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Contributors

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SOME CONSIDERATIONS
 ON THE
 NATURE AND PATHOLOGY
 OF
 TYPHUS AND TYPHOID FEVER,
 APPLIED TO THE SOLUTION OF THE QUESTION OF THE IDENTITY
 OR NON-IDENTITY OF THE TWO DISEASES.

BY ALEXANDER P. STEWART, M. D.

(Read before the Parisian Medical Society on the 16th and 23d of April 1840.)

(From the Edin. Med. and Surg. Journal, Vol. liv. No. 145.)

It is my object, in the following pages, to attempt, by a reference to facts, the elucidation of a question, which is confessedly surrounded with difficulties, and has called into the field of controversy some of the most gifted men of the present day,—some of them espousing one opinion, some another, while others still suspend their judgment, till the production of new facts and reasonings shall have dissipated, in part at least, the darkness in which the subject is involved. It is, indeed, not a little remarkable that, in a country like Great Britain, where typhus sacrifices yearly its thousands of victims,—where it is all but universally admitted that the local lesion of the intestinal follicles is not a characteristic sign of the disease,—and where, on that very ground, the doctrines of Broussais have never been able to establish themselves, there should still be wanting any standard work upon the nature, symptoms, and necroscopic appearances of typhus, such as those of Chomel, Louis, and others, who, on this side the channel, have achieved the triumph of pathological science in a class of diseases, of which the anatomical lesions were so long unknown. Hence the uncertainty that prevails in France in regard to the question I have chosen for consideration—a question which, beyond a doubt,

has been for a dozen years, and is still, exciting an interest here, to which British medical men are comparatively strangers. Hence the tours of inspection undertaken from time to time by distinguished continental physicians, with a view either to weaken the conclusions that may be drawn from facts already published, or to dispel a scepticism which these facts, so limited in number, have not been able to overcome. Hence also the objections naturally enough raised to the information collected by them, on the ground of defective opportunities, hurried observation, and imperfect knowledge. Hence, in short, such reflections on the doubtful character of the information received from English authors, as are made by Chomel in his Lectures on Typhoid Fever, those of MM. Gauthier de Claubry and Montault, in their prize essays on the Analogies and differences between Typhus and Typhoid Fevers, and the following words of Valleix,* in apology for citing American instead of English authors, in reference to fevers prevalent in Great Britain. "We admit that it would have been better to have presented a series of observations on typhus made in England itself; but where can we find them?"

These observations will derive great additional weight from a brief consideration of the work of Dr Tweedie, entitled *Clinical Illustrations of Fever*, which is referred to by most of the French writers on the subject, and whence a great part of their ideas on English typhus is drawn. I confess I have been puzzled, in perusing the above named treatise, to find out what definite meaning the distinguished author attaches to the word "fever;" whether he understands by it a symptom produced by a local affection, or a distinct disease arising from a specific cause. His valuable remarks upon contagion, and his strictures upon the doctrines of Broussais, would lead to the latter, while the cases detailed give countenance to the former supposition. I cannot better or more shortly give an idea of the cause of my difficulty, than by giving an analysis of the cases referred to.

Of 73 patients who died, 54 were inspected. Of the latter number, 10 seem referable to typhus and 17 to typhoid fever. Of the remaining 27, I find that 4 died of well-marked and extensive meningitis, 1 of phrenitis, 2 of erysipelas of the head and neck, 5 of phthisis, 4 of pleuro-pneumonia, 3 of pneumonia, 2 of pleuro-pericarditis, 1 of pleuritis, 1 of pulmonary apoplexy, 1 of abscess of lung, 4 of peritonitis. Two of the above cases were sequelæ of scarlatina and small-pox. Of those cases which were not inspected, there are, as far as can be gathered from the brief account of symptoms, 5 of typhus, 3 of typhoid fever, 3 of meningitis, 2 of pneumonia, and 1 of phthisis. Of several, it is stated that they were evidently cases of purely head, chest, or abdominal affections, and in one,

* Arch. de Medecine, 3d series, Vol. iv. p. 216.

which was proved by dissection to depend on intus-susception and gangrene of a considerable portion of gut, it is remarked, that "the symptoms were referable to some obstruction in the intestines, rather than to any form of fever." In the above summary, I have been careful to exclude those that were evidently, or appeared to be *sequelæ* of continued fevers, and to include those only in which the feverish symptoms clearly depended, throughout their course, on local affections. Deducting, however, those I have alluded to, and which form rather more than the half of the cases detailed, there still remain 35, more or less clearly marked out as belonging to two kinds of specific fevers, the one differing from the other in the extraordinary gravity of its symptoms, the rapidity of its course, and the absence of local lesions.

This leads me to remark, in concluding these preliminary observations, that when I began, in 1836, the practical study of fever, I was much struck with the simultaneous occurrence, in the wards of the Glasgow Fever Hospital, of two sets of cases, in which the symptoms (however little most of them might seem to differ, when viewed individually,) presented, when taken collectively, characters so marked as to defy misconception, and to enable the observer to form, with the utmost precision, the diagnosis of the nature of the disease, and the lesions to be revealed by dissection. More particularly, it was remarkable to observe that while, in the one disease, the affection, in those who presented no eruption, was so slight and of so short duration, as to make it very questionable whether it deserved the name of typhus, and while the fatal cases presented an abundant and generally a profuse eruption; those labouring under the other, which equally, and even in a much higher proportion, went on to a fatal termination, rarely presented any, and then only a very scanty eruption. It was further remarkable, that while in the one several successive patients had either been restored to health, or fallen victims to the severity of the affection, the disease, under which those laboured, who lay side by side with them, though characterized by much less urgent symptoms, pursued its gradual course through weeks and months consecutively, and in the majority of cases to a fatal issue. And finally, it was more remarkable still, that, to complete the contrast already so striking, dissection proved the existence, in the one disease, of most extensive local lesions, in the other, the absence of all prominent local lesion whatsoever.

Another circumstance, I must not forget to mention, viz. that though, during the summer and autumn of 1836, the cases of typhoid fever were numerous; from the month of November in that year, (at which time both the type and the amount of typhus became much more formidable,) till June 1838, the period that my connection with the hospital ceased, not more than a dozen cases—if there

were even so many, and these at long intervals, were admitted for treatment. In speaking of the two varieties of fever as totally different diseases, before going into the proof of the assertion which is the point in dispute, I have merely employed a phraseology in common use, of which even those who maintain the complete identity of the affections have not been able to rid themselves, and whereon, if I were disposed to insist upon words, I might find a strong argument in support of my position; and have expressed a conviction forced upon me by the consideration of facts and reasonings, which I now proceed to unfold.

It is far from my intention, in this paper, to take up in all its bearings the vast subject of the "analogies and differences between typhus and typhoid fever." That task I hope to be able, along with my friend and fellow-worker, Dr Anderson, to undertake hereafter. My present object is to develop, as shortly and as clearly as I can, some of their leading features of difference, in support of the position I have taken up. I propose, then, to consider, 1. The probable origin of the two affections; 2. Their proximate causes; 3. Their course; 4. Some of their symptoms; 5. Some of their anatomical lesions; 6. Their treatment.

1. Without entering upon the abstruse question of infectious germs, or discussing theoretically the probability of the opinion that ascribes the origin of continued fever to animal, as intermittents are known to proceed from vegetable miasmata, I proceed at once to examine the question in a practical point of view. Dr Copland, in his able article upon Infection, places exanthematic typhus under the third class, viz. that of "specific infections," characterized as "immediately or mediately disseminating and perpetuating their kinds," &c. and directly refers that form of typhus to "emanations from the secretions, excretions, and surfaces of persons already affected." Under the second class, viz. of "conditionally perpetuating or contaminating infections," he places "animal effluvia," or more particularly, "emanations from living bodies in close and unventilated situations." Among the "diseases resulting therefrom," stand "adynamic, putro-adynamic, and malignant fevers." Now, in this classification, it is plain that while we are informed of the origin of the latter, we are only told the mode of propagation of the former. Exanthematic typhus is said to be the result of "emanations from persons already affected;" but whence the original disease? In reference to this question, I observe, that Dr Roupell* had given the weight of his name to the opinion that exanthematic typhus does not originate from vitiated air, extreme filth,

* Roupell's Treatise, &c. p. 64, quoted at p. 107. Vol. xxxi. of Med. Chirurg. Review.

closeness, &c. I cannot but express my surprise at this opinion, as the amount of evidence against it seems quite overwhelming.

On no two points, (and so far I must anticipate my remarks on the eruption in typhus,) are the distinguished authors who describe the frightful epidemics of army typhus during the last six centuries more completely agreed, than on the influence of impure air in the production of the disease, and the very frequent presence of the rubeoloid exanthema in those affected with it. Dr Tweedie, in his interesting observations on the Influence of Impure Air on the generation of Typhus, refers to the opinion of Sir John Pringle, who states, "that he has observed the hospitals of an army, not only when crowded with sick, but at any time when the air is confined, and especially in hot weather, produce fever of a peculiar kind, which is often mortal; and he remarked, that the same thing arose in full and crowded barracks, and in transport ships, when filled beyond a due number, and detained long by contrary winds, or when the men had been long kept at sea under close hatches in stormy weather." He also refers to the experience of Mr Pearson, who, he says, "told me, that when he was surgeon of the Lock Hospital, he uniformly observed, when more than a certain number of patients were placed in any of the wards, fever became prevalent in the establishment; and that, from repeated observation of this fact, he was induced to restrict the number of beds in each ward, and never afterwards witnessed the recurrence of fever in the house." It appears to me that the instance cited by Dr Roupell, (at p. 54 of his work,) of fever coming on, in the Refuge, open in 1830-31, on the north side of the Thames, at a time when the wards were crowded to excess, might alone suffice to cast strong doubts on the correctness of his opinion.

If we consult the details given from p. 7 to p. 42 of Gauthier de Claubry's elaborate treatise, of various epidemics of typhus, described by such men as Pringle, Roux, Hufeland, Ducastaing, Reveillé Parise, Tresat, Tort, Gilles de la Tourette, Laurent, Ardy, Magnin, &c. and his summary from p. 100 to p. 114, we find the unanimous voice referring the generation of the disease to vitiated air, arising from the crowded state of the hospitals, the carnage among troops and inhabitants, and the want of burial; the scarcity of provisions, depressing passions, exposure to cold and fatigue are likewise more or less frequently mentioned; but we are entitled to regard these as merely accessory; for we have already seen the developement of typhus where these conditions were wanting. Montault, from an extensive induction of facts, likewise comes to the same conclusion; Copland,* expressly points out the influence of the circumstance in question, in producing "highly infectious and typhoid forms of fever."

* Art. "Infection," p. 352.

How, indeed, is it possible, except on this supposition, to explain the appearance of typhus in a country, and in the midst of armies, perfectly healthy, till the developement of the conditions in question, but which, under their influence, were desolated by a pestilence, whose ravages, more frightful than those of war, consigned many, whom the sword had spared, to "an ever-yawning and never-satisfied grave?" We cannot, in order to account for this, admit the idea, that, by some strange and inexplicable principle of election, infectious miasmata direct their course from distant shores to the seat of war, especially when we see in their invariable antecedence and sequence, the relation of cause and effect so clearly made out between the conditions referred to, and the appearance of the disease. What reasonable cause can be assigned for the murderous epidemic that arose, in the spring of 1810, on board the Plymouth prison-ships, and cannot be referred to contagion or infection, as the disease first declared itself among the prisoners confined in what have been well termed "these floating tombs," if not the very natural one of the long continued confinement of hundreds of persons, in a situation uniting all the conditions most adverse to health? These considerations are borne out by the undisputed fact, that the type of fever becomes much more malignant in hospitals crowded to excess, and by Gauthier de Claubry's curious statements at p. 148 of his work, where, speaking of the treatment of typhus, he mentions that, during "the wars of the empire," patients previously despaired of, notwithstanding the assiduous use of therapeutic agents, were quickly restored to health, on the evacuation of the hospitals, with fresh air, barley-water and lemonade. How completely does this agree with the experiments of Pringle, as quoted above. Let me further refer to a paper by Dr Peebles, in the forty-fourth volume, (No. 125) of the *Edin. Med. and Surg. Journal*, as detailing the results of his own experience in Italy, and giving a remarkably clear, succinct, and comprehensive digest of the mass of evidence bearing on this interesting subject. Dr Alison's statements in his recent pamphlet on the *Scottish Poor Laws* fully confirm the general opinion. I have only to add, in again partly anticipating details, to which I shall presently advert, that 22 of 30 patients who positively declared to me that they had not in any way been exposed to infection, replied as follows to my inquiries respecting the number who lived together. In three cases there were 4; in seven 5; in two 6; in three, 7; in two 8; in one 9; in one 10; in one 12; in one 13. Many of those, who traced their affection clearly to contagion, stated the number of those who lived together as 12 and 13, and in one or two instances, as "16 at least." This, be it recollected, in one, or at most two small rooms, in the most miserable, darkest, and worst aired purlieus of a large manufacturing city. After the facts I have adduced, I think we may safely conclude, that "animal effluvia," to use the term of Dr

Copland, or "emanations from living bodies in close and unventilated situations," possess the property of generating the specific poison, whatever be its nature, that gives rise to exanthematic typhus.

With regard to the producing cause of typhoid fever, all is vague and uncertain. Chomel, after his analysis of the causes assigned by patients, remarks, (p. 306), "Thus, scarcely, out of a number of patients so considerable, (115), is there one in four who attributes his state to some slightly energetic cause." But what chiefly concerns us to know is, whether the conditions we have seen to be so powerful in the production of typhus are the same, or anything like the same, in regard to typhoid fever. Exactly on this point, Louis remarks,* "No more can the dwelling in places, low, and inhabited during night by too great a number of individuals, figure among the causes in question, one-eighteenth only of the patients being in that condition;" and he concludes, from a comparison of all the commonly assigned causes with the facts ascertained by himself, in these remarkable words; "The deepest darkness then prevails regarding the causes of the affection under consideration." Chomel likewise (p. 300) uses almost the same words in beginning his third article, "The causes of typhoid fever are wrapt in the greatest obscurity." It were difficult to find a more striking instance of the little effect of the circumstances we have mentioned, in producing typhoid fever, than the epidemic at Bischofsheim, in the department of the Lower Rhine, in August, September, and October 1832, described by Ruef in the *Gazette Medicale* for 1834, and referred to by Gauthier de Claubry, who maintains the identity of that disease and typhus. (P. 133, et seq. of his essay.) The disease first showed itself, without suspicion of being transmitted, in the upper, best aired, and, as is expressly said, "the most healthy part of the village," which is situated partly on and partly at the foot of a rising ground, and spread successively to the middle and lower quarters. Again, the result of Lombard's researches, conducted at Geneva throughout several successive years, is, that the cases of typhoid fever are numerous during the autumn, and comparatively few during the winter months; whereas the ravages of typhus, as every one knows, are most formidable during winter.

II. I come now to consider the proximate causes of the two diseases. To avoid all ambiguity in the use of the terms, infection and contagion, which have caused, perhaps, a greater war of words than any two terms in any science, I shall merely state, that with Dr Copland, I take infection in its classical sense, and understand by it the imparting of any taint, pollution, or contamination what-

* Vol. ii. p. 457.

soever, to a person previously in health. By contagion, I understand a mode of infection, viz. infection by contact. As applied to the subject of fevers, it will at once be seen that infection has not merely a reference to disease, but to miasmata, or specific poisons, independent of disease, to the contaminating agent, as well as the individual contaminated, and communicating disease. In this point of view, the question is set at rest by the remarks offered under the first head. Hence, we consider typhus as a truly and eminently infectious disease. The same, we have seen, cannot be said of typhoid fever. The question, then, we have now to consider, reduces itself into one of transmission or propagation from a diseased to a healthy person. The difference, in this point of view, between infection and contagion is one of degree, and not of kind; and there are cases of morbid impression, which it would be very difficult, if not impossible, to refer either to the one or to the other, so imperceptibly do they glide into each other. It is natural, however, to conclude that, as the genus includes the species, so a disease that is first produced by the action of a specific infectious agent on the animal economy, must be directly transmissible from a person diseased to a person in health. To argue at any length in favour of the transmission of typhus, whether through the medium of the atmosphere, or by the direct inhalation of the morbid matter from a person labouring under its effects, would be a waste of time. It is admitted all but universally. The fearful rapidity with which it extends its ravages; the great number of cases in which patients refer their complaint to living with, or attending others affected with it; the all but universal occurrence of the disease in nurses and resident medical attendants, put beyond a doubt the communication of the disease. Lassis, shortly after publishing his ingenious work, *Recherches sur les Veritables Causes de Typhus, ou de la Non-contagion des Maladies Typhoides*, fell a victim to the contagion, the existence of which he denied. Need I further refer for the proof of this position to the names of Sydenham, Morton, Pringle, Huxham, Hildenbrand, Chomel, Tweedie, Roupell, Gauthier de Claubry, Montault, Lombard, indeed to almost all the writers on the subject? The facts stated by Tweedie, from p. 85 to p. 99 of his treatise, by Gauthier de Claubry, (p. 100 to 114), and by Dr West in his able paper on Exanthematic Typhus, in the fiftieth volume of the *Edinburgh Medical and Surgical Journal*, are conclusive as to the communication of typhus to medical attendants, nurses, and patients labouring under other complaints, and they are completely borne out by my own experience. It is well known, that, for many years past, every resident clerk in the Glasgow Infirmary, with very rare exceptions, many students who frequented the fever

wards, several of the acting physicians, and almost all the nurses, have, at one time or other, been attacked with typhus, and that not a few have fallen victims; and Dr Cowan, under whom I had for a long period the honour of serving, in his pamphlet, entitled *Statistics of Fever and Small-pox in Glasgow*, says, (p. 10), of the district surgeons, "Few of those gentlemen escape an attack of fever." Dr West's statements as to the spread of the disease among patients admitted for other complaints to St Bartholomew's, but placed in the same wards with others affected with typhus, and the necessity of at length closing the ward, show the groundlessness of the objection that contagion arises merely from concentration. During the winter of 1837-38, an isolated case of typhus in one of the medical wards of Glasgow Infirmary, communicated the disease to most of those in the same ward, and several died. The following are the results I obtained from the statements of 139 patients, whose cases I investigated with particular care in the years 1837-38. Much more extensive researches subsequently made by Dr Anderson and myself, the details of which, unfortunately, I have not with me, will, I doubt not, bear out this statement.

	Doubtful or cause unknown.	From in- fection.		{ Typhus in family.	In same house.	Conta- gion.
Males,	21	- 39	of	27	- 8	4
Females,	18	- 61	whom	42	- 12	7
	<hr/> 39*	<hr/> 100		<hr/> 69	<hr/> 20	<hr/> 11

I have already spoken of the conditions in which 22 of the patients were placed who denied the influence of infection in the production of their complaint. Gauthier de Claubry remarks, (p. 113,) that it requires "some degree of medical courage" to uphold the propagation and perpetuation of typhus by specific infectious germs, according to Hildenbrand. On the supposition of its identity with typhus fever, this may be so; the difficulty vanishes on supposing them different.

What is the state of the case in typhoid fever? It is well known, and stated both by Chomel and Gauthier de Claubry, that the all but universal opinion in France is against its being infectious. Louis does not even mention it; Andral (p. 728, English edition,) gives his verdict against it; Montault (pp. 12 and 186,) recapitulates the arguments against it. On the other hand, MM. Bretonneau, Gendron, Chomel, Lombard, and Gauthier de Claubry, maintain, more or less decidedly, the contagious nature of the disease. It is rather amusing that the last named author, while arguing for the contagion of typhoid fever, in order to prove its

* Of these 39, there were 30 who positively denied contagion, 6 are not marked, and 3 were doubtful.

identity with typhus, adduces the contagion of the latter in the English hospitals, in support of his position, while Montault, holding the difference of the two affections, feels constrained, in order to get over an imaginary difficulty, to express a doubt (p. 12,) as to the cleanness and salubrity of the British, as compared with the French hospitals, and to assert that "the English physicians are not perfectly acquainted with the anatomical alteration of typhoid fever;" while Gauthier de Claubry, in order to get over the real difficulty arising from the absence of anatomical lesions in the Glasgow and Edinburgh typhus, speaks of the "serious circumstances of misery and exhaustion of strength, which have so deplorably modified the constitution of the inhabitants of these wretched countries, Scotland and Ireland." Those who know the admirable cleanness and ventilation of our fever hospitals, and the acquaintance of our medical men with the lesions referred to, will readily dispense with any serious refutation of such gratuitous suppositions; and the too sweeping assertion of Gauthier de Claubry, as we shall afterwards see, (though partially borne out by the recent statements of Dr Alison, regarding the condition of the lower classes in the large cities of Scotland,) even were it admitted in its fullest extent, proves nothing in his favour. Chomel's third conclusion (p. 339,) is, that, if the identity of the anatomical lesions in the two diseases were proved, the question of contagion would be set at rest. We shall see hereafter what is the testimony of facts in regard to the lesions in typhus. Meantime, let us consider the facts in reference to contagion.

Louis, I have mentioned, does not allude to the subject. From this we might conclude, that he had found no facts in support of the theory of contagion, for we can scarcely suppose that an observer so scrupulously accurate would neglect so important a branch of his subject; but we prefer an appeal to positive facts. Chomel, we have already seen, states how very few patients ascribed their illness to any energetic cause. Five only out of 115 were placed "in circumstances favourable to contagion," 79 could "assign no appreciable cause," (p. 306.) In accordance with this I may state, that in no case, though questioned with the greatest care, either in Scotland or in the hospitals of Paris, have I ever found the disease referred to contagion. On the other hand, it appears from the statements regarding the epidemics of Nancy by Leuret, of Chateau de Loire by Gendron, of Andlau and Stolzheim by Mistler, of Bischofsheim by Ruef, and those contained in Bretonneau's memoirs; (commented on by Gauthier de Claubry, from p. 118 to 135,) and from the clear statements borne out by dissections of Lombard, (in his valuable paper upon the Geneva epidemic of 1835, inserted in the *Gazette Medicale*,) seem to prove the possible transmission of typhoid fever, when epide-

mic. Without hazarding any decided conclusion on a point still so keenly debated, I would simply direct attention to the differences as regards the probable origin and propagation of the two diseases, which the foregoing facts appear to establish; and also to the interesting question, whether, if typhoid fever really depends on, and is propagated by a specific poison, that poison is, or is not generated in the same circumstances as the infection of typhus?

III. There are circumstances connected with the course of the two diseases well worthy of attention. The fact, as undoubted as it is remarkable, that the mean duration of typhus is about one-half that of typhoid fever, is one that perplexes considerably the advocates of their identity. Chomel argues (p. 337,) that it only shows a difference of intensity. Gauthier de Claubry, who very judiciously does not venture to face the statistical details on the subject, and treats of it only under the head of the relative intensity of the two diseases, meets the objections on the same ground as Chomel. I have already remarked the extraordinary difference of the duration in the Glasgow epidemic of 1836. Hildenbrand states the mean duration of typhus at about 22 days, and that of the Glasgow typhus, from the results of many thousand cases, during successive years, approaches very nearly to this result, being about 21 days. An analysis of my own cases gives me a mean duration of 20.82 days, or deducting three cases, one of which terminated fatally from phthisis on the 42d, another not convalescent from consecutive paralysis on the 60th, and a third restored to health about the 100th day, after severe attacks of glossitis, pleuro-pneumonia, and a variety of other complaints, 19.6 days. Valleix, in his third memoir on Typhus and Typhoid Fever,* states the mean duration of six cases of the former, and seven of the latter, (observed in London by Dr Shattuck of Boston,) as respectively $24\frac{1}{2}$ and $22\frac{1}{5}$ days. To account for this discrepancy with the unanimous voice of other authors, it is enough to refer to the small number of cases, and to state that the cases of typhus were of great severity, (five out of six having died,) while those of typhoid fever were very mild, and only one in seven terminated fatally. It appears, on the other hand, from Chomel, (p. 44,) that three-fourths of the cases cured began to show signs of amendment, only between the 15th and 30th, and ~~18~~ of the whole number only between the 31st and 40th days. Louis again, (Vol. i. p. 9, 13,) mentions the duration in cases of death to be from eight to beyond 40 days; in serious cases which were cured, from 15 to beyond 50; or on an average 32 days; while that of the light cases was 28 or 29 days. Montault (p. 22,) states, that of 63

* Arch. de Medecine, 3me série, T. vi. p. 135—138.

cases observed by him, the mean duration was 36 days. The mean duration of nine cases of all different shades of intensity, very carefully watched by myself, chiefly in the wards of M. Chomel in the Hotel Dieu, is 38.7 days. One of these, admitted on the 13th of last December, has not yet terminated. I take the duration of the fever from his first convalescence, (on the 75th day,) after which he had a relapse without any marked local affection, and his recovery is very doubtful. M. Chomel in his summary on the 22d of last March, of all the fever cases (25 in number,) that had occurred during the previous six months, stated

The mean duration of the very serious cases as	- 31 days.
those of moderate intensity, as	18
the lightest cases, as	- 10

giving an average for the whole number of 19.6 days. He, however, called particular attention to the fact, that the cases were much less numerous, and the affection greatly milder, and much less fatal than usual, the mortality, commonly 1 in 3 or 4, being only 1 in 12.6.

Being firmly persuaded that the remarkable length of typhoid fever often depends upon conditions not commonly adverted to, I beg the reader's indulgence while I go into some details on the subject. What I wish to establish is, that typhus, when uncomplicated with any secondary affection, terminates in convalescence or death in the course of a single attack; in other words, that a second attack of typhus does not occur in the course of one and the same illness. On the other hand, it shall be my object to prove that such is not the case with typhoid fever.

With respect to typhus, I allége generally, notwithstanding the high authority of Dr Copland, who maintains directly the contrary,* that, (apart from secondary complications, which often completely change the course and termination of the disease,) however long may be the period of excitement,—however long the adynamic stage,—however tedious the period of convalescence, I have never, among thousands of cases, seen a single case of relapse, in the proper sense of the term, after the symptoms had begun to decline. Is this objected to as vague? I appeal to the following facts. Of 139 cases, examined during the course of the disease with great minuteness, I find that 119 presented no secondary complications. Of these 20 died at the height of the fever, the symptoms having presented a constantly increasing intensity till the fatal termination. The remaining 99 offered, without a single exception, a steady aggravation of all the symptoms till a certain period, (more or less distant, according to circumstances, from the beginning of the disease,) after which they ex-

* Dictionary, p. 1012, Art. "Typhus Fever."

hibited as steady a decline. The remaining 19 were complicated in varying proportions with phthisis, pneumonia, chronic bronchitis, hemiplegia, erysipelas, bed-sores, diarrhœa, &c. to one or other of which diseases four fell victims, the secondary affection generally declaring itself after a temporary amendment.

Let us now inquire into the state of the case in typhoid fever. I find Montault, (p. 18,) when treating of convalescence, remarking, that "relapses are easy" in that period. Louis,* speaking of the same period, uses these words: "Yet several of them (the healthy functions) were re-established very slowly in several individuals, and convalescence was thereby retarded. The heat was more or less great; the pulse continued more or less quick; the diarrhœa persisted, though it could not always be referred to imprudences in diet." This is all I have been able to find in the various authors I have consulted. I shall now cite several cases in point.

On the 26th of February 1838, a young man, 17 years of age, was admitted to the wards of the Glasgow Fever Hospital. He had been ten days ill with most of the symptoms of follicular enteritis. On admission he had a few rosy spots on the belly, very red and papillated tongue and fauces, circumscribed flushing of cheeks, disturbed sleep, tickling cough, extensive bronchitis, dull pain, tumefaction, and gurgling sound in the belly, with moderate diarrhœa. He had epistaxis on the 12th, 18th, 19th, and 23d days of the disease, and on one occasion to the extent of one or two pounds. The eruption became pretty copious, then declined, and on the 30th day disappeared. At that time the pulse, which had ranged between 104 and 90, fell to 80 and 76; the bronchitis all but disappeared; the tongue, which had been excessively red, ~~chopped~~, and apthous, became smooth and less florid; the gurgling noise less; and the appetite good. On the 20th March, (the 32d day,) he had rigors, continued on the 21st with rise of pulse to 104, and followed on the 23d by pretty copious eruption on breast and back, by return of thirst, clamminess of tongue, loss of appetite, sibilant ~~whoze~~ *rales* in chest, renewed tenderness of belly, frequent bilious vomiting, continued for five days, and epistaxis for six days running. The symptoms followed their regular course, and the eruption had nearly disappeared, when, on the 4th of April (47th day), a fresh, elevated, lenticular eruption appeared on the chest and epigastrium. The only altered indication after the appearance of this eruption was a temporary rise of pulse from 92 to 104. On the 11th of April, (54th day,) he was dismissed by his own desire; and I heard some time afterwards that he had rapidly convalesced after his removal.

The phenomena observed in this case scarcely require any com-

* Vol. ii. p. 11.

ment; and I therefore proceed to detail others that have fallen under my notice.

On the 9th of last December, a young woman, aged 22, entered M. Chomel's female ward, the Salle St Augustin, with symptoms of typhoid fever of ten days standing. The affection was mild, the eruption abundant, the intelligence good. Amendment soon took place. The countenance, previously very languid, became more lively, the pulse sunk to 92, and she seemed rapidly convalescing, when, on the 24th of December, (25th day,)—as I learn from the reports furnished me by my friend M. Gueneau de Mussy, to whose experience and kindness I am much indebted—the pulse rose to 108. There were marked and sudden prostration, pain, and crepitant rattle in the right subscapular region; deafness, which had been considerable, became complete; stupor and involuntary discharges came on after some days, and death followed on the 28th of December. Inspection disclosed the following appearances. Incomplete splenisation of lower part of right lung; diseased aggregate glands at the lower part of the ileum, some ulcerated, some going on towards cicatrization, others not ulcerated, and in the state in which they are described about the sixth day of the disease. Who can ascribe the supervention of such serious symptoms as those I have sketched to the inflammation of a small portion of the lower lobe of the right lung? Is it not much more rational to refer them, and to ascribe the morbid appearances, as M. Chomel did in his lecture on this case, to a fresh intestinal eruption of a few days standing.

Again, on the 27th of last February, a young man, aged 21, entered the Salle St Louis in the Hôtel Dieu, on the eighth day of typhoid fever. The eruption appeared next day, became pretty copious, and on the 13th of March, (the 23d day of the fever,) it is reported as all but gone. All the symptoms had diminished greatly in intensity. The pulse had fallen to 76. Convalescence took place. He was eating and relished the half-diet of the hospital, when, on the afternoon of the 31st, the pulse, in the morning 90, rose to 136. Violent diarrhœa came on. The pain of belly, formerly slight, became considerable, and a pretty copious eruption of well-marked "typhoid spots" appeared. The chest, minutely examined, presented nothing unusual. On low diet the symptoms declined; and I find him marked in my notes convalescent on the 4th of April. He is about to leave the Hôtel Dieu.

I refer further to a case detailed at p. 30 of the first volume of Louis's work. The patient had been three weeks ill. During the first fortnight the ordinary symptoms, it seems, were observed; more particularly headach, relieved by epistaxis, during the first week; then gradually increasing diarrhœa and pain of belly.

In the third week renewal of rigors and heat of skin. Appearance of the eruption and recurrence of epistaxis is on the 23d day. These phenomena were followed by violent delirium, terminating in partial stupor, and death on the 26th day. On dissection solitary glands, not ulcerated, in the upper; both solitary and aggregate glands ulcerated in the lower part of the small intestine; "a great number of small tumours flattened, not ulcerated," in the large intestines. Do not the symptoms fully bear out the opinion, that the formation of the tumours described by Louis was coincident with the aggravation of symptoms in the third week, and that the extensive ulceration at the foot of the ileum was the result of disease of much longer standing? The invaluable researches made of late years into the pathological anatomy of this affection by Bretonneau, Petit, and Serres, Andral, Louis, and Chomel, would necessarily lead to this conclusion. And, in point of fact, Louis says, (p. 37,) that they were "most probably developed at the same time as those in the first half of the small intestines, being, like them, not ulcerated." Now, according to the generally received theory, the glands in the upper half of the small intestine are developed at an advanced period of the disease.

I conclude my remarks on this head with a reference to Chomel's work. At p. 361 is the case of a patient, admitted on the eighth day of the disease. Prostration, nausea, some epigastric pain, slight diarrhoea, "a few rosy spots," were the leading symptoms. They quickly yielded; the diarrhoea had almost ceased, and a great amelioration had taken place, when, on the twenty-third day, all the symptoms returned, in a much graver form, accompanied by stupor, violent diarrhoea, "inclination to vomit, (it is expressly said), *like that* at the beginning of the disease;"—"slightly marked" improvement on the twenty-seventh and twenty-eighth days; recurrence of same symptoms, with delirium and erysipelas, on the thirtieth day; extension of erysipelas to whole right side of face and neck, and also to scalp. Almost every trace of it had disappeared on the thirty-fourth day, when epistaxis and stupor, approaching to coma, were observed. Death ensued on the thirty-fifth day. Dissection disclosed *a lobule* of gray hepatization in one of the lungs. In the upperpart of the ileum, "patches partially ulcerated;" in the last 15 inches, numerous ulcerations of very small extent, and which appear to depend on the alteration of the isolated follicles; besides, that a great number of the last-named glands present an abnormal volume, without ulceration," (p. 364.)

The symptoms mentioned as recurring on the twenty-third day, after the almost complete cessation of the diarrhoea and other unfavourable signs, cannot certainly be accounted for by the "lobule of gray hepatization," found after death, nor by the erysipelas,

which did not come on for seven days afterwards, and was then ushered in by a new exacerbation, after a second decline of all the symptoms. In fact, we have there all the phenomena of a fresh attack on the twenty-third day, followed on the thirtieth by erysipelas as a *sequela*. Am I not warranted, then, in concluding, that the exacerbation observed at the former date was accompanied by a fresh eruption of isolated follicles? This is strongly supported by another *post mortem* appearance observed, viz. the uniform rosy tint of the whole of that portion of the small gut that lay in the lower pelvis.

After the facts that have been adduced, I feel almost entitled to expect assent to the likelihood of the opinion, which, I am convinced, future observation will confirm, that in *typhus*, when uncomplicated with any secondary affection, a second attack does not take place, while in *typhoid* fever the contrary is the case.

I have a few more remarks to offer upon the duration of typhus. There are cases which are said to run their course in a few days. I do not speak of the *typhus siderans* of Saragossa, Torgau, and Mayence—a disease that was often mortal in a few hours. It is not to these cases of frightful intensity that I allude, though I may remark with Montault, that the earliest death recorded in typhoid fever is, I believe, that of a patient of M. Bretonneau, on the fifth day. The cases I refer to are those in which a cure is said to take place on the seventh or eighth day, or even earlier. Many authors of distinguished eminence detail cases wherein certain modes of treatment, early applied, have cut short, as the phrase is, attacks of continued fever. In short, it has been often laid down, that an early bleeding, an emetic, a strong purgative, can crush the hydra in the birth, and restore the patient to health. Dr Henderson in his part of the Report on the Epidemic Fever of Edinburgh,* states expressly, “If the presence of maculæ, red spots, or typhoid eruption, be considered as conclusive of the nature of the disease, though not necessarily a symptom in all cases of continued fever, then I can have no hesitation in deciding that this fever may run its course in a few days; for instances of convalescence have occurred to me on the seventh and eighth days, in which the eruption had existed, without which the diagnosis would have been perplexed, as in other cases.”

Without denying the possibility of the above, I remark, that it would have been desirable to know, whether, in the cases spoken of, very minute inquiries were instituted respecting the exact days on which the patients were first attacked;—for every one knows that the premonitory period of languor and uneasiness, extending often over a week, or even more, is seldom taken into account

* Edinburgh Medical and Surgical Journal, No. 141.

by patients in describing their complaints. But I object to the general position on other grounds. It is matter of notoriety, that very many cases of catarrh at all seasons, of influenza when it is epidemic, and of bilious affections during the heat of summer, are sent to the fever hospitals, because they simulate, in a most remarkable degree, the symptoms of commencing typhus. So it was in Glasgow Fever Hospital. These cases yielded in a few days to the exhibition of one or other of the above-mentioned remedies, while the same means were used, apparently without any effect, either as regarded their duration or intensity, in very many cases of genuine typhus. So firm is the belief in Glasgow, that those which terminate in a few days are not cases of continued fever, that they are not permitted to go into the convalescent wards, where sad experience has shown the much greater contagiousness of the disease. In fact, (and this militates strongly against Hildenbrand's opinion, that typhus is most contagious during the "nervous stage," beginning to be so when the eruption appears,) scarcely one of the hundreds dismissed from the *acute* wards ever returned labouring under typhus, though they had remained for a week or ten days in wards sometimes crowded to excess; while, of the few who, by mistake, went into the *convalescent* wards, scarcely one escaped the disease, and several died. No patient who presented the eruption was ever known to contract the disease anew, even among the convalescent patients.

I cannot omit to notice another circumstance connected with the course of the two diseases, which appears to me one of considerable interest; I mean the occurrence of *crises*. I do not refer, by this expression, to the dark and debated question of *critical evacuations*, nor will I institute any inquiry as to whether certain discharges, on certain days, are or are not attended with an improvement in the general symptoms. All that I insist upon is the frequent, I may say, the common occurrence of a perceptible crisis, or what is vulgarly termed a turn in typhus. I think I may appeal to the experience of every physician, and more especially of every resident clerk in a fever hospital, (for they have more constant opportunities of observation,) whether they have not often been struck at seeing, during their morning visit, the glassy eye, the haggard features, the low muttering delirium, the stupor approaching to coma, the tremor, the subsultus, the carphology, the rapid, thready, tremulous, and intermittent pulse, of the previous evening, the formidable array of symptoms, in short, which seemed to indicate a speedy and fatal termination, exchanged for the clear eye, the intelligent countenance, the steady hand, the comparatively slow and firm pulse, and the returning appetite of approaching convalescence. To such cases as these we might almost apply the scripture phrase, "At such an *hour*, the fever

left him ;” and if the crisis is not *very* frequently so marked, we can, in the great majority of cases, point with precision at least, to the *day* on which amendment began to take place. In reply to the question whether this is the case in typhoid fever, I can only adduce my own experience, when I state, that neither in the numerous cases I saw in Scotland, nor in those I have watched in Paris, (about a dozen of them very carefully,) have I ever seen anything approaching, in the remotest degree, to what I have noticed so frequently in typhus.

IV.—I proceed to a fourth and very important branch of my subject, viz. the consideration of symptoms. I shall not be tempted, by the highly ingenious and visionary conclusions drawn by Valleix, from his six cases of typhus,* to make any lengthened digression. That author actually considers the absence of headach and affections of the organs of sense as important diagnostic marks of typhus. Instead of wearying the reader with the laboured refutation I had prepared of this strange fancy, I shall merely state, regarding my own cases, that, excluding those in whom the headach ceased before the 5th day, it was present after that period in 98. In between one-sixth and one-seventh of this number it ceased before the 10th day; in the remaining five-sixths it continued throughout the advanced stages of the disease, being present in many till the 20th and 25th days; and in 11 throughout the whole course of the affection. Hearing, again, was either more acute, or perverted, or impaired in 69 patients. And finally, while I refer to the almost unanimous testimony of the multitude of distinguished authors cited by Gauthier de Claubry in pp. 5 and 39 of his work, I find recorded in my notes, as among the most constant symptoms, excessive intolerance of light, (of which I can add my own experience); suffusion of the eyes to an extent rarely if ever witnessed in typhoid fever; and minute injection of the conjunctival vessels. In not a few cases chemosis, corneitis producing ulceration and onyx, iritis, and in two or three cases Egyptian ophthalmia, occurred in the course of the disease. What, again, are more common in typhus than the glassy eye, (considered by Rochoux peculiar to it, from its very rare occurrence in typhoid fever,) the immensely dilated and extremely contracted pupil, and even total blindness? If reminded that, in adducing those facts, I am proving the identity of the two diseases, I reply, that if to prove the existence of analogies is to prove identity, there is an end to all conclusive reasoning and accurate knowledge. In order to establish from analogy the identity of any two diseases, the resemblances must be so numerous, so striking, and, above all, so important, and the differences so few, so

* Arch. de Med. 3me Serie, T. vi.

trifling, and so easily accounted for, as to warrant the overlooking of the latter conditions, which have certainly not as yet been answered in the case before us.

1. I have now to consider symptoms that have a material bearing on the decision of this important question. The state of the abdomen and bowels during life is naturally regarded as worthy of particular attention. The common use of purgatives in Great Britain throughout the course of typhus, and their well-known beneficial effects, while they render the declaration of Montault, (p. 93,) that Hamilton's method "still ~~converts~~ some partisans in England," not a little amusing and "passing strange," the sweeping condemnation pronounced against their repeated administration by Gauthier de Claubry, (p. 156-172,) prove at the same time the existence of certain symptoms to be combated. Though not called upon here to undertake a defence of the English mode of practice as applicable in all cases of typhus, I refer to it as furnishing some very remarkable facts bearing on the question at issue. Dr West, in his able paper on Exanthematic Typhus, (in the fiftieth volume of the Edin. Med. and Surg. Journal,) makes the following observations:—"The action of the bowels was not disturbed in the great majority of cases; in fact, the administration of mild laxatives was necessary, in most instances, in order to obtain an evacuation, once in 48 hours; and in some of the most severe cases the bowels were very constipated. Diarrhœa occurred only in ten of these sixty cases; in three of which the patients died, and it was only four times that it lasted for longer than 48 or 60 hours. * * * In thirteen cases the bowels were constipated. The following is an abstract of the statements of Dr Henderson in regard to the state of the bowels in 154 cases of typhus.

	Males.	Females.	Total.
Bowels easy in	26	73	99
loose in	—	5	5
costive in	19	31	50
	—45	—109	—154

In reference to the following results obtained by myself, I have only to remark, that in many of those cases in which diarrhœa is described as spontaneous, the requisite information was unfortunately not got as to the previous administration of purgatives.

	Males.	Females.	Total.	
Diarrhœa {	Spontaneous in	13	10	23
	From medicine in	26	27	53
	Doubtful in	1	—	1
	—	—	—	
	40	37	77	
Costiveness notwithstanding the exhibition of purgatives,	20	42	62	
	—	—	—	
	60	79	139	

Thus, according to Dr West's observations, the bowels were constipated in nearly one-fourth; in Dr Henderson's in about one-third; in my own in between one-half and one-third of the cases. Again, spontaneous diarrhœa occurred in one-sixth of Dr West's; in only one-thirtieth of Dr Henderson's, or, adding three, in which it came on very late in the disease, and might be referred to secondary affections, in one-nineteenth; and in nearly one-sixth of my own cases.

How different is the state of matters in regard to typhoid fever? Chomel (pp. 6, 10,) mentions diarrhœa as a symptom scarcely ever wanting, and states it as occurring (p. 230,) in 40 out of the 42 fatal cases recorded by him. In the still more precise observations of Louis, we find it wanting in only 3 of 128 cases, and in 58 of these it was contemporaneous with the commencement of the affection. Spontaneous diarrhœa was present in every one of the 9 cases observed by myself; in 3, it was one of the first symptoms, having declared itself on the first day, along with the general premonitory symptoms. In one, it began on the 4th; in another, on the 6th; in two more, on the 10th; in an eighth, on the 11th; in the ninth, it was observed after a few days illness, the precise day not being specified. This symptom was present, in one case, from the 1st till the 22d day, recurred on the 36th and continued, during a second attack, till the 46th day. Its shortest and longest durations were respectively 10 and 43 days; and its mean duration above 22 days.

As to its characters, in two it was very violent, and opium had little effect in checking it. In two, it occurred in severe attacks, which assumed a periodic character; and in the rest it was moderate but always or nearly always present. Without insisting further on the comparative duration of the diarrhœa in the two diseases, though it would, if minutely investigated, bring out still more clearly the extraordinary contrast already presented,—I proceed to the consideration of the state of the abdomen in the two complaints. This I propose to do under two heads, relating, *first*, to the feelings of the patient, and *second*, to the sensations of the medical attendant.

2. In thirty out of the sixty cases observed by Dr West, there was pain in the abdomen, but in only nine was it severe. Dr Henderson remarked it in 46 out of 198, and never in the right iliac fossa. The following are the results of my own observation;

Abdomen painful in 109 out of 139 patients.

	Males.	Females.	Total.
Pain intense, or at least severe in,	17	27	44
slight,	24	41	65
	41	68	109

The duration of the abdominal pain was as follows :

	Males.	Females.	Total.
Throughout the disease, in	18	27	45
a considerable part of it, in	6	9	15
From two to six days, in	12	24	36
One day only,	5	8	13
	—	—	—
	41	68	109

Deducting the thirteen, in whom this symptom was present only one day, and in whom it may fairly be attributed to some accidental cause, we have 96 cases out of 139, in whom abdominal pain was somewhat permanent, and in 60 only did it continue throughout the greater part of the illness. In the great majority, the pain was general,—in 32, it was chiefly or entirely confined to the region of the liver, a circumstance noted by Dr West ;—3 of those in whom this was observed had long laboured under chronic hepatitis. In 17 of the 32 alluded to, (between one-half and one-third of those in whom the pain was severe) the liver was exceedingly tender to pressure ; and it was the seat of pain more or less acute, in 20 out of the 60 patients, in whom the abdomen was tender during the greater part of the disease. Dr West mentions the usual duration of the abdominal pain as “about four days, though once it continued for fifteen days, and in three other instances from six to eight days.” In 11 only of my own cases was pain observed in the right iliac region, eight of these presenting it during one day, two during two days, and only one for three days consecutively. Valleix notices the presence of diffuse pain on pressure, for three or four days, in two of sixty typhus patients; and of pain on pressure and constant uneasiness in four, and of colic in a fifth of the sixth labouring under typhoid fever.

According to Louis, abdominal pain was present in 112 cases only of 128, was rarely general, occupied in the vast majority either in the iliac fossæ or the hypogastric region, continued generally throughout the greater part of the disease, its duration being commonly from 10 to 25 days. In six cases it lasted a whole month. The results of my own observation are, that abdominal pain was present in every case examined by me ; and that in six, in whom pain was observed during their residence in the hospital, it was either confined to the ileo-cæcal fossa, or more severe there than elsewhere, having been diffused over the belly in three of them during a part of their disease. It begun in four on the 1st, in one on the 6th, in one on the 10th, in one on the 14th day of the fever. In the remaining two, it was present, in a decided degree, on their admission to the hospital, on the 11th and 13th days, and had most likely been present for some time before, though not felt by the patient.

This leads me to observe that the leading diagnostic symptoms in the Glasgow epidemic of 1836 were, the red, chopped, and papillar tongue, the dull, circumscribed abdominal pain existing for weeks together, along with constant diarrhœa, harsh skin, and obstinate bronchitis. But what chiefly merits attention is, that while in typhoid fever the pain accompanies diarrhœa, in typhus, the pain is often most severe when the bowels are costive, and is relieved on the exhibition of a purgative, proving its frequent connection with and dependence on a loaded state of the intestinal canal. In typhoid fever, on the other hand, it exists independently of any such condition; a circumstance which, along with its fixed seat, demonstrates its dependence upon local lesion. This is particularly pointed out both by Dr West and Dr Henderson, and it is fully confirmed by my own experience.

To sum up what I have stated on this head; abdominal pain occurred in 186 of 403 cases of typhus, or, deducting those in whom it was altogether transitory, in 173, *i. e.* between one-half and one-third of the whole; while in the 128 cases of Louis, it occurred 112 times, (about five-sixths,) or, adding those detailed by Valleix and myself, in 126 out of 143 cases, *i. e.* in about ten-elevenths of the whole.

3. The next subject that demands attention is the feel of the abdomen. Dr West mentions eleven cases out of sixty, as presenting "some fulness and tympanitic distension;" Dr Henderson, only 8 out of 198. In those observed by myself, I find that the abdomen was

	Males.	Females.	Total.
Highly tumid and tympanitic in	12	3	15
Slightly so,	12	27	39
	—	—	—
	24	30	54
Of natural feel,	35	50	85
	—	—	—
	59	80	139
This symptom lasted throughout the disease in,	9	3	12
During a considerable part of it, in	3	2	5
From two to six days,	7	16	23
One day,	5	9	14
	—	—	—
	24	30	54

Deducting, again, as I am fairly entitled to do, those cases in which the symptom was very transitory, there remain only 40 in whom it was at all permanent, and in 17 only out of 139 did it continue during the greater part of the affection.

In typhoid fever, on the other hand, Chomel (p. 12,) remarks its almost constant occurrence, and Louis discovered it in 89 of 134 cases, in the great majority of which it lasted during the

greater part of the disease. In one only did it disappear in 24 hours. Valleix* remarks its transient existence in one out of six or seven cases of typhus observed in London by Dr Shattuck, whereas it was present at an earlier stage, in a much higher degree, and for a much longer period, (its mean duration being about nine days,) in five out of six patients labouring under typhoid fever. It was remarked in eight of the nine cases examined by myself, and was present from 4 to 34 days, or on an average a little more than 19 days. In the remaining case it was so very slight as to render it doubtful.

Adding together all the cases of typhus above referred to, we find the symptom in question occurring in 74 out of 463 cases, or between one-fifth and one-sixth of the whole. Deducting those in which it was transitory, it occurred in 59 only, or in less than one-seventh of the whole number; whereas it was observed in 102 out of 149, or in rather more than two-thirds of all the cases of typhoid fever—no small difference assuredly.

4. Yet the statements I have made are very far from exhausting the evidence on the subject. I, therefore, proceed to inquire into the relation subsisting between the symptoms spoken of; and *first*, in regard to the coexistence of pain of belly and looseness of bowels in typhus. Of fifty patients in whom pain and diarrhœa existed at one period or other of the fever,

	Males.	Females.	Total.	
Diarrhœa was	{ spontaneous in	8	8	17
	{ consecutive in	15	18	33
	—	—	—	
	23	27	50	
Pain and constipation were observed in	16	41	57	

The numbers in which diarrhœa and constipation were observed were respectively 77 and 62. Therefore had abdominal pain occurred in the same proportion in those who had diarrhœa, as it did in those who had constipation, instead of having 50 only, we should have met with it in 70.8. In other terms, the occurrence in the same cases of pain and diarrhœa is as 1 : 1.54, while that of pain and constipation is as 1 : 1.08.

Remarkable as this difference is, it is in reality much greater. I find from a further and very careful analysis of the cases before me, that abdominal pain and diarrhœa were

	Males.	Females.	Total.
Coincident in	14	16	30
Not coincident in	3	4	7
Pain was relieved by supervention of diarrhœa in	} 4	8	12
		—	—
	21	28	49

* Arch. de Medecine, 3me serie, T. vi. p. 142.

Again, abdominal pain and constipation were

	Males.	Females.	Total.
Coincident in	16	35	51
Not coincident in	0	6	6
	—	—	—
	16	41	57

Thus, in only 30 out of 77 cases in which diarrhœa, either spontaneous or consecutive, was noticed, did abdominal pain and diarrhœa exist simultaneously; whereas, out of 62 cases in which the bowels were confined, pain in the belly and constipation co-existed in no less than 51. Had the latter proportion been kept up, we should have had 63.3 instead of 30, so that, according to our present data, the coincidence of abdominal pain with constipation in typhus is to its occurrence along with diarrhœa as three is to one. I might still further heighten the contrast, by deducting one who died of phthisis, in whom the abdominal pain did not supervene till after the fever had run its course, and in whom it was found on dissection to depend on very extensive tubercular suppuration and ulceration of the intestines; another in whom it supervened about the 20th day, along with a profuse discharge from a very large bed-sore; and a third, in whom the evacuations, though frequent, were mixed throughout with scybala;—but the difference is striking enough without any such refinement.

I have marked a considerable number of the above as relieved by diarrhœa; a circumstance which naturally provokes inquiry into the previous state of the bowels. I find, then, that of the 30 in whom pain and diarrhœa were contemporaneous, 10 may be called cases of spontaneous diarrhœa, *i. e.* the purging was not, so far as I know, brought on by medicine; while in the remainder it may be ascribed to that cause. In 28 of those in whom diarrhœa, whether spontaneous or consecutive, was observed, the bowels were more or less obstinately constipated during the first stage of the fever; and in that number are included almost all those in whom diarrhœa gave relief to pain. And finally, of the 51 in whom constipation lasting throughout the disease was accompanied by pain, (in many cases exceedingly acute,) the operation of purgative medicine,—of castor-oil, turpentine, jalap, calomel, or the neutral salts, given according to the strength of the patients, and the indications presented in the course of the disease, was followed by decided and immediate relief in 20 cases. Dr West mentions, that of 30 who had pain, eight only had diarrhœa, while 15 were constipated; a result that fully bears out my statements.

Abdominal pain and spontaneous diarrhœa were coincident in all of the nine patients labouring under typhoid fever to whom I have alluded. The shortest period of coexistence was 4, the longest 32, and the average is a little more than 15 days. So con-

stant did Louis find the coexistence of these two symptoms, that (Vol. ii. p. 34, 38,) he considers abdominal pain, both in typhoid fever and other diseases as pathognomonic of an inflamed state of the mucous membrane. In opposition to this I may quote Dr Henderson, who remarks in his report, "In eight, a degree of general tenderness of the abdomen coexisted with tumidity and tension, in only one of which was there diarrhœa."

These statements form the best possible apology for the judicious administration of purgatives in typhus, while the spontaneous supervention of diarrhœa in many whose bowels were confined, demonstrates its propriety beyond the shadow of a doubt, and fully warrants, as it appears to me, a most important conclusion, in proof of which I have adduced the above rather dry and tiresome details, namely, that whereas in typhoid fever there is an early and almost uniform tendency towards looseness of the bowels, there is as constant a tendency in typhus to constipation; that while in the former diarrhœa is a symptom, and a natural, I might almost say a necessary, result of a constant local lesion, it arises in the latter, from the presence of an irritating substance, which it is the object of the *vis medicatrix* to eliminate from the system.

5. I shall offer only a few remarks on the connection between the state of the bowels and the feel of the belly. Of 54 patients in whom, as has been seen, more or less swelling and tympanites, (*meteorismus*,) were observed, 25 had diarrhœa, and 29 were constipated. The following tables will show much more concisely and clearly than I could express it in words, the duration and extent of the tympanites in these two groups of cases.

	Bowels loose.			Bowels bound.		
	Males.	Females.	Total.	Males.	Females.	Total.
Tympanites continued throughout the disease in	6	1	7	3	2	5
Throughout the greater part of it in	1	0	1	1	2	3
From two to six days in	5	4	9	2	12	14
During one day in	4	4	8	2	5	7
	—	—	—	—	—	—
	16	9	25	8	21	29

On this table I have only to remark, that 7 of the 25 had spontaneous diarrhœa, but of these seven only two had tension of the belly for more than one day. But, by a careful analysis of the cases, we arrive at still more remarkable conclusions. The following are the results of my investigations;

	Males.	Females.	Total.
Tympanites and diarrhœa coexisted in	12	5	17
Did not coexist in	3	2	5
Tympanites was relieved by diarrhœa in	1	2	3
	—	—	—
	16	9	25

Tympanites and constipation coexisted in	8	18	26
Did not coexist in	0	3	3
	—	—	—
	8	21	29

Of the 29, the distension was relieved in 3 by the exhibition of purgatives. The 5 in whom diarrhœa and tympanites did not coexist were constipated at the time the tumefaction was noticed, so that in point of fact distension of the belly was coexistent with diarrhœa in 17, whereas it existed along with constipation in 31. Finally, of the 7 mentioned above as having meteorismus and spontaneous diarrhœa, the two symptoms existed simultaneously only in 4. Dr West, I observe, states,* that of 11 cases in which “there was some fulness and tympanitic distension of the abdomen,” diarrhœa was coexistent with it in 7, “while in 3 the bowels were constipated.” As to the extent of the tympanites, I find that

	Bowels loose.			Bowels bound.		
	Males.	Females.	Total.	Males.	Females.	Total.
The abdomen was highly tympanitic in	8	0	8	4	3	7
Moderately or slightly so in	8	9	17	4	18	22
	—	—	—	—	—	—
	16	9	25	8	21	29

6. In 46 (17 males and 29 females,) out of 54, in whom meteorismus was observed, abdominal pain was also present at one period or other of the complaint, but the two symptoms were coexistent only in 37 patients, in several of whom the pain was so very transient and so slight, in comparison to the duration and extent of the distension, as scarcely to merit notice. To show still further that there is no proportion between the extent of abdominal tension and the pain experienced, I would merely state that, of 15 patients in whom the belly was highly tumid, 6 only felt severe pain; in 5 there was slight tenderness; in 4 none whatever. Of 44, on the other hand, in whom the belly was acutely painful, we have seen that only six presented any remarkable tension; in 18 it was slight; in 20 there was none at all. Dr West's statements are widely different. He states, (*loc. cit.*) that pain was present in all, except one of those in whom the belly was tympanitic.

I shall conclude my remarks on this head with a few statements regarding the simultaneous occurrence of the above symptoms in my own cases of typhoid fever. Tumefaction of the belly and diarrhœa coexisted in eight patients, from 4 to 30, giving an average of $15\frac{3}{4}$ days. Tumefaction was unaccompanied by pain in one patient, and lasted from the 12th to the 33d day. In an-

* Edinburgh Medical and Surgical Journal, Vol. 1. p. 134.

other, abdominal pain was present on admission, but disappeared next day, though the distension persisted throughout. In a third, the abdomen was swollen, but not painful, from the 9th to the 24th, both swollen and painful from the 25th to the 31st day. In the remaining 5, the symptoms coexisted from 4 to 34 days, or on an average about $20\frac{1}{2}$ days.

I go on to speak of the eruptions observed in two diseases. This subject has lately begun to excite much interest, more, perhaps, in Great Britain than in France. The proposal which has been made to overturn long-received opinions, and to blot out a long and elaborate section from the nosological dictionary, by placing typhus among the exanthemata, could not fail to awaken a most interesting controversy, and to bring under review the whole history, and much of the symptomatology of typhus. Hildenbrand was the first to propose this innovation. But it was started in England and Scotland by two physicians, who had neither had any communication with that distinguished author, nor with each other, viz. by Dr Roupell, who, it appears from his work, brought the subject under the notice of the London College of Physicians in 1831; and by my friend Dr Peebles, who, on his return to Scotland in 1832, after a long residence in Italy (where he had been struck with the constant occurrence of an eruption in the epidemics of contagious fever,) drew the attention of the profession in Edinburgh to its presence, previously unnoticed, in the typhus of that capital; and after two years of attentive inquiry, published the result of his researches, accompanied with a comprehensive sketch of the history of eruptions in contagious fever, in the forty-fourth volume of the Edinburgh Medical and Surgical Journal,—being the first, as I believe, to submit his sentiments to the medical world. I observe that Dr West, in an able and learned paper in the last number of the same periodical, maintains the opposite opinion. For several reasons, I shall not enter upon this controversy. Besides that the proof of the positions I mean to take up does not require it, it appears to me that we are not yet in possession of data sufficient to demonstrate the truth of the doctrine. That must be the work of many years of patient and very extensive inquiry. I may remark, however, that there is everywhere a growing conviction in its favour, and that, if the claim of typhus to rank among the exanthemata has not yet been fairly made out, its remarkable analogies with that class of diseases are attested, alike by the uniform experience of the past, and the observation of present times. Nor can I consent, without reserve, to conclusions drawn from the alleged absence of eruption; for the fact I have already referred to, (viz. that the eruption in the Edinburgh typhus was unheeded before 1832), shows how appearances may escape the eye of the most distinguished and practised physicians, when their attention

is not particularly drawn to them. It is also well-known to many that, previous to a visit which Dr Peebles made to the Glasgow Fever Hospital in the spring of 1835, the exanthema of typhus, then found to be of general occurrence, had neither been looked ^{upon} for nor registered in that institution, and was ~~received~~ as a new discovery. And finally, I must express my conviction, founded on observation, that a very large proportion of those cases mentioned in fever reports, as having no eruption, consists of patients (such as I have already referred to) in whom the disease is only of a few days duration, and does not, for the reasons stated above, seem to merit the name of typhus fever. These considerations, it appears to me, have an important bearing on the decision of this debated question.

While, however, I decline making any further remarks on this branch of inquiry, I must refer shortly, for the practical elucidation of my subject, to the history of the typhous eruption. Much confusion prevails on this point. The meaning of the term *petechiæ* has, in the course of time, undergone a great change. That it was used by the old authors, (loosely enough, doubtless,) to denote what would have been much better called an eruption, is evident from their writings, and has been very clearly shown, by Dr Peebles, five years ago, and by Dr West in the April number of the Edinburgh Journal. Dr Peebles accordingly proposed to retain the old term in its altered but now generally received acceptation, and to consider the rosy eruption as an exanthema. Yet many, from the use of the term petechial by the older writers, persist in denying their acquaintance with the eruption; some, on the other hand, (Rasori, Pinel, Tweedie,) having used the same word in reference to the eruption, are blamed for want of "precision and exactness in the use of terms;" Chomel (p. 336) considers "the true petechiæ or purple spots" to be very much more frequent in typhus than in typhoid fever; Valleix* maintains the eruption to be always dark and persistent, and takes it as synonymous with vibices; while Copland† and Montault (p. 154) describe "ecchymoses and petechiæ as common to typhus and other diseases," and the measly eruption as peculiar to it. Again, Roupell quotes Louis and Chomel in support of his views, as to the true exanthematous character of typhus. One of his reviewers,‡ reasoning on the generally admitted difference of the French and British continued fever, retorts Dr R.'s arguments as proof positive against the truth of his theory, while, with praiseworthy consistency, (p. 103), he cites Chomel's statements regarding the eruption in support of his own opinions.

* Arch. de Med. 3me Série, T. vi. p. 133-4-6—148-9.

† Dictionary, p. 1010. Art. Typhus.

‡ Med. Chirurg. Review, Vol. xxxi. p. 99.

I shall now endeavour, in proof of the two following propositions, to present, in small compass, the very numerous statements of authors, to reconcile their apparent contradictions, and to give the results of my own observation regarding the exanthema of typhus. The points I wish to establish are;

First, That the rash in typhus is permanent; that in all cases it presents the two periods, longer or shorter, according to circumstances, of increase and decline; that, in the more severe cases, it may exhibit, during the period of increase, four different states, being florid, dark, livid, and petechial; and finally, that it is quite different from vibices and purpura, which are of rare occurrence.

Second, which partly flows from the foregoing, that the abundance, and particularly the darkness of the eruption, may be said to be proportional to the severity of the disease.

1. When I say that the rash is permanent, I mean that it does not consist of successive eruptions of spots, each of which disappears in the course of three or four days, but that the same eruption continues throughout the disease. As the accuracy of this position will be clearly brought out in treating of the changes the eruption undergoes, I shall only state that it was ascertained by surrounding a certain number of spots with ink, and observing them carefully several times daily.

Dr Copland, and Dr Peebles, to whom he refers, lay down the proposition that the "eruption usually appears from the third to the seventh day of the fever, but may be delayed till the twelfth or fourteenth day." Dr West, in his last paper, mentions that in the Vienna fever of 1757, when the disease ran its regular course, it appeared "on the fourth, or, at latest, on the seventh day."* Montault (p. 154) refers to Drogart, Biett, Rochoux, and Pringle, as having observed it generally on the fourth, fifth, or seventh, and sometimes (according to Pringle) on the 14th day. Gauthier de Claubry, (p. 75) remarks, "that of seventeen observers who make express mention of it, four noticed it on the fourth, two on the fifth, eight in the course of the sixth and seventh, and generally, all point out its manifestation between the seventh and tenth days." Dr Roupell maintains its almost uniform appearance on the third or fourth day. Dr Cowan, in his "Statistics of Fever and Small-pox in Glasgow," p. 24, says that it "generally makes its appearance from the fourth to the ninth day of the disease, occasionally at a later period." Dr Henderson states that in eight out of twelve, in whom the day on which it appeared could be ascertained, it appeared from the third to the sixth, in "the remaining four severally on the seventh, ninth, and eleventh day." I have been able to ascertain the exact date of the appearance of the eruption in a much larger number than Dr Hender-

* Edinburgh Journal, No. 143, p. 295.

son, to whose statements, in addition to the following tables, I refer for the proof of what I have advanced respecting its increase and decline. The first table refers to the time of its appearance, the second to the period at which it began to decline, the third to the date of its final disappearance.

	Males.	Females.	Total.
It appeared on the 2d day in	1	- 1	- 2
3d,	- 2	- 1	- 3
4th,	- 3	- 2	- 5
5th,	- 4	- 12	- 16
6th,	- 4	- 9	- 13
7th,	- 1	- 4	- 5
8th,	- 2	- 1	- 3
9th,	- 1	- 3	- 4
13th,	- 0	- 1	- 1
	—	—	—
	18	34	52

From this statement, it results that in more than half of the entire number it appeared on the fifth and sixth days, and that, in exactly three-fourths, it appeared from the fourth to the seventh day. Taking an average of the whole, it appears most commonly on or about the sixth day. I must, however, direct attention to the fact, that in not a few cases it was so very copious, and in several so dark, on the sixth day, as to warrant the belief that it had appeared some days before.

Dr Henderson states that the eruption began to fade "in eleven before the tenth day; in twenty between the tenth and twelfth, inclusive; in one, as late as the seventeenth."* The following table gives the results of my own observation. It refers only to forty-eight of the fifty-two cases above referred to, four having died before the eruption began to recede.

	Males.	Females.	Total.
It began to decline on the 8th day in	1	- 0	- 1
9th,	3	- 3	- 6
10th,	0	- 5	- 5
11th,	2	- 3	- 5
12th,	1	- 4	- 5
13th,	5	- 3	- 8
14th,	3	- 3	- 6
15th,	1	- 3	- 4
16th,	0	- 2	- 2
17th,	0	- 2	- 2
18th,	1	- 2	- 3
19th,	0	- 1	- 1
	—	—	—
	17	31	48

It is evident from the above table, that we cannot fix any particular day, from the ninth to the fifteenth inclusive, on which, much more frequently than any other, the eruption begins to fade,

* Report, &c. p. 13.

It did so, however, on one or other of these days, in 39, or five-sixths of the entire number, the average of which, (12.8), assigns the thirteenth as the day that the change most commonly takes place.

The length of this paper, already too great, renders it impossible, as its object happily renders it unnecessary, to analyse the circumstances whereon the variations I have noted depend, such as the age, the constitution, the strength, the habits of the patient, and the greater or less intensity of the disease, all of which have a material influence in modifying its duration.

I shall now, in a third table, show the various dates of the disappearance of the eruption, in forty-five cases, three more having died, very shortly after it began to recede.

	Males.	Females.	Total.
It disappeared on the 13th day in	0	3	3
14th, - - -	0	4	4
15th, - - -	2	1	3
16th, - - -	2	2	4
17th, - - -	0	2	2
18th, - - -	2	5	7
19th, - - -	0	5	5
20th, - - -	1	2	3
21st, - - -	1	1	2
22d, - - -	3	2	5
11th, 12th, 23d, 24th, 25th, 26th, and 31st days in	4	3	7
	—	—	—
	15	30	45

The differences in this are still more striking than those in the preceding table. It would be easy, were such a part of my present purpose, to demonstrate that the long continuance of the eruption, depends, in almost every case, on its having been dark, livid, or petechial during the disease; but, as the proof it would not aid me in my present inquiry, I go on at once to deduce some more practical inferences from the data I have furnished. Where so many disturbing forces are at work, as those I have alluded to, it would be vain to look for anything like an exact proportion between the periods of increase and decline in the typhous exanthema. Accordingly, I find, from a laborious analysis of the above cases, that though in eight the two periods agree exactly, and in eight others, very nearly, in length, they vary, in the remainder in all different proportions. As it would be quite useless here to enter into details, I shall simply subjoin the results of my calculations:—

Shortest period of increase,	3 days.
Longest do.	13
Average, do.	7.08
Shortest period of decrease,	2

Longest period of decrease,		13 days.
Average	do.	4.45
Shortest duration of eruption,		6
Longest	do.	24
Average	do.	11.59

These conclusions form a considerable contrast to Dr Copland's statements: "The duration of this eruption is from three to five days." Dr Henderson, infers "that the whole duration of the eruption occupied commonly nine or ten days,"—a result which approaches much more nearly to that I have arrived at. Though I could have adduced a much greater number of facts regarding the decline and disappearance of the eruption, I have preferred confining my attention to those patients alone, in whom the date of its commencement also was ascertained, for, having been selected for examination solely because they presented themselves in the early stage of the disease, they form a perfect group, whence general conclusions may with safety be drawn.

Before entering on the consideration of the various states of the eruption, let me refer to some of the opinions put forth respecting it. Valleix, I have already stated, holds the eruption to be always dark, and persistent under pressure, both during life and after death. "It is," says Copland, "of a florid, reddish, or reddish-pink colour; disappearing on pressure, but soon returning when pressure is removed. This circumstance is sufficient to distinguish it from petechiæ." Again, "If petechiæ occur in this fever, they seldom are observed before the eighth or tenth day, and then this eruption has usually disappeared. When the petechiæ are earlier, or the eruption continues longer, so that both exist together, they are quite distinct and different in their appearances; for the latter is never so dark or livid as the former generally is." Further on, he remarks, "In the more malignant cases, or when petechiæ appear early in the disease, the colour of the eruption may, however, become deeper." Still further, "the petechial affection * * * may occur in the advanced stage of any fever, * * * and is not in any sense of the word, an eruption, as it has been very improperly denominated by some writers. * * * The petechiæ or cutaneous ecchymoses vary in dimensions from minute stigmata to large patches and vibices, and in the deepness or shade of colour. * * *

But the exanthematous eruption attending true typhus, * * * has been confounded with petechiæ, with which it is often associated in the advanced stages of the fever." These opinions agree entirely with those expressed in his paper by Dr Peebles. Dr Henderson (p. 16) has the following remarks: "Purple petechiæ, the result of ecchymosis, were frequently present in the second week of the disease. In some cases, they were

very abundant, but in very few exceeded the number of typhoid maculæ, which commonly existed along with them. One vibex only was remarked in the whole number of cases. The petechiæ continued often distinct, though the eruption was declining." To disprove, as far as my observation enables me, what appears to be incorrect in these opinions, even though upheld by such high authority, is the task I now propose to myself.

The statements of authors on this subject are very numerous. Dr West, in tracing the history of the eruption downwards from the fifteenth century, cites Fracastorius and Ambrose Paré, who both speak of the eruption assuming a dark colour. Conradinus is more explicit. In the article referred to,* I find these words; "In addition to these symptoms, however, spots like flea-bites appeared on the skin of all the patients. In some cases, these spots were of larger size than others; occasionally they occupied the whole body, but usually were confined to the chest, the spinal and interscapular regions, and the arms; and they were observed to assume a livid or blackish colour in dying persons." Willis, (same paper, p. 289,) mentions as one of the symptoms of the epidemic among the troops near Oxford in 1643, an eruption of spots, some of them small, and of a bright-red colour, while others were larger, and of a more livid hue. The reviewer of Dr Roupell, formerly alluded to, says,† "the rash is not accurately described by any of the authors cited, so that it is impossible to say whether it was the same in all. One author (Pringle) does not even mention its existence." Now, as it so happens that Pringle has given, both in his work on Diseases of Armies, and in his letter to Mead, a description of the eruption so clear as to make it be referred to by most writers on eruptions in continued fevers, it may be as well to quote his own words, which have an especial bearing on this and other portions of my subject. "There are," says he, "certain spots, which are the frequent, but not inseparable, attendant of the fever in its worst state. These are less usual on the first breaking out in the hospitals; but when the air becomes more corrupted, the spots are common. They are of the petechial kind, of an obscure red colour, paler than the measles, not raised above the skin, of no regular shape, but confluent. The nearer these spots approach to a purple colour, the more ominous they are, though not absolutely mortal." The most of the authors quoted by Gauthier de Claubry mention the existence both of the "rosy and petechial eruptions," the latter coming on at a later period of the affection, and in adynamic patients.

Dr Roupell, in speaking of the opinion of some authors, that

* Edin. Journal, No. 143, p. 282.

† Med. Chirurg. Review, Vol. xxxi. p. 103.

the "conversion of petechiæ into the rash is a common occurrence," has evidently, as his reviewer remarks, said the reverse of what he meant to convey to his readers. My friend, Dr Staberoh of Berlin, from observations, of which I can attest the accuracy, made in Glasgow Fever Hospital, where I then resided, drew attention to the frequent conversion of the rosy eruption into petechiæ, in a paper published in the Medical Gazette for March 1838, and in reference to it, Dr West has the following remarks: "My attention having been called to the point by a paper of Dr Staberoh, I have endeavoured to ascertain, whether, as he asserts, the spots often change into ordinary petechiæ, but have not yet been able to satisfy myself upon the point, though, from one or two cases which I have seen, in which the spots greatly resembled petechiæ, I should think that his statement is very probably correct." Dr Staberoh, in his thesis on the Exanthematic Typhus at Halle, in 1834, maintains the same opinion. Having thus, by quotations, shown that my opinion, so far from being original, has been partly recognized for centuries, and has been fully developed, of late years, by other writers, it only remains for me to offer the fruits of my own observation.

Having observed with care, several times daily, the cases referred to by Dr Staberoh, in which a considerable number of spots were circumscribed with ink, I have seen the changes referred to going on in the most gradual manner; indeed, the fact was so clear, that I find, in my daily reports of numerous cases, taken months before his arrival, and when my attention was not particularly directed to the point, the phrases, "pale, florid, darkening, dark, livid, petechial," applied successively to the eruption at various stages of the disease. In the details I am about to give, the general principle is all I wish or need to establish; the periods at which the changes take place, I do not now seek to determine. Having specified the four states of florid, dark, livid, and petechial, I would have it borne in mind, that, when florid, the eruption disappears readily under pressure; when dark, it still disappears, but more slowly; when livid, semipetechial, or pseudo-petechial, (as it has been called,) it is only partially effaced; when petechial, it is not in the least affected by pressure. In many cases, it remains florid throughout; in others, it presents one or more, and in not a few, all these alterations; and after it has reached its height, the process is inverted, and it passes through the various phases of lividity, darkness, redness, and paleness, before its evanescence. When death took place at the time the eruption was petechial, the ecchymoses, (for they are ~~termed~~ ecchymoses,) remained after death—a circumstance noted by Dr West, and particularly insisted on by Valleix in the paper already

true

referred to. Dr Henderson says, (p. 12,) "In a few, the tint was somewhat purple; and this colour could be assumed by an eruption that had formerly been scarlet." By including in the following tables, the deaths in the various groups of cases, they will serve for reference, when I come to speak, as I shall do presently, of the relation that subsists between the states of the eruption, and the gravity of the cases.

The colour of the eruption was :

	Cases.	Deaths.	Rate of Mortality.
Pale throughout, in	34	3	1 in $11\frac{1}{3}$
Florid	25	2	1 in $12\frac{1}{2}$
Darkish in	16	2	1 in 8
Dark	32	8	1 in 4
Livid or semipetechial in	15	7	1 in $2\frac{1}{3}$
Petechial	17	4	1 in $4\frac{1}{4}$
	139	26	1 in $5\frac{1}{3}$

Hence it appears that the eruption was pale in about one-fourth; florid in between one-sixth and one-seventh; darkish in between one-eighth and one-ninth; livid in rather less than one-ninth; petechial in about one-eighth of the whole cases. Dividing them, then, into two groups, we have the following result :

	Cases.	Deaths.	Rate of Mortality.
Eruption light-coloured in	59	5	1 in $11\frac{2}{3}$
dark-coloured	80	21	1 in $3\frac{2}{3}$
	139	26	1 in $5\frac{1}{3}$

I have been thus minute in my subdivisions, in order to show more clearly the almost insensible gradations by which the rash is transformed from its florid or exanthematous, into its petechial state; and in order to demonstrate at once its difference from purpura and vibices, and the truth of my concluding position. I met with purpura spots in three, and vibices in only two cases. In one of the former, a plentiful crop of purple spots appeared suddenly on the 14th day, on the chest and belly, amid a "pale, scanty, and very small" eruption. The pulse, which had before gradually increased in frequency, was 128 on the 12th day; on the 13th, it fell to 116. It continued to fall steadily, by about six beats each day, becoming gradually fuller and firmer; the intensity of all the other symptoms diminished as steadily, and convalescence was completely established on the 21st day. In another case, a large crop of very minute spots, differing completely in colour from the eruption, which had been petechial for days before, appeared on the 12th day, on the right arm, and faded away long before the eruption. The pulse, 120 on the 10th day, fell on the 11th to 104, and on the 13th to 96, and during the four

following days to 54; on the same day, epistaxis occurred, and the violence of all the symptoms diminished, but the severity of the attack left the patient exceedingly weak, and convalescence was not fairly established till the 31st day. In the third case, very numerous spots, similar to those already described, appeared around the left elbow on the 10th day, amidst a "darkish" eruption. On the same day, the patient died. The countenance, which the day before was wild, flushed, and much oppressed, is reported "languid, much better;" and the pulse had fallen from 136 to 128, and was of better strength, but an intense bronchitis and the supervention of pneumonia, with great pectoral oppression, proved fatal. The difference in the appearance of the spots is well described by Dr Staberoh. The spots of petechial eruption are ill-defined and shaded off at their margins, and have a dusky, reddish-brown colour, while the spots of purpura, besides having abrupt and very clearly defined margins, present a decided shade of blue, which, as Dr S. remarks, renders the name applied to them very appropriate. *Vibices* appeared upon the chest in one case on the 10th day, amid a pale eruption, that became afterwards more marked, while the ecchymoses grew paler. The pulse, on the 9th day 124, had fallen to 112; the state of the patient improved rapidly, and convalescence took place on the 18th day. In another, the vibices were pretty numerous, large, and very irregular in form, and appeared on the 12th day, amid livid and petechial eruption; the pulse fell from 132, the number of pulsations on the day before, to 122, and convalescence was complete on the 20th day. From these details, it appears clearly, that purpura and vibices are of rare occurrence in typhus, are quite distinct from the typhous eruption, and that, instead of being attended with peculiar danger, they would actually appear to be critical. We may also conclude that if, as Dr Copland insists, "the petechial affection is not in any sense of the word an eruption,"—"the exanthematous eruption is" not only "often associated with" petechiæ, but is often itself petechial.

I now proceed to my second proposition. Dr West, (loc. cit. p. 141,) says, that in the cases he observed "the abundance or scarcity of the eruption was not at all in proportion to the severity or mildness of the different cases." He considers petechiæ not to be particularly ill omened," (an opinion somewhat borne out by the facts stated above,) "and certainly by no means so much to be dreaded as the dark and livid hue of the eruption, and of the skin generally, which is sometimes observed." To the first assertion, so vague and so general, I oppose his own words, (at p. 290 of the Edinburgh Journal for this year,) where, speaking of Willis's account of the epidemic that broke out in the spring of

1643, among the troops near Oxford, and was for some time unaccompanied by eruption, he says, "about midsummer, the disease increased in fatality," and then goes on to give the description of the eruption already referred to. I oppose to it also the general fact, that the eruption is seen in its greatest perfection in extensive epidemics, during which the disease is much more violent and fatal; and the fact, that in Glasgow, when the cases were few, the disease very mild, and the deaths 1 in 10, or 1 in 12, the eruption, if present, was never observed; whereas, when the cases became more numerous, the disease more malignant, and the deaths amounted to 1 in 8 and 1 in $6\frac{1}{2}$, the eruption became steadily more and more abundant. Dr Peebles accounts for the scantiness of the eruption in Edinburgh, at the time he made his observations, by typhus not being epidemic. Dr Henderson states the deaths among those with abundant eruption, as 1 in 5; among those with scanty eruption, as 1 in 8.3. Two of the three fatal cases in the latter class presented "extensive visceral disease of long standing;" while not one of the thirteen in the former class had any such disease. The duration of the cases was, on an average, between two and three days longer in the one than the other. My own observations, likewise, show the relation between the abundance of the eruption and the severity of the disease. The eruption was:

			Cases.	Deaths.	Rate of Mortality.
Copious.	Universally in	-	96	19	1 in 5
	Partially in	-	32	5	1 in 6.4
Scanty	-	-	11	1*	1 in 11

The dark tint of the eruption is noted as ominous by all authors. The quotations I have given above prove the general opinion; and I may likewise refer to most of the authors cited by Dr West, and Gauthier de Claubry, to Dr Tweedie, (p. 73,) and Dr Henderson, (p. 12.) The existence of a mortality in those with dark, three times greater than in those with light-coloured eruption, in the tables I have presented above, shows in the clearest manner the truth of these opinions, and renders needless any further comment. This fact ascertained, we cease to wonder at Val-leix's sweeping conclusions, drawn from 6 cases, 5 of which were fatal.

Typhoid fever has also its eruption,—a circumstance much insisted on by Gauthier de Claubry in proof of its identity with typhus. It is generally agreed, (Chomel, Louis, Gauthier de Claubry, Montault, and my own experience leads me to assert it,) that it appears later than the exanthema of typhus, but that is a point of very minor importance; the main question is, are its charac-

* In this one case, the eruption was petechial, and on inspection, I found disease of old standing, and aneurismal dilatation of the aorta.

ters the same? According to all the best authors on the subject, (Chomel, Rayer, Biett, Rochoux, &c.) it is distinguished from the morbilliform eruption of typhus by being distinct, rounded, slightly elevated above the skin, and of nearly uniform size. In only one case (which I saw in the Hotel Dieu last winter,) have I noticed any thing like an approach to the irregular, and very often confluent, rash of typhus, which is generally level with the skin, or if raised, is very slightly so, and that only during the stage of excitement. In all the other cases of typhoid fever in which I have seen the eruption, I should say that it appeared in small spots, rounded, and almost papular in form, being considerably elevated in the centre. Dr Perry of Glasgow was the first whom I heard maintain the complete difference of the two eruptions, and I am now fully satisfied of the accuracy of that opinion, for the following reasons.

1. The typhoid eruption is not permanent. That the typhous exanthema is so, has, I think, been shown by abundant evidence; but it is generally agreed that the rash in typhoid fever is "composed of several successive eruptions," that its duration varies from 3 to 17 days, † the mean duration being $7\frac{1}{2}$, and that each rosy spot is not commonly visible for more than three or four days, and sometimes less." I have often been able to verify this remark.

2. I have never seen a single case in which the typhoid eruption became petechial or even dark, and no author I have consulted pretends that it does. On the contrary, it always retains the same characters, the last crops (and I have seen them appear the day before death, in cases where there was the most complete prostration,) being as florid as the first. Reflections on the necessary relation between the state of the blood, and the appearance of the eruption, which I cannot now develop, but which are strongly confirmed by the frequent occurrence of a buffy coat, and a pretty firm coagulum, in typhoid fever, will naturally suggest themselves to every mind.

3. It is beyond a doubt that there is no relation between the eruption and the severity of the cases. I confess that I long felt inclined to believe, that the more plentiful the eruption, the less serious the case. I was particularly led to think so by the total absence or extreme scantiness of the eruption (though carefully sought for) in the Glasgow epidemic of 1836, which was very deadly, and by afterwards finding it more or less copious in sporadic cases presenting a much less intense form of the affection. After further inquiry, I find, that though in its fullest sense the opinion is incorrect, it is to a certain extent true. From a careful perusal of the valuable work of Chomel, I find that a large pro-

* Chomel, *Leçons* &c. p. 19. Also Louis, Vol. ii. p. 232, 241.

† Chomel and Louis.

portion of the worst cases, and of those who died, had either a scanty eruption or none at all; and it appears from the lucid statements of Louis,* that it was wanting in 9 out of 35 fatal cases, and that it was scanty (*en petit nombre*) in 18, or three-fourths of the remaining 26, some of them presenting only 5 or 6 spots; a number reckoned by Chomel (p. 8,) of no value, 15 or twenty being necessary, according to him to "characterize the typhoid affection." Fifty-four of fifty-seven patients who had severe attacks, but recovered, had the eruption, and of the remaining three, two came to the hospital on the 14th and 40th days of the disease. "In some cases," he adds, "there was only a small number of spots." In 12, "the eruption was very abundant." It was present "in all the cases where the affection was slight," whence he infers, that "its cause is special" or specific, and that it is not like other secondary phenomena, proportioned to the gravity of the disease and the febrile movement." Hence we may at least conclude, that in severe attacks of typhoid fever, the eruption is less frequent, and generally less plentiful than in those that are less serious. Does any one object that these reasonings prove not the difference of the eruptions, but merely a different determination of one and the same eruption, in the one case to the skin, in the other to the mucous surface, which has been called "the inverted skin?"—I reply that, according to this hypothesis, (for it is a pure hypothesis,) there should be in typhus with scanty eruption, a decided abdominal determination; but the fact, as undeniable as it is conclusive, is exactly the reverse; for numberless cases of typhus with scanty eruption are remarkable only for their mildness, and the absence of any visceral complication. This is particularly the case in children in whom the eruption is seldom seen.

We may also conclude generally, that the facts detailed go against the opinion of Chomel, (p. 336,) that "the cutaneous exanthema offers the same characters in the two affections; the only differences being in the number of the spots, and the time of their appearance," and that, on the contrary, the typhous differs from the typhoid eruption in its form, its duration, the changes it undergoes, and the relation it bears, as to colour and quantity, to the severity of the disease.

V.—In the plan I laid down, I proposed to consider some of the anatomical lesions. To enter on a comparison of the whole pathological anatomy of the two diseases, were a task as tiresome as it would be barren of any definite results. Instead, therefore, of inquiring into the state of all the organs in all the different cavities, I shall at once meet the question, Is the lesion of Peyer's glands, so constantly met with in typhoid fever, frequently, or is it ever found in typhus?

* Louis, Vol. ii. p. 231, et seq.

Chomel (p. 339,) appears to me to put the question in its true light. His third general conclusion is as follows; "If further observations demonstrate in typhus anatomical lesions similar to those met with in the typhoid affection, the identity of the two affections would be put beyond a doubt." To the philosophic caution of the inference of the French professor the following remark offers a strong contrast. A reviewer of various works on typhus and typhoid fever, in the *British and Foreign Medical Review* for last year, after asserting (p. 432,) that the symptoms during life are the same, and quoting a passage from Gauthier de Claubry, (referring to the experience of four out of twenty-two observers who described the epidemics of army typhus during the wars of the empire, as decisive of the question,) thus proceeds, "we do not know that any additional facts from the morbid anatomy of the intestinal canal are required. In our own country we do not require any argument to persuade us of the identity of these supposed distinct diseases; the evidence is constantly before our eyes!" Montault, notwithstanding the assertion of his reviewer, that his remarks are valueless, points out clearly the absence of the intestinal affection in typhus, and cites numerous authors to that effect. Roupell considers the absence of this lesion, which he fully admits as accidental; Tweedie combats strenuously the opinions of Broussais, (p. 57,) and adduces the frequent absence of intestinal lesion as proof of their incorrectness. I remarked at the outset that, in the cases detailed by Dr Tweedie, there were evidently two kinds of fever, the one distinguished from the other during life, by difference of intensity and duration, and after death by the lesions observed. I point of fact, I find 13 cases in which ulceration of Peyer's glands had taken place, and nine in which there was no intestinal lesion whatever, but general congestion. Dalmas, in his account of what he heard and learned (not saw,) at Dublin, cannot deny that the lesion is frequently absent; but only says, "it is perfectly well known" there. So it was in Glasgow, but even to me, a perfectly inexperienced observer, the distinctive symptoms were well known also. Gauthier de Claubry, evidently feeling the force of Chomel's remark, has laboured hard, throughout a very large portion of his work, to get rid of this very strong objection to his opinion. Reveillé Parise, on whose testimony he lays much weight, observed in those who died at Saragossa, "some gangrenous points in the interior of the intestines, and on the exterior violet patches," (p. 18.) Ducastaing (p. 16,) found in many of those who fell victims at Gaëta in 1802, "the mucous membrane ulcerated in several points." Tresat, at Walcheren, in 1809, found "gangrene of some parts of the intestinal canal," (p. 19.) Tort, at Dantzick, in 1813, found "the small intestine sprinkled with gangrenous

eschars," (p. 23.) Gilles de la Tourette, at Torgau, in 1813, found serous and bloody effusions, inflammations, ecchymoses, gangrenous spots of the mucous and serous membranes," (p. 24.) MM. Laurent and Ardy, Magnin, and Fauverge, mention inflammation, ulceration, gangrene in some points, violet patches, scirrhous indurations of the mucous membrane of the small gut, as occurring in the Mayence epidemic of 1813, (p. 26-7.) Thouvenel, in the department de la Meurthe, the same year, found "generally some gangrenous spots in several parts of the membranes of the intestines," (p. 35.) In the epidemic of the Salpêtrière in 1814, Pellerin, "in adynamic fever with diarrhœa, which," says Gauthier de Claubry, "he apparently believes a distinct disease in this circumstance," describes ulcerations that seem to correspond very closely with those found in typhoid fever, as "often," not always met with.

After the foregoing investigations are we not warranted in considering it, with Pellerin who witnessed it, "a distinct disease?" Chomel, also, with his customary caution, when speaking (p. 337-8,) of the same epidemic, remarks that he "cannot place entire confidence in his recollections." If such disease, as is daily found in the intestines of persons dying of typhoid fever, had been present, it could not, I should think, have been easily forgotten. Is it, then, really on such statements as these, on the existence of "violet stains" and "gangrenous spots," (which might, for aught we know to the contrary, have been the effect of decomposition, in a disease that often terminated with frightful rapidity,) that Gauthier de Claubry and his reviewer would rest the decision of the question at issue? It was but yesterday, that I saw at M. Barth's rooms, large elliptical patches of ulceration in the jejunum of a female, who had died of puerperal fever, with rupture of the uterus and peritonitis,—ulcers which, had they been found in a patient who had presented the ordinary symptoms of typhoid fever, would certainly have been looked upon as characteristic of the disease. Is, then, puerperal fever identical with the typhoid affection? I also saw a few days since, at M. Barth's, in a portion of gut taken from a patient who had, I believe, died of phthisis, large patches, of no determinate shape, extending around the whole calibre of the tube, the mucous membrane being in these parts immensely swollen, raised a couple of lines above the surrounding surface, traversed by deep rugæ, and having a greenish-black colour, with the characteristic gangrenous fœtor. Is it beyond the range of possibility, that such may have been the state of the intestines in the army epidemics referred to? Is it not rather highly probable that, where everything indicated a deeply altered and corrupted state of the blood, and local gangrene was of frequent occurrence, the gut, as alleged

by all the observers cited, may have been actually sphacelated? All that I have in view in these remarks is to demonstrate that, even were the existence of such lesions proved beyond a doubt, the alteration of the glands of Peyer, and the isolated follicles, now called characteristic, is very far from being proved thereby.

But are there no facts that bear against the reasonings of the French essayist? On the contrary, there are many. Lombard has graphically described his surprise on finding no lesions whatever in several cases he saw inspected while in Scotland and Ireland. We have seen the supposition Gauthier de Claubry has recourse to, in order to explain away his testimony; and, even granting that "the circumstances of misery and exhaustion," in which, according to him, the people of Scotland are placed, do, as he supposes, produce a strong resemblance in the prevailing fever to army typhus, (p. 143,) does he advance one step towards a removal of the difficulty, by gaining such an admission? Nay, rather, if M. Gauthier de Claubry has made out his position, that the intestinal lesion is present in typhus as well as in typhoid fever, whence this allusion to army typhus to explain the want of intestinal lesion—an allusion by which he demolishes the structure his work was intended to raise, and unwittingly discloses his own distrust in his own opinions? Again, M. Delbosc, (cited at p. 38,) in his account of the epidemic at Alby in 1823, mentions only "a rosy colour of the peritoneal tunic," and M. Keraudren and the Toulon physicians, "though their attention was specially directed to that point by the questions addressed to them by the Academy of Medicine,* never observed any intestinal lesion in the typhous epidemic at Toulon in 1829–30. I have already referred to Dr Tweedie's data, and have cited M. Dalmas's silence as impartial evidence, though in the united kingdom very few require evidence to convince them, that in Dublin, as elsewhere, a form of fever exists, of highly malignant character, and most extensively fatal, in which no intestinal lesion is found even on the most careful inspection. Dr Alison of Edinburgh, (from whom I cannot quote accurately, not having access to his work,) corroborates the experience of other observers.†

I now proceed to give some more circumstantial details upon this interesting subject. Dr West† gives the results of ten inspections. In five, (I merely speak of the state of the bowel,) "there was no morbid appearance whatever;" in the other five, "there was increased vascularity of the intestinal canal, extreme in one instance." "Once the glands of Peyer appeared enlarged, and

* Chomel, p. 338.

† Edin. Journal, Vol. 1. p. 132–3.

‡ Dr Alison has published no separate work, but *Observations, &c.* in the twenty-eighth Volume of this Journal, p. 233.—ED.

twice there was very considerable enlargement of the solitary glands ; but I never," says he, "found them ulcerated. Once the mucous membrane of the cæcum was very much softened and congested, and there was slight abrasion of the surface of some of the congested patches." Dr Reid* states, that of 101 cases examined in the Edinburgh Infirmary, by his predecessor, the late Dr Home, the elliptical patches were well defined in 29 ; they were more or less ulcerated in seven of that number," and in two out of the seven, perforation had taken place." He next gives† a summary of the morbid appearances in 41 cases inspected by himself. In 24, Peyer's glands were apparent and distinctly defined ;" in 6, "scarcely visible ;" in 11, invisible to "the naked eye." In 4 only were they "distinctly elevated ;" and in 2 of the 4, "this elevation was to no great extent, and limited to a few patches." In 2 only was there "any appearance of ulceration." In 4, the solitary glands were "distinctly visible." Regarding the connection of the symptoms during life with the *post mortem* lesions, he says,‡ "in 9 cases only out of the 24, in which the elliptical patches were distinctly visible, were there any abdominal symptoms during life, and in some of those cases, these certainly could not be referred to any affection of the elliptical patches of Peyer." "In one case in which they were not visible, the bowels are stated to have been rather loose, the stools watery and dark-coloured. In other two cases, there was considerable abdominal tenderness, without diarrhœa."

I shall preface with a few remarks, the two following and concluding tables, which give the results of my own observation. They refer to 22 inspections,—but a small number, it is true, yet made with as great care and attention to accuracy as I could bestow on them. The solution of the interesting question, Is there any connection between the abdominal symptoms during life, and the state of the intestinal follicles in typhus? may be attempted in different ways. We may compare the symptoms, whether positive or negative, with the mucous follicles, as regards 1. their number, and 2. their degree of developement. And, after having, as far as our data enable us, determined these points, we must not be unmindful of the objections that may be urged, as, for instance, that our terms are vague ; that our cases are too few to draw conclusions from that absorption of morbid matter may have taken place before death, or, it may be, that death has arrived before its deposition was possible. I shall meet some of these objections before-hand. The cases, I acknowledge, are few, but I maintain that they are a fair sample of hundreds that occur yearly, and at the same time direct attention to the numerous facts I have already cited. To take away all vagueness from the terms I

* Report, &c. p. 33.

Ibid. pp. 30, 31.

Ibid. p. 34.

employ, I make the following statements respecting the state of the intestinal follicles. It was very rare, in the cases of typhus, which I either inspected or saw inspected, during a two years residence in Glasgow Infirmary, to see these glands elevated a quarter of a line above the surrounding mucous membrane, and in the vast majority, it was very difficult to determine whether they were really elevated or not, the slight roughening or irregularity of their surface often causing a visual deception. When, therefore, we speak of the elevation of the aggregate glands in typhus, we are no longer occupied, as in typhoid fever, in the measurement of palpable magnitudes, but for the most part, with elevations, which French pathologists, as I have frequently witnessed, scarcely reckon any deviation from the healthy state. To avoid overlooking, not one, but many, of these (enlarged?) glands, often requires very minute attention. The other objections I shall leave to be answered by the facts about to be adduced, only remarking that the removal by absorption of the morbid matter from the follicles is a very rare termination, even in typhoid fever. Chomel mentions two cases in which that process had probably taken place,* and I cannot see why it should be more common and more speedy in typhus. The first of the following tables refers to the number of follicles met with in the small gut; the second to their state of development.

No. of enlarged follicles.	Cases.	Diarrhœa.		Constipation.	Tympanites.			Abdominal pain.			State of follicles.			
		Spontaneous.	Consecutive.		Slight.	Considerable.	None.	Slight.	Acute.	None.	Elevated.	Very slightly so.	Not elevated.	Scarcely seen.
none,	1	1			1			1						
1 to 5,	4	2	1	1	1	1	2	1	2	1				4
5 to 10,	4		3	1	1	1	2	4			2			2
10 to 15,	4	1	1	2	1	2	1		3	1	2		2	
15 to 20,	3		2	1		2	1		3		3			
20 to 30,	4		1	3			4	2		2	1	1	2	
30 to 40,	1		1		1			1			1			
40 to 50,	1		1		1				1				1	
	22	4	10	8	6	6	10	9	9	4	2	8	5	6

Intestinal follicles.	Cases.	Diarrhœa.		Constipation.	Tympanites.			Abdominal pain.		
		Spontaneous.	Consecutive.		Slight.	Considerable.	None.	Slight.	Acute.	None.
Distinctly elevated in	2		1	1	1		1		2	
Very slightly elevated,	8	1	4	3	1	3	4	3	4	1
Not elevated,	5		2	3	2	1	2		3	2
Scarcely seen,	6	2	3	1	1	1	4	4	1	1
	21	3	10	8	5	5	11	9	8	4

* Observ. 14 and 15, pp. 169, 173.

Thus, we see that 4 only of the 23 in whom diarrhœa was spontaneous, rank in the above list. In one of the 4, the diarrhœa depended on an attack of simple enteritis, which came on after partial convalescence, and carried off the patient on the 34th day. No trace of follicular disease was found on inspection, but "universal softening of the mucous membrane from the duodenum to the rectum." Two of the remaining 3 may almost be said to have presented no trace of follicular disease, for the 3 or 4 follicles observed in each were barely perceptible to the naked eye. In the fourth, 14 Peyer's glands, scarcely at all elevated, were detected. But, what chiefly concerns us to notice is the striking fact, that, as the number of enlarged follicles increases, the cases of spontaneous diarrhœa diminish, and those in whom consecutive diarrhœa and costiveness were observed, become more numerous. In fact, not one of those in whom the greatest number of enlarged follicles was observed, but was either constipated, or had diarrhœa brought on by medicine, during life. This confirms our former deductions, agrees entirely with those of Valleix, and shows that the appearances observed in typhus depend upon local irritation, and not on specific disease.

It will also be observed that, in 10 of the 22, (including 5 of those in whom from 15 to 30 enlarged glands were observed,) the feel of the abdomen was natural throughout the disease. Of the 6 in whom the belly was slightly tympanitic, that symptom was observed only one day in 4 patients, (the one in whom the greatest number of enlarged follicles was found being among them;) in one with forty enlarged glands, it was present during two days; the sixth was the patient who died of enteritis. Of the six, on the other hand, who presented a high degree of tympanites, in not one were there so many as twenty, and in one whose abdomen was excessively distended, scarcely a trace of follicular disease was detected.

There seems, at first sight, to be more connection between the presence of abdominal pain, and the number of altered glands, but this is only apparent, for a reference to the figures given above shows, that, in eight of the nine in whom the pain was acute, not more than 20 were found in the whole course of the gut; that, of the nine who complained of slight pain, they were either not at all, or very slightly affected in six; and that, if two of those, in whom they were noticed in large number, (20 to 30), had slight pain, two more of the same group had no pain at all, at any period of the disease. The concluding part of the table proves that quite as little analogy exists between the number of follicles and their degree of developement. As a fitting commentary on what I have now advanced, I find that in the only case in which considerable elevation of Peyer's glands, (about half

a line), and a few ulcers (not follicular) in the cæcum, were observed, the bowels were obstinately constipated, the feel of the belly was natural throughout, and the abdominal pain was either exceedingly slight or absent, till within a few hours of death, having come on after the administration of a clyster, composed of equal parts of turpentine and castor-oil.

These remarks are still further confirmed by a reference to the second table, in which the symptoms are considered with respect to the degree of developement of the follicles. The same dependence of the intestinal lesion on local irritation, the same entire disproportion between the extent of the lesion, and the intensity of the symptoms, are brought out so clearly by it, as to render any further reflections needless. The utter futility of the objection founded on the possible absorption or non-deposition of morbid matter, will appear from the following facts. Comparing the number of follicles with the days of death, I find that, of those belonging to the first group, one died on the 12th, another on the 13th, a third on the 14th, and a fourth on the 16th day. Of the second group, two died on the 12th, another on the 17th, and a fourth on the 36th day. Of the third, one died on the 10th, one on the 14th, the other two on the 21st and 24th days. The three belonging to the fourth group died severally on the 13th, 15th, and 19th; those in the fifth, on the 10th, 11th, and 13th, the two in the 6th and 7th, respectively on the 11th and 22d days of the fever. Thus, then, of two who die on the same day of the affection, one presents scarcely a trace of disease, another as many as thirty enlarged glands. In one who dies on the 11th day, no less than forty Peyer's glands are observed, while in others who die on the 12th, 13th, 14th, and 16th, scarcely any morbid appearance is detected; in another, who dies on the 22d, no less than 45, while in a 7th, who dies on the 24th, only 10 or 11 Peyer's glands are obscurely visible.

Again, if we consider the developement of the glands, in reference to the day of death, we find that, of the first group, one died on the 10th, another on the 11th day; of the second, one on the 10th, another on the 11th, a third on the 13th, a fourth on the 15th, and the remaining four, respectively, on the 17th, 19th, 24th, and 36th days; of the third, one on the 11th, another on the 13th, a third on the 14th, the remaining two on the 21st and 22d days; of the fourth, three died on the 12th, and the remaining three severally on the 13th, 14th, and 16th days. Thus two die on the 10th and 11th days with Peyer's glands distinctly elevated, (in one of them to the extent of about half-a-line;) they are scarcely discernible in four who die on the 12th and 13th days; while they are in a state of slight elevation in eight, who

die on all different days between the 10th and the 36th. Does this, I ask, look like the steady course of a constant lesion, depending upon a constant and specific cause?

The solitary glands were visible, in small number, in three cases; in one, in the small, in two, in the large intestines. I have, however, seen them very numerous, both in the one and the other, though they were never larger, and seldom so large as a pin's head. They were white, sometimes surrounded with a vivid red areola, and each had a small black point in its centre. The aggregate glands, situated on the free border of the gut, were of all sizes from two or three lines to two or three inches in length, and varied from a couple of lines to nearly an inch in breadth. They were sometimes white, sometimes grayish, sometimes slate-coloured, frequently dotted over with small black points, often reticulated, while sometimes neither the one nor the other appearance was noticed. It is a remarkable fact that, in several cases, the glands were much more numerous in the jejunum than the ileum, and that in two there were none near the ileo-cæcal valve. Not unfrequently, the mucous membrane around them was pale, at other times they were situated amid patches of very dark congestion; in which case they partook more or less of the dark colour of the surrounding membranes. Sometimes, but very seldom, the mucous membrane itself was reddened, and injected with minute vessels; the congestion of the veins of the submucous cellular tissue was, on the contrary, very common.

No one denies the very frequent affection of the mesenteric glands in typhoid fever. The experience of every one who has seen the inspections of patients cut off by that disease must confirm the facts stated by Chomel, Louis, and Petit and Serres. Though very rare in typhus, it is nevertheless sometimes found. I find it noted by Dr Tweedie in two cases, (1 and 44). In the former, "the lungs and abdominal viscera were healthy, excepting the mesenteric glands, which were enlarged and partially affected with suppurations of a scrofulous character (p. 108.*)" In the latter, "there was no ulceration of the intestines, but the glands of the mesentery were enlarged." Of my own cases, the mesenteric glands were enlarged in four; in one very slightly, in two in a greater degree, in a fourth to a large extent, but not suppurated. In the first, death took place on the 11th day, and twenty or thirty Peyer's glands, not at all elevated, were found in the small gut; one of the next two presented eight very slightly developed, the other none at all; and in the fourth, there were nineteen very slightly elevated. In all of those, then, in whom the enlarged follicles were very numerous, including the only patient in whom they were considerably elevated, the mesenteric glands were unaffected.

I have been enabled, by the kindness of Dr Reid, to present the reader with a much larger mass of information. From the publication of the Report, till the end of June, 33 cases of fever were inspected in the Edinburgh Infirmary. Of that number only two presented the characteristic lesion of the intestinal follicles met with in typhoid fever. In both of them the lardaceous deposit (*plaque gaufrée*) was most distinctly seen, and both the aggregate and solitary glands were affected in great numbers, the latter forming the whitish "flattened tumours" so well described by Louis. Neither of the patients belong to Edinburgh; both were workers on the Edinburgh and Glasgow Railway, and were sent from Linlithgow. If, then, *dothi-enteritis* is the same disease, and depends on the same causes, as typhus, how does it happen that the former is produced at Linlithgow and at Anstruther, and not in Edinburgh, where the latter is constantly occurring, and where so many circumstances favourable, not only to its production, but, (by the hypothesis,) likewise to that of *dothi-enteritis*, are always at work? The duration of these cases is also remarkable. The one, it appears, had been four, the other five, weeks ill. One of them had convalesced, but had a relapse, of which he died; and, on dissection, the intestinal follicles and mesenteric glands were the only parts diseased, so that the relapse cannot be referred to any lesion of any other organ. It is further remarkable, that the aggregate glands were in a state of advanced ulceration, while most of the solitary glands around them were quite entire. In the remaining 31 cases—

Peyer's glands were distinctly elevated, but not ulcerated, in	4
visible, but not elevated, in	9
scarcely visible in	7
not visible in	11
	—
	31

The solitary glands were slightly developed in two of the above cases. The mesenteric glands were healthy in 25, slightly enlarged in 4, considerably enlarged and softened in 1, and in a 6th increased in size, and partially converted into chalky matter. Dr Reid also informed me that, in a man who died lately of the effects of compound fracture, Peyer's glands were more distinctly diseased than in any case of typhus he had met with.

Having thus entered pretty minutely into the pathological anatomy of typhus, I put it to every one who has ever seen the two diseases, or read the descriptions given by the best authors, of the lesions so constantly observed in typhoid fever, whether those found in the former disease can with any truth be called "perfectly identical" with those of the latter. That the existence of trifling

intestinal disease in typhus attests its general family resemblance to typhoid fever, as the presence of organs in a rudimentary state shows the analogy subsisting between individuals of different species in the animal kingdom, might be more readily admitted; but that they are one and the same species, numerous well-established facts seem most clearly to disprove. If asked to describe shortly the pathology of typhus, I might sum it up in these words:—general congestion, no prominent local disease—a congestion so general and so excessive as is rarely, if ever, met with in typhoid fever or any other disease,—a congestion singled out by most authors as one of its leading characteristics—a congestion that is evident, during life, by the livid skin and petechial eruption, and is found, after death, to have affected more or less every organ in the animal economy,—a congestion so constant as to be often passed over as almost valueless, but which future researches may prove to be the grand peculiarity of typhus; and which, in common with many other considerations, directs attention to the blood as the essential seat of the disease. If required, on the other hand, to give a brief account of the pathology of typhoid fever, I should be inclined to sum it up in these words:—prominent local lesion, comparatively little general congestion.

VI.—From the treatment of the two diseases we can infer but little. The interesting discussions held at various times on the subject in the Academie de Medecine,* strikingly show the discordance of sentiment that prevails in France on the treatment of *dothineritis*. One upholds the purgative system as the best and the only proper one, another condemns it as fraught with the greatest danger; one insists that the rational method, which consists in combating symptoms as they arise, is the only rational one, but another condemns it as most irrational, because it kills one in three; one strongly recommends the frequent use of the lancet, according to his formula, even in advanced stages of the disease; while another, condemning not only the lancet, but all treatment whatever, advocates a method in the purest sense expectant, and unlimited faith in the workings of nature, any attempt to interfere with them being attended with evil results. And every one has facts on his side. The most remarkable statements, perhaps, of any, are those of Professor Bouillaud, who, employing his “bleeding formula,” (viz. small bleedings repeated two or three times daily, sometimes even during the adynamic stage, when the patients presented “fuliginous teeth and tongue, and the last degree of prostration,) and including only those cases that were somewhat serious, found a mortality among his patients

* See more particularly those on Delarrouque's Memoirs, in the “Bulletins de l'Academie” for March 14th, 21st, and 28th, and April 4th and 11th 1837. Also Bouillaud's statements at Vol. i. p. 250.

of one in 6.5.* Again, at one of the meetings in March 1837,† he declared, that, “counting all the cases, he had lost only one in 24, and including all the slight cases, one in 16 or 17‡” Who that is acquainted with typhus, and has seen the sudden and alarming asthenia that often follows the abstraction of a few ounces of blood during a state of great excitement and oppression, but must feel convinced that, treated after M. Bouillaud’s method, 99 out of every 100 would die? Gauthier de Claubry, while he concludes, from his own experience of typhus, and the statements of numerous distinguished authors whom he cites that “blood-letting (p. 153) may be useful, if not absolutely indispensable in certain cases, indifferent in a great number, hurtful in many others,” (an opinion he shares with Pringle, Hildenbrand, and the immense majority of British physicians,) confesses, (p. 167,) that the general opinion is in favour of its use in typhoid fever; and besides referring to the constant employment of it in the beginning of typhoid fever by Louis and Chomel, states, that “twenty years of an extensive practice, and of observations made in the hospitals of a large city, as also a comparison of the results of the private practice of a great number of physicians,” have led him to adopt a similar treatment. As to the use of purgatives, I may state, that while in Glasgow they were generally used in typhus, the marked difference in the state of the bowels in typhoid fever led to the general use of opium, either alone, or combined with mercury and chalk. All, in short, that we can infer from the practice at present in use is, that the treatment generally proscribed in the one is generally adopted in the other disease.

On a review, then, of all that has been advanced, it would appear that typhus and typhoid fever present important differences, as regards their probable origin, their proximate causes, their course, many of their symptoms, their diseased appearances, and the treatment applied in each. Are they then identical, or are they not? I feel that it would be presumptuous in me to hazard a direct reply; nor do I demand an answer in the affirmative merely on the faith of what I have stated. All I can ask or wish for is careful, extensive, and minute inquiry, without prepossession or love of system, and a satisfactory solution must soon be arrived at.

“Notwithstanding all the works,” says Chomel, (p. 338,) “which the last few years have produced on the typhoid affection, its identity with typhus, though probable, is not yet certain; and yet there are few questions of which the solution would be so highly important.” I have accordingly attempted in the preceding pages to concentrate attention, as far as lay in my power, on the leading peculiarities of the two affections; I have endeavoured, by fre-

* Séance de l’Academie, Octobre 16, 1835.

† See Bulletins de l’Academie, Vol. i. p. 520.

quent reference to the works of approved authors, by collecting together valuable facts hitherto widely scattered, and by adding the results of my own observation, to bring as large a mass of information as possible to bear on the question in debate; I have attempted—with what success others must judge—to establish certain important distinctions between the two diseases; I have particularly sought to found my conclusions on practical knowledge rather than theoretical reasonings; and if I have contributed towards the elucidation of this obscure subject, or shall be successful in drawing the attention of abler and more experienced observers to its investigation, my trouble is more than repaid.





