joint may gradually acquire a great degree of compensatory movement. I lately saw a man, aged 19, whose ankle-joint, after urethral arthritis, had become ankylosed in a position of rectangular talipes, but who, owing to the increased movement in the mediotalar joint, walked without appreciable defect. ⁴ *Gant's Science and Practice of Surgery*, 2nd edition, vol. ii, p. 16. ⁵ *Lancet*, vol. ii, 1889. ⁶ vol. xvi, p. 127. ⁷ vol. xviii, p. 186.

A REPLY TO THE ACCUSATIONS MADE BY MR. LEONARD HILL AGAINST THE HYDERABAD CHLOROFORM COMMISSION.

By T. LAUDER BRUNTON, M.D., F.R.S.,
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THE action of anæsthetics is of constant interest to the medical public; but the admirable address of Dr. Augustus Waller and the other papers read at the annual meeting of the British Medical Association have increased the attention which the subject usually excites. For some time back I have been anxious to discuss some questions regarding the causation of death during anæsthesia, especially in relation to the work of the Hyderabad Commission; but the subject is so large and the time at my disposal is so limited that my intention has been postponed from week to week. I now feel that before either discussing the subject or replying to criticism I must first of all answer certain accusations which have been brought against me by Mr. Leonard Hill.

In the BRITISH MEDICAL JOURNAL for April 17th, 1897, a lecture was published by Mr. Hill on the Causation of Chloroform Syncope, and it was favourably noticed in a leading article of the JOURNAL for April 24th. In this lecture Mr. Hill not only criticised severely the work and conclusions of the Hyderabad Commission, but he brought against it charges of (1) prejudice, (2) carelessness, (3) ignorance, and (4) incompetence. At the time when Mr. Hill's lecture appeared the pressure of other engagements made it absolutely impossible for me to do more than write a few lines in the BRITISH MEDICAL JOURNAL of May 1st, asking readers to suspend their judgment on the question.

Had Mr. Hill been a medical student trying to gain a scientific reputation for himself by trying to detract from that of others I might have left his accusations unanswered; but Mr. Hill has already gained for himself a name as a scientific worker; he is a lecturer on physiology at the London Hospital; he is Secretary of the Physiological Society, and the opinions he expressed in his lecture have not only been quoted with approval by a number of subsequent writers, but they were endorsed by a leading article in the BRITISH MEDICAL JOURNAL of April 24th, 1897, and consequently, however disagreeable the task may be to me, I must reply to his charges.

Although nominally directed against the Hyderabad Commission, all these charges, with the exception of the first, are really directed against me personally, for, as Surgeon-Lieutenant-Colonel Lawrie says in his letter to the JOURNAL of September 4th, 1897, p. 617: "The work of the second Commission was directed throughout by Dr. Lauder Brunton." For this work I am, in fact, responsible, and I am ready to admit that for any faults or shortcomings I alone am to blame. The most important deficiency in the research is one to which Mr. Leonard Hill refers, not in his lecture, but in a letter to the BRITISH MEDICAL JOURNAL of July 31st, 1897, p. 313, where he says that the Commission failed to throw any light on "the causation of those accidental deaths which take place in the primary stage of anæsthesia." These deaths in man are probably due in great measure to fear, anxiety, or other emotional excitement, and may arise from the respiratory and circulatory changes, either caused directly by these emotions or indirectly through the increased susceptibility to shock from pain or suffocation which they produce.

The members of the Commission were quite conscious of this deficiency, and they carefully considered whether it was not their duty to attempt experimentally to produce in animals, by causing pain and terror in them, a condition similar to that which may occur in nervous human beings previous to an operation under anæsthetics. They unani-

mously decided against doing so on the ground that the pain and terror they would have to inflict upon animals in order to produce in them a state at all like that which occurs in exceptionally sensitive human beings would be so great that they did not feel justified in doing the experiments. In coming to the decision to apply the anæsthetics so as to cause neither pain nor terror to the animals, they were perfectly conscious they were excluding two of the most powerful factors of shock from their experiments, and laying themselves open to the charge which Mr. Leonard Hill brings against the Commission in his letter.

That there are some other deficiencies I am perfectly ready to acknowledge. It would have been advisable to repeat several experiments if we had had time, and it might have been advantageous on several occasions to use other apparatus if it had been available. But there are few, if any, researches which are free from defects, and I hope to show that in that of the Hyderabad Commission there are none of sufficient gravity to warrant the sweeping condemnation Mr. Leonard Hill has passed upon it. I have no excuse whatever to offer for these deficiencies except the shortness of time at my disposal. This time was shorter than might at first be supposed. I had less than a fortnight in which to collect apparatus, for though I received notice of Surgeon-Lieutenant-Colonel Lawrie's proposal a few days before it was published in the *Lancet* on September 21st, yet the P. and O. steamer *Kaisar-i-Hind* left Southampton on September 27th, 1889, and luggage had to be on board a few days before. I reached Hyderabad on the evening of October 21st, and the Commission began work without apparatus on October 23rd, with apparatus on the 25th, and continued until December 18th. I had *carte blanche* from the Nizam's Government with regard to instruments and with the aid of my friend the late Professor Roy I collected quickly together all the apparatus which I thought would be requisite. I had in all, however, less than a fortnight in which to collect the equipment of a laboratory, four days in which to fit it up and, Sundays being excluded, forty-seven days in which to do the work with apparatus. But with the exception of time, nothing was left to be desired. The generosity of the Nizam, the intelligent interest taken both by himself and his Ministers in the work, the courtesy and zeal of Surgeon-Lieutenant-Colonel Lawrie, as well as of everyone connected with the Commission, afforded every possible assistance in carrying out the work. In his letter to the *Lancet* of September 21st, 1889, Surgeon-Lieutenant-Colonel Lawrie undertook to place the Hyderabad Commission entirely at the disposal of the representative of the *Lancet*, and to act under his direction. The Commission, he said, "will provide all instruments and appliances, and everything which may be required for the experiments, and will, without bias, do all in their power to assist the representative of the *Lancet* in arriving at the truth." This promise Surgeon-Lieutenant-Colonel Lawrie loyally carried out. He placed everything at my disposal, so that (1) I drew up the general plan of the research, and (2) stated what experiments I thought were necessary; (3) I did the first one or more of every set of experiments myself; and (4) I was present and superintended all the experiments till near the end of the research, when I was obliged to be absent for some time owing to an attack of dysentery. I am therefore personally wholly responsible not only for the plan of the research in general, but for the plan of each experiment in particular, and for the execution or supervision of each experiment, as well as jointly responsible with my colleagues for the general conclusions. Having now shown how it is that Mr. Hill's accusations, although nominally directed against the Hyderabad Commission, are really made against me personally, I shall take them up and answer them *seriatim*.

His first accusation is that of prejudice, and of a desire on the part of the Commission to prove the correctness of a pre-formed conclusion, instead of trying to find out the truth. He says on page 957, "The prejudiced enthusiasm of Surgeon-Lieutenant-Colonel Lawrie," and page 960, "The Hyderabad Commission engaged on a wild goose chase to prove that respiration ceases before the heart fails, find in their tracings the proof that the failure of respiration is the primary cause of death. I, on the other hand, maintain that not only my own tracings but theirs too, conclusively prove that the

failure of circulation is the primary cause of the failure of the respiratory centre."

The first part of those quotations is directed personally against Surgeon-Lieutenant-Colonel Lawrie, but in the second Mr. Hill attacks us both. Now, whatever the views of Lawrie may have been, my own decided view when I went out, was that stoppage of the heart was certainly one of the dangers to be apprehended from chloroform anaesthesia. Nearly twenty-five years ago I was accustomed to show to my class an experiment by which a current of air from the same bellows was divided by a Y tube into two equal parts, one of which passed through chloroform into the trachea of one animal while the other passed through ether into the trachea of another similar animal. Both animals were thus thoroughly anaesthetised, and the movements of the heart in each were made visible to the class by a needle, to which a long straw bearing a flag at the top was attached. When the anaesthetic was pushed by working the bellows rapidly, the heart of the chloroformed animal was quickly stopped, while that of the etherised animal went on beating with hardly any change. In a paper on One of the Causes of Death during the Extraction of Teeth under Chloroform, BRITISH MEDICAL JOURNAL, December 4th, 1875, I had also expressed the opinion that syncope from failure of the circulation was one of the causes of death during chloroform anaesthesia.

In my *Experimental Investigation of the Action of Medicines*, Appendix, p. iv, I said: "In operations on the abdominal viscera in dogs, for example in making gastric fistulae, death sometimes occurs from shock, although the animals are completely under the influence of chloroform. For such operations ether is preferable, as it increases rather than diminishes the power of the heart."

My own expectation, therefore, was that we should find syncope from failure of the heart and circulation to be a powerful factor in death during chloroform anaesthesia, and the first experiment with a manometer made by the Commission on October 25th was on the effects of various operations likely to produce shock. To the evidence which led me to alter my views I will afterwards refer, but I think that the following annotation from the *Lancet*, December 7th, 1889, p. 1183, shows that I did not go out on a wild goose chase to prove that the respiration ceases before the heart fails, and that this accusation of Mr. Hill is unfounded.

We have just received from Dr. Lauder Brunton the following telegram, which we print *verbatim*: "Four hundred and ninety dogs, horses, monkeys, goats, cats, and rabbits used. One hundred and twenty with manometer. All records photographed. Numerous observations on every individual animal. Results most instructive. Danger from chloroform is asphyxia or overdose; none whatever heart direct." "These results apparently indicate such a complete reversal of the view held by Dr. Lauder Brunton at the time he left England—that one of the dangers resulting from chloroform is death by stoppage of the heart—that the details of the experiments made by Dr. Brunton, and the reasons for the conclusions he has evidently arrived at, will be awaited with the greatest interest by the profession."

The second accusation brought by Mr. Leonard Hill is that of carelessness. He says "upheld by a series of experiments, many so careless in execution that they could not for one moment be accepted by a trained physiologist."¹ This sweeping statement of Mr. Hill's practically condemns every experiment of the Hyderabad Commission, for he does not specify those to which the condemnation applies, and in the absence of such specification his condemnation may apply to all. It appears to me that Mr. Hill, in bringing such a sweeping accusation against the Commission was bound to support it by reference to definite experiments, and I now call upon him to adduce his evidence. I have looked through Mr. Hill's paper carefully, but the only evidence which I find he brings forward of carelessness is that of the imperfect application of electrodes to the vagus. Just at the time when I began to direct my own attention chiefly to the supervision of the mercurial kymograph, and to delegate to Mr. Chamarette the task of introducing cannulae into the arteries and veins, or of exposing and irritating the vagi, the experiments were not always completely successful. Thus in No. 68 the introduction of the cannula was not accomplished without damaging the artery, and the vagus was not rightly exposed, but on every occasion where we had the least reason to suspect any imperfection in the experiment—for example, in experiment No. 95, where there was a doubt whether the vagus had been rightly stimulated—such imperfection was carefully recorded in order that it should not mislead either

ourselves or others in the interpretation of our experiments. With the exceptions which the Commission carefully recorded, the experiments were done with the greatest care, and unless Mr. Hill can bring forward more definite evidence to support his assertion that many of the experiments were so careless in execution that they could not for one moment be accepted by a trained physiologist, I believe that every physiologist will decide that his accusation is unwarranted.

In regard to the cases of accidental death, Mr. Hill draws a comparison between the carelessness of which he accuses the Commission and his own accuracy, for, as he tells us, in all cases of chloroform syncope occurring during primary anaesthetisation he carefully observed the symptoms. The Chloroform Commission carefully observed the symptoms in a much larger number of cases than Mr. Hill, but when deaths occurred during careful observation they were not classed as accidental. I still fail to see how Mr. Hill could properly class his cases as accidental if he was observing them all the time. One of the accidental deaths occurred while I was performing tracheotomy upon an animal, and was of course in a favourable position for observing its respiration, but my attention was concentrated upon the operation, and I failed to notice the failure in respiration until too late. This may no doubt be put down to carelessness upon my part, but it was just such instances of carelessness which form one of the most valuable parts of the report as tending to throw light upon the causes of so-called accidental death occurring in actual practice.

Thirdly, I shall now take up Mr. Leonard Hill's charge of ignorance of physiological methods. Considering that I have been trained by Brücke, Rosenthal, Kühne, and Ludwig, this charge seems *a priori* improbable; but such training does not disprove Mr. Hill's charge. I will therefore take the instances which he brings forward. He says: "The Hyderabad Commission failed to obtain this effect although they injected 20 c.cm. of pure chloroform into the jugular vein in successive doses. The cause of this failure is to be found in the ignorance of precise physiological methods which was unfortunately betrayed throughout much of the work of the Commission."² Mr. Hill states that in our experiments the drug remained in the veins. If this statement of his were true the chloroform ought not to have produced either narcosis or death as it did. He adduces the fact that we injected the chloroform into the jugular vein after ligation as one evidence of my want of knowledge of exact physiological methods. I have many times injected drugs into the veins of animals in which the blood was freely circulating, but the advantages of this method do not, I think, counterbalance its defects. Mr. Hill may think that this is a mere opinion of mine founded on ignorant stupidity or prejudice, but some eminent physiologists have come to the same conclusion as myself. The method I employed is the one described by Burdon Sanderson in his *Handbook for the Physiological Laboratory*, p. 238; by Claude Bernard in his *Leçons de Physiologie Opératoire*, p. 262; by Cyon in his *Methodik der physiologischen Experimente*, pp. 49 and 50; and by Gscheidlen in his *Physiologische Methodik*, p. 538. It is true that Claude Bernard describes³ the method of intravenous injection which Mr. Hill regards as the only proper one, but the only advantages which he attributes to it is that of simplicity. Livion, who reproduces Bernard's figures and also describes both methods,⁴ mentions the advisability of having a ligature round the vein in order to prevent possible bleeding. As the two latter authors describe both methods they may be absolved from ignorance, but as Burdon Sanderson, Cyon, and Gscheidlen all recommend the method which I adopted, they must all necessarily fall with me under Mr. Leonard Hill's condemnation of being ignorant of physiological methods.

Besides ignorance of exact physiological methods Mr. Hill accuses me of incapacity to interpret aright the tracings of the blood pressure which the Commission obtained. I have already⁵ admitted that I am responsible for the interpretation of the tracings, so this accusation of Mr. Hill's is brought against me even more directly than the others. He says: "Now on examining the tracings of the Hyderabad Commission I find an absolute agreement between their results and my own. . . ." "Although the tracings are so much alike, the interpretations are widely different. The Hyderabad Commission engaged on a wild goose chase to prove that

respiration ceases before the heart fails, find in their tracings the proof that the failure of respiration is the primary cause of death. I, on the other hand, maintain that not only my own tracings but theirs too, conclusively prove that the failure of circulation is the primary cause of the failure of the respiratory centre.⁸ From this statement I think any unprejudiced reader would infer that it was upon their tracings that the Hyderabad Commission relied for proof that failure of respiration is the primary cause of death, but this was not the case. The proof that failure of respiration is the primary cause of death from chloroform narcosis consisted in the fact that 141 animals were chloroformed to death by the first Commission, and 430 by the second, and in every one of these 571 animals the respiration failed before the heart. It was evidence of this sort that led me to modify my views regarding the risk to the heart by the action of chloroform directly upon it.

A similar charge of inability to interpret the tracings correctly was brought against me by Drs. Gaskell and Shore, who say regarding the Hyderabad Commission: "Their large number of curves confirm again and again the observation of others, and point to heart failure as the cause of the fall of blood pressure."

In his letter of July 31st, 1897,⁷ Mr. Hill says: "The truth which I seek to establish is that chloroform kills by primarily paralysing the whole of the vascular mechanism." But in his lecture of April 17th, 1897, it is upon the paralysis of the vessels that he lays most stress. A comparison between Mr. Hill's lecture and the above quotation from Drs. Gaskell and Shore, will, I think, show how just was the conclusion arrived at by the Hyderabad Commission in the interpretation of the curves we obtained: for as I pointed out,⁸ "it was precisely because the second Hyderabad Commission, when going over their experiments, thought that although their results indicated paralysis of the vasomotor centre as a cause of the fall in blood pressure, yet the evidence before them was not conclusive that they did not formulate any conclusion on the subject." They left it as a point to be decided by further research.

To one statement made by the Hyderabad Commission, Mr. Leonard Hill says that he "must give a direct denial." This statement is to the effect that inhalation of chloroform has no effect on the length of the period in which the heart can be maintained in arrest by electrical excitation of the vagus. One would have thought that only an intense feeling of duty would have led Mr. Leonard Hill to the use of such language, yet later on he states that while he has "been able to kill animals by repeated and prolonged vagal arrest of the heart, such complete vagal arrest of the heart is never induced by chloroform, and therefore these experiments are of no clinical interest."

Fourthly, the last accusation may be regarded as one either of ignorance or carelessness, for he does not say which it is; but had his account been accurate, Mr. Hill would have been perfectly justified in condemning me as incompetent. He says: "The Hyderabad Commission carried out a few experiments on the effect of the alteration of the position during chloroform narcosis. The workers paid no attention to the fact of the absolute necessity of placing the arterial cannula in the axis round which the animal is turned. Their experiments were thus vitiated by the hydrostatic effect of gravity on the column of fluid in the tube which connected the cannula with the manometer. By the neglect of such a simple precaution the experiments on this point were rendered entirely worthless." I have sometimes felt inclined to ridicule the strong language in which some foreign writers, and especially very young ones, criticise each other's work and denounce each other's statements as absolutely false, and to rejoice that scientific literature in this country was free from this blemish. In his lecture Mr. Hill met one statement of the Hyderabad Chloroform Commission with a flat denial where, perhaps, less strong language might have been admissible, but I little thought it would be my own lot to use the expression "absolutely false" in relation to any statement contained in a scientific paper, yet there is no other by which I can characterise Mr. Leonard Hill's statement, for it is without the slightest foundation in fact. When I read it I thought that it must be a misprint, and that he meant to say that we had paid no attention to having the axis round which

we turned the animal pass through the indifferent point in its vascular system, which, as Herman and his scholars Blumberg¹⁰ and Wagner¹¹ have shown, passes through the apex of the heart. Had this been the objection raised by Mr. Hill to our experiments, it would have involved a matter of opinion, and something might have been said on both sides. But his statement involves a matter of fact, and not of opinion, and as such it can only be met by an absolute denial. The precaution which he says *was not taken* by the Hyderabad Commission *was taken* most carefully. For knowing the fallacy to which he refers I made it my own special charge to see that (1) the cannula in the carotid, (2) the axis upon which we turned the animal, and (3) the horizontal limb of the manometer, were in the same horizontal plane, or, in other words, were at the same level, and remained so throughout the experiment. I cannot think that Mr. Leonard Hill would publish a falsehood wilfully, but a falsehood his statement certainly is, and I shall be curious to learn how he has been led to make it.

I now leave it to the "trained physiologists," whose mouth-piece Mr. Leonard Hill has constituted himself, as well as to the whole medical profession, to decide whether the evidence that Mr. Hill has brought forward is sufficient to justify the charges of (1) prejudice, (2) carelessness, (3) ignorance, and (4) incompetence, which he has brought nominally against the Hyderabad Chloroform Commission, but really against me personally.

REFERENCES.

- ¹ BRIT. MED. JOURN., April 17th, 1897, p. 957. ² *Ibid.*, p. 958. ³ *Leçons de Physiologie Opératoire*, p. 265. ⁴ *Manuel de Visection*, p. 53. ⁵ BRIT. MED. JOURN., March 11th, 1893. ⁶ *Ibid.*, April 7th, 1897, pp. 959, 960. ⁷ *Ibid.*, July 31st, 1897, p. 313. ⁸ *Ibid.*, March 11th, 1893. ⁹ *Ibid.*, April 17th, 1897, p. 960. ¹⁰ Blumberg, *Pflüger's Arch. f. Physiol.*, 1885, vol. 37, p. 467. ¹¹ Wagner, *Pflüger's Arch.*, 1886, vol. 39, p. 371.

A CASE OF SUCCESSFUL OVARIOTOMY IN A CHILD FOUR MONTHS OLD.

By D'ARCY POWER, F.R.C.S.ENG.,
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THE following case is remarkable on account of the tender age of the patient, as well as the successful issue of the operation. It shows that abdominal section is not very serious even in the youngest children, if proper precautions be taken to minimise shock.

I am indebted to Messrs. P. C. E. Tribe and Walter Allingham for the careful notes of the case.

G. A. R., a breast-fed girl, aged 4 months, was admitted into the Victoria Hospital for Children, under the care of Dr. Carr, February 7th, 1898, suffering from distension of the abdomen. The child was in fairly good health until three weeks ago, when the abdomen began to swell. The child was not in pain. On admission the abdomen was enormously distended, and was very tense. It was dull on percussion, except in the left flank, and the area of the dullness did not alter with the position of the patient. A well-marked thrill could be obtained with ease. The child measured 20½ inches in circumference at a point 2 inches above the umbilicus, and 10½ inches from the symphysis pubis to the tip of the xiphisternum. The urine was normal, and there was no oedema of the legs.

An abdominal section was performed on February 10th, an incision 3 inches long being made in the linea alba above the umbilicus. A small quantity of fluid escaped as soon as the peritoneal cavity was opened, and a thick walled cyst immediately presented at the opening. The cyst wall was bluish in colour, and many delicate blood vessels ramified over its surface. It was quite smooth, shiny, and there were no adhesions. A very fine cannula and trocar were inserted into the cyst, and about two pints of clear yellow fluid were drawn off as slowly as possible, to enable the circulation to adapt itself to the altered abdominal conditions. The whole cyst was then drawn out of the abdomen, a large incision was made in its wall, and the remainder of the fluid was allowed to escape. The total contents of the cyst amounted to 59 ounces. The cyst was then found to be attached by a pedicle about the size of a slate pencil in thickness. The pedicle was transfixed, tied in the usual manner with fine silk, divided, and was allowed to drop back into the abdomen.