the army is to attract a number of young fellows of limited means into its commissioned ranks who are really desirous of making it their profession. It is quite right to throw the responsibility of checking all unnecessary extravagance upon the officer commanding regiments, and this the Commander-in-Chief very plainly intimates will be done in future. No one wants to see a lot of mere bookworms as officers, but the barrier of wealth should be removed from young men of physical and intellectual vigour with military tastes, predilections, and ambition.

Surgeon-General T. Tarrant, retired pay, late Army Medical Staff, to be an Honorary Physician to His Majesty, vice Surgeon-General A. H. Fraser, retired pay, deceased. Dated Sept. 9th. 1903.

Colonel P. F. O'Connor, C.B., I.M.S., who has been principal medical officer of the British forces in North China, has left Tientsin for India, and has been relieved by Lieutenant-Colonel L. A. Waddell, I.M.S.

The health of the troops in Somaliland is reported to be good.

It is reported that the 2nd Battalion of the King's Shropshire Light Infantry has been attacked by cholera at Ranikhet, Bengal, and that Lieutenant Carter and ten men have died from the disease.

Correspondence.

"Audi alteram partem."

WHAT IS SCIATICA?

To the Editors of THE LANCET.

SIRS,—I wish to thank you most heartily for your notice of my address on Sciatica which appeared in THE LANCET of August 22nd, p. 511. I esteem it a favour that you have given up so much space to the statement of my views in connexion with this complaint and I am greatly obliged for your courteous comments on my heterodox opinions as to the real nature of the pains in cases of sciatica. When I started the campaign against the received explanation of these symptoms I quite saw that I was, so to speak, flying in the face of settled beliefs—beliefs, let me add, unconsciously buttressed by the apparent results of treatment of an anti-neuralgic nature. May I say I fear that a good deal of our pathology is founded on this kind of inverted reasoning—that is, "because so-and-so does good then the disease is necessarily of a kind to correspond with the successful medicaments."

Returning to your leading article, will you allow me in self-defence to reply shortly to one or two points? First, you seem (but in this I may be mistaken) to pooh-pooh "references" as regards pain. But is not the majority of pains "referred pains"? All abdominal and many chest pains, for example, are allowed to be of that kind. Take the supreme pain of angina pectoris when the pain goes down the arm, is that merely an affection of the brachial plexus? Again, "tenderness on pressure," is there in the whole range of medicine a surer "sign" of the real situation of any disease than that elicited by careful exact pressure over the spot suspected? A third and most important criticism is where you say I deny that there is ever neuritis present. I do not think I quite affirmed that neuritis is altogether and always absent. Indeed, I do not believe it to be so. But I wished to throw down my humble gauntlet against the accepted pathology of sciatica and therefore I may have put my position a little too strongly. You will allow it is very difficult to prove the negative as against any proposition. May I ask my opponents to do me the honour to show that they have at least by careful exploration and examination tried to prove that my views are false and ill-founded.

I have to thank Mr. J. S. Mackintosh for his kind reference to my address. But I hope he will excuse me when I say that I do not see the *sequitur* in the case he gives of rheumatoid arthritis and sciatica. On his own showing the "sciatica" on the side which was not stretched equally recovered with that to which extension was applied. But, in the name of all that is reasonable, are not the sciatic pains sufficiently explained by such an irritative process as rheumatoid arthritis going on in those articulations? Surely such

deforming changes are decidedly inflammatory and thus likely to cause the symptoms which he classes as being due to "sciatica." I hope with your kind leave to publish by-and-by one or two cases caught in the intermediate stage between "sciatica" and "rheumatoid arthritis." It is all a question of names. If a cure takes place the disease is labeled sciatica, if it does not (as is too often the case, among the labouring classes especially) it is then designated "rheumatoid arthritis," both being merely stages of the same disease although set down at present in two distinct categories. I am sorry the term "barbarous" offends Mr. Mackintosh and others of my friends. But while I adhere to the word itself as the only one I know which expresses my meaning I do not at all affirm that the operations I refer to are unjustifiable. Many such proceedings, as in the days of Ambroise Paré, mark stages on the road of progress.

I am, Sirs, yours faithfully,

Sept. 1st, 1903.

W. BRUCE, M.D. Aberd.

MEDICAL DOCTRINES OF HEREDITY.

To the Editors of THE LANCET.

SIRS,—One might vary an ancient proverb, which says: "Wisdom is the principal thing: get wisdom, and with all thy getting get understanding," into "Logic is a good thing: get logic, and with all thy getting get common sense." As I did not feel justified in my letter to you of August 29th (p. 629), which dealt with the arguments of Dr. A. W. Gilchrist and Dr. J. Wigglesworth, in prolonging that letter so as to deal adequately with the dead-doing letter of Dr. C. Mercier, except to point out that his first objection was simply a piece of pedantry, I therefore ask a small part of your space so as to examine the reasoning of this most profound logician's examination of my reasoning. The first thought that must have occurred to everyone after finishing that logical (?) effusion would be to ask if a logician of the calibre of Dr. Mercier had ever heard of a fallacy called the Fallacy of Accent in which "the erroneous quoting of an author by unfairly separating a word from its context or unduly emphasising a word or italicising words which were not intended to be italicised gives rise to cases of this fallacy." Dr. Mercier disputes my statement, where he thinks I have fallen into the converse Fallacy of Accident, "that no two members of a litter of pups or kittens exactly resemble one another." He asks, "When did ever two kittens or puppies have the same number of legs or tails?" One would have thought that a man like Dr. Mercier, who has written so much on heredity, would at least have known what is meant by a character or trait. However, I suppose I must prove my statement. Permit me for purposes of this proof to make one or two quotations.

If under changing conditions of life, organic beings present individual differences in all parts of their structure, and this cannot be disputed, &c. (Darwin: Origin of Species, p. 160.)

Again, we have shown by abundant evidence, that the variation that occurs is very large in amount—usually reaching 10 or 20 and sometimes even 25 per cent. of the average size of the varying part; while not one or two only but from 5 to 10 per cent. of the specimens examined exhibit nearly as large an amount of variation. (Wallace: Darwinism, p. 81.)

Everyone knows that in each litter of kittens or of puppies no two are alike. Even in the case in which several are exactly alike in colours, other differences are usually perceptible to those who observe them closely. They will differ in size, in the proportions of their bodies and limbs, in the length and texture of their hairy covering, and notably in their disposition. They each possess, too, an individual countenance almost as varied when closely studied as that of a human being. (Wallace: Darwinism, p. 83.)

I suppose it is necessary to point out that these statements are not mere opinions but are based on exact observation and measurement. If Dr. Mercier will read "Domesticated Animals of the British Islands" by Professor Low, "Animals and Plants" by Darwin, and "Darwinism" (especially Chapters III. and IV.) by Wallace, he will not again be in such a hurry in denying the fact of variation or parading his acquaintance with the *fallacia a dicto secundum quid et dictum simpliciter*. Will Dr. Mercier tell us, or is he able to tell us, what cause or causes of variation I have ignored?

With regard to the Fallacy of the Consequent or *non sequitur* as well as the following one, the *petitio principii*, or begging the question, Dr. Mercier exhibits an acquaintance with the subject of heredity as incredible as his insistence on the transmission of acquired traits. As evolution can only proceed by one or both of two methods, by the transmission of inborn traits or of acquired traits from parent to offspring and by their accumulation in succeeding

generations, it is necessary in order to understand them to define two terms, the loose use of which has led Dr. Mercier into his amazing contribution. A *variation* or an *inborn* trait may be defined as one which arises in an organism in consequence of changes previously produced in the germ cell or cells from which the organism sprang; whereas an *acquired* trait may be defined as one arising in the body cells from the action of the environment. It is thus evident that the difference between the two is essentially one of origin. Variations are known to be transmissible; for example, I recently saw a child with a supernumerary little finger on each hand; the father, grandfather, and great-grandfather were each born with the same peculiarity. I know another family where each member has the second and third toes on each foot in their whole extent webbed and this is known to have been the case in the two preceding generations on the mother's side. Many, however, like Dr. Mercier, believe that acquired traits are also transmissible; but even supposing that they were so it would not account for all variations; for example, the supernumerary digits and webbed toes. Now, though a variation always implies a difference between an offspring and its parent an acquired trait does not, for they may both acquire the same thing. Admitting, however, for the sake of argument, that acquired traits are transmissible, the trait transmitted would produce in the germ cell not another acquirement but a variation, since the change would be in the germ cells. There is one possible way in which an acquirement could be transmitted and that is when a mother transmitted to her foetus the acquirement which she had acquired and that same trait arose in it. If, however, the acquirement is again transmitted it would be as a variation. It will also be obvious that males can only transmit variations, or, rather, can only transmit an acquirement as a variation. Remembering the above definitions, would Dr. Mercier now tell us where in the *petitio principii* I have taken the conclusion as one of the premisses. I am quite prepared to agree with Dr. Mercier "that very little of the logical faculty is sufficient for those who conduct the affairs of a nation." I also agree "that still less is needed to conduct a discussion on a scientific subject"; but it would seem that immeasurably less is needed for those who, on logical grounds, take exception to the reasoning of those who are parties to that discussion.

It must be remembered that neither Dr. G. Archdall Reid nor myself, nor anyone belonging to the Reid school, has ever alleged that parental drinking gives rise to filial drinking. What has been said is this, If the contention of the neo-Lamarckian school is right we will show what the logical conclusion of that contention must be. If a man by reason of his drinking transmits to his son a desire for drink it can only be by way of a variation, and variations, as we know, are transmissible and tend to increase generation after generation. Let us postulate, however, for the sake of argument a man in primitive times with no desire for alcohol. If he now drinks, if the neo-Lamarckian doctrine is correct, he will transmit the desire for alcohol that he has acquired to his son and this, as we have seen, can only be by way of a variation. The son would make a different start in life from the parent, for he would begin with a desire for alcohol, whereas the father did not. If this son now has a son he will transmit to him the desire for alcohol which he himself acquired from his parent, but not only that for the grandson will have an increase of desire, for he has the grandparent's desire plus that due to his own father's drinking, and this increase of desire going on generation after generation would lead to an enormous capacity to enjoy alcohol. Alcohol, as we know from the researches of Dr. J. J. Ridge among others, causes more deaths than any other agent and thus, if the neo-Lamarckian contention is driven to its logical conclusion, the race must become so drunken as at last to lead to its extinction. Instead, however, of postulating a man with no desire for alcohol we begin, as we know all men have begun, so far as we have any information, with a fierce desire for it, and if now to this fierce desire there be added an increase of desire due to parental drinking, the extinction of the race would be swifter in coming if not more certain. It is not, I suppose, necessary to labour the question with regard to filial insanity due to parental drinking, for the same reasoning is to be employed. Here again it would be a variation, &c. Dr. Mercier by his *non sequitur* alleges that the above reasoning is so loose that there is no cogency in it, for, as he says, I have neglected some

counteracting circumstances. Will Dr. Mercier tell us what these circumstances are? He must know them when he is able to say I have neglected them. Many, many beside myself, pained at the terrible cruelty of nature, would be glad of any way of escape from this horrible position into which we have been driven. Must we call to him in vain; he who knows; or must we, if no answer is given, conclude that no answer is possible?

With regard to the giraffe's neck and Dr. Mercier's "parity of reasoning," if it be true, as Dr. Mercier holds it to be true, that acquired traits are transmissible and that the long neck of the giraffe is due to former giraffes stretching their necks, I agree with Dr. Reid in "seeing no reason," if Dr. Mercier demands it, "why their necks should not grow as high as the cross of St. Paul's." If, however, the giraffe's long neck be not due to stretching and its accumulation and transmission, but is due to the Darwinian doctrine of selection, there is no reason why it should be longer than it is. It must be kept in mind that there is as great or greater tendency to retrogression as to evolution; also that evolution is not always an extension of the previous evolution: it may be reversed or it may be divergent; indeed, "evolution and progress are not synonymous terms. The survival of the fittest is not always a survival of the best or of the more highly organised. The environment is sometimes such that increase of fitness means degeneration of type and the animal and vegetable world show many instances of degeneration. One brilliant instance is that which has preserved the clue to the remote ancestry of the vertebrate type. The molluscoid ascidian rooted poly-like on the sea beach in shallow water has an embryonic history which shows that its ancestors had once seen better days, when they darted to and fro fish-like through the waves with the prophecy of a vertebrate skeleton within them. This is a marked case of degeneration. More often survival of the fittest simply preserves the type unchanged through long periods of time. But now and then under favourable circumstances it raises the type. At all events, whenever the type is raised it is through survival of the fittest, implying destruction of all save the fittest.¹"

With regard to the calf muscles and gravitation Dr. Reid has so convincingly and so ruthlessly brushed Dr. Mercier's objections aside as to leave nothing for any other one to say. And now as to the *ignoratio elenchi* or irrelevant conclusion. The statement was made that man's religious systems kept many sober. I replied, exactly so, and they also keep many celibate as well as making them barbarians at the same time. If Dr. Mercier can find a flaw in the reasoning I make him a present of it, *tra la*.

As Dr. Mercier has now had his fling, and as we shall all in the future admit his logical abilities, may I ask him to come to facts? If he can show the unreasonableness, the untenableness, of the position of the Reid school; if he can prove his own position and that of the school to which he belongs, he will be doing a greater service than he perhaps imagines. It was said at the Swansea meeting that the public was looking to the medical profession for guidance in this "horrible fatalism," as Dr. A. T. Schofield described it. How far that is true I do not know, but I do know that it would be more sensible to controvert the doctrines than simply to hold them up to execration.—I am, Sirs, yours faithfully,

Liverpool, Sept. 3rd, 1903.

C. R. NIVEN.

To the Editors of THE LANCET.

SIRS,—Curious coincidences sometimes occur. Yesterday (Sunday) morning I read through with great care the recent correspondence on alcoholism and disease and heredity. In the evening, at a church I had never before been in, the preacher took his text from the words, "The leprosy of Naaman shall cleave unto thee and thy seed for ever." Probably, I should think, to the surprise of his hearers, the preacher entered into the question of heredity and disease and the transmission of acquired characters, and did so in such a manner as showed that he had carefully studied the subject. I merely want to point out one aspect of the question which seemed to strike him. He stated that he had frequently come across cases of vice of one kind or another in which the only appeal which seemed to have any effect was the one which begged the sinner "for God's sake, if you care nothing for yourself, think of your children." Referring to the modern teaching on the matter, he said that if it were definitely proved that there was no such thing as

¹ Fiske: Through Nature to God, p. 81.

transmission of disease or alcoholism from parent to offspring one of the most powerful incentives to health and sobriety would be removed.

I think it is of interest sometimes to note how scientific and medical questions affect the thoughtful men of other professions, especially the clergy, and therefore I mention this.

I am, Sirs, yours faithfully,

Sept. 7th, 1903.

M.D. EDIN.

To the Editors of THE LANCET.

SIRS,—May I take from your columns an illustration of the importance of a study of the problems of heredity to the medical profession? In a review¹ of a work on consumption it is written: "He (the author) quotes the statistics of Dr. J. Edward Squire which show that the difference in the incidence of the disease in the offspring of tuberculous and non-tuberculous parents is very small. Dr. Latham remarks that these figures are remarkable, as the children of tuberculous parents who are the victims of active disease must in most cases, unless the necessary precautions are taken, be more exposed to the risk of infection. The statistics would appear to suggest, he says, that the children of tuberculous parents possess some natural immunity to the disease, for otherwise the percentage of children who develop tuberculosis should be much greater when the parents suffer from the disease than when they do not." I beg leave to say that in the light of a study of heredity these statistics are in no way remarkable, nor is there need for the curiously unscientific "shot" offered in explanation—unless, indeed, one is prepared to deny natural selection. Here in this country of Galileo and Bruno there are those who know not the Copernican theory—shall Darwin escape? Permit me to justify my statement as briefly as possible. Once more we start from the fact that races are resistant to a disease strictly in proportion to their past experience of it. Tuberculosis by weeding out the weak against it during "unnumbered generations" has left in England a race comparatively resistant to the disease. No immunity is *acquired* against tuberculosis by the individual, but as a race we can flourish in conditions which would decimate another race to which phthisis was a new disease. Until quite recently at least, the stringency of phthisis selection has constantly grown, but has been met by as steady a growth of race resistance through disease selection. The weak being eliminated, the strong against tuberculosis have survived and propagated the race. The factors have not, of course, worked equally throughout the country; in the Scots Highlands, for instance, the rate has for obvious reasons been slower, but the general effect is that we have lived, in regard to phthisis, under conditions which would have been impossible to our remote ancestors.

Contrast this view of our present evolution with the tragic story of the introduction of phthisis into the New World amongst races which had no knowledge of it and had undergone no evolution against it. They were suddenly invaded by races who suffered from the disease but had a high resisting power, and who carried with them the conditions of life prevailing in their own land. There was scarcely time for evolution. The bacillus revelled in the virgin soil—whole races were wiped out. Practically every individual attacked was found of low resisting power and succumbed. In England, then, the great majority of people are immune to tuberculosis nowadays under practically all conditions. They may live crowded in dark, ill-ventilated rookeries; their daily exercise may be only a ride in a subterranean railway to and from a sedentary occupation; they may be in ill-health, associate with phthisical invalids, be drunken and depraved, yet only a small minority contract the disease—at least in a diagnosable form. Those who do contract it when weakened by other diseases or depressed by some powerful cause tend to recover, and to-day such cases stand to the credit of many a sanatorium.

In the light, then, of analogy and of a study of the history of disease as it affects races the British islanders are progressing rapidly through stringent selection to a resisting power which might make them completely immune to tuberculosis and lead to the dying out of the bacillus from their midst—a slow and sorrowful process, but nature's, and therefore sure. But man has checked nature. Man himself has begun to relax the stringency of the selection. By means of improved and constantly improving sanitation, active "preventive treatment" of those who spring from

tuberculous parents, and by admirable curative measures employed on those whose resisting powers are not too low, the stringency abates and many of the weak who would formerly have perished now survive. And here biology supplies us with an illuminating fact—that cessation of selection is invariably followed by reversion to the ancestral type. Therefore, although man's well-meant interference is beyond doubt a benefit to the individual, it is providing for succeeding generations an increasingly lower power of resistance against the disease. It is unnecessary to follow here with speculations as to the remedy. On the one hand there appears the hope—a vain one to some thinkers—of the absolute banishment of the tubercle bacillus through perfect sanitation. On the other hand stands artificial selection—abstention from procreation on the part of those with weak resistance, with the "tuberculous constitution"—to other thinkers a wicked suggestion. With these data, imperfectly given I fear, it is possible to arrive at a solution of the problem which Dr. Latham finds so puzzling. "The difference in the incidence of the disease in the offspring of tuberculous and non-tuberculous parents is very small." The child of the tuberculous parent contracts the disease because he inherits a weak resisting power and is often placed in an environment favourable to the development of the disease. Possibly the numbers of these children are reduced somewhat by the carrying out of "preventive measures." But, on the other hand, the ranks of the children born of "non-tuberculous" parents who contract the disease are greatly increased owing to the fact that man's interference is raising an army of *apparently* non-tuberculous parents—i.e., of persons who have not suffered from the disease but who, nevertheless, possess and hand on weak powers of resistance. Thus, then, while a few generations ago "tuberculous parents" meant as now only those who had themselves suffered from the disease, "non-tuberculous parents" meant only those who had escaped the disease solely through their high resisting powers. To-day the "non-tuberculous" parents include also parents who have escaped the disease through the sanitary and other efforts of man and are really of tuberculous stock. Obviously a few generations ago statistics should have proved a very great difference in the incidence of the disease in the offspring of tuberculous and non-tuberculous parents, and obviously again the enrolment of the "apparently" non-tuberculous amongst the truly non-tuberculous parents has greatly reduced that difference. Therefore Dr. Squire's figures are neither so surprising nor so inexplicable as they appear at first sight. I think this explanation is more in accordance with the facts of natural selection and of the natural history of disease than the somewhat topsy-turvy one that "the children of tuberculous parents possess some natural immunity to the disease"; certainly it seems to me in accordance with the few facts known concerning heredity.

May I, in conclusion, ask how long the facts of heredity are to continue neglected by those who have the ordering of our medical curriculum? Hydropathy and hypnotism, the long despised, are now recognised; but (is the letter "H" unfortunate?) heredity, while the mere term is used in text-books as a cloak to ignorance, is passed by in contempt as a science. I cannot think this attitude is a credit to the profession, and certainly it results in a dead loss to our knowledge—a serious loss to our patients. Not long ago a gentleman possessing the qualifications of M.D. Lond., F.R.C.P. remarked to me, "Heredity! Lord, I never studied *that*!" And only a few weeks ago another gentleman in public debate condemned Dr. Archdall Reid's well-known views as the revival of an old fatalistic theory, altogether shocking! And now we have a text-book on Consumption coming before the public and advancing this "suggestion" (suggestions in modern "science" speedily become beliefs) without, as far as I can discover from my imperfect study of the subject, any ground whatever amongst the facts of inheritance. No body of men stands in so favourable a position for adding to the facts of inheritance as the medical profession; and yet we are permitted to receive our degrees and qualifications without a smattering of the elements of the science. It is a subject on which no one can at present be dogmatic and on safe ground at the same time—the views I have given are, of course, open to correction and, it is unnecessary to state, are not original—and yet in the text-book, in the lecture-room, in the sectional meeting, in the consulting-room even, and the bedroom dogmatism of the barest description, in the absence of knowledge, is rampant. There must be many who, like myself,

¹ THE LANCET, August 22nd, 1903, p. 532.

regret the want of teaching on the subject in university days, who like myself dislike our present ignorance and look forward to more enlightened days. To such, and especially to those who are in touch with our teachers and the directors of our curricula, I appeal to put an end to the present unfortunate state of affairs.

I am, Sirs, yours faithfully,
 Florence, Italy, August 25th, 1903. H. LAING GORDON.

To the Editors of THE LANCET.

SIRS,—Before this discussion dies down and the dust "is lightly laid ag'n" it would be interesting to know whether Dr. G. Archdall Reid has any practical suggestion whatever to make concerning the prevention of drunkenness.

I am, Sirs, yours faithfully,
 Mossley, Sept. 1st, 1903. SIDNEY VINES.

SURRA IN THE PHILIPPINE ISLANDS.

To the Editors of THE LANCET.

SIRS,—With reference to your annotation on p. 553 of THE LANCET of August 22nd concerning the occurrence of the horse trypanosoma in rats and the communication of surra to horses by insects, &c., in Manila, I would invite reference to the valuable reports by Dr. Alfred Lingard, imperial bacteriologist of India, who worked out with great thoroughness the conditions of the surra disease in the Bombay Presidency. The connexion of rats and insects with the carriage of the infection was fully dealt with. The volumes containing Dr. Lingard's reports can doubtless be obtained through the India Office.

I am, Sirs, yours faithfully,
 EDWARD C. BUCK,
 Late Secretary, Government of India Agricultural Department.
 Sept. 3rd, 1903.

THE CHRISTIAN HOSPITAL OF CHICAGO.

To the Editors of THE LANCET.

SIRS,—I desire to call your attention to the annotation concerning the "Christian Hospital" of Chicago on p. 416 of THE LANCET of August 8th, in which I feel I have been unjustly treated. I am somewhat surprised to find in the issue of a journal as late as August 8th an article giving credence to the statement that my name was used with my permission in connexion with this nefarious swindle. The affair occurred on May 12th and almost every American journal of prominence has treated the matter editorially and in communications, showing the outrage that has been perpetrated on me. I inclose a circular letter and clippings from the daily papers of Chicago which also show the steps which have been taken to secure the conviction of these criminals. I hope that you will make this matter clear in as forceful a manner as your annotation of the 8th August condemns me. I feel that you would not knowingly do me an injustice and I am sure that when you understand the details you will realise that my position has not been a pleasant one, as something like 80,000 of these circulars were sent out to the members of the American medical profession. The publicity which was given them by the lay and medical press aborted the scheme as a money-making project; therefore the perpetrators, in the hope of securing leniency in court, have returned the money to all who purchased their certificates.

I would say, for your further information, that the matter of the division of fees was taken up by the Chicago Medical Society and condemned before it was taken up by the American Medical Association, and I was one of the strong factors in securing the ruling against the division of fees in that society and in bringing the matter before the association to have it included in the principles of medical ethics. It seems to me that if you had seen the original circulars you could not help but recognise that it was not only fraudulent to the profession but an outrage on my name. The perpetrators of the scheme are now held to the grand jury and we hope to succeed in convicting them at the October session. I would refer you to articles as follows: *Journal of the American Medical Association*, May 23rd, p. 1443, and June 6th, pp. 1586 and 1591; *Medical Record*, June 13th, p. 946; *Medical Century*, August 1st, p. 252; *Texas Medical News*, July, p. 467; *American Medicine*, July 18th, p. 85;

or to any other reputable American journal that may be at your disposal.

Very truly yours,
 Chicago, August 29th, 1903. J. B. MURPHY.

* * We had not any idea who the medical man in question was, as no names were mentioned to us. We were perfectly aware that the Christian Hospital was an unprofessional institution, if nothing worse, and alluded to it in terms that showed our opinion of it. The extracts from Chicago papers sent us by Dr. J. B. Murphy show that his name has been used in a monstrous manner by unprincipled persons, against whom he has invoked the protection of the law. He has our most sincere sympathy. We did not see the original circulars.—ED. L.

INFANTILE AND BOVINE TUBERCULOSIS.

To the Editors of THE LANCET.

SIRS,—Dr. Nathan Raw's interesting paper is a valuable contribution to the subject of infantile and bovine tuberculosis as regards his own observations and experience. His references, however, to the Registrar-General's statistics are not quite up to date. If Dr. Raw had examined the more recent figures of the Registrar-General he would have found that although these are opposed to the often quoted statement of the late Sir Richard Thorne they do in effect offer a confirmation of his main contention that bovine tuberculosis is the principal cause of abdominal tuberculosis in children. In England and Wales the deaths from tabes mesenterica per 1,000,000 of the population increased up to 1880 but since then have steadily diminished from 290 in the five years 1881 to 1885 to 196 in the period 1896 to 1900, the last quinquennium showing two-thirds of the whole decrease. The annual summary of the Registrar-General for 1902 shows that in London the deaths from tuberculous peritonitis and tabes mesenterica have diminished by one-half since 1892—viz., from 1298 in 1892 to 661 in 1902. During the same period there has been a diminution, but a much smaller one, in the deaths from tuberculous meningitis. The latter seem to follow very closely the deaths from pulmonary tuberculosis, increasing and diminishing as the latter rise and fall. I am informed by the medical officer of the infirmary of Woolwich union and other local medical practitioners that they have observed a very great reduction in abdominal tuberculosis in children during recent years. We see thus that whatever was the case in former years tuberculous peritonitis is now diminishing much more rapidly than pulmonary tuberculosis, especially in London. Surely this is what would be expected on the theory that bovine tuberculosis was the chief source of abdominal tuberculosis in children.

Great efforts have been made by many sanitary authorities in recent years to obtain a milk-supply free from tubercle bacilli and cows with tuberculous udders have been almost eliminated from London cowsheds by means of the County Council's efforts. In the borough of Woolwich and probably in many other towns milk coming from the country is now comparatively free from tubercle bacilli. I do not know what are the figures that Dr. Raw refers to as the percentage of cows in London cowsheds infected with tubercle, but they are opposed to the report of the medical officer of health. This report for 1901 shows that out of over 4000 cows in these sheds on the last inspection only seven were infected with, or were suspected to have, tuberculous disease of the udder and the sale of milk from the seven suspected cows was prohibited. No doubt a large number of cases of tabes mesenterica are not really tuberculosis, but I know of no reason to suppose that anything has happened during the last ten years, 1892–1902, seriously to affect the proportion of correct to incorrect medical diagnosis in this respect. I think, therefore, that one is justified in saying that the marked decrease in the total deaths from abdominal tuberculosis and tabes mesenterica indicates a corresponding diminution in abdominal tuberculosis and that this decrease is probably chiefly due to the elimination of milch cows with tuberculous udders and partly, no doubt, to the increasing practice of sterilising milk before consumption. At the present time I find that direct infection (usually from parents) in this borough is the most frequent source of all forms of tuberculosis in children.

I am, Sirs, yours faithfully,
 Woolwich, Sept. 1st, 1903. SIDNEY DAVIES.