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AN INQUIRY

INTO

THE SOURCES AND MODE OF ACTION

OF THE

POISON OF FEVER.

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MUCH as has been written upon the history of fever, it cannot by any means be considered as an exhausted subject. If indeed we were to test our knowledge of its sources by the universality of their admission, and consider the general agreement of all observers as to their laws as the true proof of these being fully ascertained—a criterion which is applicable to medicine as to the other sciences of observation—we should see reason to conclude that in reality our knowledge of the causes of fever and their modes of action upon the living body is of very small account, and by no means of the most accurate description; for though, in this country at least, the doctrines of localization of fever are not advocated, nor fever considered the effect of inflammation of any particular organ or organs, we find in the most recent writers, equally as in the ancient, the widest differences of opinion as to the phenomena which constitute the origin or nature of this *essential disease*.

An analysis of the mass of conflicting statements upon this subject may perhaps be useful, if only as a preliminary step to other inquiries, by shewing how much of what has been put forward as evidence is really founded on observation, and how much is on the contrary mere matter of opinion and not of fact. Such an examination of what has been advanced upon the disputed question, it is proposed to attempt in the following enquiry.

We regard the essential disease termed fever as the effect of the action on the living body of a morbid poison—in other words of—

"One of that peculiar class of substances which are generated during certain processes of decomposition, and which act upon the animal economy as deadly poisons; not on account of their power of entering into combination with it, or by reason of their containing a poisonous material, but solely by virtue of their particular condition."*

The mode of operation of this poison upon the body is a fertile theme for disputation between the humoralists and solidists of this as of preceding ages, and whence it is derived and where generated—whether in the body or out of the body—the contagionists and non-contagionists are as much disagreed about as ever.

The humoralist holds that the very definition of a morbid poison, if correctly given by the distinguished author from whom we have adopted it, would point to the blood as the subject of its operations. Since, assuming that the *essence* of such poison is that its elements are in a state of decomposition or transposition—and its *action* to communicate that peculiar transposition to the constituents of the body with which it may be brought into contact, *He* finds in the blood a substance the most susceptible of any part of the organism of the action of exterior influences, and whose constituents are the most prone of any to form new combinations. The humoralist also points to the analogy of other morbid

* Liebig's Organic Chemistry.

poisons, which produce their specific effects upon their direct introduction into the blood. He points to the latent period common to both; and, if he be a contagionist (as he must be), he sees in the formation of the poison by the blood the consequence of the introduction of organic matter in a state of progressive transposition or decomposition (such as is the contagious miasm) into a mixed fluid in which its constituents are contained, and the reproduction in that fluid of the exciting body, exactly as yeast is reproduced when added to a mixed fluid in which the gluten from which it originated is contained. On the other hand, the solidist considers that the nervous system is so much engaged in fever, that the poison must be there, or, the phenomena of the latent period are attributable to the nervous system, or, dating this commencement of fever from the nervous shock, sometimes attendant on exposure, and *assuming* that the poison is received into the organism *then* and *there*, he sees an analogy between the action of the poison and certain narcotic substances which he *assumes* act on the nervous system without entering the circulation;—and therefore—fever so acts—or—lastly, the *source* of the poison not being apparent, and the shock preceding the fever, he finds that he can produce contagious fever by a moral impression on the nervous system without the action of a poison at all! These are some of the different opinions maintained by recent and able writers on the nature of fever, and which we shall have to glance at when considering the mode of action of the sources of the disease. As to these sources, our latest writers are so disagreed as to make an analysis of their opinions and evidence no easy matter. If we placed them in juxta position according to the doctrines propounded and denied, our index would run thus—

Fever contagious—not contagious.
Arises from putrefying animal matter—denied.
Arises from putrefying vegetable matter—denied.
Infection a direct emanation from the patient—denied.
Infection capable of being generated de novo—denied.
Atmosphere of patient infectious—denied.
Contact of ditto infectious—denied.
Fomites infectious—denied.
Fever originating in miasm contagious—denied.
Identity of foregoing with typhus asserted and denied.

These conflicting opinions will come under review successively in the course of an examination into the following questions.

1. The existence of a special animal poison arising from infection, and producing a specific disease—typhus.

2. The generation during the decomposition of organic substances of a poison capable of producing fever when applied to the living body.

3. The power of this paludal fever to communicate itself from one individual to another. Does it possess the power of infection per se? in other words, are typhus and typhoid fevers identical? or, does it acquire it by the aid of adventitious circumstances, and so become communicable by conversion into or superaddition of typhus?

4. Arising out of the preceding is the enquiry—what are the adventitious aids to the diffusion of each kind of fever? the laws which regulate their epidemics, and the sanatory measures best calculated to neutralise their operation.

CHAP. I.—OF THE INFECTIOUS ANIMAL POISON GENERATED IN THE LIVING HUMAN BODY, AND CAPABLE OF PRODUCING FEVER WHEN APPLIED TO HEALTHY BODIES.

SECT. 1.—*Proof of its tangible existence.*—It might have been supposed that the accumulated evidence of infection presented in the histories of the typhus of Great Britain, would satisfy the most incredulous; but it is not so, and a recent author has denied the existence of such a cause of fever as atmospheric contagion*—in other words, of "an atmosphere holding in solution a specific contagious poison." Because—"it has never been unequivocally manifested to any of the external senses; it has never been seen combined with the atmosphere, or precipitated from it, or abstracted therefrom to solid bodies."

It has been urged in reply, that this is equally the case with miasm and vitiated air of all kinds, which last, this author himself has endeavoured to prove, is the source of contagious fevers. But this answer is not correct, the fact being, that organic matter in a state of decomposition, or progressive transformation, is present in both. We shall hereafter adduce evidence of this fact with regard to miasm. As to its presence in aerial contagion Liebig states that—"all the observations hitherto made upon gaseous contagious matters prove that they also are substances in a state of decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water containing a certain quantity of this matter in solution. This water soon becomes turbid, and in common language, putrefies; or, to describe the change more correctly, the state of decomposition of the dissolved contagious matter is completed in the water. The odour of gaseous contagious matters," says the same author, "is generally accompanied by ammonia, which may be considered in many cases as the means through which the contagious matter receives a gaseous form.. ...... Ammonia is very generally produced in cases of disease; it is always emitted in those in which contagion is generated, and is an invariable product of the decomposition of animal matter. The presence of ammonia in the air of chambers, in which diseased patients lie, particularly of those afflicted with a contagious disease, may be readily detected; for the moisture condensed by the ice in the manner just described, produces a white precipitate in a solution of corrosive sublimate, just as a solution of ammonia does..... By evaporating acids in air containing gaseous contagions, the ammonia is neutralized, and we thus prevent farther decomposition and destroy the power of the contagion that is its state of chemical change."

"To this decisive proof of its presence may be added the fact of its being frequently recognized by one of the senses, that of smell, in those cases, in which it has proved active as a poison. For instance—a gentleman in this neighbourhood, not long since, passed through a severe and lengthened typhus fever. About the tenth day of convalescence, while walking across the room, leaning upon the arm of his son, the latter was struck by the odour from his father's body; he immediately became sick at stomach, and on the next day had rigor followed by fever of the same type and duration (21 days) as his father's."

Dr. Montgomery's† attack of fever, related by himself, gives similar proof that the aerial contagion may be occasionally recognized by this sense.

"On the 10th of August, I visited a patient in fever, and hearing from the nurse that there were spots on the patient's skin, I stooped very close to her to satisfy myself, and while so doing, I was sensible of a very disagreeable odour from the skin. At the moment, it made a considerable impression on the sense of smell, being almost as pungent as the odour from an ammoniacal salt. The smell continued in my mind all day, &c."

It is true a sceptical reasoner might argue for the possible existence in such cases of an unhealthy locality, impure air, &c.; but much of the evidence of contagion which we possess, is free from any such objection.

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* Dr. Scott Allison—Essay on Contagion.

† In Marsh's Essay on the Origin of Fever.

SECT. 2.—*The Testimony in proof of the Power of this Poison to cause Fever*—or, as it may be expressed, the proof that the disease has arisen from exposure to the emanations from the bodies of those similarly affected, requires to be of a very exact kind, since the opponents of the doctrine of infection, who, like the writer above quoted, affirm, that "those who have communication with the sick do not suffer in a greater proportion than those who keep apart," explain the many instances in contradiction of this assertion which occur among the medical attendants, nurses, and relatives of the sick, by attributing them to the "locality" and to "impure air," and add, that "it is however almost entirely on such exceptions as these that the contagionists depend for the maintenance of their gloomy doctrines."*

The great weight of the proof derived from the experience of the large fever hospitals in England, Ireland, and Scotland, has been well shewn by Drs. Tweedie,† Alison,‡ Christison,|| and Davidson,§ and the last gentleman justly observes, that "the simple relation of these facts would, with the majority of men, produce conviction that fever was at least contagious in these hospitals, provided the mind was not pre-occupied with an opposite theory." Certainly none but a determined anti-contagionist could resist the fact, that, in the large fever hospitals of the three countries, every clerk has, during some period of his attendance, laboured under fever.

It is also ably proved by Dr. Christison, that the proportion of attacks among attendants is in the ratio of their exposure to the emanations from the sick. It being observed that, in the Edinburgh hospitals, they were affected in the following order as to frequency. 1. Nurses. 2. Resident clerks or house-surgeons. 3. House servants. 4. Medical students not attached to the service of the institution. Thus, in the epidemic of 1818, of 38 nurses, only two or three escaped. Of the 15 gentlemen who filled the office of resident clerk between 1817 and 1820, only two escaped.

But, overwhelming as this argument from hospital experience appears, some have considered it open to objections.¶ It has been argued, that the typhus thus received (or rather the infection of typhus) is *factitious*, and created by causes over which we ought to have exerted due control; "that the poison can only be made effective through contamination of atmosphere under long-continued accumulation of morbific effluvia; and, in fine, that the atmosphere of the patient is infectious, and not his person." This argument receives some support from the experience of large general hospitals, which, particularly in London and Bristol, give admission to cases of typhus without its ever being observed to spread; and, from the acknowledged rarity of communication under the closest approximation among the better classes of society. It is said also that M. Louis never saw a case of communication of fever in an hospital, and Dr. Elliotson states that he never saw a case of fever infectious. It may

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* Medico-Chirurgical Review, Vol. II. New Series.

† Clinical Illustrations of Fever, and Art. Fever in Cyclop. of Pract. Med.

‡ Essay on the State of the Poor in Scotland.

|| Library of Medicine. Article, Fever.

§ Thackeray, Prize Essay.

¶ Dr. Fergusson, Edinburgh Med. and Surg. Journal, No. 112. See also a Discussion at the Royal Academy of Medicine, reported in the Medico-Chirurgical Review, Jan. 1839, in which the opinion was advocated by MM. Rochoux and Chervin, "that the disease is not communicable directly from one person to another, but is only transmissible in the way of infection, when the atmosphere around becomes loaded with the miasms which exhale from the bodies of the sick."

however be urged in reply, that the observations of the latter eminent observers apply to a different fever—an endemial; and that the argument proves no more than that the infection of typhus is weak, compared with other infections.

But there is another kind of evidence, scarcely less decisive than that derived from the records of the large hospitals. It is thus somewhat flippantly disposed of by Dr. Davidson.

"In the outset it may be stated that we do not mean to fatigue the reader by stories about fomites, and persons who have carried the contagion about them for months or years, nor to hunt out a particular individual who has conveyed it from one town to another," &c.

Now we think it is an admirable rule "as laid down by Dr. Elliotson,"*—"That for infection to be *proved,* the individual who communicates the disease must go from the place where he resides to the spot where the healthy person is, and there give it to the latter. If the healthy person go to the sick person, and the sick person be still in the place where he was living when attacked, then no one can say that the disease which the former contracts has not been produced from the *situation,* and not from the *patient.* The disease may have arisen from contagion—from the emanations of the patient—but this is not proved....... Whenever such a thing occurs as disease being produced in a healthy spot by the approach of an unhealthy person to a healthy one, or by the application of fomites to a healthy person, then it is a proof of contagion, provided the instances be sufficiently numerous, for one or two cases may be quite accidental."

The following is a fair case of importation by both person and fomites :—"A beggar from Limerick obtained admission into a labourer's cabin for herself and a dying child. In five days after she quitted the cabin fever took place in one of the family, which consisted of a man, his wife, and five children, and in succession, within a day or two of each other, every individual sickened, and two children from a neighbouring cabin, who had attended the child's wake, took the same fever within ten days after, and communicated it to their family. The beggar (*herself in good health*) went to a farmer's house two miles distant, and obtained a lodging for the night, after her child was buried—every individual in the family (five in number) also took the fever within a few days—these fevers were all severe."†

That these cases of importation of infection occasionally exercise a very great influence in the spread of fever, we are convinced by our own hospital experience. From the middle of the year 1834 to the same period in the year 1836, scarcely a single case of *contagious* typhus was admitted into the Navan fever hospital. The entire number of *fever* cases only amounting in that time to 363, and these being all instances of epidemic gastric fever or endemial typhoid fever. In the month of July, 1836, three cases of a *new fever* were admitted together. On enquiring their history, I was told that one of them was the seventh of his family who had been attacked—the other six having died. The two men who were admitted with him came from the same neighbourhood—seven miles distant—and had both had communication with the infected family. These were cases of typhus with measly efflorescence, profound adynamia, delirium, &c.

About two months afterwards an elderly man, with six of his family, were admitted labouring under typhus fever. They were from an opposite direction, about seven miles distant. The fevers, of which these were the commencing cases, spread rapidly and widely, and such was their effect upon our admissions, that the number of fever patients increased from 363 in two years to 400 in

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* Lectures by Rogers.

† Barker and Cheyne's Report, Vol. II.

1837, and 600 in 1838; when they were at their height. While these cases continued distinctly marked, and differed so much from our ordinary endemial fevers, as to be recognised at once by the nurses, as well as to be dreaded from their greater fatality and until they became merged in the epidemic of the past year, our cases of typhus were nearly exclusively derived from the districts in which these originated, or to which they had spread.

Careful enquiries were made as to the source of the epidemic in each case, and the following particulars were ascertained.

It appeared that in the first, a man had arrived in this country from America. It was stated that the voyage had been an unusually rapid one, and he had been ill the whole, or nearly the whole time. On landing, he was immediately removed to his father's house, twenty miles distant, and on his arrival there, was seen by a medical man, who pronounced his disease to be fever. He died on the second day after his arrival.

His father's house and neighbourhood was previously quite healthy; but in two days after his death, the father sickened, and, on the day following, his sister. She communicated the disease to her husband, who lived half a mile distant. He was attended by his brother, who caught the disease, and was one of the three first brought into hospital. The father was visited before his death by a brother, residing nearly two miles distant—on his return home he sickened, and in the course of his illness communicated it to his son. A brother of the importer contracted the disease, (apparently from his father,) and was sent into hospital, where he died, as did all the above, with the exception of the one who was sent to hospital. In short, of the family of the importer, eight out of nine were infected, and seven died.

In the course of a short time several other families, we have been informed, were completely exterminated. It spread with a rapidity and fatality perfectly unprecedented and long maintained its hold in the town and neighbourhood.

We, of course, have no means of determining the source from which the original case was derived; but we were much struck on meeting with Dr. Gerrhard's account * of the typhus which prevailed in some parts of America in that year, with the resemblance between this epidemic and that which he has so well described—especially in the acrid infectiousness of both.

We have not to *hunt* so far for the second case.

This man's daughter was a servant in Dublin, where she contracted typhus and died. Her brother went to see her, and remained till her funeral took place. He sickened—came home, and died of what was described to me as a long, spotted, fever.

After his death, the abovementioned seven persons sickened within a day or two of each other, and were sent to hospital. The father died; several of the others had very severe fever; typhus spread from this house, first to the immediate neighbourhood, and subsequently to the surrounding country.

These instances are by no means all of the kind that have occurred within the time mentioned, but are selected on account of their wider influence and the unequivocal nature of the testimony they afford to the infectiousness of fever.

Among the most unquestionable sources of fever, is the communication of it by fomites carried from the patient to some place previously healthy.† It is in this manner that washerwomen frequently become the subjects of fever. Dr.

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* American Journal of Medical Sciences, February, 1837, and Dublin Medical Journal, July, 1837.

† Dr, Stark's Experiments on the Power of different Colours to absorb Odorous Particles, (Edinburgh Philosophical Journal, April, 1834,) shew that woollen substances constitute the most powerful fomites.

Tweedie says, " to shew that the disease may be engendered by fomites in clothing, the laundresses, whose duty it is to wash the patients' clothes, are so invariably and frequently attacked with fever, that few women will undertake this loathsome and frequently disgusting duty."

Dr. Armstrong,* an anti-contagionist, had previously noticed the same fact.

Dr. Reid and Dr. Cheyne,† inform us that, during the epidemic of 1817, not a single person of those appointed to receive the clothes of the sick escaped the disease.

The preventive effects of an early removal of the sick is one of the strongest proofs of infection, since the same measure produces no such effect in the endemial fevers.‡ The effect of early removal of the sick and the cleansing and whitewashing of their apartments, was very remarkable in checking the progress of the disease in some families, while, from the neglect of these precautions, the number of the sick rapidly increased in others. Two neighbouring houses, in Barrack-street, afforded an illustration of this remark, viz. Nos. 41, and 47. In the former the disease began in two different families, and its progress was immediately checked by early removal, cleansing, &c. In the latter, the individual first affected remained at home and died of the fever, but not before he had communicated the disease to eighteen persons in a short time.

On the effect of early removal of the sick, Dr. Alison § remarks, we should have little difficulty in pointing out above a hundred houses where a single case of fever has occurred, where the patient had been removed speedily, and the place cleansed, and where there had been no recurrence, &c. Dr. Ferriar || states, " that formerly, when a fever began in the Manchester Infirmary, it was found necessary to dismiss almost all the patients...... but since a few rooms were built in 1792, separated from the rest of the wards, for the reception of such cases, though the infection has been more than once introduced, yet by removing such patients as shewed symptoms of fever at their first appearance into the secluded ward, and preventing all communication between them or their nurses, and the other patients and servants, the complaint has been stopped; and no reason has again occurred for a precipitate discharge of patients."

But in applying these facts to the proof of the infectious *nature* of fever, we are again met by the argument, that under all these circumstances *of* crowding, &c. they only prove that a factitious atmosphere of contagion is produced, and the anti-contagionist points to instances of typhus received into the Bristol and other hospitals, and mingled among the other patients without ever spreading the disease.

Dr. Davidson's quotation from Prichard, supports this view as regards Bristol. " In St. Peter's the wards are very small, and the beds were near each other —offensive smells, often perceptible, and, under these circumstances, the disease manifestly contagious. In the infirmary the wards are lofty and well ventilated —here also the fever patients were dispersed among invalids of every description—no instance occurred of the propagation of the fever—none of the nurses were attacked, nor any of the patients infected, though lying within two feet of cases of typhus gravior." (page 12.)

From the infectious form of the disease prevailing almost exclusively among

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* Lectures by Rix.

† Dublin Medical Transactions, Vol. 3; and Dublin Hospital Reports, Vol. 2.

‡ Report of Inspectors of House of Industry, quoted by Cheyne. Dublin Hosp. Rep. Vol. 2.

§ Edinburgh Medical and Surgical Journal, Vol. 28.

|| Medical Histories, Vol. 2.

the poor, it is difficult to obtain, in Ireland, a case not liable to the above objections. The following is perhaps as decisive, and as free from objection as may be.

In the month of March, 1839, an old man, with his son and daughter—all of them persons of cleanly appearance—and in comfortable circumstances—were admitted into the Navan Fever Hospital. The history which they gave of their seizure was, that another son, the only other member of the family, had contracted fever, by sleeping for two nights in a house eight miles distant, in which was a person in an advanced stage of the disease. On his return home, he lay down in a fever of twenty-one days. About the 3d day after his crisis, his father sickened—on the following day, his sister, and in a day or two after, his brother. A day or two before these persons came into hospital, a young man, a cousin of the parties, was admitted. He was one of a family of *ten* living near his uncle's house. He *alone*, of this family, visited his cousin during his illness. His family shewed their caution farther, by sending him into hospital early in the disease. He passed through the same fever (typhus, severe in all, and fatal in the old man,) as the others, but no one of his large family took the disease; and on enquiry, a year afterwards, I learned that they were all still free from fever.

SECT. 3.—*Varieties in the Nature or Sources of the Poison.*—The dogma of Dr. Bancroft, that the contagion of typhus*—" The original work of our common Creator must have been continued in existence by the energies of a living principle, exerted successively in the different bodies through which it has been transmitted from one generation to another,"—has met with comparatively few supporters among late writers on fever. Elliotson,† Barker,‡ Roupell,§ Perry,‖ and Davidson,¶ espouse this doctrine, but without adding in the least to the meagre facts upon which it is founded.

On the other hand, numerous observers assert the production of typhus under circumstances in which the existence of a fever poison derived from a person labouring under the disease, was out of the question; and therefore they have assumed " that certain physical and moral conditions may so act on the operations of the body as to cause it to generate within itself that which produces the phenomena of fever, independent of any exterior poison."

Dr. Ferriar ** thus enumerates the circumstances, under the combined action of which fever has been observed to arise spontaneously.

1. Want of fresh air.
2. A deficient or improper diet.
3. Want of cleanliness, and, chiefly, want of a proper renewal and change of clothes.
4. Anxiety and depression of spirits.

The second and fourth of these are probably the essential causes of the generation of the poison, and the others assist by producing its accumulation—as in typhus—the diseased emanations constitute the poison; which, however, is all but harmless, unless accumulated.

The following graphic sketch of fever, thus originating, is given by Dr. J. Hunter.†† In the month of February, 1779, I met with two examples of fever in the lodgings of some poor people whom I visited, that resembled in their symptoms the distemper which is called the jail or hospital fever.

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* On Yellow Fever. † Lectures, by Rogers, page 296.
‡ Dublin Medical Transactions, Vol. 2, p. 595. § On Typhus.
‖ Dublin Medical Journal, Vol. 10. ¶ Prize Essay.
** Medical Histories, Vol. 1.
†† Remarks on the Jail or Hospital Fever, Medical Transactions, Vol. 3.

It appeared singular that this disease should shew itself after three months of cold weather. Being, therefore, desirous of learning the circumstances upon which this depended, I neglected no opportunity of attending to similar cases. I soon found a sufficient number of them for the purposes of farther information.

It appeared that the fever began in all in the same way, and originated from the same causes.

A poor family, consisting of the husband, wife, and one or more children, were lodged in a small apartment, not exceeding twelve or fourteen feet in length, and as much in breadth. The support of these depended on the daily labour of the husband, who with difficulty could earn enough to purchase food necessary for their subsistence, without being able to provide sufficient clothing or fuel against the inclemencies of the season.

In order, therefore, to defend themselves against the cold of the weather, their small apartment was closely shut up and the air excluded by every possible means. They did not remain long in this situation, before the air became so vitiated as to affect their health, and produce a fever in some one of the miserable family. The fever was not violent at first, but generally crept on gradually, and the sickness of one of the family became an additional reason for still more effectually excluding the fresh air, and was also a means of keeping a greater proportion of the family in the apartment during the day. Soon after the first, a second was seized with the fever, and in a few days the whole family perhaps were attacked, one after another, with the same distemper. The slow approach of the fever, the great loss of strength, the quickness of the pulse, with little hardness or fullness, the tremor of the hands, and the petechiæ or brown spots upon the skin, to which may be added the infectious nature of the distemper, left no doubt of its being the same with what is usually called the jail or hospital fever. It would appear there is no great power of infection in the body alone provided the air be not confined. Remarking on the exemption from this disease which warm countries enjoy, he says,—" On the cold is the cause of the air being confined which gives rise to the poison, and thus, directly opposite to the opinions usually received, there is more danger of producing this disease in a cold country, and in a cold season of the year, than in a warm one."

A person exposed to and living in the poisonous air becomes feeble and irritable, his sleep is disturbed, his tongue is white in the morning, his appetite is impaired, and the smallest bodily exertion quickens his pulse and fatigues him. He will remain in this state for weeks together, without any formed attack of fever; yet another receiving the infection from him, shall suddenly be seized with a violent disease. In this manner it is, I much suspect, that prisoners brought into a crowded court often produced the most dreadful consequences, by disseminating the infection lodged in their clothes. An instance of this kind is given by Dr. Fordyce,* which deserves mention. Arguing for a distinction between this poison and putrefactive poisons, he says—" This is undoubtedly not the case, since infection has arisen from a person brought out of rooms in which numbers had been confined for several months, but kept clean from all putrescent matter, so that there was no particular smell or other sensible quality. In one case that came under the observation of the author, a person under such circumstances, from whom no peculiar smell arose, or any other sensible effluvia, communicated the infection to four others with whom he was carried in a coach for about half a mile, so as to produce fevers in all of them, which fevers were violent and fatal."

Dr. Ferriar properly includes *moral* causes—" because it is not proved that the mere confinement of the effluvia of clean and healthy persons, free from

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* On Fevers, Dissertation, I. page 114.

mental uneasiness can become poisonous. This view derives considerable support from the following remarkable case by Dr. Harty, of the origin of fever from a single person under such circumstances.

A gentleman* was suspected of having confined and ill-treated his wife. At length two gentlemen, one of them a clergyman, having obtained the necessary authority, visited the house, and examined every apartment for the wretched object of their humane search—at first in vain; but at length a small closet door attracted their notice, and having insisted on its being opened, both gentlemen eagerly entered, and as precipitately retreated. One was immediately seized with vomiting; the other (the clergyman) felt sick and faint. After a little, they recruited and called the wretched woman from her prison hole, in which she had been for weeks immured. It was a small dark closet without *light* or *air*, and in it she had been immured without a change of clothes. At the end of a week both gentlemen had fever; both took to their beds almost on the same day. The clergyman died, and the other recovered with great difficulty after a severe struggle. Both cases were alike throughout, except in the termination. The woman had not then or afterwards any febrile disease, and had been free from any at any period of her confinement.

Bursts of fever from this cause occur, at times, in situations where no possibility of contagion from without exists—as in prisons, in surgical hospitals, and in situations in which typhus does not usually prevail and has not been introduced from without. Dr. Harty gives unequivocal testimony of this fact, derived from his experience in the Dublin prisons. For cases occurring in crowded wards of hospitals during cold weather, we may refer to Palloni,† Currie,‡ Tweedie.§

Dr. Ferriar‖ gives a decisive instance of fever arising in the habitations of the poor from this cause at Carlisle in 1778—9. We must be content to refer the reader who may be desirous of sifting the evidence on this much disputed question, to the above writers, as a recital of the cases would occupy too much space. It cannot be doubted that this depraved atmosphere has been sometimes considered as a source, when it really only favoured the diffusion of the fever poison, whether emanating from the bodies of typhous patients or from paludal sources. We shall have occasion to recur to this subject when examining the circumstances which favour the diffusion of fever as an epidemic disease. At present it may be remarked that, the writers on both sides of the question, have relied in some instances, upon exceptionable proofs. Thus Dr. Peebles, in his valuable paper, adduces several cases which occurred on board ships, which are seldom free from some of the paludal sources. It is also sagaciously remarked by Lind, that it is in ships going from home, and not in those returning from the longest voyages, that fever is found. The reason is obvious.

But if weak cases have been adduced in proof of the origin of fever from this source, they have equally been relied on by the great opponent of the doctrine and his followers. Dr. Bancroft has rested much of his argument upon the fact, that on board slave ships, where the crowding was unprecedentedly great, fever was unknown.¶

But, as has been well observed by Dr. Fergusson,** there are two good reasons for this.

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* On Fevers, page 163.

† Quoted by Dr. Peebles, Edin. Med. and Surg. Journ. No. 125.

‡ Medical Reports, page 6. § Clinical Illustrations.

‖ Medical Histories, Vol. I.

¶ On Yellow Fever, page 127, &c. It is worthy of notice, that in the passage quoted from Dr. Lind (page 128), the liability of felons in transports to fever is asserted. ** Edinb. Med. and Surg. Journal, No. 112.

1st. The absence of all fomites—the wretches being naked there was nothing to retain the effluvia.

2nd. The high temperature, which is always destructive of the poison of typhus.

The absence of fever from the huts of Fins and Russians, may be explained in a similar way, by the high artificial heat constantly kept up in them, and the total absence of moisture. None of the advocates of exclusive contagion, from Bancroft to Davidson, add any facts to the meagre evidence upon which the argument is founded. The enquiry is altogether one of the most important connected with the subject of fever, and bears strongly in its consequences upon science and humanity.

For if it appear that the poison of typhus can be generated de novo, under the conjoint action of the above-mentioned moral and physical causes, we should institute enquiries as to the part which each performs in the production of this result, and without wishing "*to get rid of a difficulty,*" we should, on other grounds than our inability to trace contagion to its primordial source, pursue the investigation of its laws, disregarding any such affectation of strict logic as is contained in the following passage.* It is not intended, however, to enter into any speculations respecting the primordial source of the contagion of typhus, for the sources from which it, as well as that of the other contagious fevers originated, are involved in absolute obscurity; and though we could trace them to the most remote æra in antiquity, the same difficulty would be encountered. Some authors, apparently to get rid of this difficulty, and to account for the occurrence of typhus, where no contagion could be traced, have adopted the opinion, that it may be generated by common causes, such as impure air, filth, &c., and be afterwards capable of propagation by contagion. *The argument of analogy is directly opposed to this belief, for if in nature there be no exception to the law, that two causes are never required to produce precisely the same effect, it will follow that, whatever cause can be best reconciled with the phenomena of typhus, must be considered the true source of the disease.* And accordingly this writer proceeds to return a hasty verdict of "not proven," upon the claims of every cause but this "one true source," *contagion.*

The following remark of the venerable Dr. Stokes upon this subject is too apposite to be passed over without notice. "This supposition of a *single cause* of the effects we witness, is quite unsupported by nature. Every animal, every plant, every rock, requires for its production the co-operation of many causes that we know, and most probably of many more that we have not yet discovered. All nature depends ultimately on a single cause, but it has pleased the Almighty to cause that the effects which concern us immediately should arise from the co-operation of several of his creatures."†

Again, if it appears that the febrile poison can be thus generated, we need not follow Dr. Barker‡ to the Continent of Europe to look for it. Nor need we to accompany§ Dr. Lombard upon his geographico-typhoid tour in proof that the frieze coat of the Irish labourer is its depository, in which it is exported like other "native manufactures."

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* Davidson, page 2. † Essay on Contagion, page 25.

‡ Dublin Medical Transactions, Vol. 2.

§ This notion of Dr. Lombard's, along with an opinion expressed by Mr. Farr, in the article Vital Statistics, in M'Cullagh's Statistics of the British Empire, "that the poor Irish are keeping up, if they are not introducing, the fevers of their wretched country in the heart of the British cities," has been met by Dr. Cowan, and by an acute reviewer in the Dublin Medical Journal, for January, 1838. But the latter, while he confers a merited castigation upon Dr. Lombard, bears too hard upon Mr. Farr, whom he classes with certain humane political economists who wrote, that it would be well that Ireland were sunk in the =och;

But the question has a great bearing upon humanity and political economy. Take the case of an epidemic such as has prevailed in Ireland during the past year. Suppose that in a town containing a great number of poor in which fever perhaps has not yet appeared, the inhabitants meet to confer upon the best preventive measures. These will differ as their views of the sources of the disease differ; one may suppose that the contagion is in all cases *imported*, and can see no protection except in a "cordon sanitaire."

Another believes that fever is exclusively of *endemial origin*, and he says—make sewers, sweep away the dung-hills—white-wash the houses.* While the man *alone* who conceives the generation of the poison under the foregoing circumstances, possible will recommend the true prophylactics, and, by providing clothing and fuel, cause the light and air to be admitted into their crowded dwellings, and by relieving mind and body from the pressure of impending starvation, will both render them less susceptible *of* disease *if* it approach them, and less capable of generating in themselves the poison which he believes may arise among them without exterior communication.

On this question it is impossible to speak of humanity and political economy apart. The following extract from Dr. Alison's essay on the management of the poor in Scotland, will prove how even motives of economy should lead to the application of the true preventive-relief of the wants of the poor.

"'A fever which consigns thousands to the grave,' says Dr. Harty, 'consigns tens of thousands to a worse fate—to hopeless poverty; for fever spares the children and cuts off the parents, leaving the wretched offspring to fill the future ranks of prostitution, mendicancy, and crime.' 'The mortality of fever,' says Dr. Barker, 'is most frequent where it is most injurious, viz. in men advanced in life, the heads and supports of families, the increase of poverty and mendicity, and the agonizing mental distress to which it must give rise, are consequences which must occur to every reflecting mind.' There is no exaggeration in the simple and impressive statement of Dr. Cowan—that 'the prevalence of fever presents obstacles to the promotion of social improvement among the lower

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sea. And says there is not the slightest evidence that the labouring classes *introduce* fever into the hearts of British cities. Probably not. In the case of Glasgow, Dr. Stuberoh's paper, Dublin Journal, No. 39, would seem to shew that they do not—at least by *importing it*. But in an able and temperate reply in the second edition of M'Cullagh's book, Mr. Farr has shewn that, in the three great avenues by which the Irish labourers enter the kingdom—Bristol, Liverpool, and Glasgow, their crowding to excess in lodging-houses, their loathsome diet and filth, are productive of epidemic fever, and he concludes with the following wise remarks.

"In directing attention to a weighty sanatory fact, it is far from our intention to convey any reflection upon the Irish people. We shall, in treating of epidemics, shew that the English were formerly in as bad a condition as the Irish, and we must say we had imagined that any attempt to prove that England is vitally interested in the prosperity and happiness of Ireland, would be rendering neither country disservice....... Reduce your neighbours to ruin and starvation, and you inevitably give rise to diseases which lower like avenging angels over your own heads....... So God avenges oppression; it reaps the fruits of its own handiwork.—(M'Cullagh's Statistics, Vol. 2nd. p. 529).

* See Sanatory Reports of Poor Law Commissioners, p. 14, and Report of the Select Committee on Health of Towns, p. 111.

Also the following passage from a Report of Dr. Addison's Essay on Malaria, Lond. Med. Gazette, vol. 3, N.S. p. 796.

"He thought that if any *palladium* could be discovered potent for the salvation of the city, it would be found in the shape of a *scavenger*!"

classes, and is productive of an amount of human misery credible only to those who have witnessed it.' In the last situation in which I have seen fever prevailing epidemically in Edinburgh, (new land at the foot of the old fish market close) I find, on enquiry, that five families out of the inhabitants of twelve rooms in the two upper flats of the house, have been rendered fatherless by it." p. 9.

We could parallel these cases in this town, but it is unnecessary. There is one more consideration arising from this subject—it is a selfish one, and therefore not the least powerful—it is contained in the following profound reflection of the excellent Ferriar. "The diseases arising from wretcheaness differ in this respect from those of luxury; the first are generally *infectious*, the latter solitary but *hereditary*. This observation would furnish an excellent moral, but as it is needless to suggest it, I pass on to my next point."

Sect. 4.—*The Mode of Action of the Poison, and the Circumstances which assist its Operation in the Human Body.*

The opinions of the majority of physicians of the present day are divided, as to the *theory of fever*, into two parties—the solidists and the humoralists.

That of the former party is thus announced in the article fever, Library of Medicine, by Dr. Christison. "The theory of fever, then, which seems most consonant with the whole facts, with the general sentiments of the profession, especially in Britain, and with a sound and prudent practice, is probably the following. Fever is an essential or primary disease. The first appreciable event in the chain of sequences constituting fever is a functional injury of the nervous system. The only essential or invariable consequence of this affection is functional derangement of most of the important organs of the body, but more especially of the brain, the circulating organs and fluid, the alimentary canal, and the skin......... The changes which have hitherto been observed to take place in the blood and other animal fluids, are, like the local disorders, secondary and not primary. They may be the source of the phenomena remarked in the advanced stage of the disease, but they are not the source of the disease itself in the first instance."

If we turn to another recent work of high authority, we find the very reverse order of sequence is maintained. "It appears probable, if not certain, from what has been advanced, that in a certain class of fevers (typhoid) the blood is *primarily* diseased, and that certain changes in one or more organs take place as a consequence or secondary effect."*

It will be seen that neither of these distinguished writers assigns the phenomena of fever exclusively to his system; and it has been well remarked, "that all febrile disturbances are disturbances of such vital actions as are the *joint* product of these two great factors of vital phenomena—for example, the primary phenomena of all fevers are—1, disturbance in the formation of animal heat; 2, disturbance in all the secerning functions; 3, disturbance in the process of nutrition. But the formation of animal heat, secretion, and the nutritive process, are all dependent on the conjoint action of the nerves and blood-vessels. Either of these two systems may receive the first morbific impression, but the one soon participates in the changes of the other."†

This last sentence involves the proper terms of the controverted question, for, while all must admit that the phenomena of fever *established* are due to the conjoint operation of the nervous system and the blood, the solidists maintain that it is upon the nervous system the morbific impression of contagion acts primarily; while the advocate of a modified humoral theory holds that the source and primary seat of typhous fevers, properly so called, is proved to be in

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* Dr. Tweedie. Art. Fever. Cyclopædia of Practical Medicine.

† Ferguson on Diseases of Women. Part 1. P. 97.

the blood; and that the order of sequence is, first, a vitiation of the blood by the commixture of deleterious substances; next, in consequence of such vitiation, an alteration of the functions of the nervous system; and, lastly, the blood that supports the organs, and the nervous system that animates them, having suffered a general injury, a constant though not always appreciable modification of these organs in their function or in their texture."

The advocates of each theory construe the phenomena of the latent period in accordance with their peculiar views: thus, while the humoralist regards it as the time intervening between the absorption of the poison and the manifestation of its effects on the great nervous centres—the advocates of the opposite theory consider that "the symptoms which characterise this period, whether they be slight, or whether they be severe, indicate a disturbance affecting primarily the nervous system.*

Again,—"We are not of opinion that the time between exposure to contagion and the formation of the disease thereby caused, is a period of health: the nervous system was affected previous to any disorder of the circulating system."†

From these extracts it will be seen that it is to the explication of the phenomena of the access and latent period of fever, and not to the *formed* disease, that each theory is to be applied, and its agreement with these phenomena tested.

This narrowing of the question deprives the humoralist of all support from the fact of changes detected in the blood subsequent to the latent period, since these may be owing to the changes in the nervous system; while, on the other hand, it reduces the available arguments for the nervous theory to two. That from the analogy of the morbific impression of contagion to the action of certain poisons—"such instantaneousness of action being supposed to be incompatible with the previous absorption of a poison into the circulation;" and, that deduced from the fact, that "a single mental shock often produces protracted disease, without the presence of any known source of the febrile poison."

By thus limiting the dispute, much is given up by the humoralist; since he holds, "that the fluidity or diffluence of the blood, and the violent colour observed in typhus, is not the result of the disease, but, on the contrary, that they are the immediate effects of the specific cause of the fever;"‡ while, on the contrary, it is on the phenomena of the access that the very strongest arguments for the nervous theory are founded.

Passing by the many writers who have rested satisfied with stating their opinions of the origin of fever, without giving the grounds upon which they are founded, we shall examine the arguments for the nervous theory contained in Sir H. Marsh's able paper on the Origin of Fever,‖ which are rested upon a number of histories of the access of the disease, which Dr. Tweedie has pronounced to "contain a body of evidence which should alone decide the question of the contagiousness of fever."

It will be our endeavour, as advocating a humoral theory, to shew that the evidence does not support the conclusions of its distinguished author. These conclusions are founded upon a supposed analogy of the morbific impression of contagion (or infection) to the action of certain powerful narcotic poisons which is *supposed* to be exerted upon the nervous system immediately, and not through the circulation. "Though there can be little doubt," says he, "that

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* Marsh. Dublin Hospital Reports, Vol. IV.

† Barker and Cheyne's Report.

‡ Vide Rostan's Clinical Lectures on Typhoid Fever, in Johnson's Review for January, 1841.

‖ Dublin Hospital Reports. Vol. IV.

prussic acid, when applied to the surface of the body, is ultimately absorbed, yet the rapidity of its action leads to the conclusion, that its first and instantaneous effect is on the nervous system." And Dr. Law, in arguing for a *mental* origin in one of his cases, in which the person was exposed to contagion before and at the time of seizure, says, "How are we to explain the mode of this individual's attack of fever? If we are to suppose it was contracted from exposure to contagion, we would avail ourselves of the argument of the toxicologist, who reasons that, from the very short period of time in which some poisons exhibit themselves in the system, these poisons affect the system through the medium of the nerves, rather than through the circuitous route of the circulation."

This theory of poisons being assumed, the analogy of the action of infection is thus stated by Sir H. Marsh. "From these facts it appears that the poison of contagion produces its effects *with the same rapidity as the narcotic poisons to which we have alluded.* Headache, debility, sickness of stomach or vomiting, are among the symptoms first perceived; these sensations, with the rapidity of an electric shock, are at the instant produced," &c.

This specious argument from analogy will be somewhat weakened by the following considerations :—

1. It is by no means proved, that any poison, however rapid, produces its effects upon the system, without being received into the general circulation, or before it can be carried to the brain through the medium of the circulation.

Müller's* conclusion upon this question is—"These experiments, as well as many others instituted by well-known physiologists, prove that, before narcotic poisons can exert their general effects on the nervous system, they must enter the circulation." And again,—"The rapid effects of prusssic acid can only be explained by its possessing great volatility and power of expansion by which it is enabled to diffuse itself through the blood more rapidly than that fluid circulates; to permeate the animal tissues very quickly, and in a manner independent of its distribution by means of the blood, and thus to produce the peculiar material changes in the central organ of the nervous system more rapidly in proportion as it is applied nearer to it." But even this explanation of Müller's—while it falls very far short of furnishing the desired analogy—would seem incorrect, since Mr. Blake has found that the poisonous effects *of* prussic acid in a large dose introduced into the stomach will not take place so long as the circulation through the vena porta is carefully interrupted. He even found that, on the effects of the poison being produced by removing for an instant the impediment to the circulation, the animal could be recovered upon the circulation being again stopped, though the three drachms of prussic acid was still in the stomach. Mr. Blake's conclusions from his interesting experiments are†—

1. That the time required by a substance to permeate the capillary vessels may be considered as *inappreciable.*

2. That the interval elapsing between the absorption of a substance by the capillaries and its general diffusion through the body may not exceed nine seconds.

3. That an interval always more than nine seconds elapses between the introduction of a poison into the capillaries or veins and the appearance of its first effects.

4. That if a poison be introduced into a part of the vascular system nearer the brain, its effects are produced more rapidly.

5. That the contact of a poison with a large surface of the body is not suffi-

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* Elements of Physiology, by Baly. Vol. I. p. 246.

† Edinb. Med. and Surg. Journal, Vol. 53.

cient to give rise to general symptoms, as long as its general diffusion through the body is prevented.*

But secondly—*the suddenness of action of the febrile poison is generally speaking only apparent and not real.*

The infection of continued fever (says Christison) is, for the most part, by no means virulent. And again—fever is usually communicated by long exposure to the emanations from the sick, and seldom by any single short exposure, however decided. It is a common notion that single, brief, decided exposures often occasion an attack; and, in support of this notion, reference is made to cases where individuals can trace the infection, as they imagine, to a particular fever patient, by having experienced some very peculiar morbid sensation at the time of exposure. There is much room for fallacy, however, in observations of this kind, and besides their proportion is small compared with the far more numerous instances where no such sensations can be recalled as having ever been experienced.

It is unnecessary, though it would be most easy, to multiply testimony to the same effect. Even Dr. Marsh says very truly, that " by far the greater number of patients labouring under contagious fever, are not at all aware of the circumstances connected with the origin of their complaint; the impression made at the time of their exposure being in general unheeded or forgotten. Indeed the impression is often times so slight, as to lead one to think that contagion does no more than predispose to fever, and determine the nature of the disease, of which, exposure to cold, fatigue, or some such accidental circumstance, is the immediately exciting cause; so that there appears much reason to believe that, many are so mildly affected, that, were it not for the superaddition of an exciting cause, they would altogether escape fever; hence it happens that numbers affected with contagious fever, trace the origin of their complaint exclusively to

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* Sir H. Marsh states, that in some experiments performed by himself and Dr. Jacob, the poisonous effects of prussic acid were observed to commence in FIVE seconds; there is therefore a discrepancy between his results and those of Mr. Blake, but the following experiment of the last gentleman would seem to shew that even this short time would allow of the entrance of the poison into the circulation.

" A drachm of the strongest liquor ammoniæ, mixed with five drachms of water, was injected into the jugular vein of a dog. A glass rod which had been dipped in hydrochloric acid, was held immediately under the nostrils; four seconds after the introduction of the first drop of the solution of ammonia into the vein, it was plainly detected in the air expired from the lungs, by the white vapours that were formed upon its coming in contact with the vapour of the hydrochloric acid."

Dr. Christison's experiments on prussic acid (at page 657 of his work on Poisons), do not support Sir H. Marsh's views of the extreme rapidity of action of this poison. While at page 660 he admits that every argument but this is in favour of the theory of its action through the blood, *in which* it was detected by analysis in the case of a cat killed in a few seconds by the acid applied to the tongue.

But in the text I have neglected to notice the fact, so prejudicial to Dr. Marsh's analogy, that the blood in these cases of sudden poisoning is *fluid.* We are also told by Dr. Christison, that in cases of sudden death from the emanations from Parisian privies, the blood is found *black* and *fluid.*

A similar effect is observed in cases of sudden death from other kinds of miasm—for an instance from animal putrefaction, see the Medico-Chirurgical Review, for January, 1825; and for an instance from marsh miasm, see Evans on the Endemic Fevers of the West Indies, p. 22.

cold, wet and other exciting causes of the disease, the time and circumstances of exposure to contagion having been entirely forgotten. Cases of this kind, *which are by far the most numerous*, throw but little light on the origin of fever. It is only by a careful observation of facts of occasional and rare occurrence, such as those recorded in this paper, in which the effects of contagion are well marked and striking, that we can hope to obtain certain and satisfactory results."

There is much truth in the foregoing passage, especially in that part of it which assigns to contagion the action of a *predisposing cause;* but how can this view be reconciled with Dr. Marsh's own theory, that the action of contagion is an "injurious impression upon the sentient extremities of the nerves?" and how far is he justified in assigning the *cause* and *commencement* of fever to sudden and brief exposure, even by cases of rare occurrence, (exceptions he admits to the general rule), such as he has collected? These are questions deserving consideration. We shall return to the first when examining the argument for the humoral theory derived from the latency and cumulative property of the poison; but how do Dr. Marsh's cases support his opinions as to sudden exposure being the *cause* of fever? It is obvious that when it is committed, that the general rule is, "that no perceptible impression is made by contagion," we cannot admit the conclusion that the impression was the cause of the disease, except it appears that no other exposure took place; the more so since the medical and other attendants of fever patients in private houses, and where cleanliness and ventilation are properly observed, frequently perceive these impressions—arising from the odour of the patient or his excretions;—such impressions, however *sickening* at the time, seldom leading to any further ill-consequences; but of twenty-two cases adduced by Dr. Marsh, ten were nurses or porters of fever hospitals, seven were physicians, one a clergyman, and one appears merely to have suffered the nervous shock, as fever did not follow.

The remaining three appear to be unexceptionable instances of fever, arising from a single and concentrated dose of the poison, two, if not all of them being cases of communication by fomites, (usually containing a concentrated poison.)

But again, we have to enquire whether the moment of exposure was that of the *commencement* of the fever? since the argument rests mainly on "such instantaneousness of action of the poison as is incompatible with the idea of absorption into the blood." Here we might remark on the rapid diffusion of gaseous poisons through the blood, and appeal to Mr. Blake's experiments in proof that the poison may enter the circulation even before the impression is felt; but admitting that this impression is a purely nervous one—a shock, or "reaction," as it has been termed—"a resistance offered by the vital powers to chemical action"—it is *not* the commencement of fever. For it may end where it began; the impression may not, and very often is not followed by fever; and in many more cases goes off altogether for a longer or shorter period before fever commences. True, it may continue, especially in persons whose imagination has become alarmed—in which case some writers have attributed the imagination to the influence of the poison upon the nervous system—and in a manner hereafter to be explained, it may shorten considerably the latent period; but we repeat, this latent period will be found to exist in any case in which a *previous* imbibition of infection is not to be admitted. "The symptom," says Sir H. Marsh, "which is generally considered to mark the commencement of a febrile movement in the system, is that commotion of the nervous functions which has been technically termed a *rigor*." This commencement of the *febrile movement* is only mentioned in twelve of his cases, and in these it occurred in four at an interval of from one to two days, in six after several hours, and in two only it is said to have come on "a short time after" exposure to the poison.

The third consideration which may be urged against this analogy is, "that the poison with which contagion is compared is not *reproduced*." As this reproduction of contagion is one of the strongest arguments for the humoral theory, we shall not dwell upon it here, but merely observe that the toxicological argument, while it sets up a forced and false analogy with poisons which are not reproduced, strives to weaken and destroy that which naturally exists between the infection of typhus and that class of morbid poisons to which it may be said to belong—the exanthemata. This has not escaped Dr. Marsh's observation, who admits that "the opinion that to maintain a protracted fever, an internal cause of disease (such as absorbed or generated morbid matter) is necessary, would arise from the phenomena which manifest themselves in the course of an exanthematous fever." But he meets this by the second of the objections we have enumerated to the humoral theory.

"Yet that to excite and maintain continued fever, an *abiding* cause is *not* necessary, might be proved in various ways, but the fact that a *single* mental shock often produces protracted disease, is decisive upon this point."

As Sir H. Marsh adduces no fact in support of the above assertion, turn we to another able physician who, in a recent paper adduces seven cases from his own experience, in proof of the opinions expressed in the following passages: *

"We quite agree in the wisdom of the precaution of satisfying the absorbents, but deny that they are more the channels through which the morbific matter enters the system, in this instance, (fever from contagion), than they are in other cases where there is no reason to suppose either that they are in an unusual state of activity, nor if they were, can we discover any contagion to serve as a *materies morbi* for them to exercise themselves upon. These are cases in which a strong moral impression acts as a direct and immediate cause in the production of a fever, similar in all respects to one from contagion," &c.

And again:—

"We shall proceed to detail some cases of fever which seem to us calculated to throw some light upon the mode in which the first morbid impression is made upon the system in the production of the disease; and see how far these cases tend to confirm the opinion that fever is the result of a miasma conveyed to the system by the absorbents: *or* if it be not, in some cases at least, the effect of a *moral impression* acting upon the nervous system, and exhibiting itself in symptoms indicating a derangement of the functions of this system."

The advocate for the theory of absorption may reasonably require that in such cases the *materies morbi* shall not appear to have been within reach. But of five cases the subjects were exposed to infection, at or before the seizure. The sixth was not (as Dr. Law admits) a case of fever; and we have only one in which fever followed a mental shock, without evidence of infection at the same time existing. To explain away this case, a determined opponent of the nervous theory might adduce evidence of the general diffusion of the fever-poison through the atmosphere of a city, when fever is prevalent in it; he might maintain that at such times †"certain changes take place in the constitution of the atmosphere imperceptible to our senses, and eluding chemical tests, which predispose human bodies to febrile diseases in such a way, that circumstances which in ordinary times would only give rise to a catarrh, an attack of rheumatism, or even occasion no indisposition at all, will now in many individuals become the exciting causes of continued fever."

If it be said that this is begging the question, the humeralist takes higher ground, and asserts that such cases, instead of disproving, strengthen his own theory; inasmuch as he can shew that fever follows strong nervous impressions, in consequence of their lowering the vitality of the blood, and so favouring the transformations in that fluid upon which fever depends. He believes that ‡ "no other component part of the organism can be compared to the blood in respect of

* Observations on Fever, by Dr. Law. Dublin Med. Jour. Vol. XIV.
† Prichard, on the Epidemic Fever of Bristol. ‡ Liebig, p. 360.

the feeble resistance which it offers to exterior influences. The blood is not an organ which is formed, but an organ in the act of formation; indeed, it is the sum of all the organs which are being formed. The chemical force and the vital principle hold each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood. Every chemical action propagates itself through the mass of the blood; for example, the active chemical condition of the constituents of a body undergoing decomposition, fermentation, putrefaction or decay, disturbs the equilibrium between the chemical force and the vital principle in the circulating fluid: the former obtains the preponderance. Numerous modifications in the composition and condition of the compounds produced from the elements of the blood, result from the conflict of the vital force with the chemical affinity in their incessant endeavour to overcome one another."

He admits that * "perhaps there are cases in which the modification of the blood is only secondary to a modification of the nervous system. If, for instance, under the influence of a strong mental emotion, this system being suddenly perverted in its action, ceases to exert its proper influence over the different organs in which the blood is elaborated, deposited, and receives new materials, must not that fluid itself become altered in its turn? If so, thence must arise a number of organic and functional derangements varying greatly, according to the mode and intensity of the primitive alteration of the innervation. In such cases we may observe to occur sporadically those same diseases, typhoid or other, that we have just now seen prevailing epidemically under the influence of manifest causes of infection of the blood."

To prove that Dr. Law's case belongs to this formula, let us place it by the side of another in which *precisely* the same mental impression, acting more intensely, produced death. Eliza J——, æt. twenty-six, was admitted under Dr. Law's care, March 28, 1836. She had been in perfect health a week since, when, on missing a piece of linen which had been committed to her care to make shirts, from the apprehension that her honesty would be called in question, she was seized with a violent rigor and sickness, which confined her to bed ever since. Petechial fever, with prominent hysterical symptoms, followed. She recovered with difficulty and slowly.

Sometime ago, I was present at the examination (*post mortem*) of a man who died suddenly under the following circumstances.

He had committed a very trifling theft, for which he was apprehended and carried before a magistrate. He was a person rather above the lower order, and manifested great shame and grief at this exposure. While sitting before a table waiting for his case to be called on, he leaned his head forward on the table and was observed to snore; in a few minutes, the sound of his breathing ceased, and on raising his head, those near him found that he was dead. It was supposed, that apoplexy was the cause of death, and the brain was first examined. It was, however, perfectly healthy. The other viscera were then carefully examined. The only one which discovered anything which could account for his sudden death, was the heart, which was distended with *dark fluid blood.*

Let us suppose that the mental impression had not been so intense in this case, and the life of the blood not so completely and suddenly destroyed,—what would have been the probable consequence? This question is answered by a comparison of the two histories. In the last, the vitality of the whole circulating mass was destroyed, and the symptoms were those of a brain suffering the influence of *a strong narcotic poison.* In the other, the livid, petechiæ, spongy, and bleeding gums, &c. showed to what an extent the vitality of the blood had been destroyed, The immediate occurrence of a rigor shewed that the self-generated poison had

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* Andral, Pathological Anatomy, Vol. I. p. 671.

reached the nervous centres, and that the struggle had commenced which was to end with either the death of the whole mass of blood or the elimination from it of the portion so affected. It is worthy of remark, (and is noticed by Dr. Law) that the rigor was immediate,—not after an interval of hours or days, as in cases of exposure to infection, in which the operation of the poison is gradual and often (generally, indeed,) accumulative.

In fine, typhus, or a disease resembling it, but differing, according to Dr. Cheyne, in the very important particular that it is not communicated by contagion—in other words, that the poison is not *reproduced*—is but one of three modes, or degrees, in which the blood suffers from a strong mental impression. It may be killed at once, or it may suffer in a degree insufficient to produce *formed* disease—loss of appetite and depraved secretions, with slight derangements of animal heat, being perhaps the only indications of the injury it has received,—or it may act upon the system in a manner similar to the fever poison. But this cannot be said to prove that the fever poison acts by producing a moral impression; and, therefore, instead of agreeing with Dr. Law, that "even in cases where there was most reason to suspect absorption, where a person having exposed himself to contagion, fasting,—and then contracted the disease,—even here the symptoms exhibited by the disease so resemble those where there is no possibility of suspecting infection, that we cannot but believe that the mode of absorption is the same in both cases, and that as it is not absorption in the one case, neither is it in the other,"—instead of going to this length of denying the existence of a *materies morbi* altogether, we would reduce the two cases to the same formula by an opposite method. As thus: violent nervous shocks kill the blood or modify it, and occasionally produce fever. Contagious and other miasms also, in some rare instances, kill the blood, and, in general, modify it, so as to produce fever. But they may do so without causing a nervous shock. Therefore, they act *directly* on the blood, by being absorbed into that fluid and not through the intervention of any derangement, functional or otherwise, of the nervous system.

The principal arguments for the nervous theory derived from the mode of access of fever, having been examined, we shall submit some of those which tend to support a modified humoral theory, and then offer a rationale of the action of the *causes* of fever in accordance with this theory.

The explication of the accession of the disease having been taken as a text of the opposite thories, we are deprived of any support from two arguments which have been much used by humoralists: viz. the changes which the blood undergoes in the course of fever, and the production of fever or a disease perfectly analogous, by the introduction of substances into the circulation.

Another argument, of a similar kind, is derived from the known power› sources which ordinarily produce fever, to kill the blood at once when their poison is introduced into it in sufficient quantity. We give the fact on the highest authority.* The inference has been met by the toxicological argument already considered, and by a distinction asserted between mephitic poison and the fever poison. This distinction we shall examine along with the source itself, hereafter.

But there are certain peculiarities in the action of the febrile poison which in their general character resemble other morbid poisons, and favour the idea of its absorption into the blood.

The first of these is its occasional latency in the system, in which it will lurk for a longer or shorter period, until called into action by some accidental cause.

"In several instances," says Dr. Graves,† "I have observed that certain diseases, which seemed to have been lurking in the constitution, may suddenly

* Christison on Poisons, p. 700, 2nd edition.

† Lectures, London Medical Gazette, vol. III. N.S. p. 186.

make their appearance in consequence of the operation of causes apparently unconnected with the disease in question ...... I have witnessed several bad cases of bad secondary venereal, in which the attack was traced to excessive fatigue, or a common cold. You will also meet numerous examples of an analogous fact among fever patients: examine them, and you will learn that in a majority of cases their disease arose from exposure to cold. One person fatigues himself by too much exertion in business, and gets an attack of spotted fever; another attributes his disease to over-anxiety; some to intemperance, and some to fright. In all these cases, it is very probable that the poison of fever has been lurking for some time in the system, and has been called into active existence by the operation of some sudden accidental cause, as fright, fatigue, intemperance, or cold."

Something similar, Dr. Graves justly observes, is remarked in the case of the Irish labourers employed during summer and autumn among the fens of Lincolnshire (and we may add Cambridgeshire). During their stay in England, they appear free from disease; but on their return home, if they happen to be exposed to wet, fatigue, or the derangements of health consequent on intemperance, they are very often seized with intermittent fever.

He continues, " Does it not often happen, that many of us escape fever although exposed to its contagion month after month? Do we not go on for years untouched, although subject every-day to the imbibition of the poison? and do we not, rendered bold by our impunity, consider ourselves, as it were, fever proof, until some accidental cause convinces us of the contrary, by giving rise to a sudden and violent attack? Who is there that has not observed this repeatedly among the students attending a fever hospital?"

Similar proof of the latency of the fever poison is afforded by the cases recorded by Lind, of sailors, who apparently escaping from the fever which was raging on board, went ashore, and in some time afterwards, in consequence, apparently, of exposure to cold or debauchery, were attacked, *not* with the fever prevailing there, but with that of the ship they had left. In this respect, then, the febrile poison resembles other morbid poisons.

Again: *it is a cumulative poison.* The exposure of a single moment is probably insufficient, in any case, to cause fever. A few inspirations may accumulate sufficient in cases of great concentration of poison; but there is abundant proof that daily and continued imbibition of the poison is, in general, requisite. Thus, we find the attendants on the sick attacked in proportion to the frequency of their approaches to the infection, the very reverse of what would be the fact if the poison were *not* cumulative, since it is a law constantly observed, that agents which act by single impressions lose their power of producing those impressions in proportion as they are frequently repeated. It is true that some eminent writers aver this of infection, as Dr. Copland, who says, " when a person has escaped infection upon the first or the earlier exposures to several infectious maladies, he will generally continue to possess an immunity, unless circumstances should occur to increase his predisposition." Observations made on a large scale, however, tend to disprove this, as regards typhus.

Thus, when fever prevailed during the retreat of the British army through Holland, we are informed by Dr. Fergusson,* that few, indeed, of the medical staff escaped the typhoid contagion; and, again, in the retreat from Talavera to the confines of Portugal, it was seen that the *best seasoned* of the medical staff were the principal sufferers. Dr. Christison, too,† (a solidist) maintains that it is not improbable that the severity of the disease bears some proportion to the *amount* of exposure."

And ...... " In many instances, fever breaks forth apparently from gradual

* Edinb. Med. and Surg. Journal, No. 112.
† Library of Medicine, Art. Fever.

charging of the constitution under *constant exposure* to the morbid emanations and without any other co-operating cause."

This is very like humoralism, as is the illustration given by Dr. Haygarth. "A pint of yeast will excite fermentation in a barrel of ale, but a hundredth or a thousandth part would not have the same effect."

Again. The *reproduction of the poison of contagion*, is a fact "not dreamt of" in the philosophy of the solidists. Here their analogy is at fault, for the poisons from whose action it is derived are not reproduced. Neither will any supposable impression upon the nervous system explain the continued reproduction of the same febrile phenomena, and the same miasm through an indefinite series of individuals. We have admitted the production of fever by a strong mental impression. We have endeavoured to reconcile this occurrence with the theory which refers the source of fever in all cases to the blood. We have, however, noticed the fact, that such fever does not reproduce itself, and referred to the testimony of one, whose accuracy of observation has seldom been surpassed, who says, "The most remarkable part of the disease is that it does not spread. I have no recollection of a second case of this kind of fever occurring in a family."*

But the humoral theory has its analogy for the reproduction of the poison.† "The mode of action of a morbid virus, exhibits such a strong similarity to the action of yeast upon liquids containing sugar and gluten, that the two processes have been long since compared to one another, although merely for the purpose of illustration. But when the phenomena attending the action of each respectively, are considered more closely, it will in reality be seen that their influence depends on the same cause."

Now, when yeast is introduced into a mixed liquid, containing both sugar and gluten, such as wort, the act of decomposition of the sugar effects a change in the form and nature of the gluten, which is in consequence also subjected to transformation. As long as some of the fermenting sugar remains, gluten continues to be separated as yeast, and this new matter, in its turn, excites fermentation in a fresh solution of sugar or wort. If the sugar, however, should be first decomposed, the gluten which remains in solution, is not converted into yeast. We see, therefore, that the *reproduction of the exciting body* here depends :—

1. Upon the presence of that substance from which it was originally formed.
2. Upon the presence of a compound, which is capable of being decomposed by contact with the exciting body.

If we express, in the same terms, the reproduction of contagious matter in contagious diseases, since it is quite certain that they must have their origin in the blood, we must admit that the blood of a healthy individual, contains substances, by the decomposition of which, the exciting body or contagion can be reproduced. It must further be admitted, when contagion results, that the blood contains a second constituent, capable of being decomposed by the exciting body. It is only in consequence of the conversion of the second constituent, that the original exciting body can be reproduced.

When a quantity, however small, of contagious matter, that is of the exciting body, is introduced into the blood of a healthy individual, it will be again generated in the blood just as yeast is reproduced from wort. Its condition of transformation will be communicated to a constituent of the blood ; and in consequence of the transformation suffered by this substance, a body identical with, or similar to the exciting or contagious matter, will be produced from another constituent substance of the blood. The quantity of the exciting body newly produced, must constantly augment, if its further transformation or decomposition proceeds more

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* Dr. Cheyne's Account of Fever from Mental Causes, in Sir H. Marsh's Paper on the Origin of Fever.

† Liebig.

slowly than that of the compound in the blood, the decomposition of which it effects."

These substances are the organic matters existing in the blood, either in the state of transition from blood into the constituents of the tissues, or from food into blood. Which changes, it is argued, cannot take place without the formation in the blood of new compounds, which require to be removed by the organs of excretion.

"When the organs of secretion are in proper action, these substances will be removed from the system; but when the functions of these organs are impeded, they will remain in the blood, or become accumulated in different parts of the body. The skin, lungs, and other organs, assume the functions of the diseased secreting organs, and the accumulated substances are eliminated by them. *If when thus exhaled, they happen to be in the state of progressive transformation, these substances are contagious, that is, they are able to produce the same state of disease in another healthy organism, provided the latter organism is susceptible of their action; or in other words, contains a matter capable of suffering the same process of decomposition.*

"In the abstract chemical sense, reproduction of a contagion depends upon the presence of two substances, one of which becomes completely decomposed, but communicates its own state of transformation to the second. The second substance thus thrown into a state of transformation, is the newly-formed contagion.

"The second substance must have been originally a constituent of the blood; the first may be a body accidentally present.

"If *both* be constituents indispensable for the support of the vital functions of certain principal organs, death is the consequence of their transformation. But if the absence of the *one* substance, which was a constituent of the blood, do not cause an immediate cessation of the functions of the most important organs, if they continue in their action, although in an abnormal condition, convalescence ensues. In this case, the products of the transformations still existing in the blood, are used for assimilation, and at this period, secretions of a peculiar nature are produced."

Having submitted this chemical analogy of the reproduction of contagion in the words of the highest living authority on animal chemistry, it only remains to attempt a rationale of the action of the causes of fever, in accordance with its principles, which may be thus stated:—1st, That the principal character of the blood consists in its component parts being subject to every attraction; the chemical forces of this fluid, and the vital principle holding each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood.

2nd. That bodies, the elements of which are in a state of decomposition or transposition, when produced from the blood, as contagions are, will communicate *their state* to the sound blood, exactly as gluten in a state of decay or putrefaction, (yeast) causes a similar transformation in a solution of sugar and gluten (wort.)

Assuming then, that the primary action of the febrile poison is upon the blood, there can be but one *essential cause* of fever, viz., *The introduction of the poison into that fluid.* Its activity, or the occurrence of the peculiar transformations which it has a tendency to excite in the blood, will be determined by the existence of certain accessory or accidental causes, which disturb the equilibrium between the chemical forces in the blood and the vital influence; either by their action on the blood, causing the increase of compounds subject to those transformations which the poison produces—as depraved diet, bad air, &c.; or by their action on the nervous system, withdrawing permanently or temporarily more or less of its influence, and so favouring the chemical action of the poison. Such are the depressing effects of cold, fatigue, anxiety, debauchery, disgust, fear, &c. These are usually termed *exciting causes*, the former *predisposing causes.*

The occurrence of fever—the length of the interval which may elapse between the imbibition of the poison, and the first febrile movement; in other words, the length of the latent period—the severity of the disease, and the facility with which infection is received and communicated, will depend upon the relative power of the poison, and its combination with one or more of the foregoing predisposing and exciting causes.

Thus, the continued imbibition of the poison will sometimes, apparently without the co-operation of any accessory cause, result in an attack of fever. This, however, is a very rare case, as though deranged health, and particularly disorder of the receiving functions, may exist, the poison is in this case usually eliminated from the blood, unless the balance of forces in that fluid be disturbed by some one or other of the exciting causes.

The occurrence of the exciting cause may be, or may not be, accompanied by exposure to contagion. In the case of nurses, and the other attendants of the sick, some single exposure being marked by the presence of an exciting cause, it has been supposed that the infection was then and there received into the system, when, in reality, it was before latent, and only rendered active by the *circumstances accompanying* this particular exposure. Again, when the occurrence of the exciting cause is not attended with exposure to infection, the fever is often wrongly attributed to cold, excess at table, mental emotion, &c., the latent presence of the predisposing contagion not being recognised by the patient, and sometimes, as we have seen, being denied by the physician.

The exciting cause may act, not only by determining the occurrence of fever, but also by shortening its latent period.

This is a frequent effect of exposure to infection. " In these cases, the ascertained laws of incubation," says Fergusson, " will so far be set at nought, that a terrified patient will not only fix the precise moment of infection, but will actually sicken prematurely with small-pox, (a latent infection must of course have been previously received), through the spectacle of the disease in the person of another, or through the disgust (and nothing worse), of an excremental smell, strongly affecting his alarmed imagination, or through the same impression, he may fall down the victim of an impossible contagion, like that of yellow fever."

The apparent shortening of the latent period of morbid poisons, seems to occur under these circumstances:—

1. A strong impression made on the nervous system at the time of exposure. If this be so powerful as to affect seriously the vital principle, the effects of the poison will follow with proportionate rapidity. The poison of ague, usually so long latent, affords a good illustration:—Dr. G. Bird relates, "that being employed in some experiments upon the gas in marshes (near Woolwich), having suddenly disengaged a quantity of most offensive gas, he was seized with nausea; and on the day following, with intermittent fever."

A similar instance, in his own person, is related by Mr. Evans; and another, in which death followed in forty-eight hours. *In this last case, the blood was found fluid.**

2. A less powerful impression upon the nervous system may accompany exposure, and be followed by a latent period, apparently shortened, but admitting of the supposition of infection previously latent. Several of Sir H. Marsh's cases afford illustrations of this fact. And it is very probable that exposure to contagion in this way, often produces merely the same effect as an exciting cause, that cold, or any depressing agent would exert.

3. The circumstances accompanying exposure to one kind of poison, instead of acting as accessories to the action of that poison, may cause the immediate action of another, previously latent.

This is the only reasonable mode of explaining the cases of irregular contagion,

* On the Endemical Fevers of the West Indies.

related by Marsh and others, of typhus, received from small-pox patients, scarlatina from typhus, *ague* from typhus, and typhus from puerperal fever.

Some of these cases we might truly term *impossible* contagion, unless explained by the supposition of a previously latent poison. The facility of reception of the disease depends upon two conditions; 1st, the presence in the blood of compounds capable of undergoing the transformation of the poison. This constitutes susceptibility; and when it exists in a great degree, and conjoined with, 2nd. diminution of the vital influence, it constitutes the highest degree of predisposition to disease. The proneness which the living body may thus acquire to infection, may be so great (as seen in crowded collections of wretched beings in large cities, deprived of air, light, fuel, clothing, and sustenance), as to resemble that incapacity of resisting the progress of decay, (a true contagion) which is exhibited by dead animal matter, placed in a putrefying atmosphere.*

The severity of the disease depends partly on the above circumstances, but principally on the *dose* of the poison. This may be illustrated by comparing small-pox and measles, received in the natural mode, with the same diseases communicated by inoculation. Individuals may suffer as severely from the latter as the former, but the generality of persons do not. The following passage in a recent work of great ability offers inducements to consider this subject somewhat in detail:—"The modifications in disease dependent *on the mode of introduction* of the morbific cause, is, however, a subject too difficult for me to grapple with, and the observations are too few too offer any precise result. Cruveilhier, in the article 'Phelibitis Dict. de Med. et Chir. Prac.' points out the increased intensity of effect when pus is introduced into the circulation at once, and as compared with that caused by gradual absorption from an abscess. The modification which small-pox undergoes by inoculation, as compared with that malady acquired by inhalation, is very remarkable."† From this last observation it would appear that the author considers the modification of small-pox as not *consistent* with Cruveilhier's observation. Such an idea must have arisen from confounding the *matter* of the small-pox pustule with the *poison* of small-pox,‡ when in reality it only contains the poison in common with the blood and all its excretions.

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* A fact noticed by Parent Duchatelet, in some infectious places in Paris; and by Senac, see Wilson Philip on Fevers, Vol. I, page 210.

† Ferguson on Diseases of Women, page 104.

‡ The distinction between them is well stated by a writer in the 'Edinburgh Medical and Surgical Journal,' Vol. LIII, p. 206.

"Rayer mentions 'pus and miasm' as two distinct agents which should never be confounded. If the contagious effluvium and the matter of the pustule were one and the same thing, how could we account for the circumstance of the *fœtus in utero* becoming affected with the small-pox? Besides, Dr. Waterhouse and others have recorded cases in which persons exposed only to the exhalations from the blood of small-pox patients have been afterwards attacked by the disease."

The fact marked in italics would also serve to prove the distinctness of the poison from the ponderable matter of lues. Other considerations would lead us to extend it to all morbid poisons. For

1. The peculiar action of a morbid poison on the blood presumes its possessing great diffusibility in that fluid; and this quality is known to exist in all substances universally, as the cohesion of their atoms, or in other words, their ponderability.

2. The power of permeating tissue depends upon the same condition; and while we find that all the morbid poisons *may* act without abrasion of surface, we find that those which do appear to permeate the skin, act with more certainty if presented at a temperature which admits of their volatilization. This is noto-

The poison of small-pox is equally subtle and imponderable with the other morbid poisons, *an aura*, present it is true in the matter of the pustule, but equally present and equally capable of communicating the disease in the gaseous exhalation which arises from the blood drawn from a variolous patient. The same is

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riously true of small-pox, as the dissection of subjects who have died of this disease, though not harmless, is much less infectious than the handling of the living body. It is also well known that the examination of any dead body is most likely to be followed by the bad consequences of a dissecting wound, when the body is warm and contains the halitus of its cavities uncondensed........ the next in point of danger being that which is next in diffusibility; the exposure of the surface of the hands to the liquid contents of the serous cavities in particular cases, especially in puerperal peritonitis.

I need only refer to Mr. Stafford's paper on this subject in the 20th Vol. of the "Medico-Chirurgical Transactions" for instances of the imbibition of this poison in puerperal and other cases, without any abrasion of surface. The following circumstance bearing on this subject occurred to myself a short time since.

I was sent for to see a lady in the latter end of her first pregnancy, who I was informed had been for some time suffering much painful anxiety of mind and fatigue of body, and had been laboriously occupied with the arrangements for entering on a new residence, which had kept her constantly upon her feet. For some weeks the legs had swelled considerably, and pitted under pressure. This swelling had rather suddenly increased, and extended to the thighs and pelvic region, with a feeling of stiffness and inability to walk up stairs. Her pulse was quiet, tongue clean, and general health apparently perfect. This was on the morning of the 21st of December. All appeared to go on well, and the swelling seemed to diminish a little till the night of the 23rd, when she slept none, and was attacked with vomiting. On the morning of the 24th I found her remarkably changed; the countenance haggard and anxious, with a quick irritable pulse, thickly furred tongue, restlessness, and vomiting of a dark green fluid. Labour pains came on at 10 a.m. and continued regularly during the day. About 10 p. m. she had an attack of convulsions, and in a few minutes another. Delivery was immediately effected by the assistance of the forceps. It was observed that the labia had since morning become very dark coloured, and the perineum tore upon the slightest stretching like wet brown paper, but *without bleeding*. The delivery of the child was followed by that of a second, unassisted; both being quite dead and flaccid. The uterus contracted firmly, and there was no hæmorrhage; but the patient became less and less capable of being roused, the abdomen enormously distended, respiration laborious, and she sunk at 2 a.m. three hours after delivery.

About four in the evening of that day, I felt a hot painful itching upon the back of my right hand, where I perceived a small transparent vesicle. In a couple of hours I had pain in the axilla, and an uncomfortable, chilly feel. I applied a number of leeches to the hand and took an emetic, followed by calomel and James's powder. These means removed all unpleasant general symptoms, but the part itself did not recover so speedily, as an ill-conditioned obstinate sore formed on the hand which was long in healing. Not the slightest scratch or puncture existed before the application of the poison.

But whence this poison? It was ingeniously suggested to me by my friend, Dr. Clifford, who assisted me through this most distressing case, that the vital powers being over-taxed for the nutriment of two children, had given way, and this decomposition before death was the consequence. Perhaps this is the only explanation which can be admitted.

But I am inclined to believe that the whole was the effect of phlebitis, by which a morbid poison was generated, which produced *death in the fœtuses*, distrue of measles, which has been propagated over and over again by Home, and others, by inoculation with the *blood* of the patient, and with the same result,—a milder form of disease. By thus separating the poison from its vehicle, the difficulty of explaining the modification of these diseases by inoculation is got rid of, since, to recur to the simile of Dr. Haygarth, a hundredth part of a pint of yeast will not excite fermentation in a barrel of ale, though a pint will do it; and it must be obvious that a single inspiration in the immediate neighbourhood of a small-pox patient may introduce more of the *aura* into the blood than the direct introduction by inoculation of an atom of matter, in which only a small proportion of the poison can be present. The correctness of this view could be readily tested; and if it were found that, as in typhus, the amount of exposure had an influence in determining the severity of the attack of small-pox, the explanation must be admitted. One fact is strongly presumptive in its favour; it is the less complete removal of the susceptibility to the disease after inoculation than after natural small-pox. The analogy to the fermenting process is too obvious to need suggestion, and the same remark holds good of fever, short and mild fevers being notoriously more prone to recurrence than a long and severe form of the disease.

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organization in the mother, and being presented under circumstances favourable to absorption, rapidly permeated the skin to which it was (only for a few moments) applied. Every thing was favourable to the occurrence of crural phlebitis and to the absorption of the poison into the patient's system, as will appear from the history of the case, without again enumerating particulars. Dr. Wilson's paper, in the "London Medical Gazette" for April 1838, proves that crural phlebitis in women is not confined to the puerperal state.

Note.—It was not till after the section upon the Theory of Fever had been sent to press, that I met with Dr. Hodgkin's remarks upon the nature of the fever in his recently published volume on diseases of the mucous membranes Lecture 23rd. "I shall now proceed to state what I have conceived to be the condition of the system which constitutes fever, whether it be produced by the influence of some local inflammation or lesion, or exist by itself, independently of such exciting cause. This latter form, however, if it have an existence, I regard as of much rarer occurrence than has generally been supposed. *Fever, I imagine, to depend on the suspension, or at least very considerable interruption of that process by which during health, the various parts of the system are continually undergoing a change*, the old materials being removed, whilst others are substituted in their place. . . . . The process of incessant and universal change of the particles constituting our frames is what we imply by the terms nutrition and "interstitial absorption," it is not merely in its character closely allied to secretion, they are, I believe, essentially parts of the same function," &c. This view is supported by strong facts derived from the phenomena of fever, and by much ingenuity of reasoning. And Dr. Hodgkin proves, *at least*, that such an arrest of the molecular change takes place with reference to the secretions and nutrition of the body in fever. Thus far, there is a coincidence between his theory and that advocated in the foregoing section; the same suspension of secretion and accumulation of organic matters in the system, being part of both explanations, and the phenomena of solution or crisis being explained similarly in both.

The difference is as to the *initiatory movement*. While Dr. Hodgkin would consider the *factor* of the disease to be in all cases a local lesion or inflammation, the theory of a morbid poison supposes it to be a *molecular change in the blood* caused by the dynamic force of the decomposing particles of the poison, from which arise disturbance of the process of innervation, and of the molecular changes of nutrition, interstitial absorption, and secretion.

The theory does not assume to *determine* whether the changes in innervation (such as rigor) which mark the commencement of *formed* fever, are the direct effects

SECT. 5.—*The Characters of the Disease produced by the Infectious Animal poison of Typhus.*

The argument for the foregoing theory of fever, would obviously be much strengthened if it could be made to appear that the phenomena of Typhus are so analogous to those of the other morbid poisons, as to entitle it to a place among "those special contagions, which do not amount to more than five or six, and are all comprehended under that class of which it is the general distinguishing characteristic to occur once only during the life-time of the individual;" in other words, to be classed with the exanthemata.

We find medical writers much divided upon the question whether the petechial eruption of typhus is a primary and essential, or a secondary and accidental character of the disease. We may refer to De Haen,* Hoffman, and especially to Bruserin's elaborate argument for the former opinion, and for the consequent classification of typhus among the exanthemata; and among more recent writers the same view is ably supported by Dr. Copeland and Dr. Peebles, Dr. Roupell, and Dr. Davidson. Dr. Alison seems inclined to adopt it, though his language is reserved and cautious. "Such cases of spotted fever may be said to form the link that connects the order of fevers with that of the contagious exanthemata."†

If it be found that the analogy is complete in every essential particular, and that the objections which have been urged against the classification of typhus with the exanthemata are founded upon supposed discrepancies, which have no real existence, we shall be entitled to substitute for this cautious approximation, the decided definition of Dr. Peebles: "This contagious febrile eruption is an exanthematous affection, the production of human effluvia where society is placed in circumstances favourable to its developement, and should be considered the effect of a poison *sui generis.* It arises from a miasm, which generates in the human body *an eruptive fever distinct from all others, as other exanthemeta are distinct.*"‡

The first point of resemblance, and one much insisted on by the older writers, is the *primary* nature of the eruption. In this particular it differs from the petechiæ which occur in the advanced stage of many fevers, and cannot be considered essential to them. "The petechiæ," says Bruserius,§ "besides that they break out in all patients, or at any rate in by far the greatest number, as I have already said, likewise appear sooner in particular instances, generally about the fourth day, sometimes even earlier; but very seldom if ever at all delay breaking out beyond the seventh day, unless they be very anomalous, while the secondary and symptomatic ones appear much seldomer, and in fewer patients, nay, very late," &c.

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of the poison carried through the circulation to the nervous centres, or whether, as Dr. Hodgkin infers, from a conversion of Edwards' proposition, "since cold has the effect of retarding, especially that function by which particles to be rejected from the body are thrown off, *a suspension of this process from another cause should be attended with a sensation resembling in a degree those caused by cold.*" This seems rather a doubtful conversion of Dr. Edwards' fact. I shall hereafter return to Dr. Hodgkin's ingenious speculations, merely observing for the present, that while some parts (see page 491) support a humoral theory, his theory will by no means explain the phenomena of *infection* as the humoral theory does.

* Ratio Medendi. Vol. II. Chap. I.

† Edinburgh Medical and Surgical Journal. Vol. XXVIII.

‡ Idem. Vol. XLVII. § Institutes. Vol. III.

Hoffman* also describes them as appearing "in nonnullis *quarto* vel *circa septimum* diem in dorso potissimum pectore et brachiiscum vel sine levamine maculæ in aliis copiosiores in aliis pauciores coloris varii," &c. Modern observations are consistent with these. Thus, Dr. Barker, after taking much pains to prove by a reference to older authors, that this eruption was not peculiar to the Irish epidemic of 1817-18, says, "From a comparison of many cases, I would infer that it generally makes its appearance between the fifth and seventh days inclusive of the fever," &c.†

If we refer to descriptions of the jail or hospital fever, we find Monro enumerating the fourth, fifth, sixth, and seventh as the most frequent days of the measly eruption; and Sir J. Pringle states that he frequently saw them as early as the fourth or fifth day.‡

Another resemblance is presented in the phenomena attending the progress of the disease; more especially the attenuation which may be observed between the eruption and the affections of mucous membranes. In exanthematous typhus the same dry harassing cough is observed previous to the appearance of the eruption as in measles. On the coming out of the eruption this subsides, unless a catarrhal complication exists. Again, if the mucous membrane of the bowels be the seat of irritation,and a diarrhœa (whether the effect of the disease or of medicine exist) the eruption will fade. This is analogous to what has been observed in scarlatina,§ and it has been urged as an argument for the free use of purgatives in typhus, that they clear the skin from spots. In the definite nature of its progress, and its disposition to terminate critically and at once, typhus resembles the exanthemata as much as it differs from the intermittent and remittent fevers with which it has been confused and compared. Neither does it appear that when once the febrile movement has commenced it can be arrested any more than the action of other morbid poisons. Most of the cases in which this is supposed to have been done have been merely cases of strong nervous shock from exposure to infection, without evidence of the infection having been imbibed into the system.

The last resemblance upon which it is necessary to dwell, is the mode of communication.

The fact of typhus being communicated from one person to another, is a powerful argument for classing it among the *special contagions.* An examination (hereafter) of the circumstances which *favour* infection, will shew them to be the same in both, and the time at which they become infectious seems to be the same in both, viz. at and after the period of maturation or crisis. The argument adduced by Dr. Ferriar against the humoral theory, "that neither would a patient after recovering from a nervous fever, cease to infect others till the whole mass of his fluids were changed," is thus deprived of its weight.

The histories of patients admitted into our fever hospital afford frequent illustrations of this fact, as they constantly attribute their infection to some neighbour, or member of their family, who has returned home cured from hospital; and there is at present in the hospital a man who has suffered severely from this cause, having lately lost his wife by a typhous fever which commenced on one of his children, who was hugged and kissed by a man upon his discharge from the hospital after passing through a most severe typhus.

But as evidence on a large scale is to be preferred to individual instances, let us take Dr. Perry's very strong and satisfactory testimony to the fact with reference to *both* diseases.||

Into the fever house in Glasgow are admitted cases of measles, scarlatina and small-pox, and patients are very frequently sent in labouring under bronchitis,

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* Medicinæ Rationalis. Tom IV. p. 120.

† Dublin Medical Transactions. Vol. II.

‡ Monro on Hospitals, page 10; and Sir J. Pringle on Diseases of the Army, page 299.

§ By Fothergill, and others.

|| Dublin Medical Journal. Vol. X.

&c. &c. I found by experience that when the latter class of patients were sent into the convalescent ward, where they necessarily mixed with the others, almost all who had not previously had typhus fever were either seized with it before leaving the house, or returned soon after labouring under it. The period intervening between the time of their being sent to the convalescent ward and the attack being never less than eight days. Although means were taken to keep those recovering from small-pox, scarlatina, &c. in a separate room from those convalescing from fever, the rooms being adjoining the non-intercourse was incomplete, and the result was, that these diseases occasionally spread among the typhous convalescents, and the convalescents from small-pox and scarlatina caught typhus." He states that "the result of a trial of the plan of keeping non-febrile cases in the acute wards till able to go to their homes was, that *not one* so detained ever caught fever in the wards, or returned with it afterwards." Dr. Perry's statement is confirmed by Dr. Stewart, who says, "In fact, 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." *

Such are some of the most striking analogies between typhus and the class exanthemata; others not less important arise from a consideration of the supposed discrepancies which exist between the laws and phenomena of the two diseases.

Each writer who has opposed this classification of fever, has urged some objection or other which he considered fatal to it. We shall examine them in detail, and endeavour to show that they belong to two classes. 1. Those which apply to the exanthemata as well as to typhus; and 2. Those which *do not* apply to typhus, but to other fevers.

In both cases the argument from discrepancy must be ill-founded, as in the first the differences become anologies, and in the second, typhus, by being separated from other fevers, becomes more completely identified with the "specific contagions."

To commence with the latent period of typhus. Its variable length has been urged against the classification. That of the exanthemata appears to be equally so. In scarlatina it may extend from a few hours to twenty-one days, according to Dr. Williams and Dr. Maton. In measles from a week to a fortnight, and in small-pox from five to twenty-three days.

II. The eruption, it is said, is not invariably present. This objection is not as strong as it appears, and since it is admitted that the eruption of typhus has only very lately been attentively examined as a diagnostic character of the disease, we cannot think the question likely to be illustrated by the kind of testimony which some opponents bring to bear upon it. †

The answers to this objection are, 1. It is often present, though so indistinct as to escape a superficial examination. "On such occasions," says Dr. Barker, ‡ "*the suffusion of the eyes* is a pretty certain indication of its presence." "They sometimes," says Bruserius, § "lurk under the epidemics, scarcely perceptible, and are only seen through it on attentive examination; nay, they sometimes do not appear unless cupping glasses be applied, by which they are called out."

Similar is the observation made by Sir J. Pringle, || and repeated by Dr. Roupell, upon the arm on which a ligature had been appiied for bleeding.

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* Edinburgh Med. and Surg. Journal, No. CXLV.

† *Vide* Dr. West's paper.

‡ Dublin Medical Transactions, Vol. II. Monro also remarks, that though many had no petechiæ, in all who were very bad the countenance looked *bloated*, and the *eyes reddish and somewhat inflamed*, page 12.

§ Institutes. || Page 300.

2. In the returns from which the comparative frequency of appearance of the eruption is deduced, there are two sources of error which have been well exposed by Dr. Davidson. The first is, that they contain a large proportion of cases *not* typhus; the other, that many of them entered hospital at an advanced stage of the disease, after the retrocession of the eruption.

Dr. Davidson observes that one fact powerfully supports the opinion that contagious typhus, in the great majority of cases, particularly in adults, is attended with the eruption, viz. that almost all the instances of fever which have occurred during the last six or seven years among the physicians, clerks, nurses, &c. of the Glasgow fever-hospital, have been accompanied with this exanthema. *

The following remarks of Dr. Stewart on this subject deserve consideration.

"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 typhus in Edinburgh was unheeded before 1832) shews 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 for nor registered in that institution, and was received as a new discovery." †

3. We reply that the occasional absence of the eruption is in truth an *analogy*. "For," says Burserius, "as the variolous fever, or the variolous disease unaccompanied with small-pox, sometimes occurs, I should not consider it at all absurd to suppose that the petechial fever may in like manner take place without petechiæ."

In another place this author remarks: "This is generally observed to happen when they prevail epidemically. But it does not occur so frequently and decidedly to the observation of any one as that of the inoculators. For not unfrequently at the usual time after the inoculation, a fever comes on which continues several days, and then goes off without being followed by an eruption of pustules. Who would not call it a variolous fever? ‡

I am acquainted with a family in which small-pox made its appearance, affecting different individuals in the following modes. One with confluent eruptions, another with scanty, two with variolous fever without eruption, and another with intense vomiting and delirium, but no subsequent fever or eruption.

The same occurrence of a peculiar fever without eruption, has been remarked in epidemics of measles, by Sydenham and others. Rayer states that Guersent has observed individuals in families where measles prevailed, exhibiting all the other symptoms of the disease except the eruption, and that he has himself several times seen cases in which the eruption was incomplete, and which might have been referred to the morbillary fever of Sydenham. §

Every one who has had any experience of epidemics of scarlatina, must have observed fever and sore throats of the same character as that of scarlatina, but without eruption, occurring in families in which this disease prevailed. Rayer quotes the testimony of a number of authors upon the subject, and Dr. Tweedie introduces it as a variety of the disease into his classification. This scarlatina sine exanthemate is very frequently met with in practice.

III. A want of uniformity of the character and time of appearance of the eruption has been alleged.

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* Essay. page 22. † Edinburgh Med. and Surg. Journal, Vol. LIV.
‡ Institutes, Vol. III.
§ On Diseases of the Skin.

"Of the varying characters of the eruption," says Dr. West, * "almost every quotation has afforded an illustration, and we have seen the date of its appearance vary from the second to the seventeenth day."

We are by no means convinced that the subject has been *illustrated* by Dr. West's quotations, which appear to be descriptive not so much of typhus as of every other variety of fever. On the other hand, testimony is not wanting of observers who have explained these apparent irregularities in the character and periods of the typhus eruption, and reconciled their apparent inconsistency with an exanthematous theory of fever.

Such we meet in the following passage from Burserius's admirable chapter on petechial fever. "Le Roy also observes that there is some distinction between the primary and secondary petechiæ, which consists in the difference of their colour, namely, that the former are of a palish red and rosy colour, and in general break out in great numbers, principally on the loins and legs, that the latter on the contrary are generally of a purple colour, like deep red wine, and are sometimes also brown or black, and fewer in number."

But we must also remember that the primary ones break out soon, and when they are epidemic appear not only in all affected with the same disease, but are likewise very frequently combined with other diseases called intercurrent ones —(for these last are not always wanting, as some contend)—while on the other hand, the secondary ones break out later, and generally about the height or towards the end of the disease, and not in all patients, but only in those whose blood is so vitiated as to become almost putrid and occasion gangrenes here and there on the skin, or being thrown into violent commotion by a heating regimen and medicines, is effused into the spaces of the skin, *but not by the wisdom of Nature endeavouring to free herself from the noxious miasma.* Hence I would say that the primary differ from the secondary petechiæ, because the former arise from a peculiar and poisonous miasma, and the secondary from the crasis of the blood being deranged by the violence of the disease, or from its increased motion, or lastly, from a heating regimen having been employed." Such also we meet in Dr. Staberoh's paper on the eruption attending epidemic fever. In which he shows that not only do petechiæ of the ecchymotic or secondary kind occur after and *apart* from the exanthema, but that spots of these are capable of being converted into ecchymotic spots.† Attentive observation has convinced me that not only are the above statements correct, but that we may add that a third variety of late petechiæ occur in cases in which from diarrhœa or hypercatharsis in the beginning of fever the exanthema lurked under the epidemics. The conversion of this *indistinct eruption* into ecchymosis taking place, or the latter being superadded in the course of the disease, *and appearing to be primary.* A fourth variety is thus alluded to by Dr. Peebles; "Petechiæ may be mixed with the exanthema, and in some epidemics the exanthema has been prevented from showing itself by the disease passing so rapidly from the sthenic state to the putrid, that it has not had time to make its appearance."

Of course under any of the foregoing circumstances the late appearance of a petechial eruption is no argument for a want of uniformity in that of the exanthema. The frequency of occurrence of these secondary petechiæ is only an additional reason for believing that the two forms have been by many writers confounded together.

IV. It is objected, "That the disease often occurred more than once during the life-time of an individual."

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* On Exanthematous Fevers. Edinburgh Med. and Surg. Jour. No. CXLIII.

† London Medical Gazette, Vol. I. N. S. p. 973.

The objection assumes that typhus confers no immunity from subsequent attacks and that the exanthemata do confer an immunity. The answer is, that experience warrants our belief in a considerable power of destroying the liability to subsequent attacks in typhus, and that, though there can no doubt of the exanthemata possessing this power, exceptions to it are frequent in all of them.

It must be admitted that in this country there is a general belief in the protecting power of a seasoning or initiatory fever, and though we rarely meet with a medical man who has not had typhus, we certainly meet with few indeed who have had it more than once. The nature of the subject does not admit of very precise proof. We can only obtain the *general results* of experience. Dr. Barker* states as the results of his, "that he has for some time entertained the opinion that sufferers from fever attended with this eruption, if they are not altogether secured by it from a second attack, are not at least so liable to it as those who have had a fever of the ordinary kind; and, though he frequently made the enquiry, he never found a patient in whom this symptom was distinct who had suffered from the same fever on any former occasion." Dr. Perry† states as the result of an extensive series of observations his opinion, "that typhus generally is taken but once in a life-time, and that a second attack does not occur more frequently than of small-pox, and less frequently than of measles or scarlatina." Dr. Davidson states that of 609 patients in the Glasgow fever hospital only 74 stated that they had ever had fever previously. He justly observes that when we take into account the various diseases which are confounded with typhus, this small number can be easily accounted for.

But the protective power of the exanthemata has been much overrated. Three instances of second attacks of small-pox came to my knowledge in this county very recently. In two of them the patients had suffered the disease from inoculation very few years before. In one in which the inoculated disease was severe a most confluent eruption accompanied the second attack seven years after. Another instance has been related to me of a lady living in this country who has had the disease three times. Dr. Roupell refers to the case of one who had it seven times.

Instances of second attacks of measles are given by Dr. Bailie, who attended five children in May, and again in the following November, by Dr. Webster,‡ and by Rayer, who states that he met with three instances of second attacks of measles in the interval between the publication of the first and second editions of his work. The remarkable case of a second attack by Dr. Graves § should, perhaps, be termed relapse into measles. The second eruption appeared twenty-one days after the commencement of the first illness, in which the eruption had been copious and severe.

Cases of second attack of scarlatina are stated by Roupell to be not at all uncommon. Several have fallen under my own observation.

V. The liability to relapse in cases of typhus has been urged as an objection to the classification by Harty and others. It might be replied that cases of measles, such as that of Dr. Graves just referred to, and cases of *reversio*, as it is termed by Rayer, after scarlatina, would tend to shew that the exanthemata are not exempt from relapse. But the true answer is that typhus *is peculiarly exempt* from relapse. Two kinds of cases are erroneously considered such: 1st. Relapses from typhus into fever symptomatic of a visceral irritation—generally gastro enterite.—"I am persuaded, says Cheyne, || that obstinate and fatal

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* Dublin Medical Transactions. Vol. 2.

† Dublin Med. Journal, Vol. 10, and Edin. Med. and Surg. Journal, Jan. 1836.

‡ Medico-Chirurgical Transactions. 2nd Series. Vol. 4.

§ Dublin Medical Journal, Nov. 1840.

|| Dublin Hospital Reports. Vol. 1.

relapses after typhoid fevers are often attributable to inflammation and, perhaps, ulceration of the villous coat of the intestines." And Broussais asserts that "when the frequency of pulse in 'convalescence' does not diminish, and the strength does not increase, it may be suspected that a form of latent inflammation exists. It may be discovered by permitting an excess which generally changes this frequency into a *real fever*, and developes the pain *of* the irritated part;"* but, 2ndly, cases of fevers not typhoid will under exposure to infection relapse into typhus. Dr. Davidson gives a tabular view of the relapses in the Glasgow Fever Hospital among 686 cases, in which no case of relapse from typhus into typhus occurred, but two of febricula and one of intestinal fever into typhus.

In 500 cases of fever admitted into the Navan Hospital in 1840, two cases only of relapse into typhus occurred, both were cases of febricula, which after a few days were sent to the convalescent ward, where they relapsed into maculated typhus, one in four days and the other in fourteen days after their removal thither.

VI. Lastly, the following extraordinary objection is put forward by Dr. West:—†

"The type of the fever itself varies, being sometimes intermittent, sometimes continued, changing from the one to the other form, and being occasionally converted into other diseases."

In other words there is no such disease as typhus!

To this the supporter of the speciality of typhus replies, that the disease is here, and in numerous quotations throughout the paper, confounded with other fevers; typhoid it may be in their nature, or becoming so in their progress, not arising from an animal infectious poison, but from a variety of sources, which contain a variety of poisons, the identity of *any one of which* with the *typhus* poison is a matter in dispute, and to be argued upon the conclusion of our investigation into these sources in the following chapter.

Meantime the pertinent remarks of Dr. Copland, upon this subject, are not unworthy the notice of those who rely for the means of drawing accurate distinctions upon such sources as Dr. West has explored:—"*True* or *contagious* typhus has been confounded with synochoid and nervous fevers on the one hand, and with putrid or malignant fevers on the other. It has been already stated that putridity or malignancy, not only may characterize a particular form of fever or certain epidemics even at an early period of their course, but, also, owing to various contingencies, may take place in advanced stages of any other fever. As the circumstances favouring the generation and spread of typhus are often such as also tend to develope those changes which have been usually named putrid or malignant, and as these changes are frequently observed in the latter stages of typhus—the symptoms distinguishing this fever becoming associated with, or followed by, those indicating the putro-adynamic state—so has it been often confounded with other fevers in which this state has predominated more or less. If we refer to the numerous histories of epidemic typhus, recorded by writers from the close of the 15th century up to the present time, we shall find that although many of these, owing to the concurrence of circumstances, developing a putrid or malignant disease, were instances of fever either identical with or very closely resembling that which I have described as such in the preceding section, yet many others, or even the majority, were true typhus, in which the putro-adynamic state was either early or predominantly developed. The exanthematous eruption, characteristic of typhus, being succeeded or accompanied by the petechiæ, indicating the approach of the septic condition, and being either

* Chronic Phlegmasiæ, Vol. 2, p. 53.

† On Exanthematous Typhus.

mistaken for them or for an eruption of miliaria. Owing to this circumstance, especially typhus, low, nervous, and putrid fevers, have been very generally confounded together."

The reader of the foregoing, and many other passages in Dr. Copland's admirable article on typhus, must be startled with the following passage in Dr. Roupell's recent work on the disease, when he finds that Dr. Copland is not like Peebles and others, who have described exanthematous typhus, passed over in silence, but is actually mentioned by name as belonging to the authors referred to.

"In the above description *typhus* is considered to belong to the continued fevers. It is looked upon by the most recent authors, in this and other countries, not as an individual disorder, but as one into which others may be and frequently are converted!" (page 5.)

Here for the present we leave the subject, since that portion of the argument for the classification of typhus with the exanthemata, which is derived from the differences between it and other continued fevers, will properly come under consideration when discussing the identity of typhus and typhoid fever.

Chapter II.—Of a Fever Poison, generated during the Decomposition of Dead Organic Matters.

The difficulties attending an examination into this part of the enquiry into the sources of fever are very great, and are confessed by all who are familiar with the conflicting statements advanced on the subject. Our difficulties are increased by the indeterminate character of much of the evidence offered in proof of the paludal origin of fever, some of which not only claims to prove the power of such sources to cause continued, but *infectious* fever—by the fact that much of this evidence is moreover inadequate, as it proves only the occurrence of fever in situations and among persons which might be considered equally obnoxious to contagion as to miasm, and by the silence, or mysterious, or contradictory language of those to whom we might look for assistance and direction in a scrutiny of the mass of conflicting testimony, from which our conclusions are to be drawn.

Thus, Dr. Christison says, "the great questions involved in the investigations into the causes of continued fever are three in number :—Does the disease originate in infection? Does it originate in other causes? Granting that it does originate in other causes, may such fevers propagate themselves by infection? It will be seen that they cannot be all answered by any means with equal confidence;" and, accordingly, while he is full and illustrative on the subject of contagion, he treats of other causes in a most cursory and unsatisfactory manner, and while he admits that "the general conclusion from the whole facts seems to be that a disease, undistinguishable from true infectious fever, may sometimes arise without infection," adds, "that on descending from the general question to the more special one—what the other cause or causes of fever may be?—the difficulties are greatly increased, indeed they become insurmountable, without such limited and vague facts as are at present possessed on the subject," and "it appears a needless waste of time and labour to attempt anything further on this head."

Nor are we more enlightened by Dr. Davidson, who, while he states that he is not prepared to assert that febrile affections may not, under peculiar circumstances (what he does not inform us), arise from paludal sources, effectually excludes them from consideration by putting forward the following conclusions under the head of "Alleged sources of continued fevers, *not* typhoid."

"From a consideration of the whole evidence that might be adduced respecting this point, it may be drawn as a conclusion, that although putrid

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matters when injected into the veins of animals cause death under symptoms similar to those of typhus fever, yet that the effluvia arising from similar matters do not, under ordinary circumstances, produce any deleterious effects on man." Again—"Before concluding this part of the essay we shall notice an hypothesis, which has lately been somewhat confidently brought forward to account for the prevalence of typhus in some large cities, viz.: that a peculiar malaria is generated by the animal and vegetable filth, which accumulates along the sides of rivers running through large towns, and that the inhabitants who live in their immediate vicinity become thereby subject to fever. We are quite aware that very disagreeable and sometimes fœtid effluvia occasionally arise from such situations, particularly during hot weather, but that it is capable of causing continued fever has not even been rendered probable by any satisfactory evidence."

We have it stated upon high authority that gaseous contagions contain organic matter in a state of decomposition or progressive change. We have it also announced that from certain decomposing animal and vegetable substances, organic matter in a state of "progress to decay" is evolved, which, when collected and retained in a manner similar to the former, completes the stage of decomposition, or, in other words, "putrefies."

By evidence of the most unexceptionable kind the former of these is proved to be capable of communicating the *state of change*, of which it is the subject, to the healthy human organism.

We have to enquire whether the analogy of *action* of these bodies is as perfect as the analogy of *condition* appears to be, and, whether, "when the process of respiration is modified by contact with a matter in the progress to decay, when this matter communicates the decomposition, of which it is the subject, to the blood—disease is produced."

We shall first state the analogy of condition of the *tangible poison*, evolved from decomposing organic substances, in the words of Dr. Southwood Smith—not only because it is clearly stated by him (so far as relates to its tangible existence), but, also, because this passage has furnished the text for some of the objections which we shall have to consider*:—"It is known to every one that the putrefaction of vegetable and animal matter produces a poison, which is capable of exerting an injurious action on the human body. But the extent to which this poison is generated, the conditions favourable to its production, and the range of its noxious agency, are not sufficiently understood and appreciated. It is a matter of experience, that during the decomposition of dead organic substances—whether vegetable or animal—aided by heat and moisture, and other peculiarities of climate, a poison is generated, which, when in a state of high concentration, is capable of producing instantaneous death by a single inspiration of air in which it is diffused. Experience also shews that this poison even when it is largely diluted by an admixture with atmospheric air, and when, consequently, it is unable to prove thus suddenly fatal, is still the fruitful source of sickness and mortality—partly in proportion to its intensity, and partly in proportion to the length of time, and the constancy with which the body remains exposed to it, &c.

"But this poison was too subtle to be reduced to a tangible form. Even its existence was ascertainable only by its mortal influence on the human body; and although the induction commonly made as to its origin, namely, that it is the product of putrefying vegetable and animal matter, appeared inevitable, seeing that its virulence is always in proportion to the quantity of vegetable and animal matters present, and to the perfect combination of the circumstances favourable to their decomposition, still the opinion could only be regarded as an inference.

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* Poor-law Commissioners' Fourth Report, page 130.

But modern science has recently succeeded in making a most important step in the elucidation of this subject. It has now been demonstrated by direct experiment, that in certain situations, in which the air is loaded with poisonous exhalations, the poisonous matter consists of vegetable and animal substances in a high state of putrescency. If a quantity of air in which such exhalations are present be collected, the vapour may be condensed by cold and other agents, a residuum is obtained, which, on examination, is found to be composed of vegetable or animal matter in a high state of putrefaction. This matter constitutes a deadly poison. A minute quantity of this poison applied to an animal, previously in sound health, destroys life with the most intense symptoms of malignant fever. If, for example, 10 or 12 drops of a fluid, containing this highly putrid matter, be injected into the jugular vein of a dog, the animal is seized with acute fever, the action of the heart is inordinately excited, the respiration becomes accelerated, the heat increased, the prostration of strength extreme, the muscular power so exhausted that the animal lies on the ground wholly unable to stir or to make the slightest effort, and after a short time it is actually seized with the black vomit, identical in the nature of the matter evacuated with that which is thrown up by a person labouring under yellow fever. By varying the intensity and the dose of the poison thus obtained it is possible to produce fever of almost any type, endowed with almost any degree of mortal power."

In this last sentence we recognise the echo of Majendie's questionable assertions; the preceding statements are confirmed by the account of experiments upon "le mauvais air," given by Devergie:*—"The gas, which is disengaged from putrefying animal matters, extracts with it a particular odour, infectious 'infecte,' characterized by the general term putrid odour. We attribute this odour to miasma, that is to say to a cause void of meaning, because we are ignorant of the nature of the object which it represents.

Guntz has endeavoured to enlighten the phenomenon by the following experiment: he placed a bell glass over a portion of a putrefying dead body, in such a manner as to permit the air to penetrate, he submitted the apparatus to a temperature of 26° Cent. (equivalent to about 78° Far.) and, after a period sufficiently prolonged, he suddenly cooled the bell glass; immediately the product of the vapour assembled itself into drops, which evolved a strong odour of miasma, he treated these drops with chlorine, when the odour disappeared. He was thus led to suppose that the gas in escaping from the putrefying animal matter carried with it the vapour of water combined with a certain quantity of animal matter, very minutely divided, and this constitutes what has been named miasma.

This is not the only experiment calculated to lead to this opinion—others have been made with respect to vegetable matters. Moscati entertained the first idea of condensing the water dissolved in the atmosphere, for the purpose of detecting the principle which occasioned "le mauvais air." He suspended at some distance from the soil mattrasses full of ice; the water which became deposited upon their surfaces condensed itself readily, when limpid it presented many small flakes which possessed all the essential properties of *animalized matter*. After a few days they putrefied completely. In the course of the year 1812, M. Rigaud undertook, in the marshes of Languedoc, a series of essays directed to the same end. He condensed dew on glasses, and the water which he obtained by this means presented all the phenomena obtained by Moscati.

In 1819 M. Boussingault observed that *sulphuric acid* placed in the proximity of a well, in which he had caused animal matter to putrefy, *blackened* very rapidly. He repeated this experiment in many infectious places, and found con-

* Medicine Legale. Tom. 1, p. 100. I am indebted to my friend Dr. Aldridge for referring me to this account of experiments on the subject.

stantly that the coloration of the acid was more prompt according as the air was more infectious," &c.

The inference naturally deduced from such experiments as the foregoing, taken in conjunction with the fact of the occurrence of fever in situations where these putrid exhalations have been found to exist—namely, that they contain a *fever poison*—has been met by numerous objections. The principal seem to be the following :—1. That a mephitic poison is confounded with the fever poison. 2. It is denied that these sources ever generate fever, because the number of cases does not bear a sufficient proportion to the number of instances of exposure, and because they generate several diseases differing in their nature from fever, and it would amount to a confounding of fever with these,* if we attributed its origin to the same poison. 3. It has been objected to the evidence of the frequent occurrence of fever from this source—that it is furnished from the experience of persons who deny the infectiousness of fever, and is, therefore, suspicious ; † that in the recorded cases malaria and contagion have been confounded ; that it amounts only to a proof of the frequent coincidence of fever and the effluvia from filth, and does not prove that the former stands to the latter in the relation of an effect to a cause ; that, granting continued fever is ever thus produced, it is not contagious or typhus fever, &c.

I. It has been said‡—"If the statements of Dr. Smith were put into the simple form of the only proposition which they really contain, they would amount merely to this—that exhalations from certain putrescent matters have the power of producing both asphyxia and continued or typhus fever ; the former of which is a result familiar to all, and the latter, a mere assertion, deriving a little hue of probability from its juxta position to a known truth. There is a wide difference between the asphyxia, which is caused by mephitic gases, and typhus fever,—a difference which can never be explained, as Dr. Smith attempts to do, by a reference to the diversity in the doses of the poison. We presume that if a few doses of the poison, in its less potent shape, were sufficient to create typhus fever, *a fortiori*, such a quantity of it in a more concentrated form, as would be capable of producing a state of asphyxia not ultimately fatal, would commonly at least leave the sufferer for days or weeks in the toils of a highly dangerous fever, yet the reverse is the case, as the histories of mephitism amply demonstrate."

So they doubtless do, and they, moreover, shew that in some cases of recovery from mephitism, a disease, apparently the effect of a morbid poison, followed, though *not* fever.

But this writer has, like Dr. Smith, confounded the action of two poisons of different kinds—an inorganic poison, sulphuretted hydrogen, and a morbid poison, whose action depends not upon its chemical qualities, but upon the existing condition of its particles, they being at the time of their evolution in a state of decomposition or transposition.

The advocate for the malarial origin of fever does not regard the fever poison as the product of *extreme putrefaction*, capable of causing mephitism or fever, according to the dose in which it is applied. But he holds that during the *progress* to decay of organic substances, matter in a state of decomposition is

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* "Dr. Smith illustrates and supports his doctrine of the malarial origin of fever by referring to facts which relate merely to periodic fevers ; and he maintains the identity of the 'fever poison' of this country with the poison of plague ; wherefore, on the principle, that things that are equal to the same thing are equal to one another—plague and ague are generated by the same poison !"—*Forbes's Review*, No. 21, p. 13.

† Vide Dr. Christison's article—Fever. Library of Medicine.

‡ Forbes's Review, ut supra.

evolved which is capable of communicating its state to the organism with which it may be brought into contact, while, on the completion of the process of decomposition, the morbid poison ceases to be evolved and the mephitic poison is generated.

A perfect analogy to this is found in the effects of decayed sausages, which, according to Christison, "are poisonous only at a particular stage of decay, and cease to be so when putrefaction has advanced so far that sulphuretted hydrogen (the mephitic gas) is evolved." True, in mixed sources, and those which are receiving daily additions of new matters, the morbid poison may co-exist with the mephitic poison, and the latter may occasionally, by its sedative effects upon the nervous system, assist the operation of the former; but they are essentially different in their nature and action.

The second objection—"an alleged want of proof that the fever poison is ever generated in such sources"—rests, 1st, upon the relatively small number of cases of fever so produced, compared with the activity of contagion; and, 2ndly, upon the fact that several diseases of different kinds, from tic-doloureux up to plague, are attributed to miasmic effluvia. Can we (it is asked) believe that they are all owing to the same poison?

The first of these grounds is urged against decomposing animal matters; the second chiefly against mixed sources, as sewers, banks of rivers, &c.

It is true that very few observations exist which can be said to prove the occurrence of fever from exposure to animal putrefaction—still, some such cases have occurred. The following is referred to by Dr. Christison as an unexceptionable one:—

*"An American merchant-ship was lying at anchor in Wampoa road, sixteen miles from Canton. One of her crew died of dysentery. He was taken on shore to be buried; no disease of any kind had occurred in the ship from her departure from America till her arrival in the river Tigris. Four men accompanied the corpse and two of them began to dig a grave. Unfortunately they lit upon a spot where a human body had been buried about two or three months previously, as was afterwards ascertained,—the instant the spade went through the lid of the coffin a most dreadful effluvium issued forth and the two men fell down nearly lifeless: it was with the greatest difficulty their companions could approach near enough to drag them from the spot and fill up the place with earth. The two men now recovered a little, and, with assistance, reached the boat and returned on board...... One of these men died on the evening of the fourth day, and the other on the morning of the fifth, after symptoms of malignant petechial fever (the petechiæ occurring on the fourth day).

"In eight days after the opening of the grave one of those who were not engaged in the work was attacked with the same symptoms as his companions, and the fourth had a slight indisposition of no very decided character."

It is to be remarked, that in the above case two circumstances were present which we shall see have not always existed in the negative instances, brought forward to prove that this is not a source of fever; namely, "*confinement* of the effluvia," and a *not very advanced* stage of putrefaction. Ferriar, who did not consider the exhalations from putrid animal matters a source of fever, says—†"It appears from some late observations made on altering the vaults of a church in France, that the *confined effluvia* of putrid bodies produce fever when brought into action. Perhaps this is the solution of the question." ‡"Fourcroy states, that the grave-diggers informed him that the putrid process disengages

* Medico-Chirurgical Review, for Jan. 1825, p. 203. Dr. Christison also refers to the Mem. de la Soc. Royale de Med. 1. 97.

† Medical Histories, vol. 1. On New Contagions.

‡ Walker's gatherings from Grave-yards, p. 124.

elastic fluid, which inflates the abdomen and at last bursts it; that this event instantly causes vertigo, faintness and nausea in such persons as, unfortunately, are within a certain distance of the spot where it happens, &c." In the exhumations, conducted on such a large scale at the cemetery of the Innocents, and quoted by Bancroft and others in proof of his position, neither of these conditions could have existed, since no interments had been allowed for six years previously.

In many of the cases also related by Mr. Walker it is mentioned that the bodies had been buried for years, or were in an advanced stage of putrefaction; under these circumstances mephitism was produced—but not fever.

The other part of this objection, namely that so many different diseases are ascribed to this source—can they all be the effect of the same poison? can only be answered by supposing a variety of morbid poisons to be formed together or consecutively in the same source. Several considerations render it probable that this is the case in some malarial sources.

1. The progressive nature of the changes which the decomposing body undergoes, and the different circumstances under which the same organic matters (undergoing decomposition) may be placed in different places, or at different times. There is nothing improbable in the supposition, that the same source may at one time give origin to the poison of ague, and at another to the poison of fever.

2. The fact that an individual exposed to these sources will frequently become affected with two diseases. These will usually follow one another at a short interval. "In the vast horde of cases," says Dr. Addison,* "which the river side is continually sending forth, synochus and typhus are of frequent occurrence, and these are frequently followed, when the patient is convalescent, by well-defined agues." Sir H. Marsh has noticed the same occurrence in the epidemic of 1826, in Dublin, and it is well known that after this epidemic the hospitals of that city were filled with cases of ague. This is perfectly analogous to what happens from exposure to two morbid poisons, the one which has the shortest latent period takes precedence and is followed by the other, as in the case related by Dr. Williams of a boy who was inoculated at the same time with the virus of measles and cow-pock. The cow-pock first ran its course, and was then followed by measles. In the same way it is very possible that the poison of ague, imbibed at an early part of the year, may lie latent until the conclusion of a continued fever received many weeks later.

3. This power of generating different diseases, is alleged *of* those sources in particular which contain a *variety* of organic matters, and which are in a state of constant change from the superaddition of new materials or from atmospheric changes—such are sewers, the banks of rivers, &c.,—and it is to these that the great body of evidence, as to the frequent production of fever, applies, and not to the regular, uniform, and spontaneous decomposition of any single portion of animal matter, however great its bulk.†

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* On Malaria. London Medical Gazette,—vol. 3, new series.

† If the subject admitted of an explanation purely hypothetical we might draw an analogy, not destitute of plausibility, between the action of human contagion and putrefaction in this particular; and we might suppose, that as in fever generated spontaneously in the human body, there does not seem to exist any power of communication by infection, so in dead animal matter the product of any single mass of spontaneous putrefaction is not a fever poison, but that this is generated by the exposure of *fresh* dead organic matter to the contagion of the former. For this hypothesis to be consistent with the facts, the following should be the various consequences of exposure to putrefactive decomposition :—

Sect. II.—In order to obviate the objections urged against the evidence of the frequent occurrence of fever from malarial sources,—namely, that it has been confounded with contagion, and that, at all events, the evidence proves, no more than the frequent co-existence of filth, fever, and poverty.

We shall select only a few cases which have occurred under circumstances unfavourable to the supposition of such a cause as contagion, and the histories of which present contrasts to that of contagious fever in some of the following particulars.

I. The class of persons affected, not those usually obnoxious to contagious fever unless under circumstances of prolonged exposure.

II. Occurring without the presence of any of the aids to contagion and at an opposite season of the year.

III. In localities in which contagious fever does not prevail.

IV. Spreading in spite of the preventive measures, which are found to check the diffusion of contagious fever.

"In great towns," says Christison, "cases are met with during the intervals between epidemics, and in a station of life where epidemic fever in epidemic seasons of the worst kind is seldom witnessed. A fever of this description, tedious in its course, characterized by much nervous and muscular depression, without any particular local disturbance, and, especially, without the marked disorder of the functions of the brain which distinguishes most cases of epidemic typhus and synochus, was so prevalent among the better ranks in certain streets of Edinburgh some years ago, at a time when fever was not prevalent among the working classes, that a general impression arose among professional people of the existence of some unusual local miasma. A great variety of parallel facts might be referred to—all leading to the general conclusion, that a disease if not identical with, at all events closely resembling, synochus and typhus as described above, may arise without the possibility of tracing it to communication with the sick. A statement of this kind acquires great weight in the instance of such a visitation of disease as that just alluded to, which prevailed among people in easy circumstances in a great town."

Very similar is the testimony of Dr. Cheyne :—"For several years the fever appeared in families only in solitary instances, or if more than one were affected they were seized nearly at the same time, but it did not extend so as to lead us to think that it propagated itself. We were unable to assign the cause of the disease further than that we observed in several houses, in which our patients lay, that fætor which is discoverable when a sewer is choked, and, in some instances, upon enquiry it was found that the sewer leading from the house had been improperly constructed and neglected."

A similar instance of fever, apparently caused by defective sewerage, came under my observation recently in the house of a gentleman of fortune in this county. For a long time an unpleasant odour had been remarked in several parts of the mansion, more especially near this gentleman's study and in the

1. From exposure to a single mass of animal matter undergoing spontaneous putrefaction—no fever.

2. From exposure to the emanations from substances added to the former disease, varying in intensity in proportion as circumstances were more or less favourable to rapid communication of the "contagion of decay" from decomposing to recent organic matter.

3. From successive exposure of a number of individuals to successive additions of organic matter, (under above circumstances), a number of cases of disease.

men servants' sleeping apartment. The poisonous effects of the malaria were first produced in the form of obstinate dysentery in one of the female servants. Then the owner of the mansion was attacked with what he at first supposed was mere biliary derangement, but which rapidly assumed all the characters of severe gastric fever, becoming attended toward the close with purple petechiæ and terminating fatally on the 11th day.

About the same time two men servants were seized with symptoms of fever. In one it was cut short and in the other it ran its course, ending favourably about the 11th day. Two other persons who came to the house on business (from the neighbourhood) and who remained in it for a few hours, were seized with the same fever, which ran through its course at their own houses but without extending to other individuals.

After this lamented occurrence the cause of the effluvium was searched for and found to be a leakage of the soil pipe of one of the water-closets, which had allowed the filth to percolate through the wall and exhale into the atmosphere of the house. This exhalation was also much favoured by the warm temperature kept up in the house by heated flues.

About the months of October and November, 1839, I was repeatedly consulted by the inmates of a large establishment in the neighbourhood of Navan, on account of different forms of gastro-enteric affection, especially diarrhœa and dysentery. So many instances occurred at intervals, (in some cases of weeks), and the general resemblance was so great, that I thought they must arise from some local cause, and I expressed a strong suspicion that some source of malaria existed in the house or immediate vicinity. The house itself was large, airy, and commodious, so that our inspection was directed rather to the immediate neighbourhood, and it was thought that the cause had been discovered in an old sewer which had been laid open in the course of some building operations. The closing of this was not, however, attended with the effect of stopping the endemic affection, though it gradually ceased after about a dozen people had been attacked. The following spring was remarkably dry, scarcely any rain having fallen for about six weeks ; toward the close of this period an effluvium of a very disagreeable nature became perceptible in some parts of the house, and at the same time —within a day of each other—two of the inmates were attacked with exquisitely marked typhus, attended with profuse measly eruption, and in one of the patients with violent delirium. Every circumstance rendered the existence of contagion in either case highly improbable, I might almost say impossible, and on my again expressing my strong conviction that some form of malaria was the cause of the fever, I was informed of the effluvium perceived in some of the passages, and also of the fact that in the original construction of the water-closets they had been made to depend for their supply of water upon a cistern of rain water, which, of course, had been for weeks empty during the present spring and preceding autumn. These cases did not spread, and all traces of indisposition were removed by making the required alterations for ensuring a constant supply of water.

In the month of October, 1839, I attended a respectable man who resided in a large and airy mansion, as "care taker," during the absence of the family upon the Continent. His illness had come on slowly and insidiously, but, when I saw him, had all the characters of bad remittent fever, attended with much abdominal congestion. This it was attempted to relieve by leeches, &c., but it increased and led afterwards to large evacuations of blood from the bowels. He recovered slowly and with difficulty. At the time I saw him he was lying in the basement story of the house.

A few weeks after the return home of the family, the *butler* was attacked with symptoms of gastro-enterite and slight jaundice. He recovered partially in a few days, but in the course of a week after was suddenly seized with complete loss of muscular power, paleness and coldness of the surface, sickness of stomach, &c., followed by vomiting of a dark olive fluid, and in two days by large evacuations of tarry blood from the bowels, hiccup, subsultus, &c., while the skin was covered with vibices and black petechiæ—some of them of the size of large shot. The fever which followed had no perceptible remissions and perfectly accorded with the descriptions of putrid malignant fever by Huxham and others. The striking resemblance of some of the symptoms of this case to that which had occurred in the same place more than a year before, led me to attribute both to a common cause and to enquire for the source of the malaria. The following were the facts ascertained:—In a room in the basement story, occupied by the last patient and in which he had latterly sometimes slept, was a sink emptying into a pipe, which communicated at the distance of about ten feet with the main sewer of the house—into which the contents of two water-closets passed. This sewer was very large at its termination and when the wind blew from that direction towards the house, there being no smell trap under the sink, the effluvium of the sewer was carried up into this room and became so insupportable that the patient used to stuff the aperture with a piece of rag when retiring to bed. Upon enquiry I was informed that the first patient had frequently before his illness remarked the same fætid effluvium. It is worthy of remark that the sewer had *not* been cleaned in the interval between the two cases.

The following case, very similar to the last in its nature and origin, I give upon the *suspicious* authority of an anticontagionist*:—"I attended," says Dr. Armstrong,† "a very respectable tradesman, labouring under a remarkable bad attack of typhus fever. It was such a case as would have been called plague in the time of Sydenham. He had knotted glands and carbuncles, and black petechiæ. He was one of four or five individuals who had transacted some business in a nobleman's kitchen; a filthy fluid had overflowed that kitchen; he was sickened at the time, and in common with *all* the others had an attack of typhus fever."

If we looked about for a large town less liable to contagious fever than others we might probably find it in Birmingham,—yet here endemial causes of the kind, which Dr. Davidson has pronounced inadequate to this effect, have produced fever. A good instance for illustration is found in Dr. Ward's account of an endemic fever, which prevailed in certain localities in Birmingham in the *summer* of 1837.

‡ "The river Rea, that separates Birmingham from its suburb Badesley and serves as a cloaca maxima to both, carries its filthy stream onward, partly to turn a mill and partly to fill a mill pond. During the drought which prevailed last year the water was very low in the main stream and mill pond, and the mills not being regularly worked became quite stagnant and offensive. The back stream also became dry and shewed its mud banks, that were only occasionally wetted by a flush of the washings of the town after a shower, or by the small surplus accumulated during the cessation of the working of the mills. The exhalations from the half dried mud and putrid water were so disagreeable at night as to nauseate the more delicate inhabitants of the adjoining streets, and soon produce disease in the form of typhoid fever of an infectious (?) character." He goes on to state that about 50 cases—some fatal—occurred in the immediate vicinity of the stream, and "still lower down the stream, where the water was as black as ink, there were 13 pauper cases in one yard, and many others, both pauper and private, along the same line." That this fever was owing to the state of the stream is proved by the disease being confined to the locality, the small number affected in so large a population as Birmingham, the season of the year, and the exemption of this town from the causes which aid contagion—these are well-summed up by Dr. Ward.

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* See Christison, ut supra. † Lectures by Rix.
‡ Provincial Medical Transactions, vol. 6.

"There is a difference of nearly 200 feet in the elevation of different parts of the town. The streets and the courts or yards in which the mechanics live are wide and airy in general; fuel is cheaper than in any other large town in England; the water is excellent—and till within the last year there has been but little distress."

We have already adduced the effects of seclusion of the sick in proof of the infectiousness of typhus. In the fever arising from endemial sources this measure has no such influence. I was much struck with this fact when making some investigations as to the source of a fever, which prevailed in the summer of 1839 in a hamlet attached to a flax manufactory near this town, from which a considerable number of cases had been sent to hospital in the months of April, May, and June. The object of an examination which I made of the place was to obtain satisfactory instances of contagion, but I soon found that no such evidence was to be procured. For the intervals between the illness of different members of the families were too irregular to admit of communication from one to another. Thus, in one house the first case sickened on the 2nd of April, and the second on the 5th. In another, more than three months intervened between the first and second cases. And in several families in which the first case had been early removed to hospital, the second had sickened before the patient's return. Besides, there was too much cleanliness and comfort: several of the houses had been repeatedly white-washed during the time that the fever was going through the family, and the inmates were all well off—being employed in the neighbouring factory.

Several things convinced me that this fever had a malarial origin. The hamlet was built in the form of two parallel streets, terminating in a large open space, in front of which were twelve houses looking North East. This space had no drainage and was full of shallow pools of black putrescent water, into which the inmates daily threw cabbage leaves, &c. to rot for manure. In this country the East and North East winds prevail for the first three months of summer—April to June—and in consequence the inhabitants of the twelve houses described were peculiarly obnoxious to the emanations from these pools. The weather had during the summer been unusually dry and favourable to such emanations. Accordingly I found that while only seven cases of fever had occurred in twenty-two houses, forming the longer of the two streets, 30 out of 50 (the entire number) had occurred in these twelve houses.

The proof was to my mind rendered complete by the immediate effect of the heavy rains which set in in July. The disease was stopped at once and I have not heard of a case of fever in the same place since.

Similar proof, derived from the sanatory effect of the removal of the assigned cause, was afforded me in the case of a house in this county from which three servants were sent into the Navan hospital at short intervals labouring under continued fever, one of whom was also admitted a second time with severe dysentery. A very offensive smell had been long noticed in the yard adjoining the kitchen, and after the occurrence of these cases the sewer leading from the kitchen was in consequence examined, and found to be completely obstructed by a quantity of black putrescent matter. Upon the removal of this the smell of course disappeared and no return of indisposition has since occurred, either among the servants or family. There was not the slightest evidence of contagion in any of these cases.

A reason for attributing the fever to the operation of endemial causes might be found in some instances, in the fact of the great indisposition of the disease to spread in the house in which a case occurs, even though the circumstances favourable to contagion may be present. Such an instance is given by Dr. Fergusson which we shall have again to notice.

In a paper on the statistics of fever, in Belfast, Dr. Mateer states that "one street, Carricks Hill, and its continuation, Mill-field, with the adjoining lanes and entries, are found to have furnished three fifths of the whole amount of cases, and yet they are by no means the poorest or worst ventilated parts of the town." He attributes the prevalence of fever in this locality to the great want of water—"the consequences of which are want of cleanliness and bad sewerage, so that decayed animal and vegetable matter of all kinds, not being carried off by a current of water in the usual way, accumulate and generate miasmata."* This observation is of the more value that Dr. Mateer adduces it in support of the action of infection. Upon which an acute reviewer remarks :—"Surely this offers the very strongest argument against Dr. Mateer's own view of the extensive operation of contagion ; why should this be all powerful in one particular locality ? why should it not do its worst where poverty and bad ventilation flourish ? why, but that on the large scale other causes of fever are far more potent than contagion." †

Lastly, the following case by Dr. Currie illustrates this form of fever in several particulars :—

"The 30th Regiment, as is usual with troops in Liverpool, was billetted in the town but paraded and mounted guard in the fort, situated north of the town and on the banks of the river. The general guard room had been used, previous to the arrival of the 30th, as a place of confinement for deserters ; it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. *This water was now half evaporated and from the surface issued offensive exhalations.*

In a dark, narrow, and unventilated cell off the guard room it was usual to confine such men as were sent to the guard for misbehaviour, and about the beginning of June, 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours under the debility that succeeds intoxication. The typhus or jail fever made its appearance in two of these men about the first of the month and spread with great rapidity. Ten of the soldiers labouring under this epidemic were received into the Liverpool Infirmary...... The symptoms of the fever were very uniform, in every case there was more or less cough with mucous expectoration ; in all those who had sustained the disease eight days and upwards there were petechiæ on the skin, in several there were occasional bleedings from the nose and streaks of blood in the expectoration. The debility was considerable from the first...... Great pain in the head with stupor pervaded the whole, and in several instances there occurred a considerable degree of low delirium...... Our next care was to stop the progress of the infection ; with this view the guard house was at first attempted to be purified by washing and ventilation ; the greatest part of its furniture having been burnt or thrown into the sea. All our precautions and exertions of this kind were, however, found to be ineffectual. The weather was at this time wet, and extremely cold for the season ; the men on guard could not be prevailed on to remain in the open air and from passing the night in the infected guard room, several of the privates of the successive reliefs on the 10th, 11th, and 12th of the month, caught the infection...... No means having been found effectual for the purification of the guard room it was shut up, and a temporary shed erected it its stead. Still the contagion proceeded on the morning of the 13th, three more having been added to the list of the infected. On that day, therefore, the whole regiment was drawn up at my request, and the men examined in their ranks : seventeen were found with symptoms of fever upon them. It was not difficult to distinguish them as they stood by their fellows. Their countenances were languid, their whole appearance dejected, and the admata of their eyes had a dull red suffusion. These men were carefully sepa-

* Dublin Medical Journal, September, 1836.

† Medico-Chirurgical Review, Oct. 1836.

rated from the rest of the corps and immediately subjected to the cold affusion. . . . . These means were successful in arresting the epidemic—after the 13th of June no person was attacked by it."

It may seem presumptuous to offer an opinion differing from that expressed by the distinguished writer under whose observation these cases occurred, but we think there is every reason to question the existence of infection, and to regard them as of purely endemial origin. Let us consider them with reference to the circumstances unfavourable to the existence of contagion before enumerated.

1. *The class of persons affected.* British soldiers in time of peace are not obnoxious to contagious fever. The fact is stated by Dr. Cheyne, that while fever of this kind prevailed in the street contiguous to the principal barrack in Dublin, in 1817, and among a class of persons with whom the soldiers commonly associate, they escaped, because "little under the influence of the predisposing causes of fever; for the pay of the soldier is ample, he is well clothed, well fed, well lodged, and well looked after, and all his wants in health as well as in sickness are provided for." *

2. *The season of its occurrence* is another strong reason for considering this fever of an endemial kind. A contagious epidemic may live out the summer, but unless it is imported we should doubt its being generated at that season.

3. *The locality* was also unfavourable. Isolated as it was, an imported contagion was unlikely.

4. *The inefficacy of all the preventive measures,* short of removal from the locality, with the immediate cessation of the disease which followed this step, are strongly opposed to the idea of infection. In fact if it be admitted that the stage of maturation or crisis is the period of infection, an examination of the dates of these cases will shew that in no one was the disease so far advanced as to have enabled the patient to communicate it to his comrades, supposing them (which is not at all probable) to have had access to the hospital. On the other hand the positive evidence in favour of malaria is clear and decisive. Several individuals were exposed to this source during the debility which succeeds intoxication, and slept in its immediate neighbourhood. They were attacked, and others in succession as they became exposed to the same source. The malarious spot is abandoned on the 12th, and no case is observed after the 13th. Hereafter we shall attempt to shew that the symptoms of these cases were such as characterise not typhus but typhoid fever—especially the late appearance of petechiæ, the exudations of blood from the air-passages, and the form of disturbance of the sensorial functions.

The above are a few of the instances which might be brought forward to prove the occurrence of fever in situations and circumstances unfavourable to contagion, and not liable to the objection that *filth* has existed merely in fortuitous connexion with *fever and poverty*. It would not be difficult to draw from the published histories of infectious fever (so called) such a number of similar facts, as would render doubtful the justice of Christison's objection, that "as for the few instances remaining, where true primary fever appears to originate in one of the above causes, all that need be said farther is that for one instance where such fever follows such cause, a thousand instances occur where no effect of the kind ensues, and that, consequently, some more essential influence is probably brought into play, than what appears merely on the surface of the investigation." But some argue that the disease produced is not fever—for, first, it does not diffuse itself as fever does by infection. This is not the place to enter upon an examination of the conflicting statements upon this question—we shall do so hereafter; but admitting that it has been asserted too hastily by Dr. S. Smith, and others, that *infectious fever* is generated by paludal sources, we

* Dublin Hospital Reports. Vol. 2.

deny that this justifies the inference sought to be deduced, that, therefore, *continued fever* is not so generated. On the contrary, it seems more consonant with reason to infer, that if fever affects a number of individuals in a certain locality without appearing to be communicated from one individual to another, and without, in any instance, being carried from that locality, this fever must arise from some local source common to all the affected persons. And if that party are in extreme, who hold that fever of a contagious specific character is daily generated by common causes external to the human body, equally so are the opposite party who deny to these sources the power to cause fever, "not typhoid," while at the same time they are ready to admit the identity of their own "specific contagion" with a disease which, the most eminent observers maintain, is never contagious! It is surely more consistent with the doctrine of the speciality of typhus to let it stand alone, and to give a place to non-contagious continued fever, than to exclude the latter by a doubtful assimilation of typhus and typhoid fevers. Hereafter we shall attempt to shew that the most recent science is in accordance with the practical observation of Grant, that "these fevers, 'typhus,' are generally contagious which the common fevers are not, unless their nature is altered, and they are rendered malignant *by bad treatment*"—while we may see reason in the present state of society in our large cities, in the widely prevailing influence of crowding, poverty, non-ventilation, &c., and the consequently frequent and facile transition of *common* into *contagious* fever, why the most opposite conclusions are formed as to their origin and diffusion, and why it happens, as Christison truly remarks, that "the greater proportion of the discrepant doctrines of the present day as to the origin of fever are founded essentially upon the same great body of facts."

Again, by some it is urged that the disease produced by paludal emanations differs from continued fever in symptoms and in type. Thus Dr. Christison alleges that "few inquirers have taken sufficient pains to distinguish primary continued fever from irritative gastric fever." This objection cannot be allowed to have much weight so long as the *primary* nature of typhoid fever is a matter of dispute. Upon the subject of the type Dr. Christison may be quoted against his party, for if, as he asserts, "The coast remittent fever of Africa and other tropical countries seems to differ little in its characters from synochus, with a rapid and early stage of typhoid depression," * what becomes of the argument against the malarial origin of continued fevers from alleged differences in the nature of these and the intermittent and remittent fevers, also produced by malaria? Besides how can the exclusive contagionist answer the anti-contagionist who rests his doctrine on such facts as those adduced by Armstrong: "Shortly after I had published my 3rd edition on typhus fever, in which I had strenuously maintained the doctrine of human contagion, I met with a case of intermittent fever; in a few days the fever became remittent, and in a few days more put on the continued character, and the patient died with all the most malignant symptoms?" † or how will he dispose of the assertion of Dr. Elliotson, that most cases of so called typhus fever are really remittent, ‡ or explain

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* Library of Medicine. † Lectures by Rix.

‡ Lectures by Rogers. Dr. Mateer also observes—"We have the paroxysms of which fever is made up best seen in the intermittent and remittent fevers, but still by careful observation we can detect something of the same kind, though masked and often difficult to recognize, in the continued fevers of this country. These almost always assume more or less of the remittent character."—*Dublin Journal, ut supra.*

Dr. Currie remarks, whoever has watched the progress of fever must have observed the justness of the observations made by Cullen, Vogel, De Haen, and others, that even those genera which are denominated continued are not strictly the occurrence (already noticed) described by Marsh and Addison of well-defined ague, following on the subsidence of continued typhus or synochus? Was the ague also the effect of contagion? Or will the contagionist escape from the necessity of adopting so easy a solution of the difficulty as the supposition of different morbid poisons, generated at different periods in the same locality by a simple denial of the fact, and an impeachment of the accuracy of the observers who have recorded it? "In Sydenham's time," says Dr. Hancock, "and even in that of Fothergill, the quotidian of spring became continued fever in summer, while the simple continued fever of summer often changed to a malignant type in autumn. These were simple observations at a time when systematic arrangements had not put physicians in trammels. But now lest we should be guilty of medical heresy we must not insinuate that ague can change into continued fever, and non-contagious fever into contagious typhus, either in an individual case or in the course of the year."

SECT. III.—*Varieties of the Sources and Modes of Application of the Poison.*

The organic matter constituting the source of the morbid poison may be purely animal, vegetable, or a mixture of both.

It must be admitted that fever seems to be very rarely produced by exposure to purely animal exhalations, and the facts brought forward by Bancroft, Chisholm, Duchatelet, and others, shew that in the great majority of cases this exposure has been continued for any length of time with perfect impunity, but still there have occurred well authenticated instances to the contrary, some of which have been referred to; and a fact lately published, by M. Devergie, deserves farther notice. It is the occurrence of hospital gangrene in the hospital of St. Louis, which he attributes to the emanations from Montfauçon, since the disease was confined to the wards which were exposed to those emanations, and did not appear in other parts of the building.

Now if we admit the inference which seems naturally to follow from such instances as those related by Pringle, Hennen, &c., of the occurrence of typhus in the unwounded in wards, in which hospital gangrene existed, and of typhus attacking the attendants employed in washing the bandages of the same—namely, that hospital gangrene is a modification, or as it has been expressed, "a visible personification" of the typhus poison; we cannot avoid the admission that a fever poison may be generated by decomposing animal matter under certain conditions.

What the conditions required for this result may be, and why it so seldom happens that fever is thus produced, are questions to be resolved by deeper and more accurate investigations than appear yet to have been made.

There seems to be a more general belief in the activity of the vegetable poison, though why it might be difficult to say, unless from juxta-position with the known fact of their power to cause periodic fevers, since there is at least an equal paucity of strict evidence with regard to this as to the animal source. About 15 years since I witnessed the origin of a highly typhoid petechial fever in a healthy village in England, which appeared to arise from a vegetable source

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such, but have pretty regular and distinct exacerbations and remissions in each diurnal period.—*Med. Reports,* p. 16.

And Dr. Fordyce, says the similarity between these three kinds has determined practitioners of the greatest eminence, through the whole history of medicine, to consider them as the same disease. Many have thought that in a continued fever the subsequent paroxysm takes place in the hot fit of the prior paroxysm, &c.—(*3rd Dissertation,* p. 59.)

a heap of putrefying turnips. In a house close to the nuisance, a boy had for two or three weeks been complaining of headache, lassitude, and debility, but had not been placed under any medical care. On the day on which I saw him he had been attacked with epistaxis which continued till his death, on the day following. His skin was covered with small ecchymotic petechiæ. After his death petechial fever appeared in the family, consisting of six persons, and in the adjoining houses, and proved fatal in several instances. It did not spread beyond the locality and subsided in a few weeks. The season of the year (summer), with the other circumstances, were unfavourable to the supposition of contagion.

But it is to sources containing *mixed* organic matters that the experience of all observers point as most efficient in producing continued fever; such are slaughterhouses, obstructed sewers, cesspools, &c. &c. It were needless to add to the details already given of cases originating in these sources.

The modes in which the poison may be applied to the organism are by direct introduction into the circulation, by being taken into the stomach, by inhalation, and by the skin.

Fever, or a disease confessedly bearing a close resemblance to it, has been produced in the lower animals by experiments too well known to need us to dwell upon them, and occasionally the typhoid symptoms which appear in other diseases would seem to be owing to the absorption of putrid matters into the circulation. A case of the kind once occurred under my own observation: a boy aged ten years was received into hospital, labouring apparently under typhoid fever, he sunk after a few days, and on dissection the only lesion discoverable was a carious state of the petrous bone, with a minute opening communicating with the lateral sinus, through which the matter of the carious abscess had passed into the circulation.

It is but rarely that an instance occurs of fever produced by putrid matters taken into the stomach, and the immunity enjoyed by savages, who live much upon putrid flesh, &c., has been referred to by Bancroft and others, not only against the fact, but also against the supposition of putrid animal matters containing a fever poison. It is not difficult to understand that this should be the case, since digestion, an antiseptic process, precedes assimilation, and changes remarkably the matters submitted to its operation; or, as accurately expressed by Liebig, putrid poisons having an *alkaline* reaction are rendered inert by the acids contained in the stomach, while these exert no such power over poisonous sausages which have an *acid* reaction. But this rule is not without some exceptions. In Dr. Christison's work on poisons is a report of a case which occurred in Stockport in the year 1830, of a family of five persons who were poisoned with broth made of putrid beef; in three instances the disease produced was severe, and in one fatal. It is worthy of remark (and is in accordance with the mode of action of a *morbid poison)* that in the worst cases the illness did not commence till the second, and (in the fatal case) the third day after the meal. A case is somewhere narrated of a regiment in which putrid fever prevailed, and in which the disease was checked upon a discovery being made that the water used for drinking was drawn from a well in which some bodies were lying in a state of putrefaction.

Dr. Copland* remarks upon the effects of drinking the water of the Seine at Paris, and Dr. Hancock observes, that it has been frequently remarked that this water produces diarrhœa in every one except the Parisian accustomed to the use of it. † Dr. Tweedie refers to the history of a fever ascribed to the combined effect of drinking putrid water and the emanations from the same—and others ascribe

* Dictionary of Practical Medicine, art. Endemic Influences.

† Cyclopædia of Practical Medicine, art. Endemic Diseases.

the putrid fevers of Paris to the fact that "there are numerous wells in that city from which many of the inhabitants derive their whole supply of water, not a few of which are situated in the very neighbourhood where the 'fosses' are the worst constructed and the least attended to; the urine, therefore, permeating the soil must necessarily contaminate the springs from which these wells are fed."*

The evidence given before the committee on the health of towns, by Mr. J. B. Wood, bears upon this question. After stating (qs. 2150-4) that 31,000 persons live in the cellars of Liverpool—forming two-thirds of the working population—he states that, "in the districts in which these cellars are situated, there is a great deal of broken ground in which there are pits; the water accumulates in these pits, and of course at the fall of the year there is a good deal of water in them, in which there have been thrown dead dogs and cats and a great many offensive articles. This water *is nevertheless used for culinary purposes.* I could not believe this at first, I thought it was only used for washing, but I found it was used by the poorer inhabitants for culinary purposes."

The change produced in the fluids and solids of an animal by over driving seems to be capable of becoming a cause of disease in the human body. In the remarkable case given by Andral from Du Hamel it does not appear that fever, strictly speaking, was produced. The effect rather resembled hospital gangrene; but an instance is recorded of typhous fever following from eating the flesh of animals under similar circumstances. It is thus quoted by Dr. Gross: † "A few years ago a number of fattened cattle were driven into one of the New England cities, and having been pressed too hard in a sultry day were so overheated that some of them became quite exhausted. In this condition they were slaughtered, and the consequence was, as is stated by the reporter of the case, Dr. Fountain, that nearly all who partook of their flesh were seized with typhous fever."

These and similar observations would seem to shew that the morbid poison, the product of putrefactive decomposition, may be received into the system through the stomach, more especially if presented in the fluid form; but there is every reason to conclude that fever is but seldom produced in this way, and that the general mode of introduction is through the respiration of the gaseous exhalations, which, as we have seen, are found upon their being collected and condensed to contain animalized matter in the state of progress to decay, whose power of producing disease, in those exposed to their influence, has been questioned and is by many denied, but appears to be proved by evidence of a very satisfactory nature, and which applies with most force to cases occurring under circumstances unfavourable to the action of contagion.

SECT. IV.—*On the Mode of Action of the Poison.*

This does not appear to be attributable to its chemical qualities, but to its *condition.* It is the power of communicating an action, since there can be no doubt that its effects continue to be produced equally after the removal as during the presence of the cause, when once this cause has impressed its mode of action upon the organism. What the mode of this impression is, and what part of the system is the subject of it in the first instance, we have now to enquire, as also into the order of the phenomena subsequently produced and constituting the formed disease.

The generally received opinion seems to be that the nervous system is the

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* Medico-Chirurg. Review, vol. 6.
† Pathological Anatomy, vol. 1, p. 223.

subject of the first morbific impression, and its derangements the first in the morbid series constituting fever.

This doctrine is thus maintained by Dr. Southwood Smith*—"The immediate exciting cause of fever is a poison which operates primarily and specifically upon the brain and spinal chord. The diseased state into which these organs are brought by the operation of the poison, deprives them of the power of communicating to the system that supply of stimulus, (nervous and sensorial influence), which is requisite to maintain the functions of the economy in the state of health. The organs, the seats of the functions, deprived of their supply of nervous influence, become deranged, the derangement in each taking place in a fixed order and in a determinate manner.

Subsequently to the nervous and the sensorial, the organs the next to suffer are those of the circulation, then those of respiration, and, ultimately, those which belong to secretion and excretion. The condition of the nervous system, which produces this derangement in this circle of organs, occasions further, in that portion of the circulating system which consists of the capillary blood-vessels, that peculiar state which constitutes inflammation : hence inflammation is almost always established in one or more of the organs comprehended in the febrile circle and sometimes in all of them." In another passage the same writer says—"The more closely and extensively the subject is investigated the more clear and satisfactory the evidence becomes, that the great primary cause of fever is a poison, the operation of which, like that of some other poisons, the nature of which is better understood and the action of which has been more completely examined, is ascertained to be upon the nervous system. How these poisons act upon the nervous system we do not know, nor can we possibly know as long as we remain so profoundly ignorant of the nature of the action of the nervous system in the state of health."

It will be seen that Dr. Smith's argument, like that of others already examined, is rested upon a fallacious analogy to certain other poisons, and like these is open to the fatal objection, that such poisons really produce their effect upon the nervous system subsequently to their diffusion through the blood. It moreover lacks the support derived in the former cases from the occurrence of a nervous shock, since such rarely, if ever, attends exposure to the paludal sources of fever. On the other hand, some of the arguments for a modified humoral theory before adduced, apply with greater force to this than the animal miasm, and additional ones are not wanting to strengthen the proofs that all derangements of function in fever are subsequent to the introduction of the poison into the blood, and the consequent vitiation of that fluid. For, its latency is even more remarkable than that of the animal infection of typhus, while a close observation of the state of the patient, during this period, will shew a derangement of the secreting and excreting functions, which seem to be labouring to rid the system of the poison, or of those products which it has a tendency to generate in the blood ; again, we can not unfrequently trace the abrupt termination of this period, and the supervention of formed disease to a suspension of the depuratory action, by cold, intemperance, or any other cause which disturbs the order of the excreting functions and arrests the elimination of the morbid product. Thus, in an exquisitely marked case of paludal fever, which was lately under my care and which terminated fatally after large evacuations of blood from the bowels, it was remarked spontaneously by the patient's friends that, for six weeks before fever commenced, not only had he suffered from capricious appetite and irregular bowels, but that a remarkable thick copious deposit had been constantly present in his urine. During this period he had been living and sleeping in immediate proximity to a collection of filth,

* Treatise on Fever.

which at times filled the house (in other respects a cleanly and comfortable one) with an insufferable putrid effluvium.

This instance also illustrates the cumulative property of the poison, which is much more remarkable than that of the animal infection. Unlike the latter, which is often most severe upon its first introduction into a family, the poison of malaria seems commonly to affect each successive patient more severely than the preceding.

To what can this be attributed but to the accumulation in the blood consequent upon longer continued imbibition of the poison? This is similar to the explanation offered by Cruveilhier, of the general incurability of phlebitis from the absorption of pus. Experiments have shewn that, from a single injection of putrid pus into the vein, an animal may, after copious evacuations, recover; he therefore concludes that similar success might attend the evacuant treatment of phlebitis, did not the renewal of the sources of infection follow the incessant renewal of the pus. But we can not only detect the presence and agency of the poison in the circulation through the deranged excretions, but also occasionally in the physical changes of the blood itself previous to the occurrence of formed disease. The following observations of Dr. Potter* on this subject, are highly interesting and important, and from the care and accuracy with which they appear to have been made, are entitled to great weight in the discussion of the mode of access of fevers arising from malaria. He says, "it was remarkable in all cases in which it was deemed expedient to bleed, the blood wore the same general appearances. After a separation had taken place, the serum assumed a yellow shade: often a deep orange, and a portion of the red globules was invariably precipitated.

It occurred to me that if the remote cause resided in a common atmosphere, the blood of all who had inhaled it a certain time would exhibit similar phenomena. It accorded with the pathology I had conceived, to conclude, that all who lived in an atmosphere so impregnated were constantly predisposed, and that an additional or exciting cause only would be required to develope the symptoms and form. To ascertain the appearances of the blood in subjects apparently in good health, I drew it from five persons who had lived during the whole season in the most infected parts of the city, and who were, to every external appearance and inward feeling, in perfect health. The appearance of the blood could not be distinguished from that of those who laboured under the most inveterate grades of the disease. As this experiment might have been considered inconclusive unless the blood could be compared with that of those who lived in a purer atmosphere, remote from the evolution of miasmata, I selected an equal number of persons who lived on the hills of Baltimore County, and drew from them ten ounces of blood. The contrast in the appearances was so manifest that no cause for hesitation remained. There was neither a preternaturally yellow serum, nor a red precipitate; the appearances were such as we find in the blood of healthy subjects. A young gentleman, having returned from the western part of Pennsylvania on the 10th of September, I drew a few ounces of blood from a vein on that day; it discovered no deviation from that of other healthy persons. He remained in my family till the 26th of the month, and on that day I repeated the bloodletting. The serum had assumed a deep yellow hue, and a copious precipitate of red globules had fallen to the bottom of the

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* Quoted by Dr. Tweedie, Art. Fever, Cyclopædia of Practical Medicine.

Dr. Tweedie's own expressed opinion is "whether the opinion of the older writers, that in fevers originating from contagion, the contagious principle alters the properties of the blood be correct or not, we certainly think the strong analogy in the cases alluded to tends to confirm the supposition of typhoid fevers originating in diseased blood."

receiving vessel. Of the six persons whose blood assumed those indications of the remote cause, four were seized with fever during the epidemic; the other two escaped any formal attack, but complained occasionally of headache, nausea, and other indications of disease."

Similar experiments were instituted by Dr. Stevens, and with a like result, the blood being black in colour, and evidently deranged in its properties previous to the commencement of the fever.

It might almost be considered unnecessary to strengthen the inference from such facts as these, by observing on the effect upon the blood of the gases, evolved from sources which we consider the evidence already adduced proves to contain a fever poison, when these are presented in a sufficient degree of concentration to produce rapidly fatal effects.

Describing the consequences of exposure to the emanations from Parisian privies, Dr. Christison says :—"The appearances in the bodies of persons killed by these emanations are *fluidity and blackness of the blood,* a dark tint of all the internal vascular organs, annihilation of the contractility of the muscles, more or less redness of the bronchial tubes and *secretion of brown mucus there as well as in the nostrils,* gorging of the lungs, an odour throughout the whole viscera, like that of decayed fish, and a tendency to early putrefaction."

With these and similar facts before us, we cannot agree in the sweeping decision of Dr. Smith, that "changes in the fluids can only be second in the series of morbid events: they can never hold the first place in that series: they can never be antecedents or first causes, but merely sequents or effects."* We rather think that the evidence existing on the subject, if fairly examined, points to the blood as the seat of the primary operations of the morbific agent, and the subject of the changes which it is a part *of* its condition necessarily to produce. And regarding all derangements of the functions occurring in fever as the consequences of the molecular changes in that fluid, we proceed to examine into the order in which these consecutive phenomena occur—the mode of their production, and their mutual dependence one upon another.

Supposing Dr. Smith's to be a fair exposition of the views generally entertained in this country we find, upon reverting to his chapter on the theory of fever, that the doctrine maintained is, that subsequently to the supposed primary nervous impression, "the organs the next to suffer are those of the circulation, then those of respiration; and ultimately those which belong to secretion and excretion. The condition of the nervous system which produces this derangement in this circle of organs, occasions further in that portion of the circulating system, which consists of the capillary bloodvessels, that peculiar state which constitutes inflammation: hence inflammation is almost always established in one or more of the organs comprehended in the febrile circle and sometimes in all of them." If this passage admits of a precise construction it must be that in consequence of a certain impression on the nervous system a state of general inflammation exists (or at least a state approaching to inflammation) in the capillaries of all the organs, and which is equally likely to become actual inflammation in any of them during fever. Without denying this frequency of visceral inflammation in fever, or the great necessity of recognising and combatting it, it may be reasonably doubted if so variable and non-essential an occurrence—with one exception—or one so dependent for its existence and its seat upon accidental causes—as season, atmosphere, epidemic influences, states of constitution, &c.—as these local inflammations are, can be properly admitted into the discussion of a theory of fever. Either this combination of inflammation in some organ, with a peculiar state of the nervous system, is necessary to constitute fever, or it is not. That it is, seems unlikely, since morbid anatomy fails to detect it in a large pro-

* On Fever, p. 330.

portion of instances. If it be not then according to this theory, nothing will remain but a certain peculiar affection of the nervous system to account for the phenomena of fever. But in the kind of fever under consideration there is a local affection generally regarded as inflammatory, and which is so constantly present and found to exercise so great an influence on the disease as to have been considered by some eminent pathologists to be the essential cause of typhoid fever. It is that affection of the mucous glands of the small intestines described under the name of Dothin-enteritis. This affection claims a special consideration, since no one can impartially examine the evidence put forward in support of their views of the pathology of fever by Louis and Chomel, in France, or the cases incidentally published in the writings of Bright, Tweedie, Smith, Graves and Stokes, Hodgkin, &c. in this country, without being convinced that in some forms of fever—which farther examination will shew to be paludal fever—this dothin-enterite is a constant and a most important complication, if indeed it be not the pathological cause of the disease.

But if we seek an explanation of its occurrence in Dr. Smith's theory, we are first at a loss to know why an impression on the brain and spinal chord should lead to consequent inflammation in a part the most remote of any from their influence, and whose functions are under the control of a different portion of the nervous system. Then, in very many cases, we find that the symptoms of intestinal irritation preceded this nervous impression—that in others, in which death took place at an early period of fever, it was found far advanced—as by Louis so early as the 8th day—and could not therefore be regarded as a secondary phenomenon. And that in other cases, in which the poison was presented in so concentrated a form that death took place before fever could be established, the glands of Peyer exhibited the same appearance as in that disease. The following interesting case of this kind is given by Dr. Christison.

In *August* last, twenty-two boys, living at a boarding-school at Clapham, were seized in the course of three or four hours with alarming symptoms of violent irritation in the stomach and bowels, subsultus of the muscles of the arms, and excessive prostration of strength. Another had been similarly attacked three days before. This child died in twenty-five, and one of the others in twenty-three hours. On examination after death, the Peyerian glands of the intestines were found in the former case enlarged, and, as it were, tuberculated: in the other, there were also ulcers of the mucous coat of the small intestines, and softening of that coat in the colon. A suspicion of poisoning having naturally arisen, the various utensils and articles of food used by the family were examined, but without success. And the only circumstance which appeared to explain the accident was, that two days before the first child took ill, a foul cesspool had been opened, and the materials diffused over a garden adjoining the childrens' play-ground. This was considered a sufficient cause of the disease, by Dr. Spurgin and Messrs. Angus and Saunders of Clapham, as well as by Drs. Latham and Chambers and Mr. Pearson, of London, who personally examined the whole particulars." There cannot, we think, be a doubt that their opinion was correct, and that nothing but the rapid termination prevented the development of the phenomena of fever in these cases; but in fairness to Dr. Christison, it should be added, that he considers "this opinion cannot be received with confidence by the medical jurist and the physician, since it is not supported by any previous account of the effects of sulphuretted hydrogen." Perhaps these cases may receive some confirmation from the following report (certainly not a full one) of a similar accident by Dr. Arnott.* "In a mews behind Bedford-square, a stable had been let for a time to a butcher, and a heap of dung had been formed at the door, containing pigs' offal, pigeons' dung, &c. During the act of

* Fourth Report of Poor Law Commissioners, p 106.

removing this heap, a coachman's wife and her three children, of an adjoining stable, sat for a time at an open window nearly over the place until the insufferable stench drove them away: two of the poor children died of the poison before 36 hours, and the mother and other child narrowly escaped."

In Dr. Christison's cases the description of the appearances of Peyer's glands exactly corresponds (especially the first case) with Dr. Bright's description, and with the representation given in one of the plates in his great work. As also with the minute investigations of Dr. Staberoh, who regards the first stage of the follicular affection, as "an infiltration into the mucous coat, and especially the crypts called Peyerian glands," but also taking place, as he has repeatedly seen it, in different parts of the colon, "and to which he considers the inflammation of the mucous membrane secondary."—(Dublin Journal, Vol. 13.)

But farther—if dothin-enterite were a consequence of disordered circulation depending upon an impression on the brain and spinal chord, we might expect to meet it in other cases in which these organs are engaged, as in the periodic fevers and in typhus: but we do not, nor can any other local inflammation be named as similarly constant in these diseases, and filling its place in the "febrile circle." On the contrary, M'Cartney, Armstrong, and others, have fully proved that the vascular congestion commonly found in these diseases is not of an inflammatory nature, and that, though it may remotely give rise in certain cases to an inflammatory re-action for its removal, it is yet a distinct pathological condition.*

Two other opinions may be entertained of the relation of dothin-enteritis to fever. One, that it is the primary cause: that fever is the sum of the symptoms of this inflammation—the other, that it is the specific effect of the septic poison from which typhoid fever originates, and like the other symptoms of this disease, merely a link in the chain of sequences constituting fever.

Perhaps the strongest arguments for the first of these opinions are the large proportion of cases of typhoid fever in which it is found to exist,—the influence it exercises upon the severity of the disease, and the effects of antiphlogistic remedies, more especially of topical blood-letting. It will be presently seen that the two first circumstances are equally well explained upon the second opinion. With reference to the effects of blood-letting it must be admitted that a very considerable amelioration of symptoms, and not unfrequently their total removal, has followed timely and free abstraction of blood, especially by leeches applied over the affected intestine, and not only are the tenderness and pain in the part, with the meteorism and diarrhœa thus relieved, but the head-ache, thirst, pulse, and other general symptoms commonly undergo at least a temporary and partial improvement. But it may be doubted whether this is to be attributed so much to the removal of inflammation as to an impression made upon the general disease, by the new movement given to the circulation in general by the smallest local abstraction of blood, and which is felt in every part of the system.

One or two circumstances may be cited in proof of this dynamic effect of bleeding, and to illustrate its application to the present case—1st, the well-known fact that the impression made upon the central organ of the circulation by the bleeding from a few leech-bites is totally disproportionate to the quantity taken away. 2nd. The effect of leeching, or bleeding in some other disorders in which no supposition of inflammation could exist. In amenorrhœa, for instance, we have known the disorder of several months' standing removed by the application of a few leeches to the inguinal region before the leeches were themselves removed. In ague also, an effect almost equally marked may be sometimes produced by the same means. The following short case illustrates this. C. F——, æt. 20, was admitted into the Navan Hospital on the 22nd of February, labour-

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* For Dr. M'Cartney's observations, see Dub. Med. Trans. Vol. 2, p. 574.

ing under tertian ague. The fit comes on two hours earlier at each period. Has some tenderness on pressure, and fulness of epigastrium, thirst, tongue red at tip and edges—previous to entering hospital took two emetics and an aperient. A fit took place about 3 a. m. on the 21st, and might be expected to recur at one a. m. on the 23rd. I ordered 12 leeches to be applied on the evening of the 22nd to the epigastrium, and a draught to be taken at bed-time, containing 20 drops of laudanum. The fit occurred at 5 a. m. 4 hours later than it was expected. On the 25th it came on at 7, and the leeching being repeated on the evening of the 26th, he sweated copiously during the night, and had no return of the fit. There was a slight return of it five days after, but from this time he got rapidly well.

Without entering farther into the discussion of the theory, that dothinenteritis is *the cause* of the febrile phenomena, we pass on to submit certain considerations in support of the view which regards it as an effect not *of* the fever or of the state of the nervous and circulating systems produced by the fever, but as the direct effect of the poison itself—as one (probably the first) of the links in the chain of sequences, constituting fever, and one upon the occurrence of which some of the others may probably depend. This view approaches nearer to that of Louis, who is of opinion that the affection of the follicles occurs in the beginning of the disease, than to that of Chomel, who seems inclined to admit its classification among the secondary inflammations, but differs from the definition of the former eminent pathologist, inasmuch as it seeks to establish the agency of a morbid poison as *the cause of fever*, in place of his decision "that *the cause is unknown*." The following extract from his summary of the diagnostic symptoms of typhoid fever is important, as containing two particulars which we shall find to have a bearing upon this inquiry.

"Maladie aiguë accompagnée d'un mouvement fébrile plus ou moins intense, variable dans sa durée; propre aux jeunes sujets, *principalement à ceux qui se trouvent depuis peu de temps au mileau de circonstances nouvelles pour eux*, dont la cause est inconnue; debutant par un frisson violent, l'anorexia, la soif, et dans la très grande majorité des cas *par des coliques et la diarrhée*," &c.

The two circumstances here mentioned by Louis, of the subjects of typhoid fever being those newly exposed to influences, the nature of which he concludes are not known—and the diarrhœa which ushers in the complaint,—we conceive tend to support the theory that the intestinal affection is a consequence of the effort made by the excreting organs—more especially the liver—to rid the system of the poison which has been introduced into the blood. It is easy to conceive that the native of Paris, born and brought up in the atmosphere of its fosses, and drinking all his life the tainted waters of its wells and river, may habitually eliminate from the blood such products as are thus taken in unfit for assimilation or nutriment, and that to a constitution unused to such a task the consequence of taking into the circulation the same decomposing substances should be different. And, if we aid our conception of this fact by a reference to what takes place in the different classes of persons exposed to malaria of other kinds in temperate climates, we shall see why intestinal affections should be among the first consequences of the process; for it will appear that the liver is the organ by means of whose excretions the poison is attempted to be got rid of, and according to the facility with which this is performed or otherwise, will be the chances of escape or the contrary, from the effects of the poison. It is well known that, while the lungs of a native of a warm climate are liable to become diseased upon removal to a cold one, the liver is the organ prone to suffer upon the inhabitant of a cold removing to a warm climate. It is also found that the stranger from a colder country will rapidly contract fever from exposure to malaria in a temperate climate, while the person newly arrived from a warm one will not be similarly affected until that change in the order of his functions termed acclimatization has taken place, and he becomes assimilated in habits to the inhabitant of the same latitude.

(To be concluded in our next.)

The following are the data upon which the proof of the connexion of diseased mucous follicles with the peculiar effects of a morbid poison upon the biliary excretion, may be rested.

1st. It may be considered as admitted, that the special characters of substances fitted for assimilation are absence of active chemical qualities, and the capability of yielding to transformations; and that every substance may be considered as nutriment, which loses its former properties when acted on by the vital principle, and does not exercise a chemical action upon the living organ.

2ndly. That in the progress of the functions of nutrition, certain chemical and organic substances are produced, and from time to time are present in the blood; which products it is the office of the different excreting organs to discharge from that fluid,—the relative activity of these organs depending partly upon the matter to be eliminated, and partly on other circumstances; thus we have seen that, in one situation the lungs assume a disproportionate activity, in others, the liver, &c.

The foregoing propositions being admitted, the following may be regarded as convertible from them. If substances be introduced into the blood which are *not* capable of assimilation or of affording nutriment—whether from their chemical qualities or from their condition (of decomposition)—it will follow that, instead of these suffering the transformations which food undergoes to become assimilated, the blood will undergo their transformation and disease will be produced.

Also, that numerous modifications in the composition and condition of the compounds, produced from the elements of the blood, may be the immediate result of the introduction into it of these substances, and a change in the quality of the excretions may thus be the first indication of the action of the poison, as well as of the effort made to expel it.

Numerous facts and observations tend to shew that, in the case of organic or putrid poisons, the *liver* is the organ by whose excretions an attempt is made to rid the blood of the new products thus formed in it.

As first—by a reference to the experiments of injection of putrid pus, &c., into the veins of animals, performed by Magendie, Gaspard, Cruveilhier, &c., it will be seen that when the animal recovered it was after copious discharges of a vitiated character from the bowels; to these discharges the last-named writer attributes the recovery, and adds, that it is a fundamental fact of pathology that the intestinal canal is chiefly affected in diseases caused by miasmata.

Again, if we refer to the published cases of poisoning from putrid ingesta, we see that, besides those of irritant poisoning in which the rapid rejection of the substances was followed by recovery, there is another class in which, after an interval allowing of the absorption of the poison into the circulation, a different set of symptoms followed, as in the following from Dr. Christison's work on poisons:—"A family of five persons took for dinner broth made of beef, which owing to its black colour the master of the family had previously said to his wife he thought bad and unfit for use.

In the course of some hours two boys were attacked with sickness and vomiting, but appear to have got soon well, probably from the early discharge of the poison. Next morning a washer-woman, who had dined with the family, was seized with violent *pain in the bowels, diarrhœa, racking pains, and weakness in the limbs,* and did not recover for ten days. On the evening of the second day the master of the house was similarly affected and was ill for a fortnight. And a day later, his wife was also seized with a similar disorder, preceded by soreness of the throat and tongue and difficulty of swallowing, and ending fatally in fourteen days."

It is worthy of notice that the severity of these cases was in proportion to the interval allowed for absorption of the poison—altogether their resemblance to the description of the symptoms of typhoid fever, quoted from Louis, is remarkable. If we enquire why the mucous glands of the lower portion of the ileum, are more than other parts of the intestine liable to suffer from this peculiar derangement of the biliary excretion, we shall see reason to think the cause is the same as would explain their existence in greater number there than elsewhere, and that this is probably owing to the fact of a second digestion or chymification being performed in the cæcum, during which, it is believed by some physiologists,* that the entrance to the large intestine is closed, and bile collected in the lower portion of the small intestine, which does not enter the cæcum till the secondary chymification is completed. The effect of such a retention of an acrid and depraved secretion must be obviously to produce irritation in the part subjected to its influence, and the same deranged products of secretion, continuing through much of the duration of the fever, we can account for this affection not seeming to be limited to any portion of that period, but why commencing with it,—frequently even preceding it, it ordinarily survives the continuance of the most prolonged disease.

This explanation also accounts for the disease in these glands being found farthest advanced nearest to the termination of the intestine—for perforation occurring almost invariably close to the cæcum—for the lymphatic glands of the mesentery, corresponding to the diseased follicles, becoming diseased—and for the severity of these affections bearing a direct proportion to the severity of the fever, and, as it would appear, to the amount of the poison imbibed into the system.

This view of the relation of diseased follicles to the action of the septic poison differs both from that which regards dothinenteritis as the cause of fever, and from that which assigns to it a merely secondary place in this affection. To the latter are opposed the extremely frequent, and early occurrence of abdominal symptoms (as diarrhœa) in the typhoid form of fever. With the post mortem appearances in subjects examined at an early stage; while the former is irreconcileable with the occasional absence of the lesion, the frequent want of correspondence between its amount and the gravity and fatal result of the fever, (a correspondence which should exist if the other phenomena of fever were but the sympathies of the affection of the follicles,) with the occasional persistence of the local disease after the fever has subsided, and with the presence (almost equally frequent in typhoid fever) of other lesions which cannot be considered sympathetic of this, but must be ascribed either to the immediate operation of the poison, or to that state of the blood produced by it; such are the softening of the spleen, liver and heart, and the inflammatory affection of the brain and thoracic viscera.

These pathological changes, as well as the derangements of function constituting the *febrile state*, will probably be best explained by some such hypothesis as that advanced by Dr. Hodgkin, which supposes the febrile state to depend upon a suspension, or at least very considerable interruption of that process by which, during health, the various parts of the system are continually undergoing a change, the old materials being removed while others are substituted in their place."† This hypothesis will be found perfectly in accordance with that of the

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* Schultz quoted by Müller.

† Lectures on Mucous Membranes, Lect. 23.

Dr. Hodgkin's hypothesis seems to explain the great difference in the fatality of fever as affecting the higher and lower classes of society, since by the mode of life of the former, more nutriment being taken into the system, and more organic matters constantly present in the blood, an arrest of the process by which they are eliminated, must naturally be followed by more complete deterioration of the mass of circulating fluid, and more serious injury to the functions and structure of the organs supplied by it.

action of a morbid poison upon the blood, since it will be the natural effect of the molecular change produced in that fluid by the decomposing particles of the poison so to modify it as to render it unfit to undergo the capillary attractions constituting the processes of interstitial absorption, nutrition, and secretion; and thus instead of Dr. Smith's formula of the order of successive derangements in fever—namely, derangement of innervation—then of circulation, and lastly of secretions and of the animal fluids; the more correct one will probably be, first the molecular change in the blood, then the suspension or modification of the interstitial processes—or change of particles—then certain derangements of innervation and of the heart's action, and the result—formed fever.

According to this view, dothinenteritis is one of the phenomena of the second stage in the action of the poison, and immediately consequent upon certain modifications of the biliary (and probably also the intestinal) secretion. Its occurrence cannot be considered *essential* to typhoid fever, as the contamination of the blood may cause the molecular changes upon which the foregoing hypothesis supposes fever to depend without this—albeit its absence in typhoid fever if very rare—while, on the other hand, it may exist without fever necessarily following, for we frequently see that, of a number of individuals exposed to the same source of miasm, some will suffer an attack of typhoid fever, while others will be affected with diarrhœa or dysentery: a fact which is explained by a reference to those experiments of Gaspard, in which the recovery of the animal after putrid injection, was attended with profuse and offensive discharges—seemingly the mode of relieving the blood from the presence of the poison.

Reference has been already made to another set of cases, in which dothinenteritis occurs without fever: those namely, in which the poison was so concentrated as to produce a rapidly fatal effect, and where examination after a diarrhœa of only a day or two shewed the same peculiar affection of the follicles as in typhoid fever; the inference from such cases taken conjointly with those of fever from the same causes without dothinenteritis, must be that this lesion is *neither cause nor effect of the fever, but a concurrent and contingent effect of the poison.* Most of the other pathological changes of fever are to be explained by the alterations in the constitution of the blood. Such is evidently the cause of the softening of the spleen so invariably present in typhoid fever, and such a little consideration will shew to be the cause of the congestive character of the typhoid inflammations: for the occurrence most likely to follow such a change in the molecular attractions of the blood as will interfere with its capillary circulation, is *stagnation in this part of the system,* of which the consequences are venous congestion, passive hæmorrhages, and the softening of parenchymatous organs. The stagnant character of the typhoid pneumonia has been remarked by many; thus Dr. Williams says, "it may be almost a question whether in these cases the local disease in the lungs is not rather a congestion of blood in an altered state than an inflammation, and it is very commonly the sequel rather than the cause of the fever,"*—an opinion which seems fully warranted by dissection, as well as consistent. Huxham, with the modifications of the physical signs in this form of disease,† was so much struck with this connexion of the local affection and diseased blood, that he compared the state of the latter in these cases to the *scorbutic habit;* and Andral countenances this analogy in the following passages. "The ataxo-adynamic fever recognises for its commencement some alteration of the blood, whether this alteration may have taken place spontaneously, and produce a sort of acute scorbutus, or it may follow the introduction of deleterious agents, as miasms, virus, matters in a state of putrefaction; these agents after having modified the composition of

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* Article Pneumonia, Cyclop. Prac. Med.

† See my Observations on Typhoid Pneumonia, Dublin Journal, V. 7, for several dissections of this disease.

the blood come to poison the nervous centres. Then the disease is everywhere, where blood and nerves are to be found, and in every part lesions may occur which perform but a secondary part in the production of the symptoms.*

Again—"Congestions of the parenchymatous tissues and membranes are tolerably frequent during the course of fevers. These congestions seem to depend on the rupture of equilibrium between the globules and fibrine; they are very frequent in typhoid and typhus fevers, and small-pox; the spleen and other parenchymatous tissues are usually congested in these diseases, and a diminution of fibrine as compared with the globules (whether absolute or relative) is the alteration of the blood observed in these maladies.

"The ancients concluded from the phenomena just mentioned that in the diseases in question, the blood is altered, and that its elements have a great tendency to separate. They designated by the phrase *putridity*, that morbid condition in which the vital powers seem to yield to physical causes, and the blood becomes putrescent. Borden, whose opinions as to the nature of typhoid fever are remarkably sagacious and philosophical, does not hesitate to consider that malady as connected with a general condition of the system, which he designates by the name of *acute scurvy*. This phrase is not inaccurate, so far as regards the condition of the blood. A diminution of the quantity of the coagulable material of the blood is a general fact observed in all great febrile disturbances; thus in miasmatic fevers there is first absorption of the miasma, and immediately after, the only prominent phenomenon is an alteration of the blood. This alteration which occurs in typhoid fever is the effect of some cause as yet unknown."†

Hæmorrhage is well known to be characteristic of typhoid fever. That from the air-passages is enumerated by Chomel among the distinctive symptoms of the disease. Intestinal hæmorrhage is also a frequent and an unfavourable symptom, and indicative of a diseased state of the blood. This hæmorrhage is preceded by stagnation. The softening of viscera is always observed in conjunction with an altered and fluid state of the blood. In some descriptions of softened spleen by Andral and others, the blood contained in it is compared to the lees of wine.

SECT. V.—*Characters of the Disease produced by the Putrid Miasm.*

It has been remarked, that a general resemblance may be traced between the disease produced by the infectious animal poison of typhus and the exanthemata; its first and most striking analogy being the almost constant presence of a peculiar eruption. Another particular in which it resembles them and differs from the disease now under consideration is the absence of any constant internal lesion; the pathology of typhus being of a functional or physiological kind, while that of endemic or typhoid fever is anatomical and precise in its nature. The distinctive characters of the two affections may be thus stated. In typhus, a poison is generated by certain changes in the fluids of the living body, which, being received into the blood of a healthy individual, has a tendency to excite in that fluid the transformations from which it has itself arisen, and by which it will be reproduced: a process during which certain phenomena occur, as that of

* Clinique Medicale Translated, p. 610. Several cases are given, in which all the symptoms of typhoid fever were produced apparently by mental and bodily depression, but after death no lesion was discovered—for a striking case of this acute scorbutus, see Dr. Law's paper before quoted at p. 19.

† Lectures on the Blood, reported in Dublin Medical Press, Aug. 11, and Provincial Medical and Surgical Journal, Aug. 21.

eruption (an effort apparently to free the system from the presence of the poison), and the conclusion of which is marked by the presence in the excretions of the material necessary for the generation anew of the disease in any person into whose blood it may be received. All these phenomena may occur without appreciable change in the structure of any organ, and in fact death may be produced without any morbid appearance beyond that degree of congestion naturally connected with the modification of its processes of nutrition and secretion.

In typhoid fever the events following the introduction of the putrefactive poison are different; it will appear upon examination into these, that no new material of reproduction is generated, that the eruption is not a true exanthema or identical with that of typhus, being later in its appearance, less constant, more scanty, consisting of successive crops rather than persistent and uniform, as in typhus. A marked modification of the molecular changes of the system occurs in this as in every variety of febrile movement, but its continuance is evidently less uniform than in typhus, being subject to alternations and remissions, at times approaching those of the periodic fevers; and the critical change attending its resolution is more gradual and liable to be less certain and complete, as well as to recur by relapse, unlike that of typhus. But the most important distinction consists in the fact that the typhoid miasm has, like other putrid poisons, a tendency to be eliminated from the system through the biliary excretion, in the course of which process a peculiar form of irritation is set up in the alimentary canal, while no such tendency can be asserted of the poison of true typhus, in the majority of cases of which the biliary excretion suffers rather a diminution than otherwise.

But it may be said it is by no means proved that the typhoid affection of Louis and Chomel is of endemial origin, and in order to establish the connexion between miasm as a cause, and fever characterised by dothinenteritis as the effect, either this must be proved, or it must be made to appear that the fevers which in our own country may be traced to this source are to be distinguished from typhus by the intestinal lesion.

With regard to the French typhoid fever, we are led to infer this conclusion from the following facts: the existence of such miasm in abundance in the fosses, wells, and river of Paris—the almost invariable occurrence of gastro-intestinal affections in those newly-arrived there—the fact that typhoid fever attacks the same class so constantly as to make a change of circumstances regarded as one of the essential causes of the disease—and lastly, the testimony of the most distinguished physicians that it is not propagated by contagion. Let the experience of our large hospitals, with reference to the infection of typhus, be compared with the following statement of Andral. "In Paris, either in the hospitals or out of them, we never recognized in this disease (dothinenteritis) the slightest appearance of a contagious character. In the hospitals we do not see it transmitted from the individual who brings it from without to those who are lying in the beds next his own; neither do we see that the patients who lie in a bed previously occupied by a person who has recovered from, or who has died of a dothinenteritis, are attacked by it; neither are the physicians or medical students who come there attacked with it, more particularly those who have had to come in contact with patients labouring under the disease. Out of the hospitals what circumstances are more favourable to contagion than those generally found combined in the case of medical students who attend their companions when affected with typhoid fever? Shut up in a room which in general is very small, they pay them the most assiduous and devoted attention night and day; if the affection were contagious almost all of them would contract it, and yet we do not remember to have seen the disease even once arise in this way in a healthy individual."*

* Spillan's Clinique Medicale, p. 728.

Louis does not mention contagion in his observation on the causes of the disease, but Dr. Gerrhard states that, in conversation, he informed him that he had never seen a case so communicated.

But the question may be elucidated by an examination of some of the published histories of fevers occurring in this country from exposure to endemial sources, from which it will be seen that in numerous instances these were found to be attended with the characteristic dothinenteritis of the French typhoid affection. Thus in London, after making every due allowance to the advocate of the exclusive infectiousness of typhus, we must contend that the writings of Dr. Armstrong, Dr. Southwood Smith, and others, prove the frequent occurrence of continued fever from these causes, while the treatise of Dr. Smith shews how large a proportion of the fever of London is of the intestinal kind—having all the characters, symptomatic and anatomical, of the "typhoid affection." A similar remark may be made of Dr. Tweedie's work, which contains numerous cases of dothinenteritis, while it affords strong indirect testimony to the endemial source in the statement as to the period of the year at which the disease prevailed, and its remarkable subsidence under the influence of low temperature, rain, and frost—causes which exert a precisely opposite effect upon contagious typhus.*

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* Dr. Tweedie, who is by no means a strong advocate for the malarial origin of fever, remarks, that "cold and wet Summers are always remarked to be comparatively healthy, while disorders of the bowels in such seasons are seldom observed. The number of patients admitted into the Fever Hospital, in the Autumn months of the last three years, establish this principle. In August, September, and October, 1827, there were 205; in the same months of 1828, the numbers were 170; in the Autumn of 1829, only 94 were received. The cause of this progressive diminution is undoubtedly to be traced to the cold wet Summers of the last two seasons."

A similar remark has been made by many physicians as to Dublin; thus Dr. Percival says, "it has long been observed, that protracted dry weather is peculiarly productive of fever in Dublin; and that rainy weather agrees best with the general health of its inhabitants." And while he states that the worst forms of typhous fever prevailed at an advanced period of the *Winter*, and were characterised by cerebral congestion, he thus distinctly characterizes the endemial fever: "But the seat of peculiar congestion in the autumnal fever was the inner surface of the intestines, and sometimes the mesenteric organs. The type of this epidemic was more irregular than any other; its invasion more obscure; its progress and duration less defined. The subjects of the disease were often broken down and declining constitutions, in which the digestive organs had been long impaired, &c." Could any description more resemble that of the dothinenteritis of Louis? Dr. Davidson, who argues for the identity of the two fevers, meets the above statement of Dr. Tweedie thus:—"An opinion exactly opposed to that of Dr. Tweedie is given by Dr. Armstrong. He states, that in England, typhus is evidently favoured by a low temperature, being most prevalent in the cold seasons of Winter and Spring, generally abating or disappearing, as the heat of Summer advances, and often prevailing to a considerable degree in cold wet Autumns." This passage is extracted from his work on Typhus, in which Dr. Armstrong advocated the doctrine of contagion. At a later period of his life he taught the exclusively malarial origin of fever, and in his lectures adduces in proof of that doctrine the great prevalence of fever in London during hot seasons, and particularly during the dry hot Summer of 1818. The only mode of reconciling these opposite opinions of the *same observer*, is by supposing that he described the nature of each epidemic correctly as it was presented to him, but not being prepared to recognize a distinction in their nature, was natu-

Again, while Dr. Addison ascribes the numerous cases of synochus and typhus presented at Guy's Hospital to river malaria, Dr. Bright and Dr. Hodgkin prove the identity of many of the cases received into that institution with the disease described by Louis. Similar observations have been made in other places. In Birmingham, dothinenteritis is stated to be the constant morbid appearance of the few cases of fever which occur; and on referring to Dr. Ward's account, already quoted (p. 43), of a fever which he clearly shews to have arisen from river malaria, we find it stated that it was present in all the fatal cases.

In Dublin, Dr. Cheyne marked three periods occurring in his experience, during which the contagious typhus usually prevalent, gave way to epidemics of intestinal fever, in which evidence of malaria was frequently met with, but infection not so—the pathology was that of dothinenteritis. These observations are confirmed by others; thus, Drs. Graves and Stokes have published a number of cases of peritonitis from perforating ulcer of the ileum, occurring during one of these periods, 1826-29. Dr. Kennedy states that the glands of Peyer were found by him to be more or less diseased, in a large proportion of the cases of the same period, presenting, as he remarks, a striking contrast in this respect to the fever (contagious typhus) of 1837.* Dr. Stokes also says, "In the epidemic of 1826 and 1827, we observed the follicular ulcerations (dothinenteritis of the French) in the greater number of cases. In many instances perforation took place, and the whole group of vital and cadaveric phenomena corresponded almost exactly to the dothinenteric affection of the French authors."†

We meet with similar evidence of two fevers in Glasgow. In 1836, says Dr. Stewart, 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.

Dr. Stewart adds, "that during the Summer and Autumn of 1836, the cases of typhoid fever were numerous, but from the month of November in that year, (at which time both the type and amount of typhus became more formidable) till June, 1838, not more than a dozen cases, if there were even so many, and these at long intervals, were admitted for treatment."‡

This evidence is pretty clear as to the existence of two forms of disease. As

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rally led by the evidently non-contagious character of the last observed, to doubt the correctness of his views of the origin of the first.

* Medical Report of the Cork Street Fever Hospital.

† Lectures, Lond. Med. and Surg. Journal.

‡ On Typhus and Typhoid Fevers, Edin. Med. and Surg, Journ. No. 145.

to the causes, the highest authority in Glasgow on this subject, Dr. Cowan, writing during the period referred to, and with the cases before him, says—" many of the cases of the production and propagation of disease must be ascribed to the habits of our population, to the total want of cleanliness among the lower order of the community, to the absence of ventilation in the more densely peopled districts, and to the accumulation for weeks and months together of filth of every description in our public and private dunghills, to the over-crowded state of the lodging-houses resorted to by the lowest classes, and many other circumstances unnecessary to mention."

In Edinburgh, according to Dr. Christison, "the intestinal affection has repeatedly presented itself in groups—the *constitutio dothinenterica*, to speak in nosographical language, has repeatedly appeared and disappeared as a subordinate or intercurrent epidemic, in the course of the more general epidemic—typhus." And according to Dr. Reid, such cases occur not unfrequently in the country parts of Scotland, and are occasionally sent to the Edinburgh Infirmary.

In Liverpool we are informed they occur in an intercurrent way, as in Glasgow,* and we need only refer to the evidence before the committee on the health of towns for proof of sufficient endemial causes.

In the Navan Fever Hospital there have been for the last seven years almost always two distinct forms of fever present, one or other occasionally preponderating, so as at times nearly to exclude the other. Thus for the first three years the prominent features were pain, tenderness, and meteorism of the abdomen, diarrhœa, and not unfrequently these symptoms combined with catarrh; several cases of perforation of the ileum occurred towards the close of this period; petechiæ were not frequent and were late in their appearance, and we had few instances of communication by contagion. During the three following years a highly contagious fever prevailed, and the symptoms and treatment were completely different, delirium, subsultus, dysphagia, being the ordinary symptoms, and diarrhœa being rarely met with;—nearly every case presented the measly efflorescence, and instances of contagion were as numerous as they had been rare previously. During the present Summer the prevailing type has been the abdominal fever of the first period, and instances of typhus are infrequent, certainly not a fourth of the whole, and sent exclusively from a district in which the epidemic of last year still lingers.

In America the existence of two kinds of fever has been maintained by Dr. Jackson and Dr. Gerrhard. The former says, in his report on typhoid fever, " it is plain that there are at least two species of continued fever, both in Europe and in this country, and further researches may very possibly shew more."

Dr. Gerrhard states, that "from the information we possess we should conjecture that the two diseases (British or Irish typhus and dothinenteritis), are widely different in their symptoms, anatomical characters, treatment, and mode of transmission."

The following extracts from his able paper will shew that the two forms of fever exist in America at different periods and with distinct characters, just as in our own cities.

"Dothinenteritis is by no means a rare disease in Philadelphia, although less common than at Paris. In the essay alluded to, I established the identity of the anatomical characters and of the symptoms of the fever occurring at Philadelphia with that observed at Paris.... The typhous fever which is so common throughout the British dominions, especially in Ireland, is not attended with ulceration or other lesion of the glands of Peyer....... For a period of at least ten years, there has been no epidemic of this nature at Philadelphia. In the year 1827, a large

* See Dr. Lombard's Letter, Dub. Med. Journal, Vol. X.

number of Irish emigrants were ill of a typhoid fever with ulceration of the small intestines, which was probably dothinenteritis, and during several successive years there were more or less extensive epidemics of remittent and intermittent fevers occurring in the neighbourhood of the city, but not often extending into the central parts of the town. In the Winter of 1835-6, a form of fever not commonly met with at the hospitals was observed from time to time. It was characterized by pungent burning heat of the skin, dusky aspect of the countenance, subsultus, delirium, with great stupor and prostration, but there was no diarrhœa, and but few symptoms referrible to the alimentary canal. It was the disease which afterwards appeared as an epidemic............The evidence of contagion was direct and conclusive; three of the principal nurses and about a dozen assistant nurses, besides a number of patients ill with various diseases were taken with the fever. There was only one nurse of a ward in which many of the patients were collected who escaped; but several of his assistants and patients were taken ill. The wards in which the fever-patients were placed were large and well ventilated. The contrast between the two fevers in this respect (their infectious character) is obvious......... *Season of the Year.*—The epidemic began in March and continued until August—there were a few scattering cases afterwards. The Summer was unusually cool, and the Spring and Winter cold............. *Pathological Anatomy.*—In this large number of autopsies, amounting to about fifty, there was but in one case, and that doubtful in its diagnosis, the slightest deviation from the natural appearance of the glands of Peyer..............The fact that the morbid changes pathognomonic of dothinenteritis are not met with in the typhous fever, would of itself seem conclusive that the two diseases are no more identical than pneumonia and pleurisy. Although in some respects the two affections are analogous and even similar; the radical difference of anatomical lesions is at least as well marked as the distinction between the symptoms. It is indeed singular that there should be of late a strong tendency to confound two fevers which were regarded as entirely distinct by some of the older physicians."

In the above quotations we see strongly marked the differences of the two affections as to prevailing season—symptoms, pathology, and mode of transmission, and the similarity of each to one or other of the two forms of European fever.

Having endeavoured to collect and arrange the testimony of the best authorities as to the *sources* of the fever poison, we stop upon the threshold of the extensive inquiry into the laws which regulate the diffusion of the disease in an epidemic form.

To attempt this would require the fullest investigation into the differences and analogies of the two affections, their modes of combination in the same individual, and their occurrence in an intercurrent mode during the same epidemic period, all of which modifications of disease would be found reconcileable with the theory of two poisons; the one having its elements in the blood, and reproduced in it; the other a product of putrefactive decomposition, and not reproduced in the human body; while on the other hand Dr. Davidson's recent essay contains in itself proof that his own theory of a single typhoid poison is not tenable, since it involves the assertion of the identity of two diseases, one of which (according to him) requires to be kept up by an uninterrupted series of cases of contagion, while the other, according to the best observers, never propagates itself by contagion at all. In short, according to this doctrine, we must believe that the same poison, shall at the same time and place, and among the same collection of individuals, produce two diseases totally dissimilar in their mode of access, symptoms, pathology, treatment, and mode of transmission.

ERRATA.

Page 1—line 8, *for* account, *read* amount.
,, 9—line 30, *after* on the, *insert* contrary.
,, 14—line 32, *for* violent, *read* violet.
,, 17—line 17, *for* committed, *read* admitted.
,, 24—line 9, *for* receiving, *read* secerning.
,, 24—line 23, *for* phelibilis, *read* phlebitis.
,, 24—line 48, *for* universally, *read* inversely.
,, 28—line 13, *for* Brusenis, *read* Burserius.
,, 29—line 14, *for* alteration, *read* alternation.
,, 30—line 45, *for* epidemics, *read* epidermis.
,, 32—line 36, *for* epidemics, *read* epidermis.