swellings, combined with the internal administration of carbolic acid, or sulphate of quinine. In some cases, these remedies were thought to do good, while in others they proved useless, if not detrimental, and Dr. Cabiadis is convinced that we know as little about the cure of this disease as we do about its essence.

He concludes his interesting notes on this subject by expressing astonishment and regret that no public hospital exists in Bagdad, an extensive city of 150,000 inhabitants.* A large military hospital was built there by Midhat Pasha, on a fine and healthy site, but no use has been made of it. He therefore suggests that the Government should turn it into a civil hospital for the benefit of the people of Bagdad.

NOTE ON A PECULIAR CONDITION OF THE SKIN IN PARAPLEGIA.

By DAVID FERRIER, M.D., F.R.S.,
Assistant-Physician to King's College Hospital.

IN a case of tumour of the neck complicated with paralysis of the cervical sympathetic on the left side, and complete paraplegia from the mid-thoracic region downwards, under the care of Mr. Wood in the Albert Ward of King's College Hospital, Mr. Wood, while examining the patient, was incidentally struck with the remarkable facility with which a clean silver probe made dark pencil-marks on the paralysed parts, while it produced only a red mark of vascular dilatation on those parts of the same patient which were not paralysed. When I was told of the circumstance by the house-surgeon, I was at first somewhat incredulous, and inclined to attribute it to some silver compound on the probe; or at any rate that any other metal, such as a key or hard substance, would probably produce a similar effect. I was told, however, this was not the case; and, on examining for myself, I found that most undoubtedly a silver probe or the bright edge of a shilling produced a dark pencil-mark on the abdomen and legs, while a key and a piece of ivory were absolutely negative. But I also found that the edge of a bright sovereign would produce a similar mark, though less readily and not so distinct, and the same with a bronze coin.

Thinking these facts rather remarkable, I determined to pursue the subject: and, with Mr. Wood's permission, I instituted a further series of experiments. The various hypotheses which suggested themselves at first to myself and those who witnessed the facts will appear somewhat ludicrous when viewed in the light of what turned out to be the real explanation; but, as they served to eliminate the truth, they may as well be detailed.

From the fact that silver in particular marked the skin, it was supposed that it might be due to the formation of a sulphide on the skin. To test this, I provided myself with acetate of lead papers, and also with metals which do not form dark sulphides, as zinc, magnesium, and metallic arsenic. The reaction of the skin was neutral to litmus. The acetate of lead test-papers did not become blackened when applied to the skin—at once excluding sulphurous exhalations; and, as zinc and magnesium also caused dark pencil-marks, the theory of sulphides could not be entertained. The metals which were found to give distinct marks were silver, gold, copper (pure and in alloy), lead, zinc, and magnesium; while iron, platinum, and a crystal of metallic arsenic produced only a red line of vascular dilatation. The pencil-marks, though all dark, had different shades, and some were produced with greater facility than the others. With silver, the skin could be drawn on with as great readiness as with a pencil on paper, and it was easy to write or sketch on the thighs, hips, and abdomen. With a wet sponge, the marks were easily effaced, leaving mere lines of vascular dilatation. On examining with a lens the dark lines drawn with a piece of pure magnesium, it was possible to detect a fine dark powder; and, on rubbing him with a clean linen handkerchief, a distinct dark stain was communicated to it. To insure absolute brightness of the magnesium, I cut and scraped it; and, on proceeding to burnish it with a piece of wash-leather, I was struck by the fact that the leather became blacker the more I rubbed. Here was the key to the puzzle. Placing the wash-leather on my leg, I tried in succession the various metals which had produced pencil-marks on the skin, and found that all those which had been effectual also produced marks of exactly the same character on the leather, while iron and platinum produced none. The explanation was thus simple, that the skin possessed the same property as the wash-leather in rubbing down the metal in the form of a fine dark powder. That this property of the skin was not peculiar in the case under investigation, was proved by Mr. Wood writing with a silver probe on the

legs of another patient paraplegic from fractured spine lying in the next bed, while on the arms and chest of the same patient only red marks could be produced.

On testing the healthy parts in these cases, as well as the skin of healthy persons and patients free from paralysis, it was found that, though with considerable pressure some indications of pencil-marks might be made on most, yet the readiness with which the paralysed limbs could be written upon was very striking. I could detect no difference between the two sides in a case of hemiplegia, nor was there any difference between the two sides of the face in a case of peripheral facial paralysis of some standing. Thinking that the physical condition of the skin which caused it, like wash-leather, to take on metallic pencil-marks so readily was probably due to a state of oedema, I tried the various metals on the legs of a patient suffering from cardiac dropsy, and with marked success. With silver, copper, gold, zinc, magnesium, aluminium, etc., sketches could be drawn on the legs in bold dark lines. As before, iron and platinum were entirely negative—a fact easily explicable by the hardness of the metal. I found also that on the lumbar region and buttocks of a patient confined to bed in the recumbent posture, similar metallic lines could readily be traced. Here also oedematous softness of the skin, caused by the long maintenance of the recumbent posture, would readily account for the phenomena. But neither here nor in the paraplegic patients was there any evidence of oedema by pitting on pressure when tested in the usual way.

Hence it would appear that, though the property of taking on metallic markings belongs to the skin to a greater or less extent in the healthy state, yet in certain conditions, especially of oedema, which may not be otherwise evident, this property is greatly intensified, and may be of some diagnostic value.

THE ETIOLOGY OF ENTERIC FEVER, WITH AN ACCOUNT OF NINETEEN CASES WHICH OCCURRED IN THE HOMERTON FEVER HOSPITAL.

By ALEXANDER COLLIE, M.D.,
Medical Officer of the Hospital.

THE object of the present paper is to show that enteric fever is contagious, and by this we mean communicable by direct personal intercourse. It is of secondary importance, for the purpose of the paper, to determine what, if any, particular excretion may contain the fever-producing material; but we think it has been asserted without sufficient evidence that this "material" resides in the stools alone. On the assumption that enteric fever is a specific enteritis, and nothing more, there would be reason for this opinion. It is, however, something more. The severity of the general symptoms is not only not always, or even in the majority of fatal cases, in proportion to the severity of the internal lesions, but they are sometimes entirely *unlike* the symptoms produced by acute local inflammations. We not infrequently see cases of enteric fever in which the symptoms during life point only to acute poisoning, and in which it would be impossible to say to what the acute poisoning was due: cases in which *post mortem* examination shows lesions in no degree proportionate to the severity of the general symptoms. If, then, we regard the fever as the result of a poison affecting the body generally, and the small intestine incidentally, we see no reason why the poison should be limited to the intestinal excreta, and why that of the kidneys, the lungs, and the skin should be held innocuous. That there is no such limitation derives support from the fact that the anatomical changes occurring in the early period of the fever are not limited to the small intestine. The early enlargement of the liver and of the spleen indicates early changes in those organs; and the early bleedings of the nose, mouth, and bowel point to early blood-change. Add to all this that enteric fever, apart from diarrhoea and soiled linen, has its "stink", as typhus and small-pox: a stink which, to persons with good noses, is an aid to diagnosis in doubtful cases; and who will dare say that this stinking matter may not produce the disease? As we have already said, it is of secondary importance for our purpose to establish what particular excretion produces enteric fever. This is a question to be determined by future observation, on the hypothesis that one or all of them may produce it. We believe that the cases which arose at Homerton, and the record of which forms the basis of this paper, arose from exposure to *fresh* enteric stools; but, whilst this is the proposition which will be maintained in reference to the origin of these cases, we do not mean to say that cases do not arise from decomposing or decomposed stools or from emanations from the lungs and skin.

Further, it will be our object to show that, whilst we do not deny that enteric poison is sometimes contained in filth and in drains, and

* Population of Bagdad—Shiah Mohammedans, 70,000; Jews, 30,000; Christians (chiefly Chaldeans) and Soonee Mohammedans, 50,000; total, 150,000.

that these may be now and then means of its conveyance, there is no necessary connection between filth-decomposition or choked drains and enteric fever; that, in short, the pythogenic theory is not supported by the evidence, and is practically dangerous to life, inasmuch as it diverts attention from the real sources of danger, viz., the sick person and the sick person's diseased excreta. Choked-up drains and decomposing sewage exist in abundance in many places in all parts of the country, and for long periods together, without giving rise to enteric fever. It would be easy to give numerous instances of this; but better could not be found than the outbreak of "fever" in the Westminster School and the Abbey cloisters in 1848, and that of the Peckham Police Station in 1857, referred to by Dr. Murchison (*Continued Fevers*, 2nd edition, pp. 473-4). In the former, "it was found that the disease (enteric) followed very exactly in its course the line of a foul and neglected private sewer or immense cesspool, in which faecal matter had been accumulating for years, *without any exit*, into which the contents of several smaller cesspools had been pumped immediately before the outbreak of fever. This elongated cesspool communicated by direct openings with the drains of all the houses in which it (the fever) occurred". In the latter, it was found that an accumulation of sewage "had been going on for years" underneath the passage adjoining the room where the policemen sat, and where they had complained of "dreadful odours". It would, then, seem evident that sewage-decomposition *per se* cannot be the cause, or at least cannot be the efficient cause, of enteric fever, otherwise it would surely have shown itself earlier at Westminster and Peckham. Had the enteric fever appeared at a comparatively early period of these accumulations, and continued during these accumulations, now ceasing and now reappearing with change of residents—in other words, if the disease, like the sewage, had existed more or less continuously for years, and had disappeared with the disappearance of the accumulated sewage—then, we think, there would have been reason to regard such accumulation and disappearance in the relation of cause and effect. An effect which appears suddenly after the existence "for years" of the particular condition of things which is supposed to be equal to its production, cannot be reasonably regarded as an effect of that particular condition; for the particular condition had existed "for years"; the people liable to suffer from its operation had existed, too; notwithstanding, there is no effect, as there should have been if the cause, *i.e.*, particular condition, had been equal to it—in the form of an outbreak of enteric fever. Accumulations of faecal matter and stoppage of drains did then exist "for years" with the necessary second factor—suitable subjects for attack; still there was no enteric fever. How, then, may we ask, can it be said that these two factors, accumulation of sewage and suitable subjects, are equal to the production of enteric fever when they exist "for years" without any such production? The best instance of the inefficacy of sewage-decomposition to produce enteric fever is that of the Thames in 1858-9, recorded by Dr. W. Budd. "For the first time in the history of man, the sewage of nearly three millions of people had been brought to seethe and ferment under a burning sun in one vast open *cloaca* lying in their midst. Stench so foul had never before polluted this lower air; river-steamers lost their accustomed traffic, and travellers pressed for time made a circuit of many miles rather than cross one of the city-bridges." "With all this condition of the Thames", says Dr. Letheby, "the health of the metropolis has been remarkably good. In the corresponding period of last year (1857), the cases of fever, diarrhoea, and dysentery attended in the city by the medical officers of the unions amounted to 293 of the former and 181 of the latter; but, during the past quarter (*i.e.*, the quarter of intolerable stench), they were only 202 of the former and 93 of the latter" (*Typhoid Fever*: Dr. W. Budd, pp. 148-9-50).

To facts of this kind we do not see what can be replied. Here at least is filth-decomposition on a scale never before reached, and yet the health of those subjected to its supposed enteric-producing effluvia is not only generally good, but enteric fever is actually less common than usual. Dr. Budd's reasoning on these facts appears to us conclusive as against the filth-decomposition theory, and it is not open to a conceivable objection which might be taken to the conclusion arrived at from the Westminster and Peckham cases. In these, it might be urged that, previously to the outbreak, there were no susceptible subjects: an objection barely conceivable in regard of the thousands exposed on and around the Thames. If it were the rule to find enteric fever unusually prevalent where filth was most abundant, there would be reason to regard them in the light of cause and effect. But the case is just the contrary. Enteric fever is as common amongst the rich and the well-to-do as among the poor and the destitute; in the better constructed houses of the former as in the squalid homes of the latter. If it be true that enteric be a fever begotten of ordinary sewage-decomposition, how does it come to be relatively as common in the comfortable homes of the rich as in the narrow dwellings of the poor? It might, we think, be

concluded, with more show of reason, that typhus originates in some kind of sewage or filth-decomposition; for it is mostly confined to localities abounding in some kind of filth. As regards enteric fever and probably other contagious diseases, to wit, diphtheria, we do not think that the filth and drain theory will stand. This at least we will say: that, after diligent search, we have been unable to find evidence on which that theory can be supported. With Professor Trousseau, we would say: "Ce serait nous engager sur le terrain des banalités que de vous parler de l'influence d'un air vicié par des émanations putrides, de l'usage des aliments gâtés, boissons corrompues, etc., toutes causes hypothétiques que rien ne prouve." The fever is, in our opinion, contagious, sometimes severely so, and then affects, not only whole families, but even whole communities. Having regard to this part of the subject, we would say, with Dr. Budd, that contagion is the "master fact" in the history of enteric fever.

The importance of the subject will not be denied; for if, as we say, enteric fever be, like scarlatina, small-pox, and typhus, contagious—it is of little importance how—the present mode of treatment must be altered and isolation practised in enteric fever as in small-pox. We believe that our Homerton experience establishes this proposition; but, to show that this experience is not isolated, but part of a body of facts pointing to the same conclusion, before recording that experience, we shall give a short account of instances of contagion observed by eminent physicians in France and Germany.

"Opinions", says Trousseau, "are still divided on the question of the contagious nature of enteric fever; but the number of those who refuse to admit contagion diminishes daily" (*Clinique Médicale* vol. i, p. 276, Paris, 1868). He then narrates several instances of contagion, of which the following are three. Enteric fever was imported into Carriol (Gironde) by a young workman who had arrived sick at his home. The family was composed of seven persons, living in a large well ventilated house. All were severely attacked, of whom three died. From thence the disease spread amongst those who had communication with the sick, and these removing, sometimes to very great distances, carried the disease to a locality where it had never before existed.

On May 31st, 1857, a young man arrived at Audon-le-Romain (Moselle) from Paris, ill for some days of enteric fever. He spread the fever to the companions who visited him, and then to his father, brother, and two sisters. So long as field-work kept the inhabitants of Audon away from their houses, the fever, although it spread in the village, remained limited to a small number of inhabitants; but, when the harvest was finished, constant residence with the sick produced a general infection, and there were at one time forty cases in a population of four hundred and forty-two. "If", says Dr. Fourrier, Trousseau's informant, "sceptics see nothing here but a coincidence, where would they be able to see relations of cause and effect?"

A farm-lad aged 12, in the service of the Mayor of Bièvres, whose wife and daughter had had enteric fever successively, contracted the disease, and carried it to his village at Orgeval, a distance of about two miles, where no case of enteric fever existed. He gave the disease first to the relative who nursed him, and this relative gave it to another relative, who came from the opposite extremity of the village to help her. Thence the disease spread in the municipality (commune). But this is not all. A young domestic employed in a house at Orgeval, where there were cases, was attacked with the fever, and sent to her own home, a distance of three miles and three-quarters, where she carried the disease, which became epidemic. In concluding his chapter on Contagion, Trousseau admits that, in many instances, it is impossible to trace the source of the contagion (a remark, by the way, which is true of small-pox, than which there is perhaps no other admittedly contagious disease in which it is so often impossible to trace the source of the contagion); and that, whilst admitting contagion, it is not necessary to deny spontaneous origin. But he does not think that spontaneous cases arise from putrid emanations "which prove nothing", but from age, overcrowding, and climate.

Liebermeister, who denies that enteric fever ever passes from person to person, admits that he has frequently seen it arise in hospital. "If", he says, "this occur during the prevalence of an epidemic, and the number of the cases be small, nothing worthy of remark has taken place. But, even if the number of the cases within the hospital should become exceptionally numerous, this is not to be regarded without further consideration as evidence in favour of the direct transmission of the disease. It is far more likely that the causes will be found in local conditions. If there be an infectious centre (Infectionsherd) within the hospital, numerous cases of sickness may be produced thereby; and that such an infectious centre (Infectionsherd) may readily form itself in a hospital containing numerous enteric cases (Typhuskranke) is, in the present state of our knowledge, easily understood. I have myself,

since the year 1865, had frequent opportunity to observe such hospital infection. But my conviction that it does not pass from person to person has only been on that account strengthened." (Ziemssen's *Handbuch*, vol. ii, part 1, 2nd edition, p. 48, Leipzig, 1876.) We do not quite understand what Liebermeister means by infectious centre (Infectionsherd) and transmission from person to person; but, as far as we do understand him, his belief appears to us to be in opposition to his evidence. The plain fact remains that, in hospitals containing enteric cases, the disease does spread, and persons who have it not take it from those who have it; unless it be maintained that they take it from those who have it not. Liebermeister states that, in the six years he was connected with the hospital at Basle, from 1865 to 1871, there arose there forty-five cases; and that, in addition, "there appeared among the physicians, nurses, and servants so-called febrile and non-febrile abdominal catarrh, which must be referred partly to light infection with enteric poison (Abdominaltyphusgift)" [*ibid.*, p. 48]. He records, further, that, "in the years 1872-73, as I learn from a friendly communication of Professor Immerman, six to eight cases of enteric fever occurred within the hospital amongst physicians, nurses, servants, and patients; whilst almost all newly appointed assistants, nurses, and servants passed through so-called abdominal catarrh without fever" (*ibid.*, p. 49). And yet the disease does not, or rather Professor Liebermeister believes that it does not, pass from person to person. It does, however, as matter of fact, pass to persons; and, on Liebermeister's authority, we think we are justified in saying that enteric fever, when introduced into hospital, spreads as do typhus, small-pox, and scarlatina. The particular form of "set words" in which this important fact may be expressed is, we think, of minor importance.

It would be easy to multiply instances of a like kind. Piedvache's essay contains numerous such, and those of Dr. Budd are too well known to be more than mentioned. The valuable experience of the London Fever Hospital, which, as we shall show, supports our contention, will be referred to in another part of our paper.

[To be continued.]

TYPHOID FEVER: CONTAGIOUS, INFECTIOUS, AND COMMUNICABLE.

By WILLIAM THOMSON, M.D.,
Medical Officer of Health, Peterborough.

THE subject of typhoid fever is one which has of late been so often before the public and the profession, that I do not pretend to furnish anything very novel regarding it. My professional life has, however, for the last ten years, been passed in the midst of a population especially exposed to the spread of typhoid, and amongst whom it has very constantly and very extensively prevailed; so that a personal acquaintance with this fever in the joint capacity of medical officer of health and general practitioner has given me sufficient opportunity of studying its character. The result of my investigation is, and every year's experience strengthens me in my opinion, that typhoid fever is contagious, infectious, and communicable. Four years ago, I had at one time under my care two hundred and thirty-five cases. The ignorance and carelessness displayed by the inhabitants on the subject of the nature of the fever led me to publish directions for their aidance, in which I called the fever "highly contagious", believing that to be the most expressive term to use to explain its nature, and in hopes of inducing precautions to be adopted to prevent the further spread of the epidemic; but, as there are still members of the profession who state that "typhoid is not contagious in the usual sense of the word; that you may live and lie with safety in the same room with typhoid fever patients; that strangers may come to and fro without risk; that children may attend our schools as usual; that friends may visit and assist those who have typhoid in their houses without a shade of risk; further, that, with pure water and perfect drainage, typhoid fever will never spread by infection or contagion";—I consider it to be the duty of those members of our profession who, by experience and careful study, have come to hold an opposite view, to lose no time in laying before the profession the results of their investigations, believing that much suffering will thereby be prevented. With such a diversity of opinion as exists, it is perhaps more than rash to take any part in the discussion; but, as a firm believer in the eventual success of preventive medicine and in the growth of professional knowledge regarding preventable diseases in general, and typhoid fever in particular, I have collected the opinions of some who, by their opportunities, may be considered special authorities upon this subject. It is not my intention, in this paper, to discuss the origin of the disease, nor to enlarge either

upon the germ-theory or the pythogenic, but simply to give my opinion and that of others based upon investigation as to the spreading power of the typhoid poison.

When the pythogenic theory is believed to be wrong, it facilitates the view that typhoid is contagious in the usual sense of the word. No doubt, a great deal of the discrepancy of opinion which has hitherto existed and still exists concerning its etiology is due to the difficulty of diagnosing the many varieties of the disease. Enteric fever is very common in infancy, and thus many people, being protected by such an attack, not being susceptible, come into contact with typhoid patients without contracting it again; and this is used as evidence against its contagiousness when the same would equally apply to scarlet fever. In studying the subject, we must bear in mind that we have imperfect development of some of its usual phenomena: arrest or absence of some of its specific anatomical lesions, varieties as to its extent and mortality, differences in the history of its origin and so forth, according to the ever varying conditions under which it is diffused; that many cases of so-called typhoid are not really specific typhoid; that some cases registered as typhoid are not contagious, and so count as witnesses against it.

In order that there be no misunderstanding through the use of words, which anyone may convert to suit his own view, it should be borne in mind that every term should have a strict sense and be employed in that sense, and its ordinary sense should be its strict scientific sense. The terms "infection" and "contagion" denote the transmission of any poisonous principle, or any poison capable of being transmitted. When the transmission is effected by a material substance and is brought about by actual contact, the term "contagion" might be employed; but, when the transmission is effected through the agency of the winds or atmosphere and at a distance, infection might be used. But, as all diseases are more or less infectious, I hold this view to be wrong. No two words were ever fraught with greater danger to sanitary science than contagion and infection; they are very traps for the unwary and the ignorant; the terms are convertible, and should be employed as such.

Mr. Haviland, in a letter to me on the subject of the contagiousness of typhoid, writes so lucidly on this point, that I cannot do better than give the definition of these terms in his words. "By contagiousness, scientific men understand the power of a disease to communicate itself by contact to a person susceptible of taking it; but, before a disease can infect our bodies, it must come into contact with either our external covering—the skin, or our internal lining—the mucous membrane; between which the body properly lies. We all know that this inner lining is far more exposed to the contact of disease-poisons than the skin, inasmuch as it presents a much more extensive surface for poison-contact than the skin, which can take place every moment we breathe and every time we eat and drink. Besides which, we must remember that, as a rule, our bodies are well protected by clothes from contact with disease-poison. The process of infection is the sequel to the contact of the poison. I consider, therefore, that a disease is highly contagious the poison of which is so subtle as to be capable of being brought by the air into contact with the membranous-lining of our eyes, our lungs, and the whole length of our alimentary canal, even without being perceptible to any of our senses, and, after contact with the surfaces, developing within a few days a deadly disease capable of infinite multiplication."

In the summer of 1876, I was called to attend a young lady, who had arrived three days previously from Nottingham, where typhoid was very prevalent at the time. I found my patient ill with genuine enteric fever; spots, temperature, and so on. I at once took every precaution to have the patient isolated. I had the stools thoroughly disinfected, and sent for a trained nurse. It was an extremely hot summer and autumn. The house where my patient resided was near to the end of a row of twenty-eight houses, and at the lowest end; no houses opposite; good open garden at the back; well at the end of the garden; privy and ash-pit near the house; no drains. All the houses had separate water-supplies; there had been no cases of fever in the neighbourhood previously. A few days after my first visit, another case occurred in the highest house in the street, and at the other end of the row from the first. In six weeks, thirty-nine persons were smitten with fever in twenty-eight houses with a population of one hundred and thirty-three. That it could not have been spread by water or drainage; and that it did spread by atmosphere-contamination (even though the stools from the first ten cases were thoroughly disinfected), I feel convinced.

Last July, I was attending a typical case in a young lady residing in Peterborough for the purpose of attending school, her home being in Suffolk. Her mother was very anxious to come to see her; but, as she had other children at home, she would be careful. I told her not to stay long in the room, neither to eat nor drink anything in the house, and to wash her hands before leaving. All these injunctions were carefully carried out. Nevertheless, in ten days, I heard she was ill with typhoid fever. I corresponded with her medical attendant, who kindly