rhage from liver tissue after it has been incised or torn is by temporary compression of the blood vessels in the gastro-hepatic omentum, and then ligature of the divided vessels.

3. That when the liver is the seat of growths such as I have mentioned a surgeon ought to be prepared to carry out an operation for their removal.

4. That the mortality which will result from operations of this kind is not greater than in many of the recognised abdominal operations, such as resection of the intestine.

THE HYDERABAD COMMISSION: A REJOINDER TO DR. LAUDER BRUNTON.

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Two Commissions were appointed by His Highness the Nizam's Government to examine into the alleged dangers of chloroform. The first Commission "was applied for by Surgeon-Major Lawrie because having always believed in the truth of Syme's teaching that chloroform can be used judiciously so as to do good without the risk of evil, he desired to show by experiments upon dogs that in death from chloroform the respiration always stops before the heart."¹ This Commission determined "that secondary syncope has no more real existence than primary syncope. In poisoning by chloroform the heart fails when the respiration ceases and never before. With the cessation of respiration the further accumulation of the drug in the blood necessarily ceases, and the heart rapidly or gradually stops beating as a direct result of the stoppage of respiration."² "This point having been proved, the second Commission was applied for because it was felt that Syme's principles, which both experience and experiment had shown to be practically sound, must be founded upon a firm physiological basis."³ His Highness the Nizam's Government desired to offer £1,000 to the Editor of the *Lancet* to send a representative to Hyderabad to repeat and continue the experiments of the Hyderabad Chloroform Commission.⁴ The Nizam's Government had "been advised that if the experiments are continued and amplified by the Hyderabad Commission associated with a trained scientist whose position and attainments will ensure the acceptance of his opinions by the profession the subject might be threshed out thoroughly, and the question whether chloroform does or does not affect the heart directly might be settled once for all."⁵ The *Lancet* appointed Dr. Lauder Brunton as their representative. In announcing this appointment the *Lancet* fully recognised that "the question whether chloroform paralyses the heart or not is one of the greatest possible practical importance, for upon its correct solution the lives of thousands of people and the happiness of thousands of families may depend. Both in Europe and America," the *Lancet* continues, "clinical experience and physiological experiments have led to the conclusion that it 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."⁶

In Dr. Brunton's reply, in the *BRITISH MEDICAL JOURNAL* of March 5th, to my "accusations" he states that he had less than a fortnight in which to collect apparatus. "I reached Hyderabad," he says, "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 in all less than a fortnight in which to collect the equipment of the laboratory, four days in which to fit it up, and (Sundays being excluded) forty-seven days in which to do the work with the apparatus." In the offer to the *Lancet* it is stated that "the gentleman selected should take a return ticket for three months, to be extended if necessary." We learn from these statements two things:

1. That 47 days' work was sufficient to give "the direct denial" to the "conclusion at which European and American surgeons and scientists had arrived."

2. That His Highness the Nizam's Government was advised that original scientific research work, like any other public work, could be done by contract in seven weeks.

We may also infer with some justice from Dr. Brunton's statement, that the work of the first Commission was carried out without apparatus or the proper equipment of a laboratory. It is a curious excuse to bring forward in defence of deficiencies that the work of scientific investigation was done in a hurry. Why, then, if "with the exception of time nothing was left to be desired," was not the three months' ticket extended? Every trained scientist is aware how impossible it is to undertake to carry out an investigation in a given time. Weeks slip by before the best methods of observation and the most fruitful line of research is determined upon, and years of training do not lessen this hindrance that meets one when tackling any fresh subject in that vast and unknown field the study of life. Surgeon-Lieutenant-Colonel Lawrie himself states that "the Hyderabad Commission did not discover the key to the safe administration of chloroform until they had made observations upon 565 animals, and had reached their 148th manometric experiment." Now the total number of manometric experiments recorded is 186, and near the end of the research Dr. Brunton was unfortunately absent for some time owing to an attack of dysentery.

Had Dr. Brunton been allowed to carry on his research quietly and at his leisure in London, equipped with all the best apparatus and aided by the suggestions and criticisms of English scientific societies, I cannot help thinking that his results would have greatly advanced our knowledge of chloroform syncope. At this point I will take the opportunity of saying that 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. One can hardly be expected to have imagined that the whole Commission was personated by Dr. Brunton. Were Surgeon-Lieutenant-Colonel Lawrie and Dr. Lauder Brunton the first and second Commissions respectively and the Commissioners Bomford, Rustomji, Hehir, Kelly, Chamarette merely assistants? If so, why is the work of these two investigators upheld and glorified before the world in so Oriental a way? We read in the first page of the Commission "that His Highness the Nizam visited the laboratory on two special and memorable occasions. The Nawab Munir-ul-Mulk, son of the late Sir Salar Jung, the Maharajah the Peshkar, and the Nawab Fakhr-ul-Mulk also paid visits to the laboratory and witnessed several of the experiments. The same interest was displayed by the Nawabs Mohsin-ul-Mulk, Vikar-ul-Mulk, Imad-ul-Mulk, Fattah Nawaz Jung, and Mr. Furdonji Jamshedji."

The Commission, equally with the *Lancet*, insists upon the fact that His Highness's Government had carried out "a work which is not only of scientific interest, but of the greatest practical importance to the whole human race." Its object was an essentially practical one, and could not be better defined than in the words of His Highness the Nizam himself, "to save people's lives."

Considering the gravity of the subject, and the Oriental pomp with which the work of the second Commission was thrust upon the scientific world, I have felt justified in attacking with some vigour a doctrine which I believe to be erroneous and dangerous.

My object, too, was essentially a practical one, and the strength of my attack is justified to the full by Dr. Brunton's statements that he stands alone responsible for the work of the second Commission—in other words, that no such thing as an independent Commission of trained scientists ever existed—and that the work was done in a hurry in 47 days, with such apparatus as could be scrambled together in a fortnight's preparation.

My attack was directed against the work of the Commission. Dr. Brunton has chosen to make it a personal attack upon himself. The Commission invites criticism. It states that as far as possible experimental data were given, "so that at any future time, and in any part of the world these data will be as available to others as to themselves, and may be used by them either for the purpose of criticising the Commission's conclusions, or drawing conclusions of their own." Dr. Brunton protests against the vigour of my criticism; he would clap me into an Oriental dungeon for *lese majesté*, for daring to attack the Commission's experiments, and refusing to bow down the knee in spite of the witness of the Nawabs-

ul-Mulks, the Maharajah the Peshkar, and Fattah Nawaz Jung.

The Commission has set me the example of vigorous criticism. Dr. Brunton says: "Mr. Hill accuses me of incapacity to interpret aright the tracings of blood pressure which the Commission obtained." The Hyderabad Commission make this same accusation in regard to the tracings of the Glasgow Committee. For example: "They have published a copy of the manometer tracing on which their conclusions are founded, and an inspection of this leads inevitably to the belief that the sudden and unlooked-for effects on the heart's action which the Committee attributed to chloroform are not really due to it at all." Here Dr. Brunton states that Professor McKendrick was incapable of reading aright his own tracing. Dr. Brunton remarks on my expression "direct denial," and imagines "that only an intense feeling of duty would have led to the use of such language." On exactly the same subject the words of the Commission are as follows: "Other experiments were made to test the truth of the statement that chloroform increases the action of electrical stimuli applied to the vagus, and showed conclusively that it has no such effect." Does not the expression "direct denial" in two words, and with no more offensive meaning, convey exactly the same sense as the quotation from the writing of the Hyderabad Commission itself?

Dr. Brunton further takes exception to my expression that "many of the experiments of the the Commission were careless in execution." In reference to certain of Dr. Snow's experiments, the Commission writes that "death was not really sudden, but only rapid and the result of reckless administration of concentrated vapour in the first instance, and careless neglect of the condition of the respiration in the second." These are sufficient examples of the vigour of the Commission's criticism of others.

The trenchant nature of the criticism of the President of the Hyderabad Commission is almost too well known to exemplify. But here are a few instances quoted from the report of the Commission:

1. "The effect of the Glasgow Committee's report was enormous, far reaching, and disastrous."
2. "The teaching of the Glasgow Commission is wrong."
3. Professor Wood's "amazing and ridiculous contention that the heart of a dog in India is much less sensitive to chloroform."
4. Professor MacWilliam's "entire arrangement is so artificial that it is very difficult to accept any of his results, or to believe that he can make all his levers and tambours, six in number, work in the perfect way he describes, and at the same time that he can watch the condition of the pulmonary artery so narrowly (in the cat!) as to be able to assert that it was soft and compressible, and that the pressure within it was low."
5. "Mr. Braine writes as if the acceptance of the truth would be disastrous to anaesthesia specialists."

Herein we see that the President calls the teaching of Professor McKendrick wrong and disastrous, that of Professor Wood ridiculous, while he suggests that Professor MacWilliam, a scientist trained in cardiac research, is incapable of exact experiment, and lacks scientific accuracy. In the last criticism quoted there either runs a dubious innuendo, or else the sentence is meaningless to me.

In justification of my charge of carelessness, I can bring forward the following facts:

- i. That the work of the first Commission was conducted by men unskilled in experimental methods, and apparently without apparatus.
- ii. 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.
- iii. That the research was hurried through in forty-seven days.
- iv. 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. 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. On page 99 an "attempt to connect with Roy's cardio-myograph" is recorded as failing. This attempt and failure is the one mention that I can find of the use of an apparatus which might have yielded valuable results.

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. Thus no syncope occurred. Considering that 5 to 10 mm. injected in the proper manner into the jugular vein of a cat produce profound dilatation and death of the heart, it is incredible that the Commission could have injected 20 c.cm., or roughly 340 mm., into the jugular veins of dogs (several times) in such a way that it ever reached the heart in one concentrated dose. It is not the method but the manner in which the injection was carried out that is open to severe criticism. The experiment of injecting into the jugular was repeated on four animals. It has been carried out with a contrary result scores of times by Wood, Gaskell and Shore, Barnard, and myself. Moreover, it is an operation commonly employed in laboratories to produce immediate death at the termination of physiological experiments.

In connection with the charge of carelessness, Dr. Brunton says that one statement of mine is "absolutely false," and he is curious to learn how I have been led to publish "this falsehood." In dealing with the effect of the alteration of position, I say that the Commission paid no attention to the fact of the absolute necessity of placing the arterial cannula in the axis round which the animal is turned, and that by the neglect of such a simple precaution the experiments on this point were rendered entirely worthless. It is this statement that Dr. Brunton calls absolutely false, and he says in the BRITISH MEDICAL JOURNAL of March 5th that this precaution was most carefully taken by himself. I have again searched through the report of the Commission, and have utterly failed to find any mention of this precaution. On the contrary, the detail of the experiments given in the report naturally leads one to the conclusion which I formed. On page 28 we read: "When the hind feet are lowered on to the floor so as to place the animal in the vertical position, a considerable fall of blood pressure in the carotid artery occurs, but when the animal is replaced on the table in the recumbent position the pressure is fully restored." Anyone who has done any experiments on the influence of gravity on the circulation would know that it was impossible to place different animals of a different length with their hind feet first touching the floor, and then returned back on the table, and yet maintain the position of the cannula in the same horizontal plane throughout the procedure. Such an experiment could only be accurately carried out with the aid of a swing board with a cannula fixed in the axis round which the board was swung. I gladly accept the statement of Dr. Brunton that precautions were carried out, but my original statement is justified unless he can direct me to any page in the report where the description of such precaution is given; otherwise I fail to see a justification for his expression "falsehood."

Granting that Dr. Brunton himself adjusted the animal and apparatus on each change of position, so that cannulae, axis, and horizontal limb of the manometer were in the same horizontal plane, yet it is obvious that such adjustment would take time, and that the initial and immediate effects of turning an animal from the horizontal to the vertical position would not be accurately recorded.

The prejudiced enthusiasm of Surgeon-Lieutenant-Colonel Lawrie is illustrated by the passages which I have already quoted. I never extended this charge further.

My statement "that the Hyderabad Commission engaged upon a wild-goose chase to prove that respiration ceases before the heart fails," I consider to be substantiated by every line of the report of the Commission. Dr. Brunton says that the Commission did not rely upon their tracings for proof. The proof which he upholds that failure of respiration is the primary cause of death consists "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. The doctrine founded upon this fact (II) is that the heart has nothing to do with producing the fall of blood pressure "unless its nutrition fails either from imperfect oxygenation of the blood due to

abnormal breathing, or from stoppage of the respiration from overdosing." The fact whether the respiration or the heart stops first is a matter of not the slightest importance, and yet the confirmation of this one point is that on which the Commission prides itself. What the Commission failed to give us is the reason why artificial respiration, even when applied before the pulse has stopped, frequently fails to bring about recovery. If an animal be simply asphyxiated the heart continues to beat for several minutes, and by artificial respiration the animal can be surely resuscitated. Now in Experiments Nos. 56 to 60 we find that the Commission failed more than once to smother an animal by means of a mouthpiece, and to kill it by asphyxia. On the other hand, when chloroform was pumped into the inhaler during partial asphyxia, death very rapidly ensued. In Experiment 59 the pulse stopped 22 minutes after the commencement of the smothering (with an empty inhaler). In the first four experiments chronicled by the Committee in which the animals were killed by means of a cloth inhaler filled with chloroform the pulse ceased (on the average) 27½ seconds after the respiration. In the last six experiments recorded by the Subcommittee in which the animals were also killed by means of chloroform on a cloth inhaler, and struggled violently during inhalation, the pulse ceased (on the average) 24 seconds after the respiration. The shortest interval noted in this set is 7 seconds.

We must remember that the experiments were conducted at a temperature which was generally 23 to 25° C., that is to say, similar to our hottest summer day. In consequence of the heat "the Commission had almost to cut off the air before they could get anaesthesia with ether." This shows the rapidity with which chloroform would also evaporate. Hence the difficulty of obtaining cessation of the pulse before the cessation of respiration. The effect of the climate is shown forcibly in the following quotation: "There were thus three accidental deaths in the course of this one day, which it may be noted was exceptional in being a wet day, and was characterised by constant blunders and mistakes committed by everyone engaged in the experiments and in every department." Dr. Brunton brushes aside the question of these and other accidental deaths. He says, "Accidental deaths in man are probably due in great measure to fear, anxiety, or other emotional excitement, and may arise from respiratory and circulatory changes." He continues that the members of the Commission quite realised the deficiency in its explanation of accidental deaths. Surely it is necessary before coming to any such final and momentous decision that the Commission should be in a position to eliminate this "probably," and make it either a certainty or negative it altogether. Then again the use of the expressions "great measure" and "may arise" surely imply that there are other causes at work even in Dr. Brunton's opinion. These causes the Commission has on its own showing not attempted to ascertain.

The tracings of the Commission show that the blood pressure falls very greatly before the respiration ceases. The Commission affirms that this has nothing to do with the heart and that the failure of respiration is the primary cause of death. If this be true, artificial respiration should with absolute certainty produce recovery in all cases of chloroform collapse. It does not do so, because the heart cannot be made to beat again with efficiency. Deoxygenation of the blood or asphyxia cannot possibly bring about this failure of the heart in the time chronicled by the Commission in many of the experiments. I maintain with Gaskell and Shore that the tracings are wrongly interpreted. They prove that the respiration fails in consequence of the fall of blood pressure. Not only has MacWilliam proved that chloroform dilates the heart, but Barnard and myself have demonstrated with a cardiometer that the heart of a cat is enormously dilated with the smallest doses of this drug. If the respiratory centre be cut out, an animal can be maintained alive efficiently for hours by artificial respiration. Let the Commission tell us why the patient whose respiratory centre is paralysed by chloroform cannot be similarly kept alive.

The Hyderabad Commission continually dwells on the vast number of animals on which experiments were performed. It is obvious that in 47 days Dr. Brunton could have personally investigated but a small proportion of the 571 animals.

Day by day and year by year the sum of animals chloroformed by any busy physiologist grows to a far greater total than this. It is not only in experiment that he observes the effect of anaesthesia, but also when the numerous animals are killed for class dissection.

I am in entire sympathy with Dr. Brunton when he writes: "It would have been advisable to repeat several experiments if we had time, and it might have been advantageous on several occasions to use other apparatus if it had been available."

Anyone who has read the controversy between Surgeon-Lieutenant-Colonel Lawrie and Drs. Gaskell and Shore will admit that my attack was mild in character compared with the controversial methods of the President of the Commission. I regret exceedingly that vigour of criticism should be regarded by Dr. Brunton as of personal offence. I rejoice, however, that so important a question as chloroform syncope should be once more reopened.

REFERENCES.

- ¹ Report of the Hyderabad Commission, p. 1; ² *Ibid*, p. 4; ³ *Ibid*, p. 1; ⁴ *Ibid*, p. 7; ⁵ *Ibid*, p. 7; ⁶ *Ibid*, p. 7; ⁷ *Ibid*, p. 342; ⁸ *Ibid*, p. 10.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, Etc.

CONCURRENCE OF WHOOPING-COUGH, SCARLET FEVER, AND VARICELLA.

A. R., aged 3 years, one of three sisters, came under my care recently with a well-defined attack of scarlet fever, evidently contracted from her elder sister, who had developed a similar attack seven days previously.

At the time I first saw her the child had marked paroxysms of whooping-cough, from which a younger sister was also suffering, and which continued during the greater part of the period of desquamation.

During the third week of the scarlatinal attack, her temperature suddenly rose, and she developed a typical eruption of varicella, which appeared in crops, and followed the normal course of that disease. I could not trace any possible cause of infection for this, except a fresh nurse who might have conveyed it.

Hence, during the third week of scarlet fever the child was capable of conveying infection of whooping-cough, scarlet fever, and chicken-pox, which appears to me to be a somewhat unusual occurrence, and worth recording.

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THE REACTION OF HEALING AND NON-HEALING WOUNDS AND ULCERS.

DURING the treatment of a large number of cases of chronic ulcers and recent wounds by oxygen gas, it was observed that in a small number of them the healing process became arrested. This change was usually traceable to some general or constitutional disturbance, notably influenza. Careful bacteriological observations were made, and it was found that this arrest of healing was accompanied by the almost complete disappearance of staphylococci from the wound or ulcer, and the appearance of rod bacteria, especially bacillus fluorescens. The reaction of the wound, etc., was taken with ordinary litmus and reddened litmus paper, and showed that wounds that were not healing were in a high state of alkalinity. Following this up, a large number of cases, healing and otherwise, were examined, and it was found that all healing wounds were but slightly alkaline, and in those that had been stationary as soon as healing began the alkalinity rapidly diminished. On the advice of Professor Wright, of Netley, the method of estimating alkalinity by the strength of the acid solution necessary to neutralise the fluid to be tested, as devised by him for blood examinations, was adopted.¹ Twenty-three observations were then made on cases of ulcers and wounds, recent and chronic, and the results are given in the following table:

¹ *Lancet*, September 18th, 1897.