

tributed by the Mussulman population of India. In addition to sending extensive supplies of hospital stores to the armies in the field, it is constructing two large hospitals, one on the Bosphorus, and the other on the Dardanelles.

Correspondence.

"Audi alteram partem."

"HOSPITAL MORTALITY."

To the Editor of THE LANCET.

SIR,—Before closing your columns to the correspondence on "Hospital Mortality," will you allow me space to offer a few personal explanations which I would have done before had I not been absent on my usual holiday? With the remarks in your leading article I have no fault to find; they set forth many of the numerous difficulties which beset the question of hospital death-rate when it is employed by incompetent observers as a test of the sanitary condition or unhealthiness of a hospital; but I protest strongly at the construction which some of your correspondents have sought to put on my figures, and in fabricating statistical fallacies from them, without having consulted the original work. Had Dr. Greenfield, in his laudable attempt to uphold the credit of St. Thomas's Hospital, by accounting for its exceptionally high mortality, only taken the trouble to refer to my tables, and the remarks appended to them, he would have found that my experience closely coincided with his own, and that so far from assuming that a high death-rate involved an element of censure in the vital activity of a hospital, there are numerous reasons why it should be looked on as a proof of its satisfactory management. In the case of St. Thomas's Hospital the high death-rate is partly explained by the fact of the new hospital having broken fresh ground in its recent exodus to Lambeth, where the demand for its services happens to be greater than its restricted accommodation can supply, though I believe the main factor may be traced to the small number of beds used for ophthalmic, venereal, and other diseases attended with a low rate of mortality, when compared with the corresponding accommodation at Guy's and St. Bartholomew's, and at some other hospitals. When Dr. Greenfield charges me with misrepresenting the number of beds at the disposal of St. Thomas's, I have an equally strong ground of complaint. The column in my table from which you have deduced the relative hospital activity purports to give only the number of available beds in each of the institutions referred to, and not the number of beds in continuous occupation. Now, everyone acquainted with hospital matters in London knows that the new St. Thomas's was built to accommodate 600 patients, and estimates which are freely made as to the cost of the building are invariably based on this supposition; but lest exception should be taken to the number, I made the necessary inquiries in official quarters, and found that the total number of beds did not exceed 573, which I was bound to credit the hospital with in my table. By reducing this number to 400, or rather to 370, Dr. Greenfield alters materially the basis on which your estimates of relative activity have been made, and consequently invalidates the whole inquiry. At the same time it would be easy to show that the beds in Guy's or Bartholomew's are nearly if not quite 100 in excess of those in constant occupation, but although vacated for sanitary and for other equally important reasons, they are not the less available should any contingency arise to require their employment. But in truth estimates of hospital activity, though important in the highest degree, must be received with the greatest caution, since no two institutions are governed by the same well defined laws, while each preserves some special characteristics dependent on a variety of causes, which it would be necessary to specify in detail, in order to arrive at even an approximate estimate of their value.

For these reasons, notwithstanding the strong temptation to the contrary, I have studiously avoided any reference to the question so freely discussed in your columns, preferring rather a simple, consecutive record of facts obtained

from trustworthy sources, to "the masses of crude figures" from which Dr. Greenfield has chosen to construct his statistical fallacies, and Mr. McGill, F.R.C.S., to convert into a ridiculous, if not mischievous, indictment against our improved systems of nursing the sick. As some doubt has been thrown on the accuracy of these figures as regards St. Thomas's and Bartholomew's, I may be allowed to state that the tables have been carefully prepared from the official records of both hospitals, and I embrace the opportunity of thanking Mr. Walker and Mr. Cross for their courtesy in allowing me to have free access to them. The tables relating to St. Thomas's Hospital are, I think, especially valuable, as they comprise a consecutive series of annual returns dating as far back as 1734, with a sexual mortality from 1786, and a medical and surgical mortality for the past twenty-six years. Although it appears probable that many of my conclusions are at variance with Mr. Tait's (for I have not yet had the pleasure of reading his book), I fully endorse his protest against being subjected to criticism by parties who have failed to consult his work, and have contented themselves with a few stray figures, which may be construed to mean anything or nothing.

I am, Sir, yours, &c.,

Guy's Hospital, 21st August, 1877.

J. C. STEELE.

To the Editor of THE LANCET.

SIR,—In a letter in your columns last week, Mr. Lawson Tait challenges me to substantiate my statement that a large proportion of what he considers excessive hospital mortality is unavoidable. It will give me great pleasure to do so when I have had time to collate the facts and you will allow me a few spare pages of your journal for the purpose of stating them. At present I wish only to observe that my statement as to the mortality in large hospitals was grounded upon personal observation, not merely on figures from reports, and that they referred to *total* mortality, and not, as Mr. Tait seems to have concluded, to deaths after amputation only.

I must apologise to Mr. Tait if I have in any way misrepresented or misstated the character of his book, but I need hardly state that I should not have ventured to criticise it without reading enough of it to form a fair estimate of its contents. The tables to which I made reference as "masses of crude figures" (or "crude masses of figures," if Mr. Tait prefers), and to which in the main my strictures were intended to apply, occupy twenty-six pages near the beginning of the book, (p. 17, *et seq.*) To these tables, I must again contend, my remarks are strictly applicable, nor has Mr. Tait given any adequate defence of them. On the contrary, he accepts my view "that mortality statistics, to be of any value, must be grounded upon the comparison of a series of cases of a similar nature." Since he recognises the valuelessness of these tables, it is to be regretted that he should have seen fit to publish them, and should still make statements grounded upon them, such as that "these results are found to coincide with a death-rate in the total patients passing through its [i.e. St. Thomas's] wards nearly double that of Leeds."

There is one sentence in Mr. Tait's book which makes me fear that any effort to convince him by reasoning or demonstration will be hopeless, and since, although you have challenged him for proof, he still holds to the accuracy of his view, I scarcely hope to convince him that he is in error. Speaking of St. Bartholomew's and St. Thomas's he says, "*a priori* there seems no reason why there should be such a great difference in the death-rates of the two hospitals. If the death-rate of St. Thomas's was as low as that of St. Bartholomew's, 220 valuable lives would be saved every year." Now if this is anything but a rhetorical flourish, and I take it that Mr. Tait means it literally, he asserts that about 220 patients die every year in St. Thomas's from preventable causes, and their deaths are chargeable either to neglect, maltreatment, or to "hospitalism." Now, let us see what this means by a reference to facts. In the year 1873, as Mr. Tait will see by referring to the St. Thomas's Hospital Reports for 1874, there were 174 deaths in the medical wards, and 151 in the surgical wards, making a total of 325. If we subtract from these

the 220 preventable deaths, there remain 105 to be put down as due to natural causes, inevitable in fact. It is to be presumed that Mr. Tait allows that certain diseases are of necessity ultimately fatal. It is not necessary to take diseases which are absolutely fatal in a short space, as to do so would load your columns with figures. But I will take diseases which are either necessarily fatal, or of which there are sufficient details given in the report to enable Mr. Tait to judge of the prospects of recovery. He will find that in the medical wards alone there died of

Meningitis, cerebral hæmorrhage, and tumour	19
Phtthisis	23
Heart disease	35
Bright's disease	25
Cancerous and other malignant growths	7

Total 109

As of nearly every individual case of these diseases some details are given in the Reports, it will be easy to show whether my statement is accurate that these deaths were unavoidable. Hence the astonishing conclusion that every death in the surgical wards during that year was preventable, and nearly 40 per cent. of those in the medical wards. Or, in other words, that fractures of the skull, smashes, wounds, and operations of all kinds, cancer, &c., are curable if only "hospitalism" is excluded. Until Mr. Tait withdraws this statement he cannot expect that anyone will attach much value to his "facts" or "arguments," or consider them worthy of refutation.

I will now refer to the only tables in Mr. Tait's book which can pretend to any scientific value—viz., those towards the end, entitled "Amputation Mortality." I shall not enter into the question of the mortality after amputations at St. Thomas's Hospital, for upon that point I cannot speak from experience. I will merely point out that if one-half of those operated on primarily for accident every year died, there would be just 4·8 deaths a year from that cause, if Mr. Tait's tables are correct. And yet this is the main ground on which he founds his serious charges against St. Thomas's. Why does not Mr. Tait take some other serious operation, such as excision of the knee, for comparison. In the years 1872-4 inclusive 53 such excisions are stated to have been done, 14 with a fatal result, 2 of them from general tuberculosis. Still, taking the whole, the mortality is only 26·4 per cent.

I shall not here further discuss this subject, but point out one or two sources of fallacy which Mr. Tait has overlooked. In comparing the results of any operation, it is surely desirable that all source of error should be, as far as possible, eliminated. Now, in comparing the results from the eight hospitals given in the table, one fact is very striking—viz., that the highest mortality is in hospitals which have the smallest number of operations. Did it ever occur to Mr. Tait that the constitution and age of the patients have something to do with the result of an operation? In a manufacturing town, such as Mr. Tait states Leeds to be, and where the number of primary amputations is very large, a considerable number must be upon young and healthy persons injured by machinery accidents, whereas in London many of the accidents occur either in intemperate persons, often as a result of intemperance, or in the feeble and aged. Moreover, the margin of chances of recovery evidently rises with the larger number of cases taken. But let me give an instance from another disease, which shows how great care is needed before attributing to "hospitalism" a high mortuary rate.

In 1873, 48 cases of enteric fever were under treatment in St. Thomas's Hospital, of whom 2 died, or 4·16 per cent.; in 1874, 39 cases, of whom 9 died, or 23 per cent. Now these cases were nearly all under my personal observation; the care and treatment bestowed upon them were the same, and so were the wards. I have, in the Report for 1875, endeavoured to show the grounds of this difference, and will not here detail them, only observing that they depended on the condition of the patients and of their admission to the hospital. Now let us suppose that instead of in St. Thomas's the cases during 1873 had been treated, say in Leeds Infirmary. The argument drawn on Mr. Tait's model would run thus: "The mortality from enteric fever in Leeds Infirmary is only just over 4 per cent., whilst in St. Thomas's it is 23 per cent., so that if 500 cases of the disease were treated in the former hospital 20 would die, in

the latter 115. But the mortality in enteric fever at Leeds shows it to be a highly curable disease, and the fatal results in St. Thomas's must be due, therefore, either to hospital defects, or to neglect and malpraxis. And seeing that a large number of other diseases are equally or more curable, we must assume that 7 out of every 10 patients who die in St. Thomas's die from similar preventable causes." Yet here there is no mixing up of different classes of disease under different conditions to disturb the comparison.

I must apologise for occupying so much of your space in criticising statements which are hardly worth serious attention, were it not for the gravity of the reckless charges which they contain. The whole subject of hospital mortality has been very fully discussed years ago, when Dr. Farr, Dr. Richardson, and Miss Nightingale, made charges equally grave, and supported by better arguments than those of Mr. Lawson Tait. Those who are interested in the subject will find both in your columns and in those of the *Medical Times* of 1864, a discussion between Dr. Farr and Dr. B. W. Richardson on the one side, and Dr. Bristowe and Mr. Holmes on the other. The controversy was terminated by a masterly letter from Dr. Bristowe in the *Medical Times* for April 30th, 1864, to which Dr. Farr seems never to have ventured a reply. Had Mr. Lawson Tait really read and mastered this correspondence, and the Report on Hospitals by Dr. Bristowe and Mr. T. Holmes, to which he makes frequent reference, he would not have wasted his time in compiling a mass of useless tables, and making ingenious speculations on them.

In conclusion, let me point out that there is one method by which some valuable conclusions may be gained as to hospital hygiene. It is to show, in individual cases, that the death was due to preventable causes. Let each case of a particular disease or injury be separately considered, all disturbing causes, as shown by the symptoms and the post-mortem, be eliminated, and let it be proved that of such cases a number die under given circumstances who should recover, and a serious ground for urgent attention and reform will be established. The materials for this analysis exist to a large extent in the annual reports of the larger London hospitals, and would, I believe, be willingly supplemented by the hospital authorities if the task were undertaken in an impartial and scientific spirit.

I am, Sir, yours very truly,

Wimpole-street, August 20th, 1877.

W. S. GREENFIELD.

To the Editor of THE LANCET.

SIR,—I am obliged again to correct an error into which you have fallen from your not having considered the headings of my columns sufficiently closely with the context. The figures, taken from Churchill's Directory, are confined entirely to the number of beds which the authorities of each hospital stated it to possess. I employed them in order to arrange the hospitals according to size, and also to display the remarkable tendency to exaggeration which hospital management seems to induce. In no other direction am I content with the figures in the Directory.

I never asked for the reports of St. Bartholomew's Hospital, because I had them, and therefore the name of that hospital has not an asterisk before it. But you are not entitled to say that is amongst the institutions marked as those from which no reports were received, and to presume from that that its reports were unknown to me.

Your method of expressing the "activity" is entirely fallacious, because it is based upon not only an unknown but an assumed, and, in many instances, an absolutely false quantity. My method of calculating it as the daily population of each hospital throughout the year is the only one which will satisfy the statistician.

I am, &c.,

August 20th, 1877.

LAWSON TAIT.

To the Editor of THE LANCET.

SIR,—As Dr. Bristowe has, in THE LANCET of to-day, misrepresented—no doubt, unintentionally—my letter which appeared in your issue of August 4th, I should feel obliged if you would allow me space for a few words in reply. Dr. Bristowe says that I assume King's College, University College, and St. Thomas's Hospitals are the most unhealthy o

the metropolitan hospitals. If he refers to my letter, he will find that I assume nothing, but state the fact that these hospitals have the highest mortality. He next states that the nursing at St. Thomas's is not conducted on the Nightingale plan ("system" would, perhaps, have been a better word); but, as he also professes ignorance of what that system is, it would be advisable for him to study Miss Nightingale's most excellent work on the subject before venturing on a statement on a matter upon which he is apparently ill-informed. Again, he makes it appear that I think that good nursing in hospitals tends to raise their mortality, whereas I rather inferred that bad nursing raised, while good nursing lowered, the death-rate.

Dr. Bristowe and Dr. Greenfield both agree that the exceptionally high death-rate at St. Thomas's can be accounted for by the small size of the hospital. It almost makes one smile to find that a hospital with 576 beds, 400 of which are in constant use, is termed small, and placed in the same category as regards size with hospitals with 172 and 154 beds respectively. "Statistics are," as Dr. Bristowe says, "wonderful things, and prove a great deal, but I think that most people will require more proof than he adduces to convince them" that the high death-rate at St. Thomas's is due to the small number of beds which the staff have at their disposal. If it be true the death-rate ought to vary inversely as the size, why should University College, with 154 beds, have a mortality of only 11.46, while St. Thomas's with 400 reaches to 11.73? My explanation of the high mortality may be wrong, very probably it is, but it is not so palpably absurd as that adopted by Dr. Bristowe. Perhaps on further consideration he will be able to discover some other cause for a state of things which I still consider eminently unsatisfactory.

I am, Sir, your obedient servant,

Park-square, Leeds, August 11th, 1877.

A. F. MCGILL.

ON THE CONNEXION BETWEEN MEASLES AND RÖTHELN.

To the Editor of THE LANCET.

SIR,—My friend, Dr. Sharkey, published in your columns a few weeks since, a striking case which occurred in my practice, showing the non-identity of chicken-pox and small-pox. Dr. Farquharson, whose paper, read before the Clinical Society, induced Dr. Sharkey to write to you, acknowledged in your issue of the 21st ult. the importance of Dr. Sharkey's case in reference to the question then under dispute, but incidentally maintains in his letter that there is a similar identity between roseola, or spurious measles or rötheln, and true measles, to that which he still seems inclined to think exists between varicella and small-pox.

I freely acknowledge the very great resemblance there is between measles and rötheln, and that I would by no means undertake in every instance to distinguish a case of the one disease from a case of the other; but close resemblance does not necessarily imply identity, and, for my own part, having seen a great deal of both diseases, I have no hesitation in ranging myself with those who maintain their absolute distinctness from one another, and, in confirmation of my opinion, beg leave to lay before your readers the following two narratives:—

(1) A little more than nine years ago, all my children then born, seven in number, had measles, which was introduced among them by my eldest boy, who had contracted it at a public school to which I was physician, and where I had therefore full opportunity of observing the course of the epidemic. Four years ago this summer, while at the seaside, my eldest two daughters were attacked, within a week of one another, with what I regarded as rötheln in a well-marked form. It did not spread from them to any other of my children, although at that time two others had been added to their number, and these two had never had measles. About fifteen months ago, one of the two children who had not hitherto had measles, and who had been exposed to the contagion of rötheln, were brought into relation with the poison of measles and contracted the disease.

(2) Three years ago last Christmas, five of the children of a near relative of mine had measles in a characteristic

form. In the following March, the resident governess, who had been exposed to the contagion of measles at Christmas, but had not become affected by it, had a well-marked attack of rötheln, and a week or ten days afterwards one of the children, who had had measles three months previously, had also an attack of rötheln. The disease did not extend. I attended the family on both occasions.

These narratives seem to show two things—namely: 1st, that rötheln and measles are not mutually protective, as they should be if they were mere modifications of the same disease; and, 2nd, that rötheln is not nearly so contagious as measles. I may add that I have witnessed many other outbreaks of rötheln besides the above, and that the distribution of the disease has, as far as I could learn, in no case been influenced by the previous occurrence of measles.

It seems to me that the grounds for maintaining the non-identity of measles and rötheln and of small-pox and chicken-pox, respectively, are quite as strong as those on which we maintain the non-identity of measles and scarlet fever, which were confounded with one another by the physicians of the last century; or as those on which we assert the non-identity of typhus and enteric fever—diseases which Dr. Stokes still contends to be one and the same. The mere fact that sporadic cases of one infectious disease are met with during the epidemic prevalence of another infectious disease is no more a proof of their common origin, than the fact of patches of oats or barley being discovered in a field of wheat is a proof that those cereals all spring from the same seed. Dr. Farquharson must therefore forgive me if I put another interpretation than his upon the statement made in his letter, that "roseola, or spurious measles, will reproduce itself in its own kind fifty or sixty times; and the sixty-first may, as I have seen, turn out to be a true case of the major disease."

I am, Sir, your obedient servant,

Old Burlington-street, August 11th, 1877.

J. S. BRISTOWE.

"A POCKET INSULATED SURFACE THERMOMETER."

To the Editor of THE LANCET.

SIR,—Allow me to state in your columns that the surface thermometer, described in THE LANCET of August 11th, is, in its essential feature—i.e., the spiral bulb partly surrounded by a metallic cup—identical with the surface thermometer devised by myself some six years ago, and since then frequently used by me in the wards of the Westminster Hospital.

The thermometer has also been manufactured and sold by Mr. T. Hawksley, 300, Oxford street, for several years past. It has been exhibited by him at the last of the annual International Exhibitions held at South Kensington, at the exhibition of scientific instruments at the same place last summer, and on various other occasions.

On applying this thermometer to surfaces of known temperature within the range of temperature of the human body, I found that, after five minutes' contact, the temperature indicated by the thermometer is, within from a quarter to one-fifth of a degree Fahrenheit, the correct temperature of the surface.

Yours, &c.,

Westminster Hospital, August 21st, 1877.

A. DUPRÉ.

We have communicated with Dr. Mortimer Granville, and the following is his reply:—

To the Editor of THE LANCET.

SIR,—I am obliged for the private communication with respect to Dr. Dupré's letter which is to appear in THE LANCET this week, and for the sight of his instrument.

It is obvious that Dr. Dupré has anticipated me in the adoption of the principle common to both instruments. The great feature in which mine differs from his—and this, I think, makes all the difference between a "pocket" thermometer and one which can scarcely be so designated—is that I have suspended the glass tube by an india-rubber washer, and arranged the register horizontally, so that the apparatus packs in a very small compass. The ordinary difficulties attending the use of a horizontal scale have been surmounted, and the makers guarantee the perfect accuracy