

Fever, in its relations to sanitary reform, being the address in medicine, delivered at the sixteenth anniversary meeting of the Provincial Medical and Surgical Association, held at Bath, August 16th & 17th, 1848 / by W. Davies, M.D.

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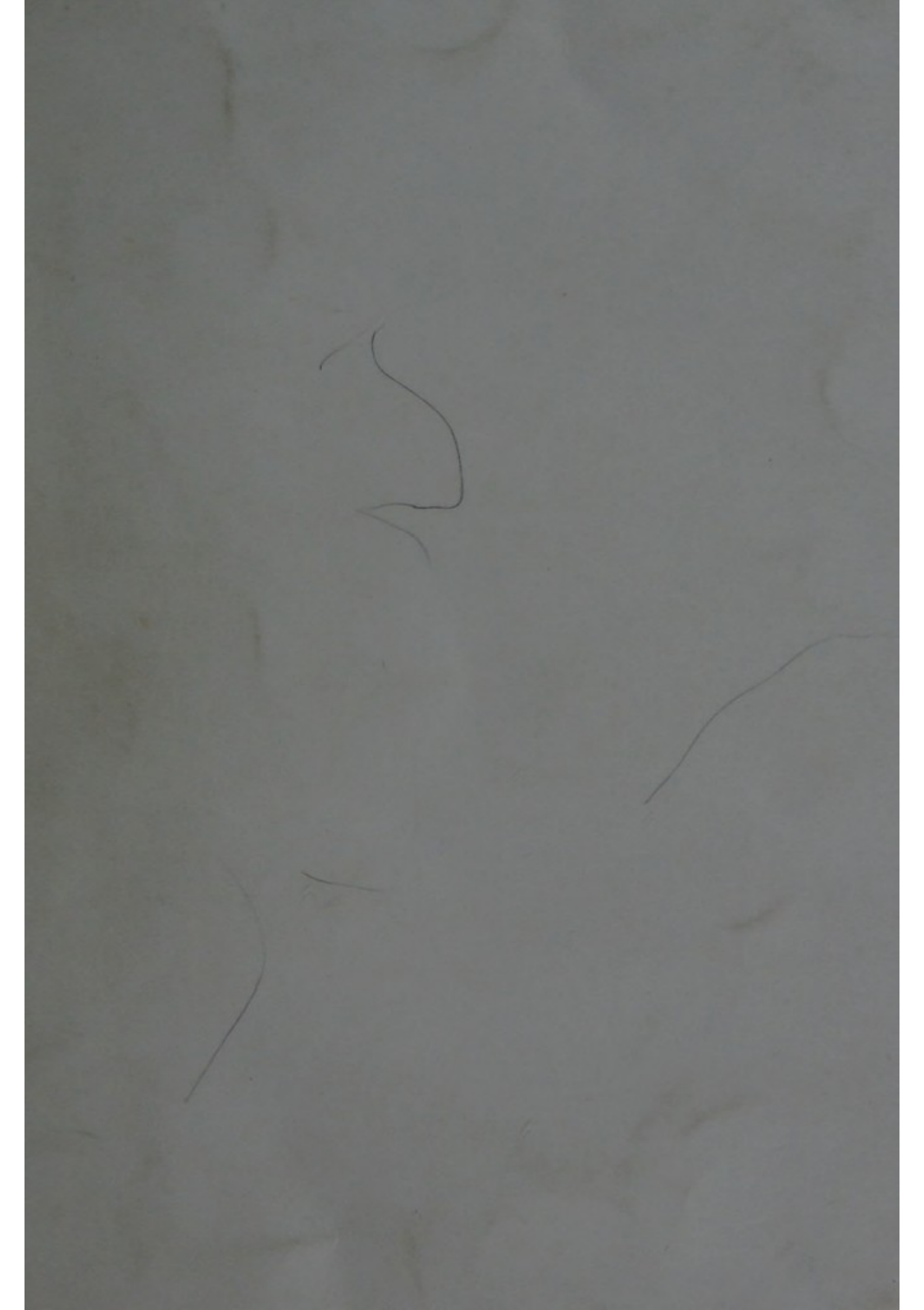
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F E V E R,

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IN

ITS RELATIONS TO SANITARY REFORM,

BEING THE

ADDRESS IN MEDICINE,

DELIVERED AT THE

SIXTEENTH ANNIVERSARY MEETING

OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION,

HELD AT BATH, AUGUST 16TH & 17TH, 1848.

BY W. DAVIES, M.D., EDIN.,

PHYSICIAN TO THE BATH UNITED HOSPITAL.

WORCESTER:

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MDCCCXLVIII.

THE RELATIONS TO SOCIETY

ADDRESS IN MEDICINE

SIXTEENTH ANNIVERSARY MEETING

PROFESSOR JOHN L. AND SURGEON GENERAL

HELD AT THE ANNUAL MEETING OF THE

BY W. BAKER, M.D. EDITOR

THE JOURNAL OF THE MEDICAL ASSOCIATION

F E V E R,
IN
ITS RELATIONS TO SANITARY REFORM.

MR. PRESIDENT AND GENTLEMEN,—I can assure you it is with feelings of no ordinary diffidence that I stand before you on the present occasion, to deliver the Annual Address in Medicine; I can only crave at your hands, that which I am sure you will grant me, great indulgence, and a lenient criticism for the numerous faults which are sure to encumber my production.

The invitation which I received from the Secretary of our Association to deliver this Address, was accompanied by a request on the part of the Council, that it should not include, as heretofore, a retrospect of the progress of medical science, as a whole, during the past year, such retrospect being no longer desirable, in consequence of the elaborate *Half-yearly Abstracts and Retrospects*, edited by Dr. Ranking and Mr. Braithwaite, but should be confined to some one or more subjects, at the choice of the writer. It is obvious that the change thus introduced into the character of the Address, has altered altogether, and much increased the responsibility of the writer; for it would only have been necessary to refer to the past volumes of the *Transactions* of the Association, to select from the many able

addresses which those volumes contain, a model whereby to fashion my own; and a form being fixed on, the value of the Address would depend on the extent of the selections from the medical literature of the past year, and the judgment with which they were displayed. All such guidance I have been deprived of, as the only Address that has been delivered on the present plan—that by Dr. Shearman, of Rotherham—remains as yet unpublished. Under these circumstances, I felt it to be no light matter to select a subject on which to address a meeting, composed, as this is, of a large proportion of the best informed members of the medical profession resident in the provinces.

It has been the practice of my predecessors in this office, to lay before the meeting some memorials of such leading members of our profession as death may have removed during the preceding year; the list on the present occasion is a long and a dreary one, and has been already appropriately introduced by our President.

It has also been usual of late years, while the subject of medical reform has been occupying much attention, both within and without the profession, to pass under review such topics as have more particularly occupied the medical mind during the past year. This task I shall altogether omit:—First, because the subject of Medical Reform is far too extensive and complicated to be advantageously discussed within the very narrow limits that I could devote to it; and secondly, because I have seen little reason to congratulate the profession on the spirit in which the various objects, by some thought desirable, have been sought to be attained. I have never been able to comprehend clearly any one specific object which the profession, as a whole, desires. I have seen a great deal of acrimony and ill-feeling set afloat, to poison the springs of that brotherly good-will which should unite in one body every honest member of a liberal profession. It has appeared to me that medical reform has been considered far too much in its individual, and too little in its universal aspect; that the small and passing interests of individuals and of corporate bodies have been allowed to occupy that attention which should have been devoted to a consideration of what is just and what is true in the nature of things. Men are too apt to imagine, if the bright anticipations which they have formed fail to be realised, that the defect lies without, and not within; and that under some different system, their career would

have been more fortunate : whereas I would humbly, but confidently submit, that the reform, above all others, calculated to promote the interests of our profession, is such as no legislature can effect—a reform of mind and will—of the one, a more diligent training in the science of our profession ; of the other, a stricter discipline in that charity which thinketh no ill. And I believe, further, that just in proportion as the minds of men are occupied in the exciting discussions of legal reform, so are they rendered unfit for the advance of that self-reform of which I have spoken above. On these grounds, I must consider the formation of the Manchester Ethical Society as a step in the right direction, though perhaps its rules are susceptible of considerable improvement. I will conclude these few prefatory remarks with a quotation from a celebrated German physician and philosopher : “ Whatever thou doest, do it with integrity ; if thou studiest, let it guide thy studies ; and then, as to whether thou shalt prosper in what thou doest, leave that to God. Thou hast most surely left it to him when thou goest to work with true and honest purpose ; with the attainment of that integrity thou wilt also attain unbroken peace, inward cheerfulness, and an unstained conscience ; and in so far thou wilt assuredly prosper.” I now pass to the special object of my Address.

The sanitary condition of the great towns of the empire has of late years occupied much of the attention, not only of the medical profession, but also of the public. Various commissions of inquiry concerning the health of the inhabitants of such towns have been formed, and various reports thereof published ; the result has been a far more accurate knowledge of the kinds of diseases most prevalent, and the amount of fatality attendant on each, than was previously possessed. Of all the diseases whose ravages have thus been brought to light, none has attracted so much attention as fever. The reasons of this are obvious. During all periods of medical history, the subject of fever has furnished a fertile source of medical discussion ; and down to the present time, opinions regarding the nature and cause of fever remain different and opposed. The great attention which has been devoted to this subject during the last few years, while it has been the means of extending to all classes certain general notions on the subject, has, I believe, very greatly complicated the matter in relation to scientific medicine, by

the introduction of a class of writers and speakers who have drawn their conclusions from the small sphere of their own immediate observation, without having studied the literature of the subject, and compared their own crude and hasty generalizations with the broader inductions of others. This defect, in my opinion, pervades more or less extensively all the reports that have been published on the cause of fever, and the means of prevention, by different commissions of inquiry, from 1839 to the present day. Hence, then, from the great practical importance of the subject, from its great interest in relation to scientific medicine, and from the numerous discordant notions by which it is surrounded, I have thought certain points in the history of fever not unworthy to occupy the attention of so important a meeting of medical men as I have now the honour of addressing.

I purpose to direct your attention especially to the relation which exists between the cause of fever and sanitary reform. But as I apprehend much of the confusion which exists on this subject to be occasioned by the confounding together, under one name, diseases essentially distinct,—an illustration in medicine of an observation of Dr. Reid, while treating of the science of mind,—“That the *ambiguity of words*, and the vague and improper application of them, have thrown more darkness upon this subject than the subtlety and intricacy of things;” and, of the remark by Locke,—that “The greatest part of the controversies that perplex mankind, depend on the doubtful and uncertain use of words;”—it will be necessary, before entering on this subject, to define, as accurately as I can, the distinctive characters of the diseases in question; and, in the first place, I will endeavour to establish that there is a form of fever to which the following description accurately applies:—

1st. That its progress is marked by a specific eruption.

2nd. That it is propagated by contagion.

3rd. That there is no distinct evidence of its ever spreading in any other way than by contagion.

4th. That the same individual is not, for the most part, liable to more than one attack during life.

In proportion to the degree of clearness with which these four points can be established will be the claim of the disease to be

admitted into the class of exanthemata. In this paper it will be designated typhus, or maculated fever.

1st. Typhus is characterized by a specific eruption. In the evidence to be adduced on this point I will omit the statements of the older authors, as not sufficiently definite for my purpose, although many of them in their description of the disease, no doubt refer to the same thing,—viz., Huxham, Hildenbrand, Drs. Barker and Cheyne, and others. Professor Alison, writing on the Edinburgh fever of 1827, says,—“It should not be overlooked in any discussion regarding the contagious nature of fever, that in those seasons when it prevails most epidemically, and very remarkably in the present, it is very frequently attended by an eruption of the skin, closely resembling what is seen in some of the contagious exanthemata, more particularly measles. A majority of the cases treated by me in the hospitals during the present year, had more or less of this eruption.” And, again, “Such cases of spotted fever may be said to form the link that connects the order of fevers with that of contagious exanthemata.”*

Dr. Peebles quotes Dr. Palloni on the epidemic of 1817, as follows:—“I have ascertained,” says Dr. Palloni, “after the most attentive inquiry, that the nature and form of the eruption are always the same, not only in the actual epidemic, but in all the cases of exanthematous fever produced by contagion, and are opposed to the spurious symptomatic appearance which sometimes accompanies fevers of a putrid or malignant character.”† Further on Dr. Peebles writes,—“When I saw the patient before the seventh or eighth day, the exanthema was more or less clearly marked; and even in some of the cases that I did not see till the ninth or tenth day, it was still visible, either alone or mixed with petechial stains. I very rarely failed in discovering it in those patients I saw in their own houses, for there I had a better chance of seeing them early in the disease.”‡

Dr. West, in a paper on “Typhus Exanthematicus,” says,—“Forty-two cases presented the peculiar measles-like eruption, described by so many authors, which, in all those cases in which I have been able accurately to note the date of its appearance, first showed itself from the sixth to the eighth day, generally on the

* *Edinburgh Medical and Surgical Journal*, vol. xxviii.

† *Ibid*, vol. xlv.

‡ *Ibid*.

former.”* He further remarks,—“That of the eighteen cases in which no eruption was observed, only five were admitted before the eighth day, so that some of those patients may have had the eruption, although it had disappeared before their admission.”

Dr. Henderson, in his conjoint report with Dr. Reid, on the Edinburgh epidemic of 1836-7-8, says, regarding the eruption, “136 cases, of both sexes, were specially inspected with reference to this eruption. In 108 cases the eruption was found; in twenty-two it was not found: six of the twenty-two cases were not admitted till between twelve days and three weeks from the beginning of the fever; therefore, as will appear from what follows, it cannot be concluded that they had not had the eruption at an early period. Of the sixteen cases in which no eruption existed at any time, the greater number were slight cases; one only could be termed a rather severe case, extending to the fifteenth day: this case was not traced to contagion.”†

Dr. Davidson, in the *Thackeray Prize Essay*, says,—“There is one fact, however, which powerfully supports the opinion that contagious typhus, in the great majority of cases, particularly in adults, is attended with the eruption, namely, that almost all the instances of fever which have occurred during the last six or seven years among the physicians, clerks, nurses, &c., of the Glasgow Fever Hospital, have been accompanied with this exanthema. We have made careful inquiries respecting this point, and have only heard of one or two exceptions, amongst, at least, 100 cases.”‡

Dr. Copland, while treating of the confusion which some writers have made between the eruption we are speaking of and petechial stains, says,—“But the *exanthematous eruption* attending true typhus, is as characteristic of it as the eruptions of measles or of scarlatina.”§

Dr. Watson says,—“The rash now described, stamps continued fever with one of the most striking characters of the exanthematous group of febrile diseases; but it certainly is less constant than the cutaneous phenomena of small-pox, measles, or scarlet-fever. It occurs much more regularly in some epidemics than in others. Fever is very rife in St. Giles’s, and in other crowded parts of this

* *Edinburgh Medical and Surgical Journal*, vol. 1.

† *Ibid.*, vol. lii.

‡ *British and Foreign Medical Review*, vol. xi.

§ *Dictionary*. Article, “Typhus.”

town, just now, (1838,) our wards at the Middlesex are full of it, and scarcely a case presents itself without these spots. We speak of it particularly as the *spotted* fever; or, from the resemblance which the rash bears to that of measles, hereafter to be described, as the *rubeoloid* fever.”*

In an elaborate essay on the epidemic fever of 1847, in the second number of the *British and Foreign Medico-Chirurgical Review*, is the following:—“It remains for us to particularize a little further with regard to the cutaneous eruption, and to show that, in all its characteristics, it maintains as determinate a relation to its proper fever as the cutaneous affection in corresponding maladies. First, it appears *definito tempore*; secondly, it manifests itself electively on some parts of the surface rather than on others; and, lastly, the spots have certain fixed physical characters.”†

A few interesting cases have lately come under my own observation with reference to this question. I will give a sketch of six of them. The first case was that of an Irish woman, of the name of Rooke, who was admitted into the Bath United Hospital, under my care, during the second week of fever, the eruption at that time being very abundant, chiefly over the chest. The nervous system was in a very depressed condition, but under a pretty liberal use of stimulants she recovered, and was discharged on the 19th of February, 1848. A woman, named Carpenter, who had been six weeks in the hospital on account of chronic disease, and who, previous to her admission, had resided in a healthy rural district, took ill of fever on the 10th of February; on the sixth day from her attack, the eruption appeared on the upper part of her chest, and in a day or two became very abundant. The nervous system became profoundly involved, and she died comatose, on the eleventh day of the disease, the 20th of February, one day after Rooke had been discharged. At the time of Rooke’s admission there was no other case of fever in the ward into which she was taken, and Carpenter’s illness commenced about a week after the convalescence of Rooke had enabled her to sit by the fire and mingle with the other patients, whose maladies were such as to admit of their being out of bed, of which number Carpenter was one. No other patient in the ward took the fever.

* “Lectures.” 1st edition.

† *British and Foreign Medico-Chirurgical Review*, No. 2.

The next case I have to mention came under the care of my colleague, Dr. Daniell. A man, named John Dunn, was admitted on the 4th of March, 1848; he was unable to give any accurate account of himself, but as far as could be judged, was then about the end of the first week of fever. The maculæ were very abundant, and the nervous system much affected; he died by a sort of compound lesion of the vascular and nervous systems on the 15th of March, eleven days after admission. Robert Stone had been in the hospital since the 2nd of February, more than a month previous to the admission of Dunn, and was taken ill of fever on the 13th of March, two days before the death of Dunn. The eruption in this case was very abundant, and the disease ran a very severe course; he recovered, however, and was discharged on the 28th of April. The beds in which the two men lay were near each other, and there was no other case of fever in the ward at that time.

The next case I will mention is that of a woman, Harriet Gates, who was admitted under the care of Dr. Daniell, on the 1st of April, suffering under maculated fever. In consequence of the delirium under which she laboured, being of a more active character than usual, the nurse of the ward was brought into more frequent and immediate contact with her than is commonly necessary. Gates ultimately recovered, and was discharged on the 4th of May. The nurse, who had been much fatigued by her attendance on this woman, was seized with fever on the 21st of April, twenty-one days after her admission. On the fifth day after her seizure, maculæ made their appearance, and became very abundant; and in her case, about the twelfth or fourteenth day, some of the spots seemed to pass into true petechiæ. She made a slow, but ultimately perfect recovery.

In attempting to estimate the import of these cases, it is necessary to understand that true typhus is a very rare disease in this city, as, until the commencement of the present year, no case of the disease had come under my observation, although I have performed the duties of physician to the hospital during more than two years; hence, when typhus does appear, more favourable opportunities than usual are presented for tracing the progress of the malady from individual to individual. Keeping this fact in view, it will be observed that I have described six cases of eruptive, or maculated fever; three of which, for the sake of distinction, I will term

original, the other three derived. It so happened that each of the original cases was admitted into a different ward, and that there was no other case of fever of any kind in either of those wards at the time of such patient's admission. Now if we consider—firstly, that maculated fever is a very rare disease in Bath—secondly, that at the time of the admission of each of the original cases described, there was no other case of fever of any kind in the same ward—thirdly, that each of the three derived cases had been in the hospital for a considerable time previous to the admission of the original ones; one upwards of a month, another six weeks, and the third a nurse of the hospital of several years' standing—and lastly, that a considerable amount of intercourse is shown to have existed between the two classes of cases:—if these facts are regarded, without the introduction of any mental idol, I do not see how any other conclusion is to be arrived at than that the two classes suffered under the same disease, and that the one was derived from the other.

In order to justify any other conclusion, it would be necessary to assume that a local cause of fever sprung up in three distinct wards of the hospital, at three different periods of time; and that the coming into operation of this cause, in each instance, agreed, both as to time and place, with the admission of patients labouring under the same disease. I am anxious to have this matter clearly apprehended, for two reasons—firstly, because it furnishes an argument in favour of the speciality of maculated fever, like producing like; and secondly, because it supplies a link in that, to my mind, perfect chain of facts and arguments by which the contagious property of typhus is proved.

It will, perhaps, be considered that I have occupied too much space in the attempt to establish the eruptive character of typhus, as the fact is to be found stated with sufficient clearness by several authors. In deprecation of this judgment, I would observe that a fact usually becomes established as a scientific verity, long before its operation on men's minds as a practical belief displays itself; and that the fact we are discussing is in the former, and not in the latter predicament, I have much reason to believe.

2nd. I now come to the second proposition, namely—That maculated fever is propagated by contagion. By the term contagion, I mean to express the means by which a specific malady is communicated from a diseased to a healthy person, without inquiring

whether obvious contact takes place, or whether the *materies morbi* passes by means of the atmosphere through an appreciable space ; it is clear that nothing can act where it is not, and therefore in either mode of communication, I apprehend the system to become affected in precisely the same manner—namely, by the entering in and multiplying of the morbid matter in the blood, up to that point of saturation when its presence becomes incompatible with the normal performance of the functions of the body, and disease is the result. I have thought it proper to premise this explanation of the meaning which I attach to the term contagion, because certain writers have attempted to draw a distinction between contagion and infection ; which distinction I believe to be not only useless, but contrary to nature.

If we admit in this discussion the dictum of Dr. Elliotson,—“That for *infection* to be *proved*, the individual who communicates the disease must go from the place where he resides to the spot where the healthy person is, and there give it to the latter,”—it will be observed that the cases I have described under the last head fulfil the conditions imposed, so that even according to the requirements laid down by Dr. Elliotson, the evidence in those cases is complete. Still I believe that perfectly satisfactory evidence of the contagious nature of typhus may be drawn from other sources ; for example :—A large building, which has been from time to time used for various purposes and by different classes of persons, and never known, under any circumstances, to become the seat of fever, is at length appropriated to the purposes of a fever-hospital ; fever-cases are admitted into it, and the necessary staff of physicians, clerks, nurses, &c., appointed ; in no long time the whole medical staff and assistants suffer under a fever. Is not the evidence here complete that the fever under which the physicians and clerks suffered was communicated to them from the patients in the house ? These are precisely the circumstances which have occurred more than once at Queensberry House, Edinburgh, and have been fully described and argued on by Dr. Alison.* To this paper I would refer for further evidence on this subject, and also to the writings of Drs. Christison,† Tweedie,‡ and Graves,§ with

* *Edinburgh Medical and Surgical Journal*, vol. xxviii.

† *Library of Medicine*. Article, “Fever.”

‡ Work on “Fever.”

§ “Clinical Medicine.”

many others; as it does not seem necessary to occupy your time with long quotations, in proof of what every man of sound mind must admit, provided he has given the subject any attention.

The third proposition is,—That there is no distinct evidence that typhus ever spreads in any other way than by contagion. I am not in a position to discuss this proposition with advantage at present, so that I will return to it at the conclusion of this Address, by which time we shall have had under our notice other matters which bear on the question.

The fourth proposition is,—That the same individual is not, for the most part, subject to more than one attack during life.

There are few facts in the history of disease more calculated to arrest the attention of the physician than this,—that there are certain diseases which do not possess the power of affecting the same individual, as a rule, more than once during life; and the more perfectly developed the disease has been in the first instance, the smaller is the chance of any subsequent attack. All known diseases which possess this peculiarity are contagious, but the converse of the proposition does not hold, as some contagious diseases do not possess this peculiarity. But, again, I believe it will be found that all contagious diseases which run a more or less definite course of febrile action, do possess the peculiarity in question, namely, that of occurring only once during life.

It appears to me to be well-nigh demonstrable that the poisons of these diseases, namely, small-pox, measles, scarlet-fever, and typhus, are taken into the blood of the individual to be affected before the disease manifests itself, and that in the blood these poisons find the necessary materials for their own increase; hence they are contagious. It is probable that the particular material of the blood, by means of which these poisons are formed, becomes destroyed during the process, and not being essential to life is not reproduced, or only rarely, and after a long interval of time; and hence these diseases occur only once during life, just as no yeast would be formed by introducing that matter into any solution which did not contain gluten.

Now if these things be so, it follows that the period between the exposure to contagion and the commencement of the disease is that

during which the poison is accumulating in the blood; that the beginning of the fever corresponds with that amount of saturation which is no longer compatible with the healthy functions of the body; that the continuance of the fever indicates the period necessary for the maturation and infusion of the poison; and convalescence the complete attainment of that end. Hence, by substituting the word fever for disease, the following quotation from Sydenham, enunciates, I believe, an important fact:—"A disease, in my opinion, how prejudicial soever its causes may be to the body, is no more than a vigorous effort of nature to throw off the morbid matter, and thus recover the patient."

Sir Henry Marsh, in a paper "On the Origin of Fever,"* brings forward a number of examples where individuals were seized with typhus, immediately on being exposed to the concentrated effluvia arising, either from the bodies of fever-patients, or from their excretions; and on these facts founds an argument that the poison of typhus does not enter the blood before it produces its effects, but that these effects follow in consequence of an impression on the nervous system. Now, in the first place, it may be replied that nearly all the cases adduced by Sir Henry Marsh in support of his opinion, were persons (hospital-physicians and nurses,) subject to continued exposure to contagion, and therefore that the particular exposure which immediately preceded the attack, was only the last pound which broke the horse's back—the last drop of water which caused the vessel, already full, to overflow; and, besides, no one doubts that a violent mental emotion, of a depressing kind, which is stated to have taken place in those cases, would act powerfully towards giving effect to the action of the poison. Again, there is one argument which has always seemed to me fatal to the notion, that the occurrence of fever could be attributed to an impression caused by the poison on the nervous system; it is this,—that even supposing the one attack to be thus explained, how are we to account for the formation of a fresh supply of the same poison in the body of the individual in whom the disease thus sprang up? It cannot be supposed that so vague a thing as an impression on the nerves can give rise to the formation in the system of a definite and specific poison; different persons will be differently affected by the same cause acting on

* *Dublin Hospital Reports*, vol. iv.

the nervous system. Shakspeare knew this when he made the Jew say—

“Some men there are love not a gaping pig;
Some that are mad if they behold a cat;
And others, when the bag-pipe sings i’ the nose,
Cannot contain their urine.”

Thus much for contagious diseases which occur only once during life. There are other contagious diseases which may occur any number of times during the life of the same individual, such as syphilis, gonorrhœa, and scabies. The last of these is well known to depend on the presence of an insect. As regards gonorrhœa, there is no reason to suppose it any other than a purely local affection; and though syphilis no doubt affects the blood, still it is not necessarily attended with any febrile disturbance of the system.

There are two contagious diseases derived from the lower animals, hydrophobia and glanders, which, as far as we know, are always fatal, so that there are no means of judging whether they have a tendency to occur more than once during life or not.

There are febrile diseases not contagious, and from the attacks of which no immunity, as far as we know, is derived from having previously suffered under them; such are marsh or intermittent fevers, and, probably, also fevers which arise from choked drains, cess-pools, and so forth, concerning which we have heard so much of late, and of which I shall have to speak more at length presently. It seems probable that these fevers are not contagious, and that they should not be so seems to me in accordance with the deductions of theoretic reasoning, anterior to all experiential evidence; for the poisons on which they depend are formed by chemical causes, acting on dead matter exterior to the living body; and although the poison thus generated may and does gain access to the blood, and by that means excite certain conditions in the system, to which we give the name of fever, still it is not probable, nor does it seem to be the fact, that this product of dead matter shall find any material in the living blood by means of which it can multiply itself, so as to form a fresh supply of the same poison. Now, this is surely a very important distinction to bear in mind while studying these diseases,—that the poisons of the contagious fevers are formed in the living blood, and hence find the materials

of increase in that fluid, so that one individual affected with any of these diseases coming into a healthy town or neighbourhood, may be the means of extending the malady to thousands; while, on the other hand, a person labouring under a fever, the origin of which had been external to the living body, on thus coming into a healthy district, injures no one; the poison which affected him, not having been a product of the living body, does not find in his body the necessary elements for its reproduction, therefore ceases with himself.

It will be observed in what I have said under this head, that I have assumed that typhus does, in common with small-pox, measles, and scarlet-fever, possess the peculiarity of only occurring once during life. I now have to show some of the grounds of this assumption. The evidence is the same in kind, and nearly equal in amount, to that on which the same fact rests as regards the other diseases mentioned. It is not professed that a second attack of typhus never occurs in the same individual, but that it very rarely does so, in comparison with the number of persons who are exposed to the contagion of typhus, after having gone through one attack.

The first evidence I will adduce is furnished by, as far as I know, the first propounder of the doctrine, Dr. Perry, of Glasgow. His paper consists—first, of the grounds on which he claims to be heard in the matter, these being the careful observation of upwards of four thousand cases of the disease; and secondly, sixteen carefully elaborated propositions deduced therefrom.

The eleventh proposition is the following:—That contagious typhus is an exanthematous disease, and like small-pox, measles, and scarlet-fever, during its course, produces some change in the system, by which the individual having once undergone the disease, is (as a general rule,) secured against a second attack, and may with impunity expose himself to the contagion of typhus, if he continues to reside in the same country in which he previously had the disease. In those cases which are exceptions to the general rule, the disease appears in a mild and modified form.”*

Dr. Davidson, in the essay already referred to, says:—“The following table shows the answers to questions which were carefully

* *Edinburgh Medical and Surgical Journal*, vol. xlv.

put to patients which were admitted into the Glasgow Fever Hospital, from November 1st, 1838, to November 1st, 1839. It includes the whole of the patients affected with eruptive typhus, from whom answers were obtained relative to any former affection with fever, as evidence from decided cases only could be made available in the elucidation of this point:—

		MALES.		FEMALES.		TOTAL.
Not previously affected,	...	284	...	251	...	535
Previously affected,	...	33	...	41	...	74
						<hr/> 609 <hr/>

This table shows that out of 609 eruptive, or decided cases of typhus, there were only 74 persons who stated that they had previously laboured under fever.”*

The above table furnishes strong evidence in favour of the proposition I am maintaining, as there were only 74 persons out of 609 who had previously suffered under fever of any kind; and when it is considered how generally the different kinds of fevers are confounded one with another, it will appear exceedingly probable that a very small proportion, even of the 74, had previously suffered under the maculated fever. And when it is further borne in mind that the table gives all the cases of eruptive typhus that were admitted into the Glasgow Fever Hospital during one year, and that typhus is almost never absent from that city, it will appear somewhat more than a coincidence, that among the admissions to the hospital, the second attacks should have borne so small a proportion to the first.

The last statement upon this point with which I will trouble you, is from the paper already quoted, in the second number of the *British and Foreign Medico-Chirurgical Review*:—“Of the many second attacks that we have witnessed, in the way of relapse, during the epidemic of last year, we never noticed all the specialities of maculated fever to be twice displayed. Still, that fully-developed instances *may* be exhibited in the same individual more than once, analogy would not permit us to doubt, even though there were no correct evidence upon this point. Scarlatina is sometimes repeated; we have ourselves twice attended the same child in measles, at an interval only of a year; and Dr. Baron, of Cheltenham, in his

* *British and Foreign Medical Review*, vol. xi.

Vaccination Report, relates authentic instances of *third* attacks of small-pox. And we are confident, that if the distinctive peculiarities of maculated fever were more generally a subject of close observation, immunity after attack would be found to obtain very much in the same proportion, and to be qualified much in the same way, as in the other exanthematous fevers."

This concludes what I think it necessary to say at present concerning the distinctive characters of typhus; and I trust it will be considered that some grounds have been established for the admission of the disease into the class of exanthematous fevers, and thus an important step will have been attained towards the elucidation of what is to follow.

Now there is distinct and unequivocal evidence, that, during the years 1843 and 44, there was a fever which spread extensively in different parts of these islands, which differed in its causes, symptoms, progress, and termination, from that we have been describing, or true typhus, and which there can be no just reason to doubt was a disease as distinct from typhus as small-pox is from measles. I cannot afford the space necessary, in order to detail the grounds of distinction; the leading characteristics were the following:—

1st. The absence of any eruption.

2nd. The short duration of the disease, generally terminating from the fifth to the seventh day.

3rd. The manner of terminating, almost invariably by critical diaphoresis.

4th. The great frequency of relapse, the relapse being the rule, and its absence the exception.

5th. The extreme rapidity of the pulse; the average in thirty-eight cases of the disease, on or before the fifth day, was 123 in the minute, while, in fifteen cases of eruptive typhus, the average was 100 in the minute. (Henderson.)

6th. The small amount of mortality, the average number of deaths not exceeding three per cent. of the attacks.

Lastly. The one disease conferred no immunity from the attack of the other, as there were several cases where the same individual suffered under both diseases within a short period of time. (Henderson.)

There were not wanting those who denied that the two diseases

were distinct; but where is the fact in medicine that has not been denied? The literature of the disease is sufficiently extensive. I may mention a few of the authorities:—Dr. Alison,* Dr. Rose Cormack,† Dr. Craigie,‡ Dr. Henderson,§ Dr. Richard Wardell,|| Dr. Perry,¶ and Dr. Smith.**

The paper by Dr. Henderson is that to which I would especially refer those who wish to investigate this subject further, for the evidence in support of the essential distinctness between the disease in question and typhus.

I now proceed to discuss as shortly as may be the doctrines set forth by various commissions of inquiry concerning the sanitary condition of large towns, in as far as their reports refer to the origin and spread of fever. It is well known to all those who hear me that the publications in question have proclaimed loudly the doctrine, that typhus is capable of being originated under all kinds of circumstances, and by all sorts of substances, animal and vegetable, provided they be placed under conditions favourable to the process of decomposition: in short, that the dwellings of the poorer inhabitants of our large towns, with their cesspools, their uncovered and stagnant drains, their imperfect supply of fresh air and pure water, and the crowded condition of their inmates, contain within themselves all that is essential for the generation of the poison of typhus. Now this is a doctrine that I can in nowise admit the truth of; and yet it is no light matter to attempt its refutation, as the belief is almost universal. Still it must be borne in mind that the advocacy of numbers, forms a very poor ground for the belief of a doctrine, apart from the inherent truthfulness of the doctrine itself. Man, if I may so speak, is a gregarious animal in the domain of thought, as well as in that of action, and thus a set of notions, supported by some external plausibilities, be it in religion, politics, philosophy, or physic, once set a going, is like a snow-ball which a boy rolls along the street until its size becomes

* *Northern Journal of Medicine*, No. 1.

† A Monograph.

‡ *Edinburgh Medical and Surgical Journal*, vol. lx.

§ *Ibid.*, January, 1844.

|| *London Medical Gazette*.

¶ *Edinburgh Medical and Surgical Journal*, July, 1844.

** *Ibid.*, January and July, 1844.

too great for him to move, the increase having taken place by the absorption into its mass of various unattached particles of snow with which it was brought into contact ; so with a doctrine, which goes rolling along through the world, and in its course meets with vast numbers of unattached minds—that is, minds devoid of definite ideas on the subject thus brought into contact with them ; and, like the particles of snow, they give in their adhesion. Nor does the parallel stop here, supposing the doctrine to be erroneous ; for, what a hot blast of sunshine is to the snow-ball, free and public discussion is to the doctrine,—namely, the cause of its liquefaction and dispersion.

But, before I enter upon the discussion of this subject, it is important that I should free myself from the probability of misapprehension as to the extent and scope of the remarks I am about to make. It must not be imagined that I have any intention of calling in question the great benefit and advantage, likely to accrue to the health and morals of the lower order of society, by means of the investigations brought about through the instrumentality of a commission of health ; on the contrary, it is because I entertain so high an opinion of the good which might result under the operation of a well-conceived sanitary reform, that I am anxious to bring under the notice of so important a meeting of medical men as the present, what appear to me to be grave errors in matters of fact, and a system of loose logic in reasoning on those so-called facts, and drawing conclusions from them, in as far as relates to the subject now under our consideration, namely, the cause of fever. And, further, it is obvious, that if the advocates of sanitary reform in their publications hold out hopes to the public, that under the system of regulations which they propose, one of the most fearful and fatal maladies incident to man in this country will be no more seen in our cities ; and if, after trial shall have been given to the plans proposed, the promised result does not follow, the reaction in the public mind will be altogether on the other side, and the good which has been effected will be overlooked in the disappointment at the non-realization of that which was promised. Therefore it is that I am anxious to bring forward evidence which appears to me conclusive, that in the nature of things, after the best sanitary regulations that can be adopted, we shall still be liable to epidemics of typhus in our large cities and

towns, especially those wherein extensive manufactories exist; and that it is Utopian, in the present state of our knowledge, to imagine that anything the wit of man can devise, shall have the effect of banishing typhus from our country, until fluctuations in labour, commerce, and the price of provisions shall cease to exist, and every individual of the community shall be so circumstanced as to have within his power a sufficiency of food and clothing to preserve his body in health and vigour; and then the predisposing causes of typhus, as of nearly all other diseases, will have no existence, and we shall cease to have epidemic diseases, because we shall have no class of our population among whom disease can become epidemic.

It is with some satisfaction that I am enabled to quote the following extract from the Report by the Committee of the Royal College of Physicians, Edinburgh, appointed to consider any bills that may be brought into Parliament for the improvement of the health of towns, and the applicability of such measures to Scotland.* The Committee consisted of Professors Alison, Christison, and Gregory, and Drs. Stark and Spittal. The remarks already made, as well as those which are to follow, were written before the Report came under my observation; some of them, indeed, more than five years ago, before the subject had attracted any great share of public attention; it is, therefore, with the greater pleasure that I give the following quotation:—

“The Committee think it necessary to add, that while they fully concur in the importance of the measures in question, they do by no means indulge in any such sanguine anticipations as to their immediate consequences, as are contained in the Report of the Committee of the Health of Towns’ Association, in London, and in other recent publications; and they consider this a point of great importance, because, if the public are led to form such expectations of the result of these measures as cannot be realized in practice, the necessary consequences must be,—*first*, that other measures, which may be equally necessary for the health and comfort of the inhabitants of towns, may be neglected; and *afterwards*, that a little experience of the state of the public health, after other measures shall have come into operation, may cause a general and hurtful feeling of distrust as to the principles on which they are founded. Thus, there is a great risk of the real efficacy of such measures being first over-estimated, and afterwards undervalued, in both cases to the public disadvantage.

“In particular, the Committee cannot concur in the statement (at 10 of the Report above quoted,) that ‘science might secure to the whole population of this country such sanitary measures, involving protection from disease, suffering, and premature death, as are actually extended to the inmates of well-regulated prisons;’

* *British and Foreign Medico-Chirurgical Review*, April. 1848.

where, be it remembered, the diet, clothing, and whole mode of life and intercourse with the rest of the community, are completely under the power of the authorities regulating those institutions.

"The members of this Committee have witnessed too many and too extensive epidemics of fever, and are too well aware of the varying circumstances under which these may be diffused, to entertain any expectation that, by all the sanitary measures here proposed, such epidemics can be prevented from recurring occasionally; or their symptoms and mortality from varying, as they often do, in all ranks of society; or their extension among the poorest people, who will inevitably crowd together, particularly in cold weather, in rooms, the atmosphere of which will necessarily become foul and offensive, be controlled as in a barrack or prison. They very much doubt whether the 'slaughter of the living by the dead,' *i. e.*, the extension of epidemic disease by the retention of dead bodies in inhabited houses, has been a material cause of the propagation of fever in the great towns of Scotland, which are known to have suffered much more from this cause than those in England. And they cannot concur in the general and unqualified assertion, that 'wherever animal and vegetable matters are in a state of decomposition, a poison is generated,' capable of producing fever or other epidemic disease. If this last assertion had been correct, there are many parts of this town in which such diseases would constantly prevail, or at least, always be apt to affect strangers taking up their abode there; whereas it consists with the knowledge of this Committee, that there are no parts of the town constantly liable to such diseases; that although these spread most rapidly and most extensively in the filthiest and worst-aired parts, yet, even in these, they prevail only occasionally; and that when they do prevail, their origin may generally be traced to *importation* from places previously affected, and their extension to the effluvia arising from the *living subjects*, applied directly, or indirectly, to those who become successively affected.

"All that these sanitary measures can effect will not purify the air of the interior of the crowded rooms, inhabited by the dissipated and improvident poor, or by the destitute poor; nor do more than diminish the variety of disease and suffering, which may be ascribed in part to the impurity of that air. And although much may be done by religious and moral instruction, by an enlightened and general system of *improved secular education*, especially as addressed to those who stand most in need of sanitary improvement, and by a well-regulated relief of destitution, to correct the evils which spring from dissipation or from indigence; yet it is only necessary to advert to the great mass of suffering, permanently included under the name of the Irish Poor, in every large town in this country,—to the effects of stagnation of trade in any of the manufacturing districts,—or to the faulty construction of large portions of such a town as this, which are private property, and cannot be rapidly altered,—in order to perceive that any such measures, for a long time to come, can only be partially successful.

"But if the public expectation, as to the effect of these measures, be not raised above a reasonable height; if it be only affirmed that the health of all towns may be gradually, but materially improved; the extension of epidemics in them be restrained; the probability of life, even in large and ill-situated towns, be very considerably increased; and the comfort and happiness of all classes of the inhabitants be materially promoted—the Committee have the greatest pleasure in saying, that they think all these benefits may be confidently anticipated from these measures.

(Signed)

"W. P. ALISON, CONVENER."

The value and importance of the above views must be the excuse for the length of the extract. I need not say anything as to the claims of the Committee to the respectful attention of the medical profession.

I now pass on to consider, as shortly as I can, some of the statements published by the Health of Towns' Commission, First Report, "On the Causes of Disease, and the Means of Prevention," published in 1844. The first person examined was Dr. Southwood Smith; at p. 19, the following question is put to him:—

"You have attached considerable importance to the subject of sewerage, so much as to state that you would be able to indicate the fever-localities by the presence or absence of sewerage. Do you mean by that to assign as a principal cause the absence of sewerage, not, of course, saying it is the sole cause?"—*Answer*. "I conceive the immediate and direct cause of fever to be a poison generated by the decomposition of animal and vegetable matters."

At p. 123, Dr. Duncan, of Liverpool, says:—"Further, it has been observed that when a poor population is densely crowded, a kind of poisonous matter, of a highly contagious character, is generated in the system, affecting with typhus and other fevers, not only those in whom it first originated, but spreading with rapidity amid such a population, from individual to individual, from house to house, and from street to street." The author had been speaking previously of decomposing animal and vegetable matters.

Now, I may observe that the modes of origin of typhus, here spoken of by Dr. Smith and by Dr. Duncan, although at first sight they may appear the same, are in reality entirely distinct and opposed. Dr. Smith conceives the poison to arise from the decomposition of animal and vegetable matter, that is external to the living body. Dr. Duncan speaks of the poison as generated *in* the system. In a former part of this Address I made some remarks on what I believe to be the importance of this distinction.

I now go back to page 3, Dr. Southwood Smith being the respondent. Speaking of what he terms "fever-districts," he says, "The fever-districts of the metropolis are situated in different parts of it; and it is in accordance with ordinary experience to find fever raging in some of these districts at the very time that others are enjoying a temporary immunity from it. In former years I have found, on my personal examination, some localities

in which there was not a single house in which fever had not prevailed, and in some cases not a single room in a single house in which there had not been fever. I observed this particularly in certain localities in Bethnal-green and Whitechapel. Now, during the present year, there has been a very remarkable absence of fever in these, its ordinary seats, while in other districts it has been more than ordinarily prevalent."

"Did you attribute that, in those districts, to bad drainage, or want of water, or some cause of that kind?"—*Answer*. "In every district in which fever returns frequently, and prevails extensively, there is uniformly bad sewerage, a bad supply of water, a bad supply of scavengers, and a consequent accumulation of filth."

Now, although Dr. Smith's answer does not contain a direct reply to the question, still there can be no doubt but that he meant his answer to be taken as an affirmative; let us, therefore, inquire if facts and theory are not somewhat at issue. Dr. Smith states that different districts of the metropolis furnish the fever-cases of different epidemics, and not always the same districts. How does this come to pass, on the supposition that these districts contain within themselves all that is essential for the generation of typhus? The draining and ventilation remain unchanged, the decomposition of animal and vegetable matters goes on as before—why has the effect ceased to follow its presumed cause? It will not do to answer that the epidemic constitution is wanting, its presence, whatever it is, being proved by fever being epidemic in other parts of the metropolis; for I presume no one will argue for the existence of such an epidemic constitution in one part of London, and its absence at the same time in another part. Now this, which seems to me an inexplicable anomaly, on the supposition that the opinions of Dr. Smith are correct, is, on the other hand, confirmatory of the views I am endeavouring to maintain; for all other things are inert towards the origination of typhus until the specific contagion is applied, and the same district is not liable to be the seat of epidemic fever twice within a short period of time, because the same individual is not, for the most part, subject to more than one attack during life; and it would, therefore, require some time to bring into a limited district a sufficient number of fresh subjects to furnish the materials of a second epidemic in that district, even supposing the specific poison to be introduced, or to spring up there.

At page 109, Mr. Liddle is being examined ; he says:—" How the sewers can benefit the health of the poor living in the courts and alleys, I am at a loss to conjecture. The poor are still exposed in the same degree to the pestiferous emanations of the overflowing of cesspools, decayed animal and vegetable matter, and stagnant water, which circumstances are proved, by direct experiment, to be the cause of fever ; and without some legislative measures are speedily adopted to remove these evils, another epidemic similar to the one of 1838 will, ere long, make its appearance."

This evidence was given in 1844, just six years after the epidemic of 1838. During those six years the inhabitants of the district in question had been living under constant exposure to the above-mentioned impurities, without any epidemic fever appearing ; nevertheless, Mr. Liddle attributes, by implication, the epidemic of 1838 to these impurities, and is prepared to attribute any subsequent epidemic that may arise to the same cause. Yet it appears to me that it would be much more in accordance with the principles of sound reasoning, to reject that which is constant and invariable (the impurities mentioned,) in an attempt to fix on the cause of an event (epidemic fever,) which occurs only occasionally, many years elapsing between its visitations.

At page 113, the following question is put to Dr. Aldis:—" What are the chief species of disease you find in the dispensaries, most commonly attendant on the physical circumstances, or upon the atmospheric impurity caused by filth, decomposing animal and vegetable matter, and defective ventilation ?"—*Answer*. "The regularly recurrent diseases are fever-cases, inflammatory affections, and derangements of the intestinal canal. When heat and moisture prevail, I anticipate the occurrence of febrile diseases in various forms. In one place, the same general state of weather will be attended with ordinary fever ; in another place, at the same time, with small-pox."

Here we have the same evidence, in kind and degree, in favour of small-pox being an effect of local impurities, as that fever is, yet I presume there are but few who would choose to affirm the truth of the former proposition, and therefore it would be more just to say, that the local peculiarities caused neither disease, but pre-disposed to both.

Dr. Duncan, in his report "On the Health of Liverpool," an

extract from which has been already given, maintains a somewhat modified view of the subject under consideration, though with some reservation in favour of going the whole length with Dr. Southwood Smith. Dr. Duncan seems to entertain no doubt but that bad drainage and ventilation, combined with an over-crowded population, are sufficient for the generation of contagious typhus; one argument in support of which view he draws from the fate of the sufferers in the black hole of Calcutta, and contents himself with the somewhat vague quotation, that the twenty-three survivors "were said to have been afterwards attacked with a fever analogous to typhus." Now, although I consider the evidence drawn from the black hole of Calcutta, and also from the African slave-ships, to be valueless in this discussion, because I am not aware that it has been shown that true typhus ever occurs in hot climates, still, if the argument had any weight, it would, I presume, tell on the side I am advocating, as it has been shown by Dr. Bancroft and others, that neither the survivors of the black hole suffered from any disease at all similar to typhus, nor that the unfortunate captives of the African slave-ships are subject to any such disease.

At pages 134-5, Dr. Duncan has the following:—"It is still a disputed question among the medical profession, whether the malaria arising from the accumulation of filth from decomposing animal and vegetable matter, such as the contents of ash-pits and cesspools, is sufficient in itself to generate fever. Although there are numerous recorded instances in favour of the affirmative, and many high authorities support that side of the question, I am myself inclined to doubt whether this is an efficient cause of fever, independently of other circumstances. But there is no difference of opinion as to the most important point in this inquiry, *i. e.*, as to the fact of these exhalations favouring, in the highest, the extension of the disease when it has once appeared in a locality where they abound. It is admitted on all hands that, although this condition may not suffice to originate fever, it invariably promotes its rapid extension." I need hardly say that I fully concur in the doubt here expressed by Dr. Duncan.

There have been cases of the so-called origination of typhus, from local causes, put on record so recently as last July, in the *Journal of Public Health* for that month, by Dr. Watson, of Liverpool, in a paper entitled "Instances of the Origin of Typhus

Fever from Defective Sanitary Condition." I feel constrained to make a few remarks on this paper, because the statements are put forth as indisputable facts; and those who do not receive them as such, are looked upon as "persons who have not directed their attention peculiarly to sanitary investigations;" and further, "that it is to deny the whole evidence on which the fabric of medical science rests." The fever is said to have been spotted typhus, therefore, coming within the definition of typhus, which I have already endeavoured to establish. Now, notwithstanding all the confidence with which Dr. Watson has expressed himself concerning these cases, a few words will, I think, suffice to show that they are very inadequate for the support of the doctrine in question. The two cases occurred at the house of a baker in the outskirts of Liverpool; the one in June, the other in September, 1847. There are few occupations more calculated for bringing the individuals who follow them into frequent intercourse with others than that of a baker. This position I need not illustrate. Now, what was the condition of Liverpool at the time these persons were seized with fever? In the summer quarter of 1847, the very time referred to, there were registered 2,227 deaths from fever, and 1,700 cases at once were supplied with hospital accommodation; while, at the same time, about 6,000 were estimated to be under medical care at their own dwellings. And yet Dr. Watson thinks it necessary to hunt up a foul drain to explain why two bakers, at an interval of three months, shall suffer under typhus.

In the commencement of this Address, I stated that the question concerning the cause of fever had been much complicated by the introduction of a popular element into the discussion,—that is, by disquisitions on the subject, by men whose education and habits of thought had not been such as to qualify them for the due appreciation of the elements out of which a judgment has to be formed. The fourth number of the *Journal of Public Health* furnishes a striking example of this fact. Mr. William Lee, an engineer, of Sheffield, has written a paper on typhus fever in rural districts. Mr. Lee, after stating that typhus and other forms of febrile disease generally have an efficient local cause, goes on to say, that "To ascertain these causes of disease, requires a considerable amount of constructive skill and experience of engineering, chemical, geological, and other scientific attainments, quite distinct

from pathological inquiries." These are great words, but they have no meaning; for I have yet to learn that the cause of any disease with which we are acquainted, has been discovered by means of any, or all of the above-mentioned qualifications, apart from their connection with pathological knowledge. The facts furnished by Mr. Lee are much of the same order as we generally meet with. Fever exists in a locality; something offensive is found near at hand, and that is put down as the cause of the fever.

Before quitting this part of my subject, I may call attention to a very able article in the eleventh volume of the *British and Foreign Medical Review*, wherein the earlier reports on the sanitary question are passed under review, and a kind of argument adduced against their conclusions, which I have not touched on, not because I do not consider it an important argument, but because it has been so fully insisted on in the article referred to. It is chiefly drawn from the writings of Parent Duchatelêt, regarding Montfaucon, near Paris, where there exists, perhaps, a greater amount of decomposing animal matter than in any other place in the world, and yet no fever occurs among the work people engaged there, nor among their families, who live in the immediate neighbourhood. This is good negative ground for denying the potentiality of such causes elsewhere, and, so far as I know, has never been properly met by those who take an opposite view of the question.

Now, it will be observed that I have spoken throughout this Address of one form of fever, the characters of which I laid down at starting, and it is the power of originating this form alone that I have denied to be possessed by local and occasional causes. That there are febrile disorders capable of being produced by local causes I do not entertain any doubt; all I deny is, that any such disease, of undoubtedly local origin, ever possesses the characters of true typhus. With those who hold a contrary opinion properly lies the burthen of proof: but something more in that way is required than the vague statements, some of which have been passed under review in the last section of this paper.

Professor Christison has described, in the first number of the new series of the *Monthly Journal of Medical Science*, the most clear and unequivocal example of febrile disorder, arising from a local cause, with which I am acquainted. The original paper is

so easy of access to all who take an interest in the inquiry, that I shall merely state some of the leading facts connected with this very interesting narrative. The disease broke out in a farm-house, in Peebleshire, in a thinly-peopled rural district, far from any other dwelling, and as favourably situated for the purposes of health as it well could be. The number of individuals who suffered under the malady was fifteen, which included all those who either resided in the house, or had occasion to be much in it during the day; six of the number resided night and day in the house, and these, without exception, suffered most severely from the disease; three of the six cases proved fatal, while none of the remaining nine, who slept out of the house, died. In neither of the cases did any eruption on the skin exist, such as has been spoken of as characterizing typhus. Nine of the individuals who suffered under the disease, lay, while ill, in cottages remote from the farm-house, but in no instance did those in attendance on the sick, who did not frequent the farm-house, suffer under the malady.

The peculiarities of the disease are pointed out in the following words by Dr. Christison:—"It appears that in the whole fifteen cases, the symptoms in their nature and succession were generically the same, and with but few specific peculiarities in each. The leading symptoms were those of great gastro-intestinal derangement, nausea, vomiting, loathing of food, an excessively loaded tongue, and obstinate constipation; the accompanying fever was slight, and in its type adynamic. Exhaustion of the nervous system, without any particular cerebral oppression, except in the single case of the servant girl, was the principal consecutive danger incurred; and in no case was there detected any trace of the petechial eruption, so general for some years past in the infectious typhus of this country."

Dr. Christison says, regarding the first case he saw:—"I was struck with the physiognomy of the disease, as presenting something very different from that of ordinary infectious typhus at the same stage, the countenance being pale, the eye lively, the expression natural, and by no means oppressed, the mind clear and alert, and the strength far from so prostrate as it is usually observed in early convalescence from our late epidemic typhus."

In only remains to state, as regards the cause of the disease, that on examination, the house-drains were found completely

choked, and that on their being opened, an intolerable effluvium was perceived by those around; for the evidence that the choked drains were the cause of the disease, I must refer to the original paper; it is sufficient to satisfy my own mind. As regards Dr. Christison's paper, I may remark that it has lately been referred to by Dr. Guy in support of the local origin of typhus; whether he or I have given the truest account of the bearing of the facts detailed in that paper, I must leave to others to decide.

In a paper by Dr. Hudson, "On the Poison of Fever," several instances are noticed from the writings of Drs. Christison, Cheyne, Addison, Armstrong, and others, and also some that had come under his own observation, where there were good grounds for believing that a febrile disorder had arisen from a local cause; but in none of these cases does it appear that any eruption on the skin existed, or that the disease spread by means of contagion. I am prevented, by want of space, from citing any of these cases at length.

I stated, in a former part of this Address, that at its conclusion I would return to the question, whether typhus ever spreads in any other way than by contagion. We shall never be in a position to discuss this subject with advantage, until medical men, in their reports on fever, shall agree as to the distinctive characters of different fevers; until the practice ceases of calling every disease that presents a quick pulse, a hot skin, and a dry tongue, fever, or typhus, or continued fever, according to the whim or habit, or any other like motive, of the writer.

It is not the object of my remarks to attempt to prove that typhus never arises in any other way than from pre-existing contagion; for, if the existence of the disease could be traced ever so far back in the history of the world, still it must, at one time or other, have had a commencement; and we are not justified in denying that the same causes that were sufficient to originate the poison of typhus, in the first individual of the human family that ever suffered under the disease, may in our own day come into operation. In this particular, typhus occupies precisely the same ground as small-pox, measles, and scarlet-fever; and, however interesting such speculations may be, they are not of much importance to the practical physician. He wishes to ascertain the

causes on which the spread of such maladies in the epidemic form depends, and he finds that while there are numerous circumstances which predispose the individuals of a community to fall into disease—such as insufficiency of food and clothing, depressing passions of the mind, and impure air, still, that the only exciting cause worthy of his attention, is the communication of the disease from the sick to the healthy, and therefore that there is no immediate object so worthy of his attention as the early separation of the sick from the healthy; and next to that, it becomes his duty to devise means for the removal of the predisposing causes. But it is a fatal mistake, if he thinks this has been accomplished when he can enter the dwellings of the poor without offence to the delicacy of his olfactories. He has then only effected the removal of one out of many evils; and but little good will follow in his steps if he leaves behind him, unmitigated, the hopelessness of destitution, with its physical and mental depression. It seems to me little better than a mockery to talk to a man about pure air when he wants bread. The room he lives in, and not the man, seems to be the grand object of the present day. Now I would have the condition of the man attended to first, and his room afterwards.

In concluding a paper, the object of which is an inquiry into the causes of an important disease, it cannot be out of place to remind the members of the medical profession of the first rule of philosophising laid down by Newton,—“*Causas rerum naturalium, non plures admitti debere, quam quae et vera sint, et earum phaenomenis explicandis sufficient.*”

The foregoing paper having been arranged chiefly with reference to its delivery in the form of an Address, is not so complete in its evidence and illustration as the Author could desire, on submitting it to the profession in the published form. It is therefore his intention to publish an Appendix in the next volume of the *Transactions*, which shall furnish such further materials as he deems requisite to a full exposition of the subject of the Address.

W. D.



