me more than eighty cases, within a small area, in the parish of St. James.

Now, Bristol is supplied with drinking-water which from its source in the country to the tap from which it is delivered under high pressure to the consumer, flows through conduits out of all reach of sewage contamination.

But, with the exception of a single household, all the fever patients under Dr. Pring's care were drinking this water—the very same water which, as far as fever is concerned, more than 150,000 of their fellow-citizens, outside the infected area, were drinking with absolute impunity.

The last example brings us back, once more, to the town of North Tawton, from which, among the incidents

¹ I exclude, as by right, the case of two inmates of the second division, who, although sleeping in their own part of the house, were employed all day in the fever-stricken part.

of a great epidemic which occurred there in 1839, our first illustrations of the contagious nature of typhoid fever were drawn.

About two years ago, after an interval of thirty years' almost entire immunity, this town was again visited by typhoid fever.

Meanwhile, a water company had been established, by which drinking-water brought from a considerable distance, in iron pipes, is delivered under high pressure to every inhabitant. Contamination of this water by human excreta is an absolute impossibility. And yet in this second outbreak of typhoid the population suffered even more severely than before. In the course of a few months, out of a population of 1,500 persons, 120 were known to have had typhoid fever, and eleven of their number died.

In conclusion, I will only observe that, if water be excluded, the air is, as I remarked at the outset, the only other possible vehicle by which a poison generated in the living body can find its way back to the interior of other living bodies, on a scale sufficiently large to cause the resulting disease to assume an epidemic form.

In the three terrible epidemics of Arno's Court, of St. James, Bristol, and of North Tawton, at least, the air was the great medium through which the infection passed.

But if so, it is further obvious that in these cases this was not the work of chance, or accident, but only represented *the effect of agencies which are always in operation where this fever is prevailing.*

The phenomenon is, in fact, merely the expression of a general law.

The contagious particles cast off, in the liquid excreta of contagious diseases, rise into the air by no power of their own, but in virtue of conditions which cause the germs of

the great tribe of Infusoria—organisms which, as their name bespeaks, breed in liquids—to rise in swarms into the atmosphere.

At some future time, I hope to show that the same law covers, by the same physical necessity, many other specific morbid products not hitherto classed among contagions at all. So wide, indeed, is its operation, that it includes not only a whole kingdom of nature, but the most populous of all, whose countless species are propagated down, through time and space, by this mode of aerial dissemination.

I have been the more anxious to bring forward these facts and considerations, not only on account of the great practical importance of the question to which they refer, but because the most erroneous doctrines have recently been put forward upon it, under the sanction of very high authority.

About three years ago, it was laid down by a very distinguished physician as an established principle, that it is impossible for typhoid fever to spread widely in any community provided with pure drinking-water. With the cases just related before us, I need scarcely say that this assertion is entirely at variance with fact.

In the same degree it is dangerous, as tending to divert attention from the one safeguard by which, alone, the spread of typhoid fever can with certainty be prevented—viz., the disinfection of the typhoid excreta immediately on their issue from the body.

Drinking-water, whether mixed or unmixed with other things, is a frequent and a very deadly vehicle of the typhoid poison. That its condition should always be looked to is of the first importance in fever outbreaks, and, where there is any suspicion of impurity, even, it should be provided against. But whoever trusts to the purity of this element alone, as a certain guarantee against the

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from H. A. Jones
water

spread of the fever, will often find, to his cost, that he has made himself responsible for grievous calamities.

The exact proportion which the cases caused by infected water bear to those caused by infected air is not easy to determine, and probably varies much under different conditions of soil, climate, season, water supply, social habits, and sanitary arrangements.

As far as my own experience goes, I can state that the worst and most wide-spread outbreaks which I have ever witnessed have occurred in communities where the drinking-water was absolutely blameless.

The one great aim to keep before us should be, by timely and effective disinfection, to prevent both air and water from becoming infected.

TYPHOID FEVER—AT WHAT PERIOD BEGINNING, AND AT WHAT PERIOD CEASING, TO BE CONTAGIOUS.

§ 14. ONE or two supplementary questions yet remain to be examined before we can obtain a complete view of the modes in which typhoid fever is disseminated.

At what precise period does the fever first become contagious, and when does it cease to be so?

To the first question a tolerably precise answer may be given.

Putting pathological probability aside, facts have repeatedly come before me which appear to show, by evidence free from all ambiguity, that the contagion begins with the diarrhœa. Or rather it would be more exact to say—for the evidence does not go beyond that—that, when diarrhœa is present, the disease is, certainly, already contagious. As this symptom often begins a considerable time before the patient takes to his bed, and while he is still up and about, it is easy to see what an important influence the

fact may have in spreading the disorder. Persons in this stage, going about from place to place, may obviously lay a train of wide-spread infection.

But it is in schools, workhouses, and other large establishments, where a common latrine often serves for a large number of persons, that this contingency tells most. Since I came to Bristol, I have seen three very considerable epidemics of typhoid, originating in the infection of the latrine of a particular day-school, by one of the scholars, who, having contracted fever and already affected with diarrhœa, still continued to attend the school for some time longer.

In my various writings on infection, I have repeatedly had occasion to draw attention to these relations in their bearing on the part which large establishments play in the spread of epidemics. This mode of spreading constitutes one of the greatest of all the difficulties in the way of prevention, and forms the ground of my proposal to make it compulsory to provide all such establishments with self-disinfecting latrines. Obviously, it is a difficulty that can only be completely met by what I have ventured, elsewhere, to call disinfection by anticipation.¹

The precise date at which the fever patient ceases to give fever to others is not so easy to define. But I have seen so many instances in which fever has broken out in a family living in a previously healthy neighbourhood, soon after the arrival of a convalescent, that I am quite sure that patients, so far recovered, cannot always be safely allowed to mix with others without precaution.

In the cases referred to, all traces of actual fever had disappeared, and diarrhœa had long ceased.

¹ See 'Cholera and Disinfection, or Asiatic Cholera in Bristol in 1866,' by W. Budd, M.D. Also 'Second Report of Royal Sanitary Commission,' vol. iii. pp. 46-7.

In what form the infection still lurked—whether in articles of wearing apparel that had become tainted in an earlier stage of the fever, or whether in the form of specific exuviae from which the diseased intestine may not have entirely cleared itself, I confess myself unable to say.

The facts suggest two important precautions. The first is, always to provide the convalescent with clothes perfectly free from specific taint; the other, to charge the cistern of the closet belonging to the house, in which he may be staying, with a powerful disinfectant.

For many years past, I have been in the habit of enforcing both, and, since I began to do so, I have never seen a convalescent give fever to a healthy person.

CHAPTER VI.

PREVENTION—DISINFECTANTS AND DISINFECTION.

‘That Man, who is rapidly subduing all the most Titanic forces of the universe to his commonest uses, should always remain at the mercy of these ignoble things, is an antithesis too extreme to be permanent. The subjection of the powers of nature to our will has always seemed to me to imply, as a strictly correlative achievement, the putting the plagues of nature under our feet.’

§ 15. IN the course of this work, I have already had occasion to speak of disinfection as the one great means whereby the spread of typhoid fever may be prevented.

What are the agents to employ for this object? There is considerable diversity, both of opinion and practice, in the profession, as to this point.

As far as disinfecting power goes, a considerable variety might be named, which are equally effective. Although, when dry, the typhoid poison seems to be very tenacious of life—if I may so speak—there is reason to think that, in the moist state, it is very easily killed.

Among the chemicals which I have, myself, used, with equal success, for preventing the spread of typhoid, the following may be enumerated:—Chloride of lime, chloride of zinc, chlorine water, sulphate of iron, carbolic acid, and the two last-named agents mixed. I have not tried chloralum; but if what has been reported of its power in disinfecting the clothes and bedding of small-pox patients may be trusted, it ought to be equally efficacious.

Perchloride of iron is another substance which I believe

to possess great disinfecting power, and which has not had the trial it deserves.

But chemicals are not the only agents by which contagion may be disarmed. It has been proved, by experiments conducted on a large scale, that a temperature of 212° —that, namely, of boiling water—entirely destroys the contagion of the small-pox, vaccine, and other poisons, even when in the dried state. Heat, therefore, is one of the most powerful of disinfectants.

Of these various agents, some are more convenient for one use, and some for another. For the disinfection of linen, chloride of lime in water is perhaps the most handy. Discredit has been of late years thrown on its disinfecting power, but entirely without reason. It was the only chemical used in the suppression of the Arno's Court outbreak, to be related in the next section, and I have used it with the same success in many other outbreaks almost equally formidable.

To make doubly sure, all tainted linen should be *boiled* as well as washed. Things not conveniently washable, such as mattresses, feather beds, pillows, or what not, should either be burnt or be raised to a temperature of 300° in a disinfecting oven. Of the various preventive measures this is the one which, in the houses of the poor, especially, is the most likely to be neglected. There is scarcely any measure which it is more important to execute. Mattresses, blankets, and feather beds—all things on which the sick lie—remain longer in close contact with sources of contagion than any other articles exposed to it. In the same proportion they are in risk of becoming more saturated with it. There is evidence to show that in typhoid fever the infective power thus acquired may continue effective for a long period of time.

On the other hand, these same articles, particularly in

the lower strata of society, are frequently changing hands. Nothing in life has astonished me more than the recklessness with which, even respectable people, commit their persons to secondhand bedding, often without any preliminary purification, or a single thought as to what dangerous taint may possibly have been deposited in it by former possessors.

After the termination of every case of this fever, to annihilate by one or other of the methods just mentioned every trace of the fever poison, is one of the foremost and most pressing duties of the medical attendant.

For disinfecting the discharges, chloride of lime, chloride of zinc, carbolic acid, and sulphate of iron, or the two last mixed, may be equally relied on. As I said before, chloralum is probably equally good. For the strength of the necessary solutions and the quantities in which they should be employed, the labels attached to the vessels or packets in which most of these agents are retailed may be generally trusted.

I have, however, given in the appendix—a code of rules for the prevention of typhoid fever which I drew up for general circulation a few years ago—some instructions as to these points sufficient for practical use.

In carrying out this system of prevention, two great principles should be kept in view. The first is to be lavish in the use of the chemicals rather than to run the terrible risk of failing by default; the second, that whatever be done, should be done in that thorough and conscientious way which alone befits acts that may issue in health or disease, in life or death, to indefinite numbers of men.

My own practice is to place in the sick room, or close at hand, a large can filled with the mixed solution of carbolic acid and sulphate of iron, whose composition is given in the Appendix; to keep the night-pan always

charged with it, and after each use of the pan to pour upon the discharge a quantity of the solution sufficient to *insure* the disinfection of what has been voided.

In addition to this, and in order to provide against possible lapses on the part of attendants, I have often thought it well to keep the water-closet cistern permanently charged with the disinfectant in use, so long as fever may last in the house.

Where the social conditions of the patient admit, certain other aids ought to be found in the sick chamber which materially help in the work of prevention.

Free ventilation by fire in an open chimney, and by open window, also, if the season allow, and, if need be—the removal of carpets and of all needless draperies—the shifting of the sick bed away from the walls of the room, so that air may freely circulate round it—and (to provide for the immediate ablution of any taint the hands of the nurse may contract) two wash-basins, one containing clean water, the other water and chloride of lime—a little scouring sand, and an array of clean towels—such are the arrangements which make up the ideal towards which the physician should always aim.

These arrangements are not only good in themselves, but good, also, as so many pledges, that the people concerned are alive to the deeply responsible nature of the business they have in hand.

Another point that should never be overlooked relates to the drinking-water.

In the infected home, itself, two cases may occur. The patient may have caught the fever from his well having become infected by some other fever-stricken member of the community in which he lives, or, having got the fever elsewhere, he may become the means of infecting the well, himself.

In yet other cases, the area of danger is greatly widened, as where, for instance, a large number of persons are supplied with drinking-water from a common pump, town pump or other. The outbreaks of typhoid fever at Richmond Terrace, Clifton, and at Cowbridge, in Wales, related in a former chapter, offer striking examples in point.

In a very fatal epidemic of fever which occurred at Guildford a few years since, the general infection appears to have been traced, mainly, to the percolation of typhoid excreta into the tank of a water company.

But it is in country villages, where the cesspool and the well are so often found in dangerous proximity, that this mode of wide-spread infection is peculiarly apt to occur. Some months ago, a lady living at the head of a large village in the West of England, being much annoyed by stench from her house drains, consulted on the subject a retail chemist, who advised her to drench the drains, daily, with carbolic acid. Soon after this, the same lady returned to the shop, and asked, with ineffable *naïveté*, if she could not be supplied with a disinfectant devoid of *smell*—her reason being, that her operations with carbolic acid had not been carried on many days, before the drinking-water in her own and some neighbouring wells stank so badly of the acid as to be quite undrinkable.

Relations of the same kind, for the most part previously unsuspected, have been more than once disclosed to myself by the same agent.

Some fastidious people object to the sanitary employment of carbolic acid, on account of its powerful and not too agreeable odour. But the possession of this odour is, in reality, one of the strongest recommendations of this valuable chemical; for it gives us, as we see here, the ready means of detecting, by scientific proof, a sanitary defect beyond all others the gravest—a defect which

to ac. &
smell

otherwise might often remain unsuspected, or beyond our power to trace by any equally ready method.

With experience of this kind before me, I have long thought it wise, where the most absolute reliance cannot be placed on the purity of the drinking-water, to enjoin all communities, in any degree infected with typhoid fever, to boil their water before drinking it. It is grievous to have to say that it is not superfluous to apply the same precaution to milk also.

So much for the cases—happily the greater number—in which the nature of the fever is recognised from the first.

But no preventive scheme would be complete which did not provide against those in which the fever remains, for a longer or shorter time, undetected. I have already shown how the occurrence in school-children of that type of fever, which the French call *la fièvre typhoïde ambulante*, may become the means of spreading the disease widely through a school, and thence through a large community. On these and other grounds, I suggested some years ago that all schools should have their latrines provided with self-disinfecting apparatus.

On still stronger grounds, the same provision should be enforced upon hospitals, in many of which, cases of this, and other self-propagating diseases, are often congregated.

Not very long since, I went to a large hospital to see the post-mortem examination of a man who had died in the third week of typhoid fever. The medical pupil who had charge of the operation, having removed the intestine from the abdominal cavity, tied the upper end to a water tap, and turned on the stream. Immediately there issued from the lower end of the gut, a flood of characteristic yellow matter, which at once passed, with whatever powers it may have had, intact, into the sewers

of the city in which the hospital was situated. If what has been said in this work of the properties of this yellow stuff be true, it is tragic to think of the possibilities that may have sprung out of this act.

In the evidence which I had the honour to give before the Royal Sanitary Commission in December 1869, I ventured to urge upon the Commission the great importance of providing by law for the permanent disinfection of the sewers of all hospitals.¹

In the course of the last session, while the Sanitary Act was passing through Parliament, I repeatedly endeavoured, by representations made in influential quarters, to get a clause to that effect inserted into it. Up to this time these representations have, however, remained unheeded.

To allow all the virulent things that find their way into a hospital drain free course through the sewers of a crowded community, is a practice which will be seen, ten years hence, to belong to the dark ages of sanitary science.

In concluding this section, I may add that, if the various

¹ I subjoin the paragraph in which this suggestion is reported:—‘Schools, public and private, constitute a special case where the adoption of this principle is urgently required, on account of the great prevalence of infectious diseases among the young. Hospitals are another, as being places where infectious diseases are being treated and congregated. I have been able myself to trace many severe outbreaks of self-propagating diseases to emanations from hospital sewers. I am of opinion that many evil things little suspected at present of such an origin have very probably a similar source, and I think that would be a very interesting subject for investigation. It is certain at any rate that many malignant forms of disease, which are but too well known within the hospital itself to have the terrible power of self-propagation, feed the sewers largely with very specific excreta, and I would desire to impress the Commission with my very strong conviction that the permanent disinfection of the sewers of all hospitals should be enforced by law. I have no doubt that a great improvement of health within the hospital itself would follow. All those recommendations have double force in hot climates, in the conditions attaching to campaigns, to emigrant ships, and so on. I am of opinion that if they were universally acted upon, their beneficial influence would soon be made apparent in a sensible reduction of mortality.’

precautions recommended in it were universally practised, my own experience fully authorises the expectation that we should see an immediate large abatement in the mortality from typhoid fever, and that at no very distant time the disease would become a pathological rarity.

Whether the neglect of them should be visited by penalties, as in the case of the neglect of vaccination, is another matter. My own feeling is that, for a time at least, it will be best if the option of adopting them be left *in foro conscientiaë*. This much, however, may be unhesitatingly claimed, that no man can, with a clear conscience, knowingly omit them, who has mastered the evidence on which they are founded.

One thing, at least, may be said of them—they can do no harm. There is no question here, as in vaccination, of ‘doing evil that good may come.’ For, even should these precautions fail of their main object, they will, at least, help to abate evils of which all men complain.

Of the permanent sanitary works, by which the spread of typhoid fever may be hindered, this is not the place to speak. The business of designing and executing these works belongs not to the physician, but to the engineer. Their chief object is to prevent the drinking-water, as well as the air of the inhabited area, from becoming contaminated by human excreta. As regards the water, we know how this may be effected, and the question is one of expense only. But as regards the air, great improvements must be made in our systems of drainage to enable us safely to dispense with the disinfecting measures detailed in the present chapter.

TYPHOID FEVER IMPORTED INTO A CONVENT—MORE THAN FIFTY INMATES ATTACKED—SUPPRESSION OF THE OUTBREAK BY DISINFECTANTS—OTHER EXAMPLES.

§ 16. ON February the 29th, 1864, I was summoned to the Convent of the Good Shepherd, at Arno's Court, near Bristol, to advise in a great emergency.

The place, which had for some years been the retreat of a Roman Catholic community, was originally the seat of a country gentleman, a friend of Horace Walpole. Built in the latter end of the last century, when the confectionery style of Gothic was in fashion, it was described in a characteristic letter from the fastidious owner of Strawberry Hill, as a model of elegance and taste.

As a dwelling-house it had some very sterling merits. The walls and floors were faithfully and solidly built, the interior was dry, the rooms were spacious and airy, and the aspect was good. Two deep wells, secure from any possibility of sewage contamination, furnished the house with an ample supply of drinking-water.

Soon after the property came into the possession of its present owners, the building was enlarged by the addition of a considerable block, which, joined to the old mansion, formed three sides of a quadrangle, of which a newly-built chapel made the fourth. At the same time, the entire drainage was remodelled, the old drains were abolished, and earthenware pipes laid in their place.

The structure when completed was divided into three principal segments. The community of Sisters, then 25 in number, lived in the old mansion; the next division was a reformatory for girls, of whom there were 126; the third was occupied by penitents, to the number of 34.

Except that each division was ministered to by the

Sisters and the resident chaplain, the three were completely isolated, one from the other.

In all important respects the reformatory was well fitted for its objects. The day-rooms and dormitories were spacious and fairly well ventilated, and the sanitary arrangements were more than ordinarily good. In the reports of the Government Inspector the establishment had more than once been certified in words of strong praise, as belonging to the first class.

For several years after its first opening the mortality was very low and the sickness slight. It is, however, worthy of note that after a time consumption became in a marked degree prevalent, and, at the same time, strumous ophthalmia became so rife, as to suggest to the Sisters the idea that it must be spreading by contagion.

In March, 1863, diarrhœa appeared in the reformatory, and, in the course of two months, more than fifty of the girls were under medical treatment for it.

But, unless the ailments here named belong to the self-propagating class, up to the date of the events about to be recorded no one of the *recognised* infections had ever attacked the inmates.

For some months past the mortality had been low, and the health of the whole establishment exceptionally good. There had, at various times, in the two or three preceding years, been some trouble with the drains, but this had seemed to have passed away, and there were no bad smells about.

In regard to the present history it is specially important to note *that at no time, whether the drains were at their best or their worst, had typhoid fever ever appeared within the walls.*

But the time had now come when this long-standing immunity was to be broken by a great tragedy.

The magnitude of this tragedy may be estimated by the fact that, before it had come to a close, fifty-six persons had passed through the protracted miseries of this fever, and as many as eight had died of it.

The origin of this terrible outbreak was to the last degree clear. The disease was introduced into the convent in the preceding November, by a young girl who was admitted to the reformatory, *while actually labouring under it.*

In the summer of that year, this girl had left the convent for service in a Roman Catholic family living in a large town some twenty miles off.

'In November,' I quote now from a written account by one of the nuns, 'we were informed by her mistress that she was ill of a fever then prevalent there. We were requested to see about her removal. On the 17th our chaplain went to the place, ascertained from her medical man that she was fit to be removed, and that he considered there was not the slightest risk to the school in her returning here to be nursed.'

The fever was, in fact, 'typhoid' fever, a fever known to be caused by bad drainage only, and notoriously not, in any way, catching. Such, in strict accordance with the well-known stereotyped traditions, was the language held.

It would, perhaps, be unjust to fix upon an individual the responsibilities attaching to a doctrine, which, in flat contradiction though it be to long recorded facts, is, in this country, still all but universally held.

But to me, at least, the spectacle of these poor helpless women, so many of whom were to be brought into untold suffering, and not a few even to death, by this assertion, is unspeakably touching. How much longer will these dangerous delusions continue to prevail?

Unhappily, the ladies of the convent were only too confiding. Blindly trusting to the assurance, endorsed as it was by their own medical man also, that the disease

was not contagious, the fever-stricken girl was admitted, 'the discharges were thrown down the infirmary cabinet, her clothes went with those of the other girls to the common wash-house, and were washed by the girls employed there. There was no idea of any precaution being necessary.'

I have already shown, at considerable length, why it happens that when (the drinking-water being pure) an outbreak of this fever is lit up in an isolated community by a single imported case, some considerable time necessarily elapses before the next crop begins to spring up. This was the case at Chaffcombe, at Loosebeare, at the farm near Cardiff, and indeed in all the cases of the same kind related in this volume. For the same reasons, the same rule applies, 'mutatis mutandis,' to all the other contagious fevers.

So it was not until January 4, as near as may be six weeks after the admission of the first fever case, that a girl, an inmate of the reformatory of some standing, after a week's drooping, was laid up with the same fever.

This girl, who was seventeen years old, had visited the fever-patient in the infirmary and was also employed in the wash-house.

To put all question aside as to the specific identity of the fever itself, I may state that in one of the fatal cases I made a *post-mortem* examination and found the typical ulcerations and other changes in the intestinal follicles which are characteristic of this species of fever.

But even without this evidence, the following extracts from a written account by one of the nuns of this girl's case, would have sufficed to settle the point:—

'About February 1, less well. Diarrhœa (which before had not been violent) increased. The tongue quite purple. On the fifth it was found that there had been considerable

hæmorrhage from the bowels for five or six days, which she had concealed. . . . Deafness came on about the middle of February. Diarrhœa ceased about March 5; at the same time phlegmasia dolens came on in the left leg.' Incidents, equally characteristic, occurred in all the other sufferers.

The dates of the succeeding cases were as follows. Two on January the 11th, one on the 13th, one on the 16th, one on the 19th, one on the 23rd (fatal on February the 8th), two on the 26th, one on the 28th (fatal on February the 11th), one on February the 5th (fatal on the 10th), one on the 13th, one on the 14th, one on the 22nd (fatal on March the 3rd), one on the 23rd, one on the 24th, three on the 25th (one of which fatal on March the 5th), *five* on the 26th, one on the 27th, *three* on the 28th, three on the 29th, *seven* on March the 1st, and on the 2nd *ten* at once.

I give these dates and numbers, thus literally, because they are characteristic.

Obviously, they represent not the sudden striking down of a multitude by a morbid cause common to them all, but the gradual growth of a contagion gathering strength as it goes.

It is the exact counterpart of what, in the other contagious fevers, under similar conditions, we see every day.

At the date of my arrival, on February the 29th, very nearly thirty young women were already in bed with the fever, and within forty-eight hours *twenty* more were added to the list.

By March the 2nd more than fifty of the inmates had been stricken, three had already died, and two more, including the chaplain, who fell a victim to his devotion to the sick, were lying at the point of death.

Such, in an establishment in which, hitherto, no single

case of typhoid fever had ever occurred, were the fruits of the doctrine that 'typhoid fever is the result of bad drainage only, and is not in any way contagious.'

It is not easy to describe the panic which prevailed at the convent. The great number of the sick, and the rapidity with which the fever was now spreading, were, in themselves, sufficiently appalling.

But there was one circumstance which added immeasurably to the anxiety. The nuns, themselves, had long become convinced that the fever was, in some way, spreading by contagion, and naturally looked to separation as the only means of staying it. But this was a reformatory, and reformatory is only another name for a prison. All the inmates were here under sentence, and could not be sent away until their term had expired. Up to this time a certain degree of isolation had been effected. Two wards had been set apart for the sick, but these were now entirely full. In the event of any further extension of the disease further separation was impossible.

More than fifty of the inmates, as we have seen, had already been attacked, seventy remained who up to this time had escaped. What was to prevent these seventy, or, at any rate, a large proportion of them, from sharing the fate of their fellow-prisoners? Such were the questions which were anxiously addressed to me on my arrival. What wonder that to the uninformed the dilemma should appear to be inexplicable.

Here was a great conflagration rapidly spreading, and a mass of combustible material close at hand which it was impossible to get out of the way. Conditions more unfavourable to prevention it is not easy to conceive.

One very important preventive measure—the pouring a disinfectant into the drains—had been adopted about a week before my arrival.

The further measures recommended for arresting the spread of the fever were the following:—

1st. Flooding again all the drains of the place with disinfectants, with a view to destroy, as far as possible, the poison already cast off.

2nd. The reception of all discharges from the sick, immediately on their issue from the body, into vessels charged with disinfectants also.

3rd. The instant immersion of all bed and body linen used by the sick into a disinfecting liquid, before its removal from the ward.

4th. Scrupulous ablution and disinfection of the hands of the nurses, whenever soiled by any offices rendered to the sick. And lastly, the burning or disinfection of all beds occupied by the sick, as soon as vacated by death, convalescence, or otherwise. All these things to be done, not in a loose or slip-slop way, but with the thoroughness and precision of scientific processes conducted in a laboratory. Under these conditions, and I am happy to say that they were fulfilled to the letter, I ventured to predict that the plague would be stayed. The prophecy was more than verified by the events. Twenty-six persons were indeed seized with the fever after the date of my first visit, but as twenty of these fell ill within the first forty-eight hours, they may at once be struck off the list. The average duration of the latent period in this fever, when, as in this case, the infection spreads through the air, being about fourteen days, these twenty persons must all have received their infection long before the preventive measures came into force. Of the remaining six, three more may probably be excluded on similar grounds. So that three only could, with any certainty, be counted as having become infected after the institution of the disinfecting plan.

Two great masses, one of infected and the other of uninfected, still remained, nevertheless, in close juxtaposition. For many weeks, yet, some forty or fifty fever-patients continued to discharge, as profusely as before, their poisonous excreta, but the chemicals placed in the vessel which received these excreta placed a barrier between the sick and the sound, which the contagion could not pass.

To complete the general history of this remarkable outbreak it is well to add that, with the exception of four cases, the fever was confined, from first to last, to the reformatory, into which it was originally imported. Of the four who, although not inmates of the reformatory, yet caught the fever, two were penitents who, alone of their class, were employed in the laundry; one was a nun who nursed the sick, and the fourth, the chaplain, who spent all his days in attendance on them. The two penitents, when they fell ill, were immediately placed in the sick ward with the other fever-patients, while in the case of the nun and of the chaplain disinfection was, from the date of my first visit, thoroughly carried out, so that the disease did not spread.¹

¹ Although the fact does not in any way qualify the conclusions suggested by the text, I think it right to state that after the fever had quite died out, the main drain of the reformatory was examined and found to be considerably out of order. On leaving the building this drain—a large earthenware pipe—traversed the court already described, receiving the surface drainage of the court through a well-trapped sink in the centre. It had been the practice to store large quantities of coal in this court out in the open, and whenever heavy rain came on small coal was freely washed into the drain. This coal mixing with the sludge of the sewage had formed a concrete, which was literally almost as hard as stone, and which completely blocked the pipe. On pursuing the examination further another defect was found, which in great degree neutralised the effect of the first. Exactly within the boundary wall of the building the drain-pipe had come to pieces in such way that the whole of the sewage of that part of the building discharged into a large excavation in the ground made when the foundations were dug. As there had never been any actual regurgitation either through the sink or further back, the breaking of the pipe must necessarily have occurred either before its occlusion further on or must have coincided with it.

It must not be supposed that the result here related, striking as it is, is in any way exceptional. My whole experience, on the contrary, extending now over a period of nearly thirty years, is in the most exact and absolute conformity with it.

In the month of November 1863 I was asked by my friend, Dr. Parsons, to come to Frome, in order to confer with him and some other medical men as to the steps to be taken to repress an outbreak of typhoid fever which had grown to formidable dimensions there, and had proved very fatal. The history of this outbreak was in one important respect the counterpart of that of Arno's Court.

The disease had been brought to Frome by a female pauper, who, having had typhoid at Bridgewater, was sent, while scarcely convalescent, to Frome, which was her proper parish. The journey brought on a relapse, and she lay for some weeks at a lodging in a small court there in a very dangerous state. Among other things it may be noted that the diarrhœa in her case was especially profuse.

A few weeks after her arrival, several persons living in

The stoppage itself was probably a very gradual thing; partial, probably, at first, possibly intermittent also, in obedience to the varying degree of pressure from behind, through rainfall or otherwise. Such a state of things would explain perfectly the recurrent difficulties which occurred in the drainage on that side of the house. In the same way the fracture of the pipe would explain the cessation from these difficulties already spoken of in the text. It is of course, in a case like this, impossible to give an exact estimate as to time, but the builder who conducted the investigation, and who was a man of great experience, felt confident, judging from the hardness of the concrete on the one hand, and the amount of sewage accumulated under the basement of the house on the other, that the state of things he found must have existed for a twelvemonth at least.

Its general effect may be stated with scientific precision by saying that it had converted the drainage of the reformatory from drainage by sewer, into drainage by cesspool. But concede the principle of cesspool and nothing could be better than this. The traps were all in good order, and the cavity into which the sewage discharged was large, and was enclosed on all sides by solid masonry.

That the conditions here described might have helped in giving effect to the contagion is possible, although by no means sure. That they were the actual cause of the outbreak is a supposition too preposterous to be worth considering.

the court and in the adjoining street were seized with typhoid fever, and the disease, spreading from that point, gradually infected the whole of the immediate neighbourhood. At the date of my visit, some forty cases, or more, had already occurred. The measures taken to prevent the further spread of the disorder were identical with those which were put in force at Arno's Court. The only other precaution taken, was to close one or two wells, which from their proximity to drains or privies, were in danger of being tainted.

As the measures were identical, the result was identical also. A few persons were attacked afterwards, but from the date of their illness it was clear that they had received their infection before the institution of the disinfecting method.

It may be interesting to add that the disinfectant used was a solution of chloride of zinc, of which, fortunately, there happened to be a manufactory near at hand. This liquid, diluted with water in the proportion of one in forty, was poured several times a day into every public and private drain in the infected neighbourhood.

Altogether, nine hogsheads of the diluted solution were used in these operations.

Of typhoid fever in schools my experience is strikingly confirmatory of these results. I could name many large schools, in which, in former years, outbreaks originating, each, in a single case, had spread through the school, causing several deaths, and well-nigh accomplishing the ruin of the proprietors. In more than one of these, fever having again occurred in the person of one of the pupils, led to my being consulted as to whether any means short of breaking up the school could be taken to prevent a repetition of the same fatal consequences. The opinion I gave was that, if isolation of the patient and disinfec-

tion were thoroughly carried out, things might be safely left as they were. It was, no doubt, a great responsibility to assume. But I had ascertained in each case, that the drinking-water was beyond all risk of contamination, I felt that I could rely on the people, and to make assurance doubly sure, and to forestall any danger from possible negligences or lapses, I had the cisterns, which supplied the water-closets, permanently charged with powerful disinfectants.

The event, in each case, entirely justified my confidence. In neither of these schools did the fever extend beyond the first case.

But why multiply instances, when the result is unvarying and can be stated in a single sentence?

In no single case that has ever fallen into my hands has this method failed to prevent the extension of this fever when applied from the first, or the further extension of it, when it has already been allowed to spread.¹ I have come, therefore, to look upon the method as infallible.

With these facts the cycle of evidence becomes complete. In all attempts to determine the relations of agents that are invisible, science is, rightly, exacting of proof. That a theory fits the phenomena is not, alone, enough. When the theory in question is of a nature to admit of being put to a practical test, we must show, also, that it will work. In the case before us we have satisfied both requirements.

The theory is, that the fever is spread by the discharges : in practice it is found that disinfecting the discharges always prevents the fever from spreading.

¹ In one, only, case (referred to in the Appendix), a servant girl, who nursed a lady in a severe attack of typhoid, herself took the fever. But in that case the disinfection was not complete.

CHAPTER VII.

THE PYTHOGENIC THEORY OF TYPHOID FEVER IRRECONCILABLE WITH THE PROLONGED ABSENCE OF THIS FEVER FREQUENTLY IN PLACES WHERE PUTRESCENT EFFLUVIA ARE HABITUALLY RIFE. ALSO WITH THE LOW RATE OF PREVALENCE OF THIS FEVER ON THE BANKS OF THE THAMES IN THE SUMMER OF 1858, WHEN THAT RIVER STANK SO BADLY.

'Non da effluvj putredinosi, non da vicissitudini atmosferiche, anco le più stravaganti e terribili è da ripetersi adunque la causa delle malattie contagiose.'
BRERA.

§ 17. It must be well known to most readers that the theory of typhoid fever propounded in the foregoing pages, whatever may be thought of it by scientific men, is not the theory which is generally accepted.

That this is a disease, which, like small-pox and the other contagions of the same order, is maintained solely by self-propagation, is a proposition which, as yet at least, is widely remote from the popular belief.

By the public at large, at any rate, organic matters, and especially sewage in a state of decomposition, without any relation to antecedent fever, is still generally supposed to be its most fertile source.

So widely, indeed, has this view been adopted, that an attempt has been made, and very nearly successfully made, to embody it in the very language of science.

'Pythogenic' fever, or, in other words, a fever born of

putrescence, is the epithet by which it has been sought to engraft this doctrine in the name of the fever, itself.

In the foregoing chapters I have brought forward evidence to show that while this doctrine is irreconcilable with what is most characteristic of the manner in which this fever is propagated, the facts on which it is founded may be readily and far more satisfactorily explained in another way.

It is the purpose of the present chapter to further show that this doctrine is untenable even on its own grounds.

In studying the march of scientific discovery, there are few lessons more strongly impressed upon us than this: that a theory which merely embodies an inference drawn from a single order of facts is, for that reason, almost sure to be wrong, however clear and inevitable the inference, itself, may at first sight seem to be.

Philosophy teaches us why this should be so; experience illustrates the fact by a long list of signal examples.

There are, no doubt, cases, and that of contagion may be named as one of them, in which the relations between events really are, in general terms at least, what from the first they appear. But this can only happen when the fact on which we chance to light is one of a high order.

From the infinite complexity of things, it necessarily follows that such cases are few in comparison with those in which the direct contrary holds. It is not often that nature 'wears her heart on her sleeve,' or delivers up her secret at the first summons. Quite as often it seems to be her mood to mislead by deceiving shows, as if in counterpart of the principle by which man is said to have invented language for the concealment of thought.

It should be especially borne in mind in connection with the question about to be discussed, that *primâ facie* evidence, which to the common eye seems irresistible,

offers no guarantee that the conclusion on whose behalf it is cited, may not be utterly false. Illustrations of this must be familiar to all who are acquainted with the history of science. The conviction held so undoubtingly, for so many ages, that the earth is immoveable; that it is the centre of the universe, and that the sun and stars revolve round it, and were made for its use, is a very striking one. Sir J. Herschel has made the pertinent remark that, in astronomy, nearly all the facts are, in reality, exactly the opposite to what they at first appear *with such certainty* to be. The notion held as undoubtingly, and for a still longer time, that mildew and kindred organisms are the actual offspring of damp and decay, is another case in point. Founded, like these discarded figments, on an induction from which the most capital facts were excluded, the theory that typhoid fever is actually caused by the chemical compounds which result from the decomposition of organic matters, is evidently destined to share in their fate.

On the other hand, it is characteristic of theories which, like these, are the expression of simple and overt relations, to obtain a strong hold on the popular mind. Suggested in the first instance by an obvious fitness, which, although superficial, is not the less striking, they soon gain general acceptance, and pass without challenge. From the nature of the case, a large number of phenomena fit the formula, and the convenient class of 'anomalies' and 'exceptions' is there to receive the rest. When a theory of this sort has once become widely current, no rival theory (unless it be within the circle of the exact sciences) has, for a time, at least, any chance of a fair hearing.

Acclinis falsis, animus meliora recusat.

The Copernican theory of the heavenly bodies had a

hard battle to fight before it finally drove the Ptolemaic view from the field ; and the ancient doctrine would, no doubt, have survived still longer, had it not been that the new one, from being founded on mathematical proof, was of a nature to compel belief. Even to this day, the Chinese, who, whatever else they may be, are a cultivated and intellectual people, treat as an insult to the understanding the notion that the earth is a moving body—a notion which, in their eyes, is in such flagrant contradiction with the plainest evidence of the senses.

As to the question before us—that of the origin and mode of spreading of typhoid fever—there can be no doubt as to which side the weight of evidence belongs.

The whole of the evidence recorded in the preceding pages concurs in showing that the power to propagate this fever which sewers and other *cloacæ* undoubtedly possess, is due, not to common impurities, but to their frequent impregnation, in the way there described, with the actual fever-poison.

To see that this is the true interpretation of the facts, all that is necessary is to contrast the experience of large towns in regard to this fever with that of country places. In large towns, typhoid fever is being continually traced to sewer-emanations : because, from the constant presence of the disease in large communities, the sewers provided for them are as constantly being infected with the fever-virus. But, in villages and other places of small population, where, especially if remote from large towns, the disease is only an occasional visitor, the emanations to which it is commonly ascribed may continue in full force for years, together, without a single case arising.¹

¹ The considerations advanced in this and one or two subsequent pages have already, in part, been brought before the reader in preceding chapters. But the facts in which they are founded are so important in their bearing on

I have already stated in a former chapter, that before the great epidemic broke out which visited North Tawton so severely in 1839, there had been for fifteen years not more than one case of fever in the village.

Instances of the same prolonged absence of fever, in rural districts, in the presence of its reputed causes, might be multiplied to any extent, if there were need.¹

But, that the occurrence of these long intervals of rural exemption is not to be traced to any want or feebleness in country places of the fever-producing power is further plain from this, that when the disease does break out in them, the ratio of persons attacked is incomparably greater than is ever seen in the city under the like circumstances.

Speaking of the fever which swept the parish of Great Horwood in 1857-58, Prof. Acland says:—‘An endemic affecting a similar proportion of the population of London would attack, in nine months, more than a quarter of a million of persons, of all ages; above fifty thousand would die of the fever alone, while the deaths from other causes during the period would not diminish.’ (*Fever in Agricultural Districts*, by H. W. Acland, M.D., F.R.S., Regius Professor of Medicine in the University of Oxford.)

The North Tawton epidemic would lead to a similar calculation; while the two Caumont epidemics described by Gendron de l’Eure, and great numbers of the village outbreaks, recorded in the admirable ‘*Rapports des Epidémies*’ of the Memoirs of the French Academy, would

the origin and mode of spreading of typhoid fever, that I make no apology for reproducing them in somewhat fuller development.

¹ The very large proportion in which, wherever the reckoning has hitherto been made, persons new from the country figure among the fever-patients of large cities, may be explained by reference to the same considerations. Louis has pointed out with his wonted clearness, that this peculiarity is mainly, if not wholly, due to the circumstance *that in the city the disease is always present, but in the country only now and then.* (*Recherches sur la Fièvre Typhoïde*, 1^{re} édit. tome ii. p. 735.)

carry a proportionate metropolitan mortality to a still more enormous aggregate.

It will be seen, from the bare statement of them, that figures like these represent an amount of havoc, in proportion to population, to which no urban outbreaks offer the most distant approach. And, as if to remove the last doubt as to what is the inference to be drawn from this contrast, this immensely greater havoc occurs, *precisely because the village is worse off than the city in regard to those very conditions out of which, according to the commonly received theory, typhoid fever is supposed to spring.*

I have already shown that the rapid and wide dissemination of the pest at North Tawton—and the remark applies with the same strictness to all similar village outbreaks—was caused by the almost entire absence of sewerage; *i.e.*, of means for preventing the discharges from the human intestine from polluting the soil and air of the inhabited area.

Nothing can well be more simple than the interpretation of these relations. Village life, not only in England, but in Europe generally, is constantly presenting to us the following conditions:—

1. Sewerage entirely wanting, or very defective; and the resulting pythogenic compounds rife in proportion.
2. Typhoid fever wholly absent for long periods of time, notwithstanding.
3. Great virulence and inordinate spread of the fever when imported, by reason of, and in proportion to, the defective sewerage.

The long continued and entire immunity from the disease amid the impurities to which it is commonly ascribed, especially when contrasted with its intense virulence when once introduced, shows, with a force of evidence which in medical problems is rare indeed, that these impu-

rities have no power of themselves to cause fever, but only when, in the course of events, they happen to become charged with its specific poison.

One of the most striking testimonies to the truth of this theory consists, indeed, in the *perfect explanation* it gives of two facts *absolutely inexplicable* on any other grounds. I speak of the incomparably greater ravages which, as we have just seen, typhoid fever commits among rural communities when it attacks them, and its vastly greater apparent contagiousness in country than in town. Both these facts had been remarked over and over again by writers on the subject, but all had confessed their inability to explain them.

In referring to the evidence of the contagious nature of typhoid fever already recorded before the date of his own memoir on it, M. Piedvache says:—‘All these observations (by Gendron de l’Eure, Leuret, Bretonneau, Ruefz, Putegnat, Patry, Jacquez, Ragaine) have been drawn from the country, or from towns of small population. The medical men of great cities, on the contrary, have scarcely observed any but facts negative of contagion. M. Gaultier de Claubry, who is an avowed partisan of contagion, could only muster, from his own practice, eight examples of it, for his paper on the subject, read to the Royal Academy of Medicine. In the discussion that followed, as well as in the discussions which took place on the occasion of the reading of the other memoirs I have cited, the members of the Academy admitted, some to have seen one, and others, two, three, or, at most, four cases, in which they could recognise the operation of contagion; others denied it altogether, on the ground that they had never seen an instance of it!’ (*Mémoires de l’Acad. de Méd.*, 1850, p. 246.)

In another passage of the same memoir, M. Piedvache

adds:—‘The observations which I have recorded as proofs of contagion were nearly all collected in the country. The third, fourth, fifth, sixth, and thirty-seventh, were the only exceptions; and in none of these did that extension of the disease to a whole family occur, of which I have related several examples, and which I have so often witnessed.’

‘These facts,’ he continues, ‘which are in accordance with all hitherto published, lead to the conclusion that typhoid fever, while almost invariably contagious in country places, is only rarely so, and by exception, so to speak, in cities. It is a general fact, which it is impossible to deny; but it is, if I may so express myself, a crude fact (*un fait brüt*), whose explanation is yet to seek.’ (*Ibid.*, p. 308.)

To the common view, so great and so strange a contrast must seem in the highest degree perplexing. For, in town and country, as I have before observed, the fever itself is one and the same. It is not, in reality, less contagious in one place than in another. To multiply and throw off a specific poison constitutes, in town and country alike, its very essence. No one has yet suggested, in explanation of the seeming anomaly, that this pest has human sympathies, and is more merciful to the townsman than to the rustic. And few, I imagine, would be disposed to follow the cautious Louis, who, in the act of admitting this fever to be contagious, thought it necessary to add—not by way of humour, but in the sober sense of the words—‘at least, in the departments’ (*‘au moins dans les départements’*).

The fact that this fever spreads almost wholly by the discharges from the intestines gives a perfect solution of the enigma.

In the city, in a large proportion of cases, these discharges are no sooner cast off than they are swept out

of harm's way, or are otherwise dealt with in a manner that prevents their taking effect on the group who watch the sick bed.

In country places, they accumulate day by day *on the open soil* which surrounds the patient's dwelling, until they envelope the whole household, and often the neighbours also, in a fever-miasm, *which is incomparably more virulent than the atmosphere of the sick chamber itself.*

In a paper which appeared in the 'Lancet' on the 6th December, 1856, on the Fever at the School of the Clergy Orphan Asylum,¹ I pointed out the working of these conditions in terms to which I have nothing to add.

In the paper referred to, after observing that the case is, happily, one in which the natural instincts suggest what science prescribes, and that the rich, in using disinfectants to get rid of bad smells, often accomplish a higher purpose, it is added :—'Amongst the poor such refinements are never thought of; and when this fever breaks out in a poor family, the discharges from the bowels are thrown, without preparation, either into the common privy, or, as I have seen a hundred times in rural districts, are cast upon the dungheap, or into the open gutter. From this point, following the line of watershed, this pestilent stuff often makes its way to considerable distances, where, appearing now under the guise of an endemic miasm, which entirely masks its true origin, it may carry disease and death into many an unsuspecting household.'

The following extract from Mr. Simon's 'Report on the Public Health in England' in 1860, will show how true to fact these statements were.

In an account of an epidemic which ravaged the village of Dronfield, in Derbyshire, he says :—'Among details reported by the inspector was one which, notwithstanding

¹ See Appendix.

its disgustingness, deserves particular notice; he was informed that, recently, below one house where typhoid fever was present, a long track of blood, descending the hill-side, marked the tract where the diarrhœal excrements had been thrown, and let run *past two rows of houses into the turnpike road.*' (p. 12.)

The need of some radical modification in the view commonly taken of the relation which subsists between typhoid fever and sewage was placed in a very striking light by the state of the public health in London, during the hot months of 1858 and 1859, when the Thames stank so badly.

The late Dr. McWilliam pointed out at the time, in fitting and emphatic terms, the utter inconsistency of the facts with the received notions on the subject. Never before had Nature laid down the data for the solution of a problem of this kind in terms so large, or wrought them out to so decisive an issue. As the lesson then taught us seems to be already well-nigh forgotten, I may, perhaps, be allowed to recal some of its more salient points.

The occasion, indeed, as already hinted, was no common one. An extreme case, a gigantic scale in the phenomena, and perfect accuracy in the registration of the results—three of the best of all the guarantees against fallacy—were all combined to make the induction sure. For the first time in the history of man, the sewage of nearly three millions of people had been brought to seethe and ferment under a burning sun, in one vast open *cloaca* lying in their midst.

The result we all know. Stench so foul, we may well believe, had never before ascended to pollute this lower air. Never before, at least, had a stink risen to the height of an historic event. Even ancient fable failed to

of Thames
1858

furnish figures adequate to convey a conception of its thrice Augean foulness. For many weeks, the atmosphere of Parliamentary Committee-rooms was only rendered barely tolerable by the suspension before every window, of blinds saturated with chloride of lime, and by the lavish use of this and other disinfectants. More than once, in spite of similar precautions, the law-courts were suddenly broken up by an insupportable invasion of the noxious vapour. The river steamers lost their accustomed traffic, and travellers, pressed for time, often made a circuit of many miles rather than cross one of the city bridges.¹

For months together, the topic almost monopolised the public prints. Day after day, week after week, the 'Times' teemed with letters, filled with complaint, prophetic of calamity, or suggesting remedies. Here and there, a more than commonly passionate appeal showed how intensely the evil was felt by those who were condemned to dwell on the Stygian banks. At home and abroad, the state of the chief river was felt to be a national reproach. 'India is in revolt, and the Thames stinks,' were the two great facts coupled together by a distinguished foreign writer, to mark the climax of a national humiliation. But more significant still of the magnitude of the nuisance was the fact that five millions of money were cheerfully voted by a heavily taxed community to provide the means for its abatement. With the popular views as to the connection between epidemic disease and putrescent gases, this state of things naturally gave rise to the worst forebodings.

Members of Parliament and noble lords, dabblers in sanitary science, vied with professional sanitarians in

¹ Several instances of this came to my own knowledge.

predicting pestilence. If London should happily be spared the cholera, decimation by fever was, at least, a certainty. The occurrence of a case of malignant cholera in the person of a Thames waterman early in the summer, was more than once cited to give point to these warnings, and as foreshadowing what was to come. Meanwhile, the hot weather passed away; the returns of sickness and mortality were made up, and, strange to relate, the result showed, not only a death-rate below the average, but, *as the leading peculiarity of the season*, a remarkable diminution in the prevalence of fever, diarrhœa, and the other forms of disease commonly ascribed to putrid emanations.

After describing in scientific and forcible terms the unprecedented state of the river, Dr. Letheby adds:— ‘With all this condition of the Thames, however, the health of the metropolis has been remarkably good. In the corresponding period of last year (*i.e.*, of the year 1857), the cases of fever, diarrhœa, and dysentery, attended in the city by the medical officers of the unions, amounted to 293 of the former and 181 of the latter; but during the past quarter (*i.e.*, the quarter of intolerable stench), they were only 202 of the former, and 93 of the latter!’¹

So that, while pythogenic compounds were poisoning the air with what may be called a forty thousand fever-power, the so-called pythogenic fever, so far from rising in proportion, fell much below its average.

The testimony of Dr. McWilliam, as medical supervisor of the waterguard and waterside custom-house officers, is still more to the point. The former, to the number of

¹ Not that there is any reason to suppose that the typhoid poison is offensive to the nose or even cognisable by it. But as according to the pythogenic theory this poison is the offspring of putrescence, while stink is the surest of all indices of it, the magnitude of the stink may be taken as a measure of the magnitude of the scale on which pythogenic products are being generated.

more than 800, 'may be said to *live on the river, or in the docks, in ships, or in open boats*; and the latter, numbering upwards of 500, are employed during the day *in the docks, or at the various wharves of the bonded warehouses on each side of the river.*' After stating that the amount of general sickness among these men was below the average of the three preceding years, and considerably below that of the year 1857, he adds, 'that as regards the types of those forms of disease (including diarrhœa, choleraic diarrhœa, dysentery, etc.) which, in this country, noxious exhalations are commonly supposed to originate, we find the additions during the four hot months of the year from this class of complaints 26·3 below the average of the corresponding period of the three previous years, and 73 per cent. less than those of 1857. In another passage this distinguished physician says:—'It is nowhere sustained by evidence that the stench from the river and docks, however noisome, was in any way productive of disease. On the contrary, there was less disease of that form to which foul emanations are supposed to give rise, than usual.'

Before these inexorable figures the illusions of half a century vanish in a moment.

To the argument against the pythogenic theory of typhoid fever, founded on the prolonged absence of the disease in places where pythogenic compounds are rife, there is only one possible objection that yet remains to be disposed of. The specific poisons, it may be said, by which the exclusively contagious diseases—such as small-pox, for instance—propagate, by the very nature of the case, never die out from among us. But, although always somewhere extant, it is only now and then, when season and other conditions conspire, that they display their full power over men. To hold, because their action is often dormant, that it does not exist, would be simply absurd.

In the same way, it might be argued that, if fever is often absent where the agencies to which fever is commonly attributed are rife, it is not because these agencies have not the power ascribed to them, but simply because the general conditions are wanting to give this power effect. It is scarcely necessary to remark that the analogy on which this objection is founded is imperfect in all the points essential to give it force. But, supposing it were otherwise, and admitting the argument to have a *primâ facie* claim to be considered, the circumstances which characterise the spread of this fever show at once that it does not meet the case. It requires but small acquaintance with the history of fever-outbreaks to know that the difficulty cannot be thus explained. Nothing can prove this more conclusively than the fact which may be witnessed every day in some part of the kingdom, that of two places in the same neighbourhood, situated exactly alike as to sanitary and other conditions, one may be the seat of a virulent epidemic of this fever, while the other remains perfectly free. In respect of defective sewerage, the place from which the disease is absent may be (as I have often seen) the worse of the two.

The admirable papers by Gendron de l'Eure on the epidemics of small localities; the writings of the illustrious Bretonneau; the annual reports of the French Academy of Medicine on the epidemics of France; and, lastly, not to add further to this list, the essay of M. Piedvache, are full of the most striking cases in point.

Examples of the same kind have occurred over and over again in my own practice. The epidemic at North Tawton, from which I have already drawn so largely, was a very complete one. I have already referred to that epidemic to show that the prolonged immunity from typhoid fever, which the village had previously enjoyed, was

fatal to the hypothesis that common sewage emanations had any hand in the scourge which at length fell so heavily upon it. The violation of all reason involved in this hypothesis was rendered still more apparent by taking a wider survey. For, at the very time when typhoid fever was so rife in North Tawton, all the other principal villages in the neighbourhood remained entirely free from it. And yet, not only were many of them far worse off than North Tawton, in respect of drainage, but there was one, the village of Exbourn, four miles away, that had long been a by-word in the country for its flagrant neglect of sanitary precautions. Pools of foul and stagnant water, dung-heaps, open cesspools, and every other conceivable nuisance, abounded in the place. If the like impurities were the cause of the fever which was raging at North Tawton, how did they happen to have no such effect here, where they were so much more rife? It could not be that the season was not favourable; neither could it be that there was no food for the disease. For when the same fever was introduced into Exbourn, fifteen months later, it spread there, also, with great virulence, and, as if to complete the demonstration, when it had already died away in its former seat.

This is a history which admits of but one interpretation. For if upon the hypothesis that typhoid fever is caused by common malaria, irrespectively of contagion, facts like these are perfectly inexplicable, few, I presume, can have failed to perceive that, as incidents occurring in the erratic progress of a contagious disorder, they are entirely, and in every point, consistent with our other experience.

The history of which the leading features have just been rapidly sketched is, in fact, emphatically, the history of a disorder of this kind. *Mutatis mutandis*, it is the history of small-pox; it is the history of measles; it is the

history of scarlet fever; it is the history of malignant cholera.¹ In all these specific contagions we meet with these same alternations of slumber and activity; of widespread prevalence in one place, while other places hard by remain free; and finally, with the same successive invasion of neighbouring places, in such wise, that the reigning disorder—be it small-pox, measles, scarlet fever, typhoid fever, or malignant cholera—often only begins to prevail in the new locality when it has already died out in the old.

It is, in fact, in a general survey of this kind that we get the clearest view of the thread which really connects all these events. Running through all these circumstances, there is plainly but one thing constant; namely, a specific morbid cause—a cause which is neither a permanent product of the soil, or air, or of particular seasons, but which is susceptible of transmission from place to place; which breeds as it goes, and then again dies out or becomes dormant, without leaving any sign to mark its track.

There is only one thing of which these can be the characteristics; and that is, the specific poison bred of the disease and by which the disease propagates, and which, in common with the other specific poisons perpetuated by the same law, possesses all these properties.

¹ The greater and more rapid diffusiveness of the contagious exanthemata constitutes a difference, not of essence, but of form merely. The law by which the poison is reproduced is the same in both cases. The exanthemata spread more widely and more rapidly, partly because (there is reason to believe) the poisons by which they are propagated are more prolific than the typhoid, and partly because, from being cast off mainly by the outward surface of the body, they are sown broadcast from the first, and not held, as the typhoid poison is, in a certain degree to the ground.

THE PYTHOGENIC THEORY OF TYPHOID FEVER, FURTHER IRRECONCILABLE WITH THE INTENSELY SPECIFIC CONDITIONS WHICH ATTACH TO THE REPRODUCTION OF THE FEVER POISON IN THE LIVING BODY. THE ENORMOUS RATE OF THIS REPRODUCTION ADEQUATE TO ALL THE PHENOMENA.

§ 18. IN a leading article which appeared in the 'Medical Times and Gazette' for February the 9th, 1861, under the heading 'Abandonment of the Doctrine of the Specificity of Contagious Diseases,' the writer states that, one by one, authors and observers are declaring their belief that these diseases 'may originate *de novo* out of the dirt and neglect which surround us.' After quoting the opinions of many eminent authorities, in proof of his assertion, he adds that the paper lately read to the Epidemiological Society by Dr. Milroy¹ applies the same idea still more widely, and 'shows a tendency in the professional mind to adopt new and what may be called liberal views.'

All the vague and untenable notions which have of late years gathered round this subject, more particularly in relation to the propagation of typhoid fever and some other epidemic disorders, may be traced either to ignorance of their contagious properties, or to want of insight into what contagion involves. 'If once a disease of this kind,' says Sir T. Watson, in his pleasant and masterly discussion of this subject, 'is decidedly proved to be the effect of contagion, we cannot help entertaining a doubt whether the disorder in question really ever has any other cause.' The more we reflect on this observation, the more just it will appear. In a former chapter I have already shown that, in typhoid fever, dissemination by contagion implies precisely what it implies in small-pox; and

¹ On the Influence of Contagion in the Diffusion of Epidemic Disorders.

that it is provided for, in the same way, by the multiplication of a specific poison in the most intimate recesses of the living body, by that most remarkable of processes which constitutes the phenomena of a contagious fever.

Even on the most superficial view, it is difficult to suppose that an agent which is evolved by such a process as this can be the offspring of any other. Still more difficult is it to suppose that an agent so evolved can be the product of merely external conditions. To see in all its force how intensely specific a thing the propagation of these poisons in the human body is, we must bear in mind that they are not one, but many. Multiplying in the same remarkable medium—bred, therefore, of the same organic materials, and, what is still more, by the same peculiar law—yet each of these several poisons sets up a series of changes which always issues in the reproduction of its own specific kind—small-pox in small-pox, scarlet fever in scarlet fever, and so on of the rest. What small-pox and measles were in the Arab in the days of Rhazes, they still are in the Londoner, in our own. What they are in the Londoner, they are in the wild Indian of the North American prairie, and in the Negro of the Gold Coast. To all the other contagious fevers, as far as our records go, the same remark applies. In races the most diverse, under climates the most various, age after age, through endless generations of men, these diseases pass down through the human body, perpetuating their own kind, and each maintaining its separate identity by marks as specific as those which distinguish the asp from the adder, or the hemlock from the poppy. To say the least, it is difficult to conceive that agents of whose propagation this is the history can be generated in any other way.

An additional argument to the same effect may be drawn from the fact that many of these fevers are

peculiar to man. This seems to be especially the case with the fever before us.

If the poison from which typhoid fever springs were capable of being bred elsewhere than in the human body, it would surely be in the bodies of animals which are made of flesh and blood like ourselves, and from whose substance we draw sustenance for our own. And yet it appears to be almost certain that this is not the case. In the most virulent outbreaks of typhoid fever, there is no evidence that even the domestic animals which gather round the fever-stricken dwelling ever take the disease. At Chaffcombe, while nearly all the *human* inmates of the infected homestead were laid low by the poison, the dogs and cats which belonged to the house, and the poultry, pigs, horses, and cattle which thronged the yard, continued to enjoy perfect health. Yet the pond from which the latter drank was being continually polluted by a drain which received the whole bulk of the intestinal discharges from the fever-patients.¹ I have observed the same striking contrast in other instances without number ; and I may add, that the whole course of observation bears out the conclusion to which this contrast so distinctly points.²

¹ Dr. Murchison fed a pig for three months with the discharges from intestinal fever, and found that the health of the animal in no wise suffered from this strange addition to its diet. The inference which the fact might, at first sight, seem to suggest—that these discharges are not infectious—is, I need scarcely say, quite unwarranted by it. The true inference to draw from it is, that the typhoid fever of man is not thus communicable to the pig. Although this animal is subject to an intestinal fever of its own, there is reason to believe that it is quite insusceptible of human typhoid.

² If anything were wanting to add force to this, it would be the fact that animals, on their part, are infested by a whole brood of contagious poisons, specific in kind, each separate from the other, and all (with one or two exceptions) incapable, apparently, of multiplying in the *human* body. It seems to be quite established, for instance, that cattle are subject to a variety of malignant and contagious fevers—many of them of typhoid type—from which man is altogether exempt.

But if the fever-poison cannot find the elements of its growth even in the animals which are the most closely connected with man—in the material conditions, that is to say, which of all others most nearly resemble those of its known mode of development—what likelihood is there of its being the offspring of conditions so radically different from these as those which are presented by common dead matter? If it cannot breed in the bodies of the cat, the dog, the pig, the horse, or the cow, how can it be conceived to breed in every ditch or dungheap?

The force of this argument is immeasurably added to when we further consider that, even in the body of man himself, the reproduction of the fever-poison requires conditions of its own.

One of the most cardinal points in the history of the remarkable family of agents of which this poison is one, is, that predisposition in the subject is essential to the development of their specific power. Deadly as they are where this predisposition exists, where it is wanting they are of none effect. A given dose of carbonic acid or of sulphuretted hydrogen is fatal to all men alike; but of fifty men who receive an equal dose of the poison of small-pox, or typhus, or typhoid fever, as the case may be, some are poisoned unto death, while others remain unhurt. This is a difference, not of degree, but of essence.

What may be the *intimate* nature of the conditions essential to give effect to the fever-poison, or of those which defeat its operation, we are as yet in no position to define. How subtle they may be we may learn from that most mysterious of facts already once referred to in these pages, which consists in the immunity conferred by one attack against a repetition of the same fever.

In small-pox, this immunity has been proved by

experiment on an enormous scale.¹ In the other contagious fevers, although not open to experimental proof, an immunity of the same nature has been deduced from observation, which may be the more readily trusted because the singularity in question (however unable we may be to penetrate the causes on which it depends) is evidently connected in some essential way with the peculiar law by which the whole family propagate.

Like many others already mentioned, this too is, plainly enough, a family characteristic. But what a marvellous characteristic it is! Here are two subjects who in their power to resist all common external agents are, as far as we can discern, precisely alike. Living on the same food, breathing the same air, nearly of an age, following the same occupation, alike in feature; the children, it may be, of the same parents; and equal, as far as can be judged, in nervous and other power: and yet, in the one a few moments' exposure to the specific poison is often sufficient to cause death, while in the other a far greater exposure may be incurred for indefinite periods of time without the slightest ill effect.

To explain so great an enigma, chemistry can as yet demonstrate nothing, and physiology can only vaguely speculate.² Neither in the flesh or the blood of the two men; neither in their form or feature, or any other attribute, is any trace to be discovered, by present methods, of a difference so momentous. But, whatever

¹ The whole practice of vaccination is based upon an immunity—less in degree, but the same in kind—acquired by the inoculation of vaccine.

² Some ingenious suggestions on the subject may be found in Liebig's 'Agricultural Chemistry,' under the head of Fermentation; and Mr. Paget has made some very interesting reflections upon it in his 'Lectures on Surgical Pathology,' as illustrating the incomparable precision of the assimilative act. Sir H. Holland has also treated the matter generally in an admirable and most suggestive essay, 'On Diseases which occur only once in Life.' But although these eminent men have no doubt hit upon important elements of the problem, their speculations must not be mistaken for actual solution of it.

may be the nature of the conditions on which this difference depends, they are all summed up in the fact that the man who suffers has never had the fever before, while he who escapes has already gone through it.

Over and over again, I have seen instances of persons who have remained perfectly well where every one else has suffered, and whose immunity could only be reasonably explained by the protection conferred by an attack of the same fever at some former period. The nurses of fever hospitals, who have once had the disease, remain with few exceptions, for the rest of their lives entirely proof against it, although constantly exposed by their calling to greater risk of infection than ever occurs in any other condition of life.

But what is most striking of all is, that the attack which confers the future protection may occur in childhood, and the protection, nevertheless, be life-long. The child once marked with small-pox is proof against it as long as he lives, however early the date of the first attack; and in this, as we have seen, small-pox is but a type of the rest.¹

The living substance, meanwhile, is in continual flux, and the body itself, to borrow the fine expression of Coleridge, is 'but the common phantom of a thousand successors.'

The case is even stronger than this. The figure of the poet, bold as it may seem, falls short of the truth. For it were vain now to trace in the finished man of forty or fifty either the form or the lineaments of the child who, in long past years, once had the fever. And yet, through

¹ When MM. Louis and Trousseau were sent by the French Government to report upon the great epidemic of yellow fever at Gibraltar, they observed a great number of cases of striking exemption in persons who were infants at the breast when the disease prevailed epidemically there before; and who, to use the expression of the Gibraltar people, had sucked the fever with their mother's milk. (See Trousseau's *Clinique Médicale*.—Lecture on Contagion).

all these changes of stature, form, and substance, the man of forty or fifty still inherits, as against this particular poison, the immunity which was purchased by the child in that one attack. Once dipped in the Styx, our Achilles comes forth invulnerable.

In the presence of relations such as these, it would seem as if science could only verify and record, without attempting to explain. So manifold, so subtle, so incomparably specific, are the conditions on which, even in the living body itself, the reproduction of the fever poison depends. If anything more were needed, in addition to all this, to show how specific a thing the reproduction of these poisons in the human body really is, and on what intensely specific conditions it depends, it would be found in the fact that the immunity acquired by one attack of these fevers holds good only as against its own particular poison, and is of no avail against the rest. Measles renders the body proof against measles, but leaves it as open to small-pox as before; and so on of the rest.

To conclude, on the evidence usually assigned for such a belief, that a poison, of whose growth this is the history, is bred in every cesspool or ditch in which there may chance to be a heap of seething rottenness, is precisely on a par with the philosophy which led the ancients to believe that mushrooms are bred of cow-dung, alligators of the mud of the Nile, and that bees, as Virgil sang, may be engendered in the entrails of a putrid ox. And signs are not wanting to show that the time is not far distant when the belief in question will take its place in that limbo of discarded fallacies to which these other superstitions have long since been consigned.

While, therefore, the great fact remains that sewers are the principal channels through which this fever is propa-

gated, the proof from all sides is overwhelming that they are so not because of their being receptacles of decomposing organic matter, but solely to their being the depositaries of the specific discharges of persons already infected.

But if this be so; if the sewer only transmits the fever because it is tainted with the intestinal discharges of fever patients, there is, as I have observed before, no need of further argument to show that these discharges are incomparably more virulent than aught else that proceeds from the sick.

So completely, indeed, in the public mind, is this fever identified with sewer-poisoning, and so insignificant, in comparison, has seemed the part which mere personal intercourse has taken in its propagation, that the sewer has come to be looked upon as the actual and primary source of the disease, while the infected man has been altogether lost sight of. To such a degree has the prejudice thus arising operated, that not the vulgar only, but (until lately) even scientific men have refused to listen to the decisive evidence by which the disease is proved to be essentially contagious, and overlooking, thus, the bond which unites the two elements, have failed to discern the great truth that the sewer only owes its fatal influence to its connection with man; that it is but the channel for the distribution of a poison which it has no power to generate, and only acts in the work of dissemination by opening a wider sphere to the contagious principle.

On the other hand, there is reason to believe that *so long as it remains in the liquid state* the virus which issues from the intestines of fever patients is very easily destructible. Much of it is, no doubt, extinguished soon after its discharge from the body in process of natural change, or by the external agents with which it comes in contact. Much, again, is rendered ineffective by dilu-

tion, or is rapidly swept beyond the inhabited area by floods, or by effective drainage, and much too is dissipated or destroyed in other ways. From these several causes, it is certain that by far the greater part of what is cast forth takes no effect.

This is but the expression of a general law. Like all other types in nature that propagate by germs cast loose upon the world, a myriad perish for one that fructifies.

The ova developed by a single tapeworm would be enough, if they all came to their full estate, to people with tapeworm every extant human intestine. The spores of some dozen mushrooms would well nigh suffice to crop the planet with that fungus.

Where the springing up of heirs depends so much on chance, and may be defeated by so many contingencies, it is only by such lavish reproduction that the great law of continuous succession can be maintained.

Autumn scatters its thousand seeds. Scarce one of the thousand
Bringeth forth fruit; and the rest back to the element turn.
Yet though one only expandeth its germ, that one only peoples
Living creation with forms that through eternity last.¹

What is true of organic life is, as I have elsewhere shown, true to the letter of these organic poisons also.

¹ Tausend Keime zerstreuet der Herbst, doch bringet kaum einer
Früchte; zum Element kehren die meisten zurück.
Aber entfaltet sich auch nur einer, einer allein streut
Eine lebendige Welt ewiger Bildungen aus.

SCHILLER.

CHAPTER VIII.

SPONTANEOUS ORIGIN.

‘Il vajuolo pare che talvolte sorprenda degl’ individui che non ebbero comunicazione co’ vajuolosi. Eppure, egli è deciso, che il vajuolo, sono undici secoli, era affatto sconosciuto in Europa, e che è rimasto sconosciuto in alcune isole del Mar Pacifico, fino a tanto che non vi penetrarono gli Europei.’—BREERA.

§ 19. THE position taken in the foregoing pages, broadly stated, amounts to this:—that typhoid fever is not only self-propagating, but that it originates in no other way.

For if it can be shown that even the products of the decomposition of organic matters, for which so strong a *primâ facie* case can be made out, have, in reality, no power to cause this disease, there is no single plea for supposing that such a power resides in any other condition.

In reply to this, it has often been objected to me that it is impossible in such a problem to prove a negative. But, on the other hand, this alternative is clearly not required. It will be time enough to examine the claims of any other alleged cause when scientific grounds can be produced for advancing them.

In regard to this point, I may further observe that, if this fever really have an origin independent of contagion, facts giving *positive* and express indication of such origin, and incapable of explanation on any other supposition, are sure to make themselves known. Meanwhile, the question admits of being put to a very clear issue. With the

exception of what relates to defective sewerage—a case already disposed of—the only plea for believing in the spontaneous origin of this fever is founded on evidence of a purely *negative* character, and consists, solely, in our inability to trace, with the eye, the continuity of a chain whose links are known to be invisible. To conclude from this that no chain exists is palpably absurd. When subjected to a careful analysis, the whole of this evidence resolves itself into the fact that instances of typhoid fever often spring up in which it is impossible to trace the disease back to a personal source of specific infection. But to this one would have supposed that it would have been sufficient to answer, that this impossibility lies in the very nature of the case. For, as the fever-poison is invisible to the naked eye, at least—its very existence being known to us by inference only—and as, from this and other causes too obvious to need special mention, provision is made for its dissemination in a thousand invisible ways, it necessarily follows, not only that the precise source of infection in any given case must often escape us, but that cases must *constantly* arise which, to the uninformed, bear all the semblance of spontaneous origin.

To distort facts which can thus be shown to be the necessary result of a *known mode of continuous propagation*, into an argument for a *de novo* origin, is to carry perverseness in reasoning as far as it can well go.

The question might fairly be left to rest on these grounds. The want of logic involved in the popular mode of solving it may, however, be placed in a still stronger light.

If we are required to believe that typhoid fever originates spontaneously, simply because in many instances we cannot trace the disease back to an immediate

prototype, on the very same, or on still stronger grounds, we are bound to believe in the spontaneous origin of small-pox and the other self-propagating fevers.

In small-pox, indeed, not only do countless cases occur which cannot be traced to contagion, but the disease not rarely springs up under conditions where the idea of such a source would seem at first sight to be altogether shut out. When small-pox, some years ago, prevailed extensively in Bristol, it attacked simultaneously three persons who had been for a long while in-patients in three different wards of the Bristol Royal Infirmary. Of these three persons, one had been confined to his bed for more than a month with a fractured leg, another had for some time been altogether disabled, and the third had not left the Infirmary for many weeks. What made this example the more telling was that, although there was a good deal of small-pox in Bristol, there was none at the time in the immediate neighbourhood of the house.

Small-pox entirely disappeared from Boston in New England, seven different times; but in only three of the seven, in spite of the greatest pains taken in the investigation, could the channel of its reintroduction be discovered.¹

Some years ago, an intimate friend of my own, living in a country house, was seized with small-pox, although he had not been away from home, and although there was, as far as could be ascertained, no other case within forty miles of the place. About thirty years since, an exactly similar instance fell under my own observation in the person of a young boy, in the village of

¹ See for this and other similar instances, an admirable Report on Contagion by Dr. Henry, of Manchester, in one of the early volumes of the 'Transactions of the Provincial Medical and Surgical Association.'

North Tawton, who had never in his life been out of the village. In that instance there was certainly no other case of small-pox within a very wide radius of it.

The course of general observation is in entire conformity with these facts.

To those who have not attended to the subject it may appear strange, but it is nevertheless strictly true, that during a period—thirty years—I have scarcely ever met with an instance in private practice in which the patient first affected could trace the disorder to its source.¹ The late Dr. Gregory stated, in his article on Small-pox, in the *Cyclopædia of Practical Medicine*, that of the numerous cases received into the Small-pox Hospital, 'not one in twenty is capable of being referred to any known source of infection, the disease being ascribed by the patient to cold, fatigue, change of air, or some other innocent circumstance.'

Judging from my own experience of the two diseases, I can scarcely doubt that, if an accurate register of both were kept in town and country, it would show ten cases of small-pox to one of typhoid fever in which the origin of the disease could not be traced.

From these facts one of two things is clear: either small-pox, as well as typhoid fever, is constantly springing up anew, or the facts which are commonly supposed, in either case, to lead to that conclusion offer, in reality, not the slightest warrant for it. There appears to be no possible escape from this dilemma. I need scarcely add that the former alternative will be rejected at once by all who have well considered the subject.

Sir T. Watson has well remarked that the history of

¹ In times of epidemic, and among the lower classes, the spread of the disease in many cases can, of course, be followed.

small-pox is decisive against the notion of its spontaneous origin. His precise words are these:—‘I say the *history* of small-pox leads to the settled belief that this disorder, of which few persons are not readily susceptible, never occurs except from contagion. *It was quite unknown in Europe till the beginning of the eighth century.* No mention of any such malady is to be found in the Greek or Roman authors of antiquity. Now, whatever may have been the deficiencies of the ancient physicians, they were excellent observers and capital describers of disease; and it is impossible that a disease so diffusive, and marked by characters so definite and conspicuous, should have escaped their notice, or have been obscurely portrayed (if known) in their writings. On the other hand, Mr. Moore, in his learned and interesting *History of Small-pox*, has shown that it prevailed in China and Hindostan from a very early period, even more than a thousand years before the time of our Saviour.’¹

If anything were wanting to show what is the true inference to be drawn from these events, it would be found in the fact that, *once imported into the West*, ‘whenever or wheresoever the disorder came, it spread with the most fearful rapidity and havoc.’ ‘What I wish you to remark,’ says the distinguished writer I have just quoted, ‘is this: that while almost all men are prone to take the disorder, large portions of the world have remained for centuries exempt from it, until at length it

¹ ‘That it did not sooner extend westward into Persia,’ Sir T. Watson continues, ‘and thence into Greece, may be attributed partly to the horror which the disease everywhere inspired, and the attempts that were consequently made to check its progress, by prohibiting all communication with the sick; partly, to the limited intercourse which then took place among the eastern nations, but, principally, to the peculiar situation of the regions through which the infection was diffused; separated, as they were, from the rest of the world by immense deserts, and by the ocean.’—Watson’s *Lectures on the Practice of Physic*, 3rd edit. vol. ii. p. 709.

was imported; and that then it infallibly diffused and established itself in those parts.'

That the conditions for the manufacture of the small-pox poison existed here in the most intense degree, was shown by the event when the disease itself was once introduced. The long lapse of ages during which we remained entirely free from it showed with equal clearness that until this introduction occurred, all those conditions were powerless to cause a single case.

The spectacle witnessed in Europe was repeated over again in the western world in a still more striking way. Our knowledge of the events in this instance is precise and sure. 'There was no small-pox in the New World before its discovery by Columbus in 1492. In 1517 the disease was imported into St. Domingo. Three years later, in one of the Spanish expeditions from Cuba to Mexico, a negro covered with the pustules of small-pox was landed on the Mexican coast. *From him* the disease spread with such desolation that within a very short time, according to Robertson, three millions and a half of people were destroyed by it in that kingdom alone.

'Small-pox was introduced into Iceland in 1707, when sixteen thousand persons were carried off by its ravages—more than a fourth part of the whole population of the island. It reached Greenland still later, appearing there for the first time in 1733, and spreading so fatally as almost to depopulate the island.'

Clearly we have here the history of a specific poison which no common conditions of human life, however various the climate, the people, the civilisation, or however defective the sanitary state, have the power to generate, but for whose propagation the actual virus of a pre-existing case is the one necessary and all-sufficient condition. Under this view, everything is explained in

the most easy and natural manner. Adopt the opposite view, and we are driven to the most preposterous assumptions in order to sustain it. This may be made evident by applying the spontaneous theory of small-pox for a moment to our own case.

When small-pox was first imported into England, it spread, as everywhere else, with the most fearful rapidity. Then, as now, we may be sure—(but no doubt to a far greater extent than now, for Jenner had not yet thrown over us the ægis of vaccination)—in addition to those who could trace their disease to direct contagion, vast numbers were attacked who had held no communication with the sick. Then, as now, we may be equally sure, cases were frequently springing up under conditions of the most complete apparent isolation.

If the idea of spontaneous origin be necessary to explain such cases now, it was equally necessary then. So that the assumption we have to make is, that concurrently with the importation of small-pox into England, *but quite independently of that event*, the conditions of English life, which, for all time before, had been powerless to develop a single case, had suddenly acquired the power to breed the disease in the most prolific manner. I need scarcely add that the bare statement of a conclusion such as this is the best exposure of its extravagant absurdity.

Our difficulties, insuperable as they already are, would not indeed end here. For, even if all this were granted, we should further have to explain how it is that the English people, having once acquired so perfectly the art of brewing this specific poison, should by migrating to Australia and New Zealand, *have again altogether lost it*. Happily, as yet this particular scourge has not once made its appearance in either of these two colonies.

I may add that the philosopher probably does not exist who would not readily hazard the prediction that, if by neglect or mischance small-pox should be imported into these communities, they would at once appear to have recovered the power of breeding it; and observers with more zeal than knowledge would there, as here, again point to isolated examples of it as proofs of its spontaneous origin.

The facts as applied to typhoid fever stand, therefore, thus:—Cases of this fever, like cases of small-pox, are constantly arising which cannot be traced to contagion. There is the strongest conceivable evidence, notwithstanding, that whatever its first origin, small-pox does not spring up *de novo* now. The occurrence of such cases is, therefore, no warrant for supposing that either of these diseases does so spring up.

Scarlet fever and measles teach the same lesson. In all countries in which these two diseases are established, cases are continually occurring in which, as in small-pox, it is impossible to trace a linear descent. But the geographical distribution of both, past and present, is fatal to the idea of spontaneous origin in either, and decisive in favour of their sole propagation by the law of continuous succession. To confine ourselves to a single illustration. The disease, measles, was quite unknown in Tahiti until about ten years ago. But, once imported, it spread like wildfire through the island, causing an amount of havoc among the natives, for which no parallel is to be found in European outbreaks of the same disorder. Even without going farther we might safely apply the conclusions drawn from the history of these three self-propagating fevers, small-pox, scarlet fever, and measles, to typhoid fever also. The four are, in the strictest sense of the words, members of the same natural family, and precisely as we may apply

what we know of the propagation of one sort of mushroom to that of another sort, so we may apply what we know of one of these specific self-propagating diseases to the rest. But direct evidence is not wanting which, although less precise and absolute than in the case of the other three, is yet sufficient to warrant the belief that in its distribution over the earth, this fever, also, has followed the same law as they.

There is reason to think that its specific contagion continued, down to a comparatively recent period, quite unknown, in many parts of the world, where it is now rife.

Like small-pox and the other eruptive fevers, this febrile type appears not to have existed in Tahiti, until after the discovery and colonisation of that island by the European. Now, it is reported, the population is being rapidly swept off by two principal scourges, typhoid fever and phthisis; the latter, also, as some believe, imported from Europe.

It has been credibly reported to me that typhoid fever was quite unknown in Queensland, until it was brought there from England, in the 'Flying Cloud,' a fever-stricken emigrant ship. Since that date it is said to have become established in the colony. For obvious reasons I am unable to verify the statement, but if the circumstances really were what they have been represented to me to have been, they would be well worth inquiring into in relation to the question before us. Some record of them is, no doubt, still to be found in Brisbane.

I have read, somewhere, but am unable to lay my hand on my authority now, that when America was first settled; it was a long time before typhoid fever appeared among the colonists, and that a still longer time elapsed before it reached the great Mississippi valley, where, at present,

it is very prevalent. Other illustrations of the same kind might be added to these, if there were need.

Taken as a whole it is impossible to conceive of anything more decisive than these facts. For—whatever may be thought of the case of typhoid—it will be observed that, in regard to more than one other of this great family of specific diseases, the evidence is as massive as it is precise.

There is no opening, here, for the thousand and one fallacies, which in experimental inquiries, for instance, are so apt to slip in undetected, through carelessness or want of skill in the operator. There is no opportunity for the distorting influence of theoretic bias. The testimony is too colossal to be warped. The elements of the argument are, in fact, Cosmic in their magnitude, and the conclusion to which they all unite in bearing witness, is, that, in whatever way these self-propagating types may have first come into being, they are never known to arise spontaneously now, but are perpetuated by the great law of continuous succession.

As if this were not enough, two other contagious fevers have lately been brought, in a memorable way, to the knowledge of the British people, under conditions that have added one link more to the chain of proof. I speak of Sheep's Small-pox and the Siberian Cattle Plague.

In these two diseases the evidence derived from their history and geographical distribution offers an exact parallel to that which we have just been considering. But here the inference drawn from it has been clenched by the crowning fact, that by destroying the diseased animals and the poison they have cast off, the plague itself is extinguished. The teaching value of the evidence thus furnished is greatly enhanced by the circumstance, that from its intimate relation to the food of the people, the

subject has been brought home to the knowledge of the humblest amongst us.

Once more, as I write these lines, our cattle are threatened by the great Siberian Plague, and so well known, now, is the right and only course to take for its suppression, that no one has any doubt that, by destroying the infection already laid, and slaying every stricken beast, this great scourge will be rooted out of England with the same certainty with which the breed of thistles would be exterminated, could we destroy every thistle seed and root up every thistle plant.¹

But if the arguments set forth in these pages be sound, we are authorised by the severest induction in anticipating a similar result from our action against typhoid fever also.

We cannot, indeed, in this case, slay the infected, but the fact that the poison which carries on the infection is cast off in a form that places it entirely within our power, enables us to bar the succession by which the disease is kept up.

There is no need of rooting up the thistle; destruction of the thistle seed is enough.

There still remains one objection to these views, on which a word must be said before I conclude. As all contagious diseases must, in the *first* instance, have occurred independently of contagion, it is often argued that there is every reason to believe they still do so.

This is the last refuge of the partisan of spontaneous origin.

M. Trousseau, who, although an avowed contagionist, is an equally firm believer in 'spontaneity,' has put the case very clearly. After observing that the spontaneous

¹ This passage was written in 1872, at the date of the last importation of the Cattle Plague into England, the Plague being speedily extinguished by the measures indicated in the text.

origin of contagious disorders is not only a fact that cannot be contested, but is an important element in the development even of the most contagious among them, he adds:—

‘In fact, as contagion implies necessarily the presence of two persons, the one giving and the other receiving the morbid germ, it is too obvious that in the *first* person attacked by a contagious malady the disease must have developed itself spontaneously, and was formed out of its prime elements, under the influence of causes which are completely unknown to us.’¹

Following up this view, this distinguished teacher suggests that many cases in which diseases of this kind spring up in isolated places can only be explained by supposing that the same thing still happens.

Small-pox, that is to say, having originated *de novo* once, must originate *de novo* now. For that, if it is to have any force at all, is what the argument amounts to.

But it is obvious, from what has gone before, that, if this argument applies to the specific contagions, no reason whatever can be shown why it should not also apply to animals and plants—not microscopic merely, but even to those of considerable size.

Itch, being a contagious disease, two persons are always necessarily implicated in its propagation. But there must have been a time when a *first* person had the itch without receiving it from another. The itch insect having originated *de novo* once, must therefore, according to M.

¹ The exact words are:—‘La spontanéité est donc un fait incontestable dans le développement des maladies même les plus contagieuses. Et en effet la contagion impliquant nécessairement la présence de deux individus, l’un donnant l’autre recevant le germe morbifique, c’est une vérité par trop banale de dire que chez le premier qui fut atteint d’une maladie contagieuse, la maladie se développa spontanément; qu’elle se forma de toutes pièces sous l’influence de causes qui nous sont complètement inconnues.’—*Clinique médicale*, tome 1er, article *Contagion*.

Trousseau, originate *de novo* now. The two facts stand on exactly the same level.

But, the truth is, that the first origin of the germs of specific contagious diseases and of the great procession of organic types which have successively appeared on the earth is alike unknown to us. By the light of science, we know no more of the first evolution of the germ of small-pox or syphilis, than we do of the acarus which inhabits the human skin—of the tapeworm which was appointed to live in the human intestine—of the cystic worm which finds a temporary home in the brain of the young sheep—of the mistletoe which fastens on the oak—or of the different species of mildew which breed in the grain of wheat.¹ We know no more, by the light of science, of the first origin of *these* living types, than we do of the higher organisms on whose substance they feed.

The precise mode in which *all* these things first came into being is hidden from us by an impenetrable veil.

On the other hand, everything tends to show that, once created, they all propagate only in one way. That small-pox is only maintained by self-propagation, there are, as we have seen, the strongest conceivable reasons for believing.

The history of syphilis tells the same story. Who among us can say when, or how, or by what process, this terrible virus first came into being? Who can name the thrice unhappy man, on whose person, innocent of any unclean contact, it *first* implanted its foul venom? We all know how it propagates now, and that inoculation by the actual contact of two persons is essential to its continuance.

I have always thought it a very significant fact, that,

¹ As a piece of natural history, the whole subject of parasites is a very curious one; and its study, if carried out, would probably lead to some very unexpected inferences. The topic is one which should commend itself, one would fancy, in a peculiar way to the fertile and ingenious mind of Mr. Darwin.

in this case, where the process of transplantation can only be effected by an act of which two persons at least must be cognisant, the idea of spontaneous origin has never once arisen.

Let an isolated case of typhoid fever occur, and, merely because he cannot readily trace the disorder to its source, many a medical man will assert, without the slightest hesitation or doubt, that the disease has, clearly, originated spontaneously. Press him, and he will be ready, at once, with the argument that there is no reason why the conditions which *first* gave rise to it, may not again give rise to it now.

But let a young sinner with the peccant mark upon him swear ever so stoutly that the disease he bears has come on spontaneously; let him argue as plausibly and as long as he will against the incredulity which refuses to see that the conditions which *first* gave rise to this disease, may again give rise to it now, and the very same medical man will laugh him to scorn.

In small-pox and syphilis, where, from the nature of the facts, the evidence bearing on this question is precise and sure, the answer is unequivocal.

The history of these two diseases is at hand to show—as by a great standing experiment—that these specific contagions, so far from being things that spring up at every turn, have, in reality, come into being under conditions hidden in primeval darkness, and of which we have no experience.

The history of many other contagious maladies is precisely to the same effect. The very significant fact, already mentioned, of the exclusive appearance of many among them, in the first instance, in particular geographical centres, whence, sometimes only after the lapse of

long ages, they have afterwards spread far and wide, leads to the same general conclusion.¹

I have argued this question thus minutely because, in addition to the momentous practical conclusion that hangs upon it, a point of high equity is concerned in it. On all accounts, it is time that the opposite parties in this great debate—the contagionists and the anti-contagionists—should be placed in their true, respective positions. Some years ago, at least, whenever and wherever this debate was opened, I observed that the contagionists were uniformly treated as if they, and not their opponents, were guilty of dealing in assumption and going against evidence.

I need scarcely remark that the truth is precisely the opposite of this.

At all events, as regards typhoid fever and the other members of the same family group, the contagionist, alone, proceeds on the solid basis of fact, and, where direct evidence cannot be had, has the support of analogy of the closest and strongest kind.

We *know* that the poisons of small-pox and of typhoid fever are multiplied in the human body, as certainly as we know that corn is raised from its own grain.

That these poisons are multiplied in any other way is not only pure hypothesis, but is an hypothesis that has besides the fatal defect of being entirely gratuitous—of being in opposition to all analogy that is of any worth, and of being, in one of these cases, at least, only tenable, at all, by heaping one extravagant assumption on another.

¹ This peculiarity is strictly parallel to the primary appearance of particular species of plants and animals in certain geographical centres, often of very limited area. Mr. Darwin's attempt to explain the fact is well known. I am not competent to say how far it is successful, or whether and to what extent the same principle is applicable to specific diseases. I may at some future time have something to say on this last point. Meanwhile, the fact itself, as connected with the origin and spread of contagious disorders, would form a very interesting subject for an essay in the hands of anyone competent to deal with it.

CHAPTER IX.

SUMMARY.

‘Ernst ist das Leben.’—SCHILLER.

THE conclusions arrived at in the foregoing pages may be summed up in the following propositions :—

1. That 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 to be the type.

2. That 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.

3. That the reproduction of this poison in the infected body, and the disturbance attaching to it constitute the fever.

4. That this reproduction is the same in kind as that of which we have, in small-pox, ocular demonstration.

5. That the 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.

6. That, 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.

7. That as a necessary result, sewers and the cloacæ 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, that, in many instances, the infected sewer, and not the infected man, appears as if it were the primary source of the specific poison.

8. That 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.

9. That, 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 the other contagious fevers, whose linear descent cannot be followed, and which spring up, therefore, under the semblance of spontaneous origin.

10. That the occurrence of such cases obviously constitutes no proof, whatever, that this fever ever does arise spontaneously.

11. That 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.

And, lastly—to crown the whole induction, by a practical test—That by destroying the infective power of the intestinal discharges, by strong chemicals, or otherwise, the spread of the fever may be entirely prevented, and that by repeating this process in every fresh case as it arises, the disease may in time be finally extinguished.

In arriving at these conclusions, I would observe:—

That the principal data on which they rest are, to the best of my belief, sure. Where these data consist in events observed by myself, I can vouch for their accuracy; where in events reported by others, I have taken, whenever this was possible, the most conscientious pains to verify them. In addition to this, all the great cardinal facts are distinguished in a peculiar degree by that precision and exactness which fit them alike for scientific statement and severe scientific deduction.

Whether the inferences drawn from the facts be logical, or whether, consistently with the laws of logic, any other inferences can be substituted for them, it is for others than myself to determine.

The whole induction is gradual. There are no great leaps, but the argument is conducted step by step; and I have striven to the utmost throughout to make one step sure before attempting the next.

The theory, in its entirety, is not only simple and harmonious, but it is in strict accordance with what we already know, of a certainty, of other members of the same family group.

Above all, its truth may be tested every day by a practical test, the employment of which can do no harm, and may do incalculable good.

Here I might fitly conclude. With this last step, as all may see,—Science passes into DUTY.

For we are not dealing here with questions of which the interest is abstract only. We are not discussing the great question of 'The Plurality of Worlds,' or the 'Darwinian Theory'—problems for whose final solution we shall have, and, many will say, we can afford, to wait—but a matter which, take the world over, for every year that passes, is life or death to myriads of men.

At this very hour scores of English homes are the scene

of the agonising anxieties with which recent experience has made the nation familiar. In hundreds, or, if we extend our view to the whole of Europe, in thousands, of cases, these anxieties have been consummated, within the month, by the death of the sufferer.

And all this the work of an ignoble thing, which, if what has gone before be true, may be disarmed of its terrors by the simplest of precautions.

In my introductory remarks I endeavoured to show, by illustrations drawn chiefly from the homes of the poor, how deeply humanity is concerned in the eradication of this fatal pest.

Since those remarks were first written (1859) the extension of its power to persons more conspicuous in rank has succeeded in awakening and directing, in a very striking way, the national attention to it. More than once since that time men or women bearing great historic names have fallen victims to it. Twice it has cast its dark shadow on the Throne. Once it has placed the entire nation in mourning; and once again it has brought the entire nation, on bended knee, to the Throne of Grace in thanksgiving for a happier issue.

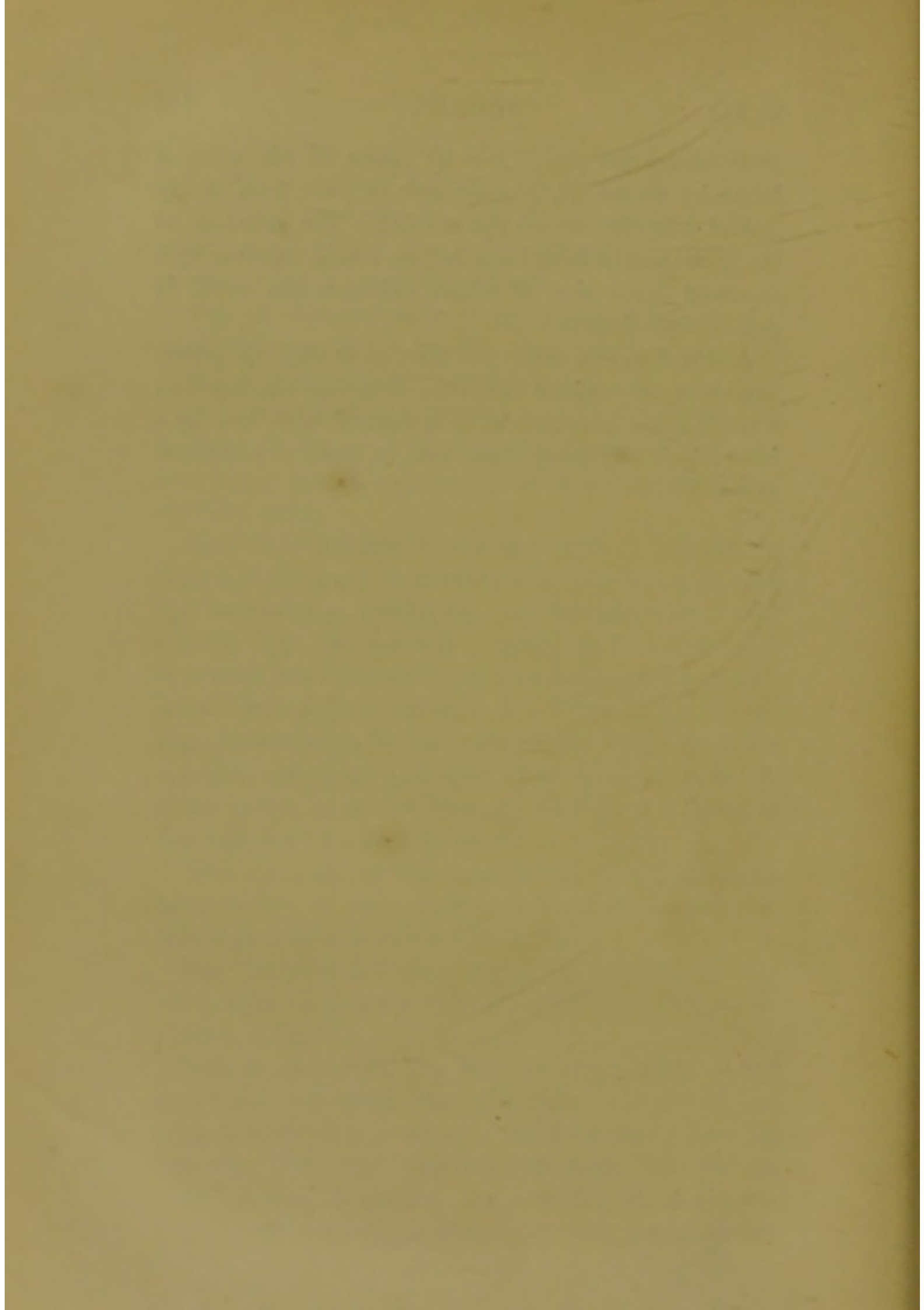
That the people of this Kingdom should testify, in this most solemn of ways, their deep sense of the responsibilities they have in this matter is well.

But if they wish to set a seal on the sincerity of their piety, there is—I say it with all humility—a still higher attitude to take.

It is, to spare from this hour no human effort to put under foot this great enemy of man. And let no one suppose that this is a matter in which he has no personal interest. The duty itself we may evade, but we can never be sure of evading the penalties of its neglect. This disease not seldom attacks the rich, but it thrives

most among the poor. But by reason of our common humanity we are all, whether rich or poor, more nearly related here than we are apt to think. The members of the great human family are, in fact, bound together by a thousand secret ties, of whose existence the world in general little dreams.

And he that was never yet connected with his poorer neighbour, by deeds of charity or love, may one day find, when it is too late, that he is connected with him by a bond which may bring them both, at once, to a common grave.



APPENDIX.



(RULES FOR PREVENTING THE SPREAD OF TYPHOID
FEVER, DRAWN UP FOR POPULAR USE.)

*How Typhoid Fever (otherwise called Gastric Fever, or Low Fever)
may be prevented from spreading.*

The means by which typhoid fever may be prevented from spreading are very simple, very sure, and their cost next to nothing.

They are founded on the discovery that the poison by which this fever spreads is almost entirely contained in the discharges from the bowels.

These discharges infect—1. The air of the sick room. 2. The bed and body linen of the patient. 3. The privy and the cesspool; or the drains proceeding from them.

From the privy or drain the poison often soaks into the well, and infects the drinking water. This last, when it happens, is of all forms of fever poisoning the most deadly.

In these various ways the infection proceeding from the bowel discharges often spreads the fever far and wide.

The one great thing to aim at, therefore, is to disinfect these discharges on their very escape from the body, and before they are carried from the sick room.

This may be perfectly done by the use of disinfectants—one of the best is made of green copperas. This substance, which is used by all shoemakers, is very cheap and may be had everywhere. A pound and a half of green copperas to a gallon of water is the proper strength. A teacupful of this liquid put into the night-pan every time before it is used by the patient,

renders the bowel discharge perfectly harmless.¹ One part of Calvert's liquid carbolic acid in fifty parts of water is equally efficacious.

To disinfect the bed, and body linen, and bedding generally, chloride of lime, or MacDougall's or Calvert's Powder, is more convenient. These powders should be sprinkled, by means of a common dredger, on soiled spots on the linen, and about the room, to purify the air.

All articles of bed and body linen should be plunged, immediately on their removal from the bed, into a bucket of water containing a tablespoonful of chloride of lime, or MacDougall's or Calvert's Powder, and should be boiled before being washed. A yard of thin, wide gutta percha placed beneath the blanket, under the breech of the patient, by effectually preventing the discharges from soaking into the bed, is a great additional safeguard.

The privy, or closet, and all drains communicating with it, should be flushed twice daily with the green copperas liquid, or with carbolic acid, diluted with water.

In towns and villages where the fever is already prevalent the last rule should be put in force for all houses, whether there be fever in them or not, and for all public drains.

In the event of death, the body should be placed, as soon as possible, in a coffin sprinkled with disinfectants. Early burial is, on all accounts, desirable.

As the hands of those attending on the sick often become unavoidably soiled by the discharges from the bowel, they should be frequently washed.

The sick room should be kept well ventilated day and night.

The greatest possible care should be taken with regard to the drinking water. Where there is the slightest risk of its having become tainted with fever poison, water should be got from a pure source, or should, at least, be boiled before being drunk. Immediately after the illness is over, whether ending in death or in recovery, the dresses worn by the nurses should be washed or destroyed, and the bed and room occupied by the sick should be thoroughly disinfected.

¹ As the discharge is sometimes much more copious than at others, the quantity of disinfectant added must of course be copious in the same proportion. A teacupful is mentioned in the rules, but the principle is to be lavish of the chemical.

These are golden rules. Where they are neglected the fever may become a deadly scourge; where they are strictly carried out, it seldom spreads beyond the person first attacked.

W. B.¹

OUTBREAK OF FEVER AT THE CLERGY ORPHAN SCHOOL,
ST. JOHN'S WOOD.

(*Lancet*, November 15, 1856.)

‘An outbreak of epidemic disease, in a public establishment containing a large number of inhabitants, must always be regarded as an event of public interest, and demanding the most searching inquiry. It is now a demonstrated law, that for epidemics of this kind to make their appearance, and spread in a community, some local special cause must be at root. It is a public benefit to give the widest publicity to every circumstance connected with the origin and course of epidemics, and, more especially, in the case where they occur in isolated communities like schools, where all the inmates are probably subjected to the same influences, and are under uniform observation; where peculiar facilities for scientific investigations exist; and where we are, therefore, likely to discover valuable facts, capable of useful application to the preservation of the public health. It is an obvious duty of those medical practitioners whose connection with institutions of a public character affords them special opportunities to add to the fund of scientific facts, faithfully to record the history of such outbreaks.

‘The leading facts that have transpired are, briefly, as follows:—The school is said to have been generally healthy, and the most scrupulous attention to cleanliness is observed. The children reassembled after the holidays on the 2nd September. The first pupil was taken ill about the 14th, with mild diarrhœa, headache, rigors, lassitude, &c. Four other cases occurred within about three weeks. Some cases of diarrhœa occurred. In the beginning of October, within a period of thirty-six hours, nineteen pupils became simultaneously

¹ Anyone who may desire to reprint these Rules for general use, has the full permission of the writer to do so.

affected, a large proportion complaining of diarrhœa, severe gastric irritation, and abdominal tenderness, but more particularly over the region of the caput cœcum coli. Some suffered from intense headache and congestion of the cerebral vessels; four or five had complications of pulmonary congestions; two with pneumonia. About the fifth day a papular rash was diffused over the body. This was observed in all but two cases. It was remarked that all the more violent cases arose within the thirty-six hours alluded to—a close, damp, oppressive state of atmosphere prevailing. The simultaneous seizure of the patients is sufficient to set aside the idea of contagion. Some local cause was, obviously, at work. It appeared that the drains had been under repair during the holidays, and, although it was considered that all source of malaria from this source had ceased, it is highly probable that some unobserved defect had been suffered to remain. We cannot but repeat, that a minute detail of every case, and a full report of the investigation into the sanitary condition of the institution are necessary, not only for the instruction of the public in a question of great moment, but, also, in vindication of the government and administration of the school.’

ON THE FEVER AT THE CLERGY ORPHAN SCHOOL,
ST. JOHN’S WOOD.

By WILLIAM BUDD, M.D., Lecturer on the Practice of Physic at the Bristol Medical School.—(*Lancet*, December 6, 1856.)

‘The outbreak of fever at the Clergy Orphan School, of which such a painfully interesting account appeared in the “*Lancet*,” of Nov. 15th, will probably prove to be no mystery if the facts are looked at from the true point of view.

‘From the description of the symptoms, as well as from other circumstances to be gathered from the narrative, there can be little doubt that the disease which is spreading among these orphans is the specific fever whose single anatomical character was first shown by Louis to be an equally specific disease of the intestinal follicles—a fever variously known in this country under the names of common continued,

typhoid, gastric fever, and so on, but of which intestinal fever would seem to be a fitter designation. Short as the summary is which your notice contains, it includes nearly all the diagnostic marks of this well-known affection. The detection of the characteristic ulcerations in the intestine seems to be all that is needed to set the seal to its identity. A great number of outbreaks of this fever, precisely similar in their leading incidents to that which has just happened at St. John's Wood, have already occurred in schools and other public establishments in this and other countries. Perhaps the most interesting case of the kind which has yet been recorded is that of the fever which committed such havoc in the military school of La Flèche, in France, in 1826. The scientific precision of the details give to this case a more than common value. As the chief amongst them lie in a small compass, it may not be amiss to reproduce them here.

'The fever was already prevalent in the town before it broke out in the school. As in St. John's Wood, it began in the school with a few scattered cases. In the course of the outbreak four of the pupils died. Their bodies were examined with great care, and, in all four, the characteristic ulcerations of the intestine were found. As the disorder soon gave signs of spreading, the governor of the school decided on anticipating the usual vacation, and sending the pupils home. Before, however, this measure could be carried into effect, sixty of their number were seized with the malady. Twenty-nine others took away the seeds of the fever with them, and were laid up by it at their own homes. Of these twenty-nine, Dr. Renon ascertained that as many as eight communicated the disorder to persons who were engaged in attendance upon them. This species of fever has two fundamental characteristics. The first is, that it is an essentially contagious disorder; the second, that by far the most virulent part of the specific poison by which the contagion takes effect is contained in the diarrhoeal discharges which issue from the diseased and *exanthematous* bowel. This, it may be said, is mere assertion; but there are, probably, few truths in medicine which rest on a body of evidence at once so copious, so various, and so decisive, as that which might be cited in proof of this two-fold proposition. So many and such striking proofs of both the facts here asserted have come before myself, that

I have looked upon them as being as sure as that itch is contagious, and that small-pox may be inoculated. When these facts are kept steadily in view, the interpretation of such events as those which occurred at La Flèche becomes to the last degree simple.

‘The first case in the series may either be casual and imported, or may be due to the local rekindling, through atmospheric or other changes, of poison which had remained as the dormant legacy of some former similar attack. In either case, in the usual course of things, diarrhoea comes on in the infected subject; and the next thing that happens is that the discharges from the bowels, which are usually at once copious, numerous, and liquid, are thrown into the water-closet or privy. In this way, the drains, or system of drains, belonging to the place become at once saturated with the specific poison in its most concentrated and virulent form. This once occurring, the poison may give the fever to the healthy inmates in one of three ways—either by percolating into the well which supplies the drinking water; by issuing through defects in the sewer into the air of the inhabited area; or, still more directly, by exhaling from the aperture of some ill-trapped water-closet or privy, which is at once the receptacle of the discharges from the sick and the daily resort of the healthy. Assuming the identity of the disease to be determined, it may safely be prophesied that the true key to the events which have lately occurred in this orphan asylum will be found in some of these simple relations. I have myself seen very striking instances, on a large scale, of all three of these modes of communication. A very memorable one, of the propagation of this fever by the bursting of a cesspool into a well, occurred in this place in 1848.

‘It is essential to a true view of the phenomena to understand that when the poison issues into the air (often, it may be, into a common day-room or dormitory), the atmosphere generated is *immeasurably more infectious than that which immediately surrounds the fever patients.* The sewer may be looked upon, in fact, as a *direct continuation of the diseased intestine*; with this important difference, however, that instead of being a narrow tube, which, except when the bowels act, is hermetically sealed, it is a large chamber, whose expanded surface, when the construction is imperfect, is either slowly distilling the infection into the soil, or constantly exhaling

it in large volumes into the surrounding air. As this species of fever, like all other contagious fevers, has what is called a period of incubation, it necessarily happens that when the first case is an imported one, a considerable interval elapses before the development of the group of cases which may chance to spring from it. From a large number of facts, bearing on the point, which have fallen under my own observation, I have been led to the conclusion, that this period ranges from ten to fourteen days.¹ There is reason to believe, however, that its duration varies considerably, partly with the nature of the medium through which the specific poison finds admission to the living body, partly by reason of conditions it meets with there, and still more, as I have been led to think, in virtue of the greater or less intensity of the metamorphosis in which the poison itself may be at the moment of its reception. The simultaneous seizure of a large number of inmates in an establishment like this, at the interval of a week or two after the occurrence of the first cases, so far from negating the idea of contagion as the writer of your notice supposes, is precisely what we should expect to witness under that mode of propagation. It is the exact counterpart, in fact, with the difference due to the different nature of the disorders, of what has been observed a hundred times before in schools, in the case of measles, scarlet fever, and, in former times, of small-pox also.

‘I may add, in conclusion, that I do not offer these remarks with the view of prejudging the inquiry at St. John’s Wood. My principal object in writing them is to avail myself of the opportunity they afford of earnestly recommending the adoption of some very simple measures which I have long been in the practice of employing for checking the spread of this fever, and by whose means, *provided they are thoroughly and effectually carried out*, I believe the recurrence of such calamities may be entirely prevented. These measures are founded on the power of chemical agents to destroy the contagious properties of contagious poisons. If it be certain that the intestinal discharges in this fever are the principal means of propagating the disease, it is no less certain, that by subjecting the discharges, on their issue from the body, to the action of powerful disinfectants, they may be entirely deprived of

¹ The facts relating to the Cowbridge outbreak, related in a former chapter, show, however that under certain conditions it may be much shortened.

this property. An actual instance of the application of these preventives will illustrate the mode of their employment better than any general statement. In the month of June, 1853, I was consulted by the head of a large retail establishment in this city, in consequence of the occurrence of several cases of this fever among his hands. Three cases had already occurred, and a fourth patient was now lying ill of the fever, and in a most dangerous state. The idea of propagation by contagion was supposed to be out of the question, because each one of the four who had fallen sick was separated from the other inmates of the house the moment the first signs of disorder appeared. It was, however, a very significant fact that these four were all *women*. There had been severe diarrhœa in all the cases, and, on inquiry, I found, as I had anticipated, that the diarrhœal discharges had all been thrown down the water-closet appropriated to the female part of the establishment. In the water-closet itself, at the time of my first visit, there were three vessels containing the three last discharges from the patients' bowels, which, in accordance with a too common practice, had been placed there without being subjected to disinfecting measures, to await the inspection of the medical men. Some linen, stained with these discharges, which had just been removed from the sick bed, lay in the vestibule. Under these circumstances, the following measures were suggested:—

‘1. That for the future all the discharges from the fever patient should be received on their issue from the body into vessels containing a concentrated solution of chloride of zinc.

‘2. That all tainted bed or body linen should, immediately on its removal, be placed in water, strongly impregnated with the same agent.

‘3. That the water-closet should be flooded several times a day with it, and that some chloride of lime should be placed there, to serve as a source of chlorine in the gaseous form.

‘And, lastly, and by way of further precaution, that so long as the fever lasted, the water-closet should be used exclusively as a receptacle for the discharges from the sick.

‘These precautions were carried out to the letter, and the result was that no other case of fever occurred among the remaining twelve or fourteen young women who lived in the establishment.

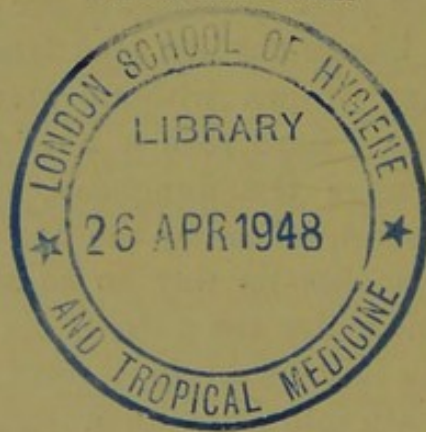
‘I may add, that since I have adopted this method of prevention, I have only met with one instance, where it was used from the first, in which the disease has spread beyond the single person first attacked.¹ Before I adopted it, it was no uncommon thing for me to see several members of a family take the disorder one after another, even in airy and well-appointed houses.

‘It may, perhaps, be alleged that the measures here recommended are already in very general use. Happily, the case is one in which the natural instincts suggest what science prescribes, and in which measures adopted to prevent noisome smells accomplish a higher purpose. It is not the only case that might be cited in which the nose is wiser than the intellect that it serves. But measures of this kind, if they are carried out at all, are only carried out in the houses of the rich, and, even in them, as far as my observation goes, they are never executed with the completeness which an insight into the real exigencies of the case requires.

‘Amongst the poor such refinements are never thought of, and when this fever breaks out in a poor family the discharges from the bowels are thrown without preparation into the common privy, or, as I have seen a hundred times in rural districts, are cast upon the dungheap, or into the open gutter. From this point, following the line of watershed, this perilous stuff often makes its way to considerable distances, where, appearing now under the guise of an epidemic miasm, which entirely masks its true origin, it may carry the seeds of fresh disease and death into many an unsuspecting household. If the practice were universal of depriving this stuff of its infectious properties before it is let loose upon the world, I believe it is not too much to say, that the lives of a large proportion of the many thousands who die annually of this fatal disease in the British Isles would be saved by the measure, and a corresponding amount of sickness be prevented.’

In the one exceptional case, a servant girl, who nursed a lady through her whole illness, caught the fever. In the lady the chief stress of the disease, as sometimes happens, fell on the lungs, and, for some time, there was much cough and expectoration. Now, although the discharges from the bowel were disinfected, nothing was done to the sputa, or the numerous pocket-handkerchiefs that were soiled with them. As there are strong pathological grounds for believing that, under such circumstances, the pulmonary excreta may convey the fever, the disinfection, in this case, cannot be regarded, therefore, as being complete.

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