How far the uterus and appendages will be able to resume their functions of pregnancy and parturition has not been practically settled by the occurrence of these states on patients operated on. There are no *a priori* reasons, however, why such occurrences should not proceed naturally, as Nature tends to go back to the normal condition when disturbing causes have been removed.

The operation is a plain, straightforward one, much easier than panhysterectomy, and very much easier than that Chinese puzzle of removing a comparatively large fibroid-laden uterus through a comparatively narrow vagina. In some cases no doubt deprivation of the uterine organs is not of great moment, and in other cases enucleation may be out of the question, so that I do not recommend it as a panacea for fibroids. But in all cases enucleation can be considered as the most desirable of all the operations for uterine fibroids, if it can be safely and conveniently performed, and I can recommend the method of treating uterine fibroids to greater consideration at the hands of the profession than it has so far obtained.

If this paper will have rendered the operative technique of enucleation safer, and extended the operation further than the very limited field enucleation has hitherto held, then an additional resource will be placed in the hands of gynaecologists whereby troublesome small fibroids can be removed earlier, because by the so much milder operation, and even in more advanced cases the disease may be removed without destroying the functions of the sexual organs, although the retention of that function may not seem to the operator of much importance.

AN ANSWER TO MR. LEONARD HILL'S REJOINDER REGARDING THE HYDERABAD COMMISSION.

By T. LAUDER BRUNTON, M.D., F.R.S.,
Physician to St. Bartholomew's Hospital.

In his rejoinder¹ to my reply,² Mr. Leonard Hill takes up a curious position, for on the one hand he says that he regrets exceedingly that vigour of criticism should be regarded by Dr. Lauder Brunton as a personal offence, and on the other he says, "The strength of my attack is justified to the full by Dr. Brunton's statement that he stands alone responsible for the work of the second Commission."

These two statements are so contradictory that the only conclusion I can come to is that Mr. Leonard Hill does not know himself what he means, but I am quite willing to believe that he does not see the whole gravity and scope of his accusations, and I shall therefore pass over their personal bearing, and confine myself as far as possible to their bearing on the character of the work of the second Hyderabad Commission, which, being more important than the first, is usually simply called the Hyderabad Commission. I have already explained in my reply how I came to be responsible for any deficiencies in the work of this Commission, but I am very far indeed from claiming the credit for the work actually accomplished, which belongs to the Commission and not to me individually.

It is obvious, however, that as I am so far responsible for the work, any accusations brought against it are really directed against me personally, whether they were intended to be so or not, and the duty of answering them devolves upon me rather than on any other member of the Commission.

Before proceeding to examine the arguments by which Mr. Leonard Hill, in his rejoinder in the *BRITISH MEDICAL JOURNAL* of March 19th, attempts to support the accusations he brought against me in the *JOURNAL* of April 17th, 1897, I will endeavour to correct some misapprehensions under which he appears to labour (a) in regard to the constitution of the Hyderabad Commissions, and (b) in regard to my reason for mentioning the shortness of the time occupied in the work.

(a) In his rejoinder Mr. Hill asks: "Were Surgeon-Lieutenant-Colonel Lawrie and Dr. Lauder Brunton the first and second Commissioners respectively, and the Commissioners Bomford, Rustomji, Hehir, Kelly, Chamarette merely

assistants?" Mr. Hill appears not to have read with care the report which he has so violently attacked, for otherwise he would have known (1) that, so far from Surgeon-Lieutenant-Colonel Lawrie being the first Commissioner, he was not even a member of it; and (2) that the second Commission continued its work during my absence through illness, which it could not have done had I been the Commission instead of being merely a member of it.

(b) Mr. Hill asks why my three months' ticket was not extended? The answer simply is that I could not be absent longer from my engagements in London, and accepted the invitation to join the Commission only for this limited time. But Mr. Leonard Hill completely misapprehends my meaning when he says: "It is a curious excuse to bring forward in defence of deficiencies that the work of scientific investigation was done in a hurry." He does not appear to understand that there is all the difference in the world between doing things quickly and doing them in a hurry, for this term implies a want of the complete co-ordination of action requisite to obtain the best result.

The scientific work of the Commission was done in a short time, but it was not done in a hurry, and I have not brought forward hurry as an excuse for any deficiency in the work done by the Commission, with one exception, which I will presently mention, and which I ought to have specified in my reply. Notwithstanding the shortness of time allowed for collecting apparatus, I was able, either by purchase or by selection from what I possessed, to obtain all the instruments I thought necessary, with one exception, namely, Ludwig and von Basch's electrodes. Those which I have had in my possession for many years, and used in former researches, were unfortunately mislaid, and I was unable to obtain new ones before sailing. The electrodes I had with me were such as had been generally in use by physiologists previous to 1874, and are quite sufficient for most work, although the possession of Ludwig's electrodes would have ensured more perfect application of the stimulus to the vagi than was attained in Experiment 95. As a doubt regarding the application of the stimulus was carefully mentioned in the report the record of the experiment was perfectly trustworthy, and affords no ground for Mr. Hill's objections. With this exception my mention of the shortness of time employed in collecting apparatus, and in the work of the Commission, was not brought forward by me as an excuse for any imperfections in the work *actually done* and recorded in the report. It was intended as an excuse for the work which the Commission in spite of the other advantages at its disposal was *obliged to leave undone*, and which is therefore altogether outside Mr. Hill's criticism. The work of the Commission has been supplemented by the researches of Gaskell and Shore, and also of Hare and Thornton, both of which researches were initiated by Surgeon-Lieutenant-Colonel Lawrie. Yet much remains still to be done, and some of the lines of research on which the Hyderabad Commission worked are still in the same state as it left them. The Commission began to investigate the influence of altered tissue change and excretion on the susceptibility of the cardiac nerves to stimuli, and on the production of shock either with or without chloroform, but this question still remains to be worked out, although a new research by Cyon on the effect of thyroid extract upon the excitability of the cardiac nerves gives reason to hope that our knowledge regarding this subject may soon receive a substantial increase.

Having now tried to correct Mr. Hill's misapprehensions, I will proceed to examine the arguments by which he endeavours to support the accusations of (1) prejudice, (2) carelessness, (3) ignorance of scientific methods; and (4) incompetence, which he brought against me in the *BRITISH MEDICAL JOURNAL* of April 17th, 1897. In his rejoinder he says, "I never intended any attack that I have made upon the conclusions of the Hyderabad Commission as a personal attack upon Dr. Lauder Brunton, for whose work I naturally have the highest appreciation." But the attack now in question was made by Mr. Hill not upon the conclusions of the Hyderabad Commission, but upon their experimental work. Their conclusions might have been wrong, and yet the experimental work perfectly right. But Mr. Hill has chosen to deny the trustworthiness of the experiments, and thereby he attempts to nullify the whole work of the Commission. In

his rejoinder he does not withdraw his accusations, but on the contrary tries to substantiate them. In order to justify this action he must prove his accusations to be true. I shall now take them up *seriatim*, and examine the evidence by which he attempts to prove them.

1. His first accusation is that of prejudice. In his lecture,³ he said the "Hyderabad Commission engaged on a wild goose chase to prove that the respiration ceases before the heart failed." In my answer,⁴ I mentioned my responsibility in connection with the work of the Commission, and showed that when I set out for India any prejudice that I had was in favour of danger arising from the direct action of chloroform on the heart, so that when the results of many experiments caused me to change my opinion the announcement was mentioned by the *Lancet* with a certain amount of surprise in the annotation which I quoted in my reply. In his rejoinder, Mr. Leonard Hill simply gives a short historical summary of the appointment of the Commissions which has no apparent bearing on the subject, and then quotes from a leading article in the *Lancet* the following passage: "Clinical experience and physiological experiments have led to the conclusion that it (chloroform) has a paralysing effect on the heart, while ether exerts such an action in a very minor degree, if at all. It is almost impossible to believe that the conclusion at which European and American surgeons and scientists have arrived is, after all, destitute of foundation, and little better than an idle dream." It was because this passage exactly expressed my opinions on the subject when I left England that the *Lancet*, in the annotation to which I have already referred, expressed surprise at the change they had undergone, and anxiety to learn the experimental evidence which had induced this change. Had any further evidence been wanted to show how utterly groundless Mr. Leonard Hill's accusation of prejudice was the quotation which he himself gives in his rejoinder would supply it.

2. His second accusation is that of carelessness. This charge is of the very gravest kind, for if it be true it entirely deprives the experimental work of the Hyderabad Commission of any value whatever. So important is it that I shall again quote it. Mr. Leonard Hill says in his lecture that the conclusions of the Hyderabad Commission were "upheld by a series of experiments, many so careless in their execution that they could not for one moment be accepted by a trained physiologist." Although Mr. Hill only states that many of the experiments made by the Commission are untrustworthy yet he practically condemns every one of them as utterly valueless, for, as his accusation at present stands, it is impossible to bring forward a single one of the Commission's experiments without being liable to the retort that it is one of the "many" which Mr. Leonard Hill has declared to be so careless in execution that they cannot be accepted by a trained physiologist. If his accusation be true, the most extensive research ever made on the action of chloroform is nothing more than a complete and useless waste of time, labour, animal life and money. It is evident that these charges are of the gravest nature, and before making them Mr. Hill ought to have been certain that he could prove them by incontrovertible evidence. Let us now see what the evidence is which he actually brings to support them. It is of such an extraordinary character that I shall quote part of it verbatim. He says: "In justification of my charge of carelessness I can bring forward the following facts:

"(1) That the work of the first Commission was conducted by men unskilled in experimental methods and apparently without apparatus.

"(2) That the second Commission was conducted in an out-of-the-way place with such apparatus as could be hurried together in the fortnight preceding Dr. Brunton's departure.

"(3) That the research was hurried through in forty-seven days.

"(4) We may turn to the internal proof to be drawn from the experiments themselves.

"(a) An absolutely insufficient method was used to record the energy of the heart.

"(b) That chloroform was injected into the jugular vein, and was not washed on into the heart by normal saline solution. Thus no syncope occurred."

From this quotation we see that the first proof Mr. Hill brings forward in support of an accusation of the utmost

gravity, which, if true, would utterly destroy the value of all experiments done by the second Hyderabad Commission, and for which I am responsible, is that these experiments were careless in execution because the first Commission, which sat nearly two years previously, and of which I was not a member, "was conducted by unskilled men and apparently without apparatus." A more remarkable *non sequitur* it would be hard to find.

His next proofs are, if possible, still more extraordinary. Mr. Hill lays down a standard of his own, by which he judges our experiments, and condemns them as careless, whilst he appears to be ignorant of the meaning of the term. He seems not to know that carelessness does not depend on the place where a thing is done, on the apparatus with which it is done, nor on the time employed in doing it, but on the way in which it is done. It is a term quite inapplicable to place, materials, or time, and applies only to the manner in which the operators employ the means at their disposal. If Mr. Hill's standard of carelessness were correct, we might at once cast aside as worthless all the observations of travellers or exploring expeditions because they have been made in out-of-the-way places. Henceforth civilised Governments may save themselves the trouble and expense of sending out expeditions to observe the transit of Venus or total solar eclipses because any results they obtain will, according to Mr. Hill's canon, be condemned beforehand as absolutely untrustworthy, inasmuch as the observations must necessarily be made (1) in out-of-the-way places; (2) without the aid of such large telescopes or such perfect apparatus as would have been available at home; and (3) in a space of time which has to be measured by seconds. The absurdity of Mr. Hill's general proofs of carelessness it would be difficult to surpass.

Let us now turn to the internal proofs of carelessness which Mr. Leonard Hill draws from the experiments themselves. His first proof is (a) "an absolutely insufficient method was used to record the energy of the heart." Even if this were true, which it is not, this statement is no proof at all of carelessness in the performance of the experiments. It is simply a question of apparatus, and I will consider it in its proper place, namely, under his third accusation, of ignorance of exact physiological methods. His second proof is (b) "that chloroform was injected into the jugular vein after ligation of the vein, and was not washed on into the heart by normal saline solution." This experiment was cited by Mr. Leonard Hill in his lecture as a proof of my ignorance of exact physiological methods.

In my reply I showed that his position was untenable, so he now shifts his ground and brings it forward as a proof of carelessness. He tries to do this by stating that it was not the method but the manner in which the injection was carried out that is open to severe criticism.

But in the two objections just quoted there is no question whatever of either carelessness or any other defect in the manner of performing the experiments, and it is only to the methods that Mr. Hill's remarks apply. These methods are (1) ligation of the vein previous to injection; and (2) omission to wash the chloroform into the heart by normal saline solution. I have already shown in my reply that in objecting to our method of ligaturing the vein before injection, Mr. Hill is in a minority, and that instead of my being ignorant of exact physiological methods, it is Mr. Hill who was either ignorant of the fact that the method I employed was the one recommended by Burdon Sanderson, Cyon, and Gschenden, or else in his superior wisdom he sat in judgment on the writers of the best handbooks on physiological methods, and included them in the same condemnation with me. It is, therefore, unnecessary for me to discuss this objection any further. His objection to the second method, namely, that we injected chloroform into the jugular vein, and did not wash it into the heart with saline solution, I will answer by two questions, (1) If the chloroform reached the heart without the injection of saline solution, of what use would such an injection have been? (2) If the chloroform did not reach the heart, how did it kill the animal as it did?

The other experiments which I dealt with in my reply under the accusation of incompetence, but which Mr. Hill now brings in under carelessness, are those on the effect of alteration of position. I have already said that Mr. Leonard

Hill's statement with regard to them is absolutely incorrect, and I will again consider it under the charge of incompetence.

As I have already pointed out more than once, Mr. Leonard Hill's accusation that many of the experiments performed by the Hyderabad Commission were so careless in execution that they could not for a moment be accepted by trained physiologists, applies in its present form to all their experiments, and must so apply until the experiments to which it actually does apply are specified so as to exclude those to which it does not apply. I asked him to specify the untrustworthy experiments, and with the exception of those which I shall deal with under the head of incompetence his "many" have dwindled into one solitary experiment which he utilises for a double purpose, employing it in his lecture as the basis of an accusation against me of ignorance of exact physiological methods and using it in his rejoinder as the basis of an accusation of carelessness; but, as I have already shown, this solitary experiment upon which Mr. Hill would found so much affords no basis whatever for either of his accusations.

Having thus examined the evidence by which Mr. Hill attempts to support his accusation of carelessness—an accusation which if proved would totally destroy the whole experimental work of the Hyderabad Commission—we find that it consists, first, in a fancy of Mr. Hill's that work is untrustworthy if it is done (1) in an out-of-the-way place, (2) with limited apparatus, and (3) in a short time, a fancy which fortunately for the world at large exists only in Mr. Hill's imagination, and does not correspond with facts; secondly, in a statement that the apparatus employed did not meet with Mr. Hill's approval; finally, we find that the "many" experiments which he denounced as untrustworthy have shrunk down into a solitary one, and even against this Mr. Hill's evidence of carelessness utterly breaks down. Such are the grounds on which Mr. Hill seeks to destroy the whole experimental work of the Hyderabad Commission.

I will now take up Mr. Hill's third accusation of ignorance of physiological methods. In his lecture he attempted to prove this charge by bringing forward the experiment of injecting chloroform into the jugular vein. In my reply I showed that this so-called proof was worthless, and that all the best textbooks on physiological methods recommended the method I used. In his rejoinder he finds it convenient to shift his ground, and say it was not the method but the manner of using it that was objectionable, and he now brings it forward as a proof not of ignorance but of carelessness, and under this heading I have already dealt with it. Another proof which he brings forward in his rejoinder in support of his accusation of carelessness, but which, as I have already shown, ought really to have come under the heading of ignorance of exact physiological methods is that "an absolutely inefficient method was used to record the energy of the heart. A heart when poisoned with chloroform is thrown into paralytic dilatation; it continues to beat, but is incapable of emptying its cavities. A needle thrust into the heart would exhibit the efforts of contraction, it tells us nothing of the efficiency of action of that organ, and yet this was the method employed by the Commission." This statement of Mr. Hill's is a most extraordinary one, for if he has read the report he must know that the Commission did not employ a needle in the heart in order to record its energy, but simply in order to ascertain whether it was contracting or not, for which purpose, as Mr. Hill himself acknowledges, it was amply sufficient.

I now come to Mr. Hill's last accusation—namely, that of incompetence. In his lecture he affirms that I, in experiments on the effect of altered position, paid no attention to the absolute necessity of placing the arterial cannula in the axis round which the animal was turned, and that by neglect of this simple precaution the experiments on the point were rendered utterly worthless. Although it is in another place that he applies the word "incompetent," yet if his accusation that I neglected this simple precaution were true, I should well deserve the epithet. But this statement is a pure product of Mr. Hill's imagination. It does not correspond to the facts of the case, and is therefore untrue. Nevertheless Mr. Hill considers that he was justified in uttering it, without taking the trouble to ascertain previously whether it were true or not because he did not find in the report a state-

ment that certain precautions were taken. The reason why no such statement appears in the report is that my long experience in physiological work has led me to look upon these precautions as so indispensable that I no more thought of mentioning them than I did of commencing my account of each day's work with the statement that the sun had risen, and consequently there was light enough to see our apparatus. For want of such a statement Mr. Hill may feel justified in rejecting all our experiments; and indeed, in order to be consistent, he ought to do so, for we have neither stated that the sun was up, nor that we employed artificial light when making our experiments, and consequently Mr. Hill has no assurance that we were not working in total darkness. But no amount of incredulity on Mr. Hill's part, nor of tenacity in holding to his fancies, will ever convert these fancies into facts, or change his statement from untruth into truth. It is curious to learn from the data with which Mr. Hill furnishes us how he came to make this unfounded statement. He is apparently unable to conceive of an axis without an axle, and so he postulates a swing board as a necessity for the experiment. His imagination, limited by the experience of the scanty supplies in a London laboratory, cannot picture to itself a supply of animals nearly of the same size, nor can he conceive that a floor may be temporarily heightened in the part required by means of blocks. Having thus laid down a canon to which experiments must necessarily conform, he condemns those which do not do so as being utterly valueless, in the same way as he condemned those which did not conform to his canon of carelessness. In neither case does it seem to have entered into Mr. Hill's mind that he could possibly be mistaken, and so he takes upon himself to act as the mouthpiece of trained physiologists, and, in this assumed capacity, to condemn our experiments without further inquiry.

I have now taken up Mr. Hill's accusations of prejudice, ignorance, carelessness, and incompetence, one by one, and have shown that all those accusations are absolutely groundless, and that the experiments of the second Hyderabad Commission are perfectly trustworthy, and may be accepted as such both by trained physiologists and by everyone else.

REFERENCES.

- 1 BRITISH MEDICAL JOURNAL, March 19th, 1898. 2 *Ibid.*, March 5th, 1898.
3 *Ibid.*, April 17th, 1897. 4 *Loc. cit.*

THE EFFECTS OF THE DUM-DUM BULLET FROM A SURGICAL POINT OF VIEW.

BY SURGEON-COLONEL W. F. STEVENSON, A.M.S.,
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PROFESSOR BRUNS is reported to have stated at the late meeting of the German Chirurgical Society, that the use of the dum-dum bullet in warfare is "brutally inhuman"; and the inferences to be drawn from certain questions lately put by members of the House of Commons to the Secretary of State for War are sure to conjure up before the minds of hysterical persons, who have no means of knowing the facts of the case, the notion that the English Government has selected for use in the English army a small-arm projectile which in the spirit, if not in the letter, contravenes the agreement come to at the Congress of St. Petersburg, in 1863, not to use explosive shells of less than 400 grammes in weight. As a matter of fact, there is no doubt that the effects of the dum-dum bullet have been exaggerated. This enhanced idea regarding the damage caused in human tissues by this missile is probably the result of reports from India, which were grounded on experiments made there by firing it through animals of greater resisting power than man, and on the natural prejudice in favour of the home-made article. Experiments on animals give only a general idea of the effects of a projectile; they supply no reliable evidence of the injuries it will produce in man or in other animals than those experimented on, inasmuch as these depend altogether, other things being equal, on the resistance offered by the tissues to its passage: the greater the resistance the greater will be the damage done by it.

All war may be said to be "inhuman"; but if the inhumanity of war is to be judged by the greater or less severity