and not clearly expressed. Nevertheless, the views which have just been detailed are certainly to be found there. Thus, "it appears to us that in endeavouring to point out the most prudent course to pursue, it is necessary to distinguish between different cases. When typhus fever is so prevalent that if it were indiscriminately admitted the wards would become crowded with it, it appears that even the best-constructed hospitals will be poisoned by it and become unfit for the purposes of a general hospital; otherwise, that is when the proportion of cases of fever to general cases is small, there seems no great risk in admitting them, if the hospital is well ventilated, and the beds a sufficient distance apart, and if patients who are well enough to go about are carefully kept away from the fever beds." (Report, p. 471.) And again, "But that the hospital should have the power of treating acute disease is the first requisite, and for this it is necessary that no rule should exist excluding fevers. Whether the cases of fever ought when admitted to be isolated or scattered is a question on which the opinion of those who have the best right to express one appears to vary very much. Our own impression, from what we have seen at the various hospitals visited, inclines us to believe that scattering the cases is the safest course if the hospital be spacious and the applications not too numerous; and in contrary circumstances that they should be placed in separate wards....." (p. 473.)

I think it will be seen from what precedes that the difference between Dr. Anstie's views and our own is not nearly so great as the perusal of his letter would lead one to suppose. We all advocate the admission of typhus fever into properly constructed general hospitals; we all advocate, in the presence of much fever-pressure on an hospital, the appropriation to this disease of special fever wards: we differ in regard to our views of the mode of dealing with typhus at times when it is not epidemic, or when, from any cause, the applicants are not sufficiently numerous to claim more than a certain limited proportion of the hospital beds.

Now it is clear that the question of the indiscriminate and unlimited admission of typhus patients amongst the general inmates of all hospitals is widely different from that of such a limited admission as Mr. Holmes and myself have contemplated; and arguments which may be decisive on the one question, by no means necessarily affect the other at all. I quite admit that many of Dr. Anstie's facts tell very forcibly against the practice of indiscriminate admission; so do many of the facts which we ourselves have adduced. But I submit that arguments derived from disasters which have occurred in years of epidemic prevalence of typhus, in hospitals which have admitted typhus cases without limit, or have admitted them without regard to the unsuitability of their construction and arrangements for the purpose, have little force when applied to the question of dealing with typhus at other times, or of admitting a carefully restricted number of typhus patients into hospitals better suited to their reception. In our report we have adduced arguments in favour of our view derived from the experience of St. Bartholomew's, Guy's, St. Thomas's, and St. George's Hospitals; and I may here add that although at St. Thomas's we have admitted, as has ever been our custom, typhus patients in small proportion from the commencement of the epidemic up to the present time, typhus has spread to other patients in four cases only, and in our temporary hospital (which we have occupied since Oct. 1862) has not spread to patients at all.

In pursuance of his arguments against the treatment of typhus patients in other than special wards, Dr. Anstie treats as chimerical the danger which the use of special fever wards brings to the medical attendants; he speaks of it as "a danger which exists only in the imagination." Yet further on he makes the odd remark that "under the present system"—that, namely, of admitting typhus cases into the general wards—"the senior physicians of hospitals run a very serious risk indeed." I might well leave these statements to neutralize one another, but that I wholly dissent from both. Experience conclusively shows that under the present system senior physicians run no appreciable risk at all; and experience equally conclusively shows that the medical attendants on special fever wards are very frequently attacked with fever, and not rarely die of it. In the year in which Mr. Holmes and myself were engaged on our inquiry, the house-surgeon of the Chester Infirmary died of typhus; the resident physician of the Manchester Infirmary died of typhus, and one of the physicians was attacked with it; and at the Dundee Infirmary two resident medical officers died of the disease in rapid succession. In all these cases (and others still might be adduced) fever was treated in special wards.

Dr. Anstie is very careful to point out—and he is right to do so—the precautions which should be taken to reduce the danger of fever wards to a minimum. I agree generally with his views on this matter. But, indeed, the whole question of the safety of dealing with typhus, whether in separate wards or among other patients, depends on the careful observance of well-understood precautionary measures. Fever will certainly spread from the fever wards to the general wards of an hospital if precautions be not systematically observed. Such spread has occurred at Manchester; it has occurred at Edinburgh, and doubtless elsewhere. Equally, even in the best-constructed hospital, fever may spread from a single case admitted among the cases, if necessary precautions be neglected; and I have very little doubt that some at least of the outbreaks which Dr. Anstie has quoted originated in such neglect.

Before concluding my letter, I should wish to say a few words in reference to a quotation which Dr. Anstie makes from our report, and to his remarks upon it. He says, "It was with a feeling of perfect bewilderment that I read the following statement in that report: 'It is very rare for typhus to spread from one patient to another occupying a contiguous bed. Such an occurrence is rare (if not unknown) even in the Fever Hospital, and we are not aware that such an event has occurred either at St. George's or St. Thomas's Hospital.'" Dr. Anstie does not seem quite to understand the point of this observation of ours, for the majority of the arguments which he has arrayed against it are beside the question. I am free to confess that its meaning is by no means accurately expressed, and yet I should have thought it would have been apparent (especially with the aid of the context*) to most readers. It was not intended to assert that fever rarely spreads from patient to patient in hospital wards; it was not intended to assert that patients occupying beds on either side of a typhus patient enjoy by reason of such contiguity a special immunity from the danger of contagion; it was not intended to assert even that in wards with beds crowded together the presence of a typhus case might not be specially dangerous to the immediate neighbours: it was intended simply to declare that in hospitals reasonably well ventilated, with beds at a fair distance from one another, the fact of a patient's occupying a bed next to that occupied by a fever case was not in itself a source of material danger to that patient. To this opinion I adhere; and though the paragraph animadverted upon may be somewhat exaggerated in expression, it embodies, I believe, a substantial truth. Certainly the danger to patients sleeping in beds adjoining those of typhus patients cannot of necessity, and need not in fact, be great, when during the whole period of the present epidemic no patients thus circumstanced have in St. Thomas's Hospital contracted typhus.

Your obedient servant,

J. S. BRISTOWE, M.D.

Queen-square, Westminster, June 26th, 1865.

THE

COMPARATIVE MERITS OF THE TREATMENT OF HÆMORRHOIDS BY THE CLAMP AND BY THE LIGATURE.

To the Editor of THE LANCET.

SIR,—In the favourable notice with which you honoured my late Lettsomian Lectures on the Surgery of the Rectum, you make the following remark in reference to the method of treating hæmorrhoids by my improved clamp:—"Mr. Smith is of opinion that this is, in a large number of cases, a safer operation than, and therefore preferable to, the ordinary one of ligature. It is too soon, however, to institute a comparison of this sort."

I hope you will permit me to state that it is not only on the ground of its far greater safety that I practise and strongly recommend the employment of my clamp in cases of internal hæmorrhoids and prolapse; but I have adopted it for this additional and important reason, that the period of convalescence after this operation is rendered so brief that the patients are

* The words immediately preceding the above extract from our report are as follows:—"But although typhus fever undoubtedly spreads very readily, both to nurses, to medical men, and to pupils, and even also to patients who come closely into contact with those suffering from it, it seems quite well established in regard to this disease that in a well-ventilated and not overcrowded hospital the poison, even where many typhus patients are accumulated, extends to a very short distance indeed around each individual; so that in such an institution, unless the poison be conveyed carelessly by fomites or by the attendant, there is no reason why the general patients should incur any risk whatever of contracting the disease. It is very rare," &c.

enabled to be up and about their business in one-third of the time which is of necessity consumed after the ordinary operation by the ligature. A reference to the cases detailed at length proves this, for it will be observed that in nearly all the instances given in my lectures the patients were moving about in four or five days. In one or more cases, it is true, the convalescence was protracted for a period of a fortnight or three weeks; but these are quite the exception, and the comparatively long period which elapsed before the patients could move about was due to the circumstance of their being exhausted by hæmorrhage of long standing.

A reference to my notes of several other cases operated upon by me since these lectures were delivered tells the same tale as regards the short period of convalescence in every instance save one. Another was in the case of a gentleman upon whom I operated at the request of Dr. George Johnson. Here, however, there was a complication of malignant stricture of the large intestine, which did not develop itself until some period after the hæmorrhoids were removed, although doubtless it existed at the time. Here the patient did not move out until two weeks after the operation.

I think, therefore, that if the number of cases on which I have operated—now close upon fifty—are not sufficient to enable us to institute a comparison between the safety of the one and the other mode of treatment, their results, as I have detailed, entitle me to draw a fair comparison as regards the period required for a successful issue after each proceeding; and although I firmly believe all the elements of danger are entirely removed from the surgical treatment of hæmorrhoids and prolapse by adopting the method recommended in my lectures, and therefore on this ground alone is far superior to the operation by the ligature, I strongly advocate the use of the clamp for the additional and weighty reason that so much time is saved by its employment. As an illustration of this truth, it will not be out of place to refer to the very latest case I have had, in the person of a gentleman engaged most actively in important business, and who was sent to me by Dr. Reginald Read. I removed in this instance a prolapsus of the whole circumference of the rectum of twenty years' standing. The patient kept quietly in bed until the evening of the fourth day of the operation, the bowels having been acted upon by castor oil; and on the fifth day he returned to his business. Now this gentleman would have had an operation performed upon him several years since, but he was unable to afford the time requisite for the treatment by the ligature.

I am, Sir, yours &c.,

Caroline-street, Bedford-square, Aug. 1865.

HENRY SMITH.

COMPENSATION FOR RAILWAY INJURIES.

To the Editor of THE LANCET.

SIR,—As I am sure your aim must be to learn and state facts, and from them to form a judgment, I venture to trouble you with a few lines, that you may have the truth with regard to my case.

Nearly everything quoted by Mr. Skey, as my statement, lacks that clearness and literalness of delineation necessary to convey to the reader a correct conception of my case. Sometimes the style of statement is so vague, incorrect, and opposite to my own, that at first reading I thought Mr. Skey was purposely altering it; but I should not say so now, as I find Mr. Skey makes mistakes as evident in details but slightly material to his argument. For instance: I told him that I am thirty-five years of age, and not forty-three, as he says. I told him that I am seventeen stone and a half, instead of "twelve or thirteen." He saw me within a few days of twelve months after the accident, and not "ten months." Then he has forgotten to state that I have lost a Government appointment of £400 a year for 180 days' work in the year, besides all the advantages of such an appointment as professional connexion to me as an artist, and the certain opportunities I should have had for placing my children out in life. All this, and more, was considered, and influenced the verdict which Mr. Skey considers so preposterous. Will he be incredulous when I declare that, compared with the loss of my former great activity of body, the money I have received is to me as nothing?—that ten times the money would not have tempted me to part with my former state for the condition I am now in? Do I over-estimate the value of health, or does Mr. Skey over-estimate the value of money? I was surprised

at Mr. Skey forgetting to name my lameness. Before the accident I was a faster runner than any man of my weight that I ever met with, and I never hesitated to run races with men of lighter or any weight; this I often did, and with almost invariable success. I now creep about slowly and painfully, and at the present moment I sit up with great distress to myself. Since the trial, the sensation of minute points being driven into the brain has been continuous, and often very much aggravated; and writing you this letter makes it worse. You may ask, Why, then, do I do it? I confess that Mr. Skey's version of my case annoys me unspeakably. He says I complained of soreness and throbbing "about" the spine. The fact is, I complained, and do complain, of soreness down the spine, and of acute tenderness at two points—the upper injury being like a severe "burn," having a centre as of a hot needle being inserted into the spine. I complained of throbbing of the spinal cord, which was too distinct and painful to me to admit of my using the slovenly term "about." The horrible sensations "as of a claw grasping my brain, and as of my skull opening and my life leaving me," occurred when about to fall asleep, which Mr. Skey will perhaps remember, now that I remind him of the facts. Mr. Skey makes me say, "I can't shake my head without feeling a liquid in it." Mr. Skey has, perhaps, made all medical England laugh by this, but he certainly has not done it by using my words. I said, "Shaking my head rapidly to indicate a negative is now impossible, because of the pain caused at the base of the skull." And then, upon being pressed to explain the kind of sensation, I said the pain seemed caused by a heavy liquid, like quicksilver, being set in motion. Mr. Skey says I have fluttering and trembling at the base of the brain, while I acknowledge that I had a ravenous appetite. Now, Sir, and Mr. Skey, I complained (I was not forced to acknowledge) that I had had a ravenous appetite. That condition had almost completely passed away when I complained of fluttering and trembling at the base of the brain on awaking out of sleep. I do not know whether this will make any difference to Mr. Skey's argument or not; but if correctness of statement is of any value in medical science, I think what I have here said ought to be considered. I ought to add that I did say that I had frequent distress and aching at the base of the brain, which I found I could relieve by eating, and that this had occurred as recently as a few days before the trial. Mr. Skey must not reply, "A distinction without a difference." I am sure that, as I tell the truth, there must be many men in England who will be able to appreciate the value of the difference, and who will share with me the conviction of the need of exactness of statement and definition in matters so important. I can excuse a man failing in this respect when he is badgered out of his depth by a Q.C.; but I lose my charity when he writes to scientific England and shows that making careless and inaccurate statements is his habit.

But I must not weary you or your readers. If you can insert a non-professional view in your valuable journal you will much oblige, Sir, yours most respectfully,

August, 1865.

£6300.

THE ASHBURTON POISONING CASE.

To the Editor of THE LANCET.

SIR,—Since the appearance in *The Times* of the 4th inst. of a letter from Dr. Ogle, having reference to the above case, a great deal of undeserved obloquy has been thrown on the medical witnesses for the prosecution.

Dr. Ogle states he has no doubt "that the Chalker family were poisoned by belladonna, and that the poison was contained in the rabbit." There was certainly no doubt of the poison being in the pie. Atropia was found by Professor Herapath, not in the rabbit, but on the surface of the rabbit's leg, and also in a little of the scrapings from the pie-dish. Supposing the hypothesis advanced by Dr. Ogle to be correct—viz., that the rabbit in question had been partaking of the belladonna plant to excess, and had so poisoned its consumers, the analysis would have brought to light the presence of malic acid (with which atropia exists in combination in the plant), and also of pseudotoxin, phyllocolla, gum, wax, chlorophyll, salts, &c. None of these, however, were found; the only substances discovered in the pie not belonging to sound or healthy flesh being common salt and atropia.

It also seems very strange that of the thousands of belladonna-