hairs; and even when these appear, it by no means proves the case to be cured, as there are generally a few diseased stumps left, which can easily be seen with a lens, their removal being most difficult, and sometimes impossible for months. Therefore, although most of the hair may be growing firmly and freely over a patch of ringworm, this alone does not prove the case to be cured.

With regard to the treatment by turpentine and iodine, though certainly a good one, yet I believe it will fail in the majority of obstinate cases. The plain truth is that there is no known remedy that is sure to eradicate any given case of ringworm, even after months of treatment; and sometimes children will have this complaint for years, though every means be taken to remove it. Oleate of copper and mercury, and all other remedies, often fail, in spite of what has been written to the contrary.

I am, Sir, yours obediently,

ALDER SMITH, M.B., F.R.C.S.,
Medical Officer to Christ's Hospital.

Feb. 24th, 1886.

HOME v. HOSPITAL TREATMENT OF INFECTIOUS DISEASES.

To the Editor of THE LANCET.

SIR,—On January 23rd you drew attention in an annotation to the section of my Report on the Sanitary Condition of Bradford dealing with the above important subject. For reasons which will appear pretty obvious, I have given in my reports to the Town Council the rates, and not the actual numbers, upon which I founded my opinion that in Bradford the proportion of recoveries among those treated in

ceeding year the advantage has been very much on the side of the hospital. These particulars will be seen at a glance in the table given below.

The explanation which you suggest of these facts—viz., that the home-rate is increased by the suppression of notification of mild cases—is, I think, not warranted. So far as concerns notifications sent by medical men I am perfectly satisfied it is erroneous. I have every reason to believe that every member of the profession continues loyally to carry out the anxious and troublesome duties cast on him by the Act requiring notification. It would be expecting greater certainty than prevails in any human affairs to expect that no case unattended by a medical man ever escaped notification. There may be some few which do, I think there are not many. But even allowing for such cases, the difference between the hospital rate and the home rate is so great that it cannot reasonably be thus accounted for. The latter is pretty nearly double the hospital rate for both scarlet fever and typhoid. With regard to the character of the cases sent into the hospital, some are sent in because they are very severe, others are kept at home for the same reason—viz., that they are too ill to be moved. Again, some cases may be sent into hospital which are exceedingly mild and only need isolation; but for the same reason many mild cases are treated at home because of the slight character of the symptoms. I am informed by Dr. S. Evans, the resident physician at the Fever Hospital, that if the severe cases sent to the hospital which die within twenty-four or forty-eight hours of being received there were deducted from the hospital figures the result would be to show a very great reduction in the hospital rate. Such cases can hardly be said to have undergone treatment at the hospital at all,

TABLE SHOWING THE MORTALITY PER 100 CASES FROM SCARLET FEVER AND TYPHOID FEVER IN BRADFORD, WHEN TREATED IN THE HOSPITAL AND WHEN TREATED AT HOME.

Year.	DEATH-RATE PER 100 CASES.					
	SCARLET FEVER.			TYPHOID FEVER.		
	Treated at home.	Treated in hospital.	All cases.	Treated at home.	Treated in hospital.	All cases.
1882	18·4	14·5	13·9	13·0	17·3	15·2
1883	15·8	7·3	10·3	32·6	14·2	22·1
1884	9·1	4·2	6·7	29·6	18·5	24·5
1885	8·3	3·9	5·9	42·1	6·3	19·80
Mean 4 years	11·7	6·82	8·95	27·7	14·1	20·44

hospital for certain infectious diseases is very much greater than among those treated at home. As you expressed a desire to have fuller information on the subject, I have much pleasure in laying the details before your readers.

As the compulsory notification of infectious disease has been in operation here over four years, already a considerable number of cases have been recorded on which to form an opinion. During 1882, 1883, 1884, and 1885 there have been 758 cases of typhoid fever and 2032 cases of scarlet fever. (I deal with these two as by far the most important diseases, the case of other diseases notified having been much too few to be of value for drawing any reliable conclusion from.) Of the cases of typhoid fever, 404 were treated in the hospital and 354 at home; the deaths among the former numbered 57, and among the latter 98, giving a death-rate of 14·10 per cent. for the cases treated in hospital, and of 27·68 for those treated at home. Of the 2032 cases of scarlet fever, 1143 were treated in the hospital (during the same period), and 889 at home; of the former, 78 died, and of the latter 104, giving respective rates of 6·82 per cent. deaths for the cases of scarlet fever treated in hospital, and of 11·69 for those treated at home; the number of cases of these two diseases dealt with is considerable, indeed very much larger than that frequently regarded as sufficient for using as the grounds for an opinion as to success in therapeutics, whether medical or surgical, and the numbers compared treated in hospital and at home are not markedly different. The period embraced—four years—is also one of considerable duration. In the case of scarlet fever the lower rate prevailed every year in the hospital; the rate for typhoid was a little higher in the hospital for the first year (1882), but during each suc-

although they increase the death-rate recorded in the institution, and lower the proportion of deaths at home. I think, indeed, the explanation of the more favourable returns from the hospital is not far to seek. Perhaps 97 per cent. of the cases occur among the poorer class, fully 96 are distinctly of this class, and of these a large proportion are of the poorest class. There is no doubt excellent professional skill available for their treatment at home; but of what use can this be in small crowded rooms, where fresh air, were it available, is not admitted, situated as workmen's houses usually are, and without the skilled nursing, constant care and supervision, and abundant and suitable dietary which the hospital can provide? If the acute stage of the fever be tidied over, there remain the difficulties attending convalescence, which are not small in the case of the two diseases I refer to. But these are only matters of opinion. I have given the facts as fully as possible, so that your readers may judge whether my strong advocacy of hospital treatment in this town is well founded or not.

I am, Sir, your obedient servant,
Town Hall, Bradford, Feb. 8th, 1886. T. WHITESIDE HIME.

ENTERIC FEVER.

To the Editor of THE LANCET.

SIR,—After reading the interesting discussion at the Royal Medical and Chirurgical Society on Enteric Fever at Suakim (THE LANCET, Feb. 13th), I thought that a few notes gained from my experience in South African warfare might not be unacceptable; the subject can be read more in detail at page

513 of the Transactions of the International Medical Congress of 1881. In the Kafir war of 1877 I was principal medical officer of the Naval Brigade, and in 1879 I was principal medical officer of the Column of Invasion commanded by Brigadier-General Sir C. Pearson, K.C.M.G., C.B., during the Zulu war, and I thus had ample opportunities of seeing a very large number of cases of both enteric and the so-called typho-malarial fever. I must say that I entirely agree with those authorities who are of opinion that the disease frequently arises *de novo*. During our investment by the Zulus in Fort Ekowe, from Jan. 23rd to April 4th, 1879, we were entirely cut off from all communication with the outer world, except that very rarely a solitary Kafir runner managed to creep through the investing Zulu army from our base of operations some forty miles away on the Natal frontier. Before the march of our column from this base of operations no case of enteric fever had been heard of there; neither was the disease known at the mission station of Ekowe before our arrival there, as I ascertained from a medical missionary who was an old resident. The earliest case of enteric fever occurred among the garrison of Ekowe about the end of the first week in March, and no runner came there from the base in Natal after the 12th of February; it was, moreover, a considerable time subsequent to that date that any case of enteric fever was heard of at the base in question. There were many cases of enteric and typho-malarial fever at Ekowe, including several deaths. Thus it will be seen that it is as nearly proved as anything of the kind can possibly be that the affection must have arisen *de novo*, unless the very dubious hypothesis is advanced that the provisions with which we were supplied contained the germs.

I believe that the cause, in these countries at least, is generally to be sought in the prolonged occupation of a camping-ground, and that the term "pythogenic" is a very happy one. I am of opinion that flies are great factors in its production; they swarm in latrines and cesspits, where the heat favours decomposition, and arise from them often with their fine hairs coated with putrescent matter, shortly to be deposited on articles of diet, or in potable fluids, in which they are often found drowned. The fine surface impurities of these camps are also wafted by the winds into the tents, covering the food and potable water, or being inhaled or swallowed with the saliva. I do not remember ever seeing a case of enteric fever which occurred in an active flying column, which is of course constantly changing its camping-grounds or perhaps merely bivouacking at night, or for some considerable time after such a column had become stationary; but whenever the disease appeared it was invariably in a camp of some permanency, where decomposition of animal matter and frequently malaria coexisted in a subtropical heat.

The colonial surgeon of the Lower Tugela Division of Natal, in which our base of operations was situated, informed me that he had never seen a case of enteric fever in his district until the advent of our troops in 1879; and he told me, a year and a quarter after the troops were withdrawn, that since their departure not a single case had been brought under his notice.

I witnessed several necropsies of cases of so-called typho-malarial fever, and, so far as the morbid appearances were concerned, they were strictly identical with those of ordinary enteric.

I am Sir, yours truly,

HENRY F. NORBURY, C.B.,
Fleet Surgeon R.N.

Plymouth, Feb. 18th, 1886.

INVERSION OF THE UTERUS.

To the Editor of THE LANCET.

SIR,—In THE LANCET for Dec. 26th, 1885, is a very interesting case by Dr. Arbuckle of the reduction of chronic inversion of the uterus by the combined method of incision of the cervix and sustained elastic pressure. Dr. Arbuckle does me the honour to cite me as the author of the treatment by incision. Dr. Neale in a subsequent number quotes his valuable "Digest" to show that "Dr. Pratt of Fife contributed a paper to the *Medical Times and Gazette* in 1868, in which he asserts that Dr. Wasseige of Liege is the sole originator of this most effectual plan." As I should be sorry to let it appear that I could in any way wish to appropriate merit due to others, I must beg of you to give me the opportunity of presenting the following explanation. I have not been able to find the memoir of Dr. Wasseige referred to by Dr. Pratt, nor do I find "Dr. Pratt

of Fife" now on the Register; so that I have no means of verifying the terms or the date of Dr. Wasseige's communication. Perhaps some of your readers may help me. But, in truth, the question of originality is not to be determined by reference to Dr. Wasseige or to Dr. Neale's "Digest." This work is almost limited to noting contributions to three medical journals, and cannot be accepted as an authority upon this or similar questions. If Dr. Neale had extended his researches beyond this range, he might have seen that I have not claimed priority in the idea of incising the cervix for the reduction of inversion. In my memoir in the *Medico-Chirurgical Transactions* (1869), after discussing the various methods in use, is the following passage:—"Another proceeding stands before amputation. For twenty years I have taught in my lectures that the unyielding cervix may be divided by incisions carried into its substance from above downwards at different points of its circumference. Pressure thus applied will cause it to yield more easily. Huguier, Professor Simpson, and Dr. Marion Sims have suggested the same plan. I am not aware that it had ever been carried into execution before 1868, when I treated a case in this manner with complete success." The case is narrated in the memoir. I published another successful case in the *Obstetrical Journal* (1873), and I have had since at least one more in St. George's Hospital. Other cases are recorded.

Aran's "Maladies de l'Utérus" (1858-60) has the following passage: "Reste à savoir, cependant s'il ne peut pas se trouver des cas dans lesquels la rigidité du col serait à son tour un obstacle à la réduction; je préférerais alors, je l'avoue, le débridement du col pratiqué avec prudence, à l'aide d'un bistouri boutonné, sur la commissure de l'orifice, voire même le débridement multiple." It is to be noted that Aran does not say he has done it, nor does he describe a case.

Thus, so far as my actual information goes, the case stands as I put it in 1869. Although I did not claim priority of publication of the plan of incising the cervix to facilitate reduction of inversion of the uterus, I did claim to have been, as I know, the first who had successfully practised it as an aid to reduction by sustained elastic pressure.

The most memorable discovery in the history of inversion of the uterus is that of Tyler Smith, who first (*Medico-Chirurgical Transactions*, 1858) successfully demonstrated the method of reduction by sustained elastic pressure, and who thus removed what had hitherto been an *opprobrium chirurgicorum*.

I am, Sir, yours truly,

Harley-street, Feb. 1886.

ROBERT BARNES.

THE TREATMENT OF DOG BITES.

To the Editor of THE LANCET.

SIR,—It has always seemed to me that it was beginning at the wrong end to pay so much attention to hydrophobia and entirely to overlook the treatment of dog bites. It is unnecessary now to plead the cause of the germ theory. Pasteur has shown that this disease is caused by germs, and that poisonous germs exist even in human saliva. The hydrophobia germs are inoculated by the teeth of the dog; why not, therefore, destroy the germs in the wound by some antiseptic? I do not speak against the method of counter-inoculation, or the wonderful skill and vast knowledge of the founder of the germ theory, but I advocate prevention instead of cure. The ordinary treatment of dog bites is unscientific. There is, first, the do-nothing treatment, based on the erroneous idea that free bleeding purifies the wound. Neither blood nor any other fluid could mechanically cleanse an inoculated ragged wound. I have noticed two cases treated in this way followed by hydrophobia. Secondly, applying mild caustics, as nitrate of silver. Though very fatal to the mould plant, it is not an antiseptic; moreover, as soon as it is applied to a wound there is a precipitation of the chloride, which is white, and silver, which is black, both of these being insoluble and inert. Thus, the only effect is to seal the germs in the deep part of the wound with a slight caustic action on the surface. This treatment is commonly followed by hydrophobia. Thirdly, immediate excision or application of some strong caustic, as fuming nitric acid. These being severe methods are not often resorted to.

The most rational treatment of a dog bite is to apply some germicide or antiseptic which will penetrate the tissues without coagulating them. The theory is perfect; therefore failure would only be due either to delay or insufficient