

**Upon the results of the Hyderabad Chloroform Commission / a communication by Surgeon-Lieutenant-Colonel Lawrie ; discussion by Dr. Gaskell [and others].**

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PROCEEDINGS  
OF THE  
ROYAL MEDICAL & CHIRURGICAL SOCIETY  
OF LONDON.

4.

UPON THE RESULTS OF  
THE HYDERABAD CHLOROFORM  
COMMISSION.

A COMMUNICATION BY *Edward*  
SURGEON-LIEUTENANT-COLONEL LAWRIE.

DISCUSSION BY

DR. GASKELL, MR. VICTOR HORSLEY, DR. SHORE, DR. LAUDER  
BRUNTON, DR. DUDLEY BUXTON, DR. DAVID NEWMAN,  
DR. HEWITT, DR. LAZARUS-BARLOW AND DR. SILK.



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BY

W. L. BARRER

AND

W. R. S. RAYNES

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PROCEEDINGS  
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MEDICAL AND CHIRURGICAL SOCIETY  
OF LONDON.

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**SPECIAL MEETING.**

*Tuesday, July 3rd, 1894.*

F. W. PAVY, M.D., LL.D., F.R.S., Vice-President, in the Chair.

J. MITCHELL BRUCE, M.D., }  
R. J. GODLEE, M.S., } Hon. Secs.

Present—63 Fellows and 43 visitors.

The minutes of the last ordinary meeting were read and confirmed.

The following gifts were announced, and votes of thanks were awarded to the donors:

Annuaire statistique de la ville de Paris, 1880-82, three vols.: presented by Mr T. W. Nunn, F.R.C.S. A Descriptive Catalogue of the Clinical Museum, and Journal of Proceedings; by Jonathan Hutchinson, LL.D., F.R.S.: presented by the Author. Catalogue of Scientific Papers (1874-83), compiled by the Royal Society of London (Gis.-Pet.): presented by the Royal Society. Index to the Periodical Literature of the World for 1891 and 1893: presented by Mr. J. Y. W. MacAlister. Spinal Caries (Spondylitis or Inflammatory Diseases of the Spinal Column): presented by Mr. E. Noble Smith, F.R.C.S. Behind the Tides; 2nd edit., by Charles Bland Radcliffe, M.D.; Muscular Action as a purely Physical Phenomenon, by Charles Bland Radcliffe, M.D.: presented by Mr. D'Arcy Power, F.R.C.S. Erste Hilfe bei Unglücksfällen, von Samuel Osborn, F.R.C.S.: presented by the Author. Transactions of the American Orthopædic Association, vol. vi, 1893: presented by the Association. Transactions of Fifteenth Annual Meeting of the American Laryngological Association (1893): presented by the Association. The Toilet; by Herman Lawrence (St. Vincent's Hospital, Melbourne): presented by the Author. The Proclivity of Civilised Woman to Uterine Displacements: the Antidote; also other Contributions to Gynæcological Surgery; by M. U. O'Sullivan: presented by the Author. Annual Report of the Sanitary Commissioner with the Government of India (1892): presented by the India Office. Official Copies of Reports by Medical Officers on Epidemic Outbreaks to the Local Government Board: presented by the Medical Officer of the Local Government Board.

**Communication from Surgeon-Lieutenant-Colonel Lawrie upon the  
Results of the Hyderabad Chloroform Commission.**

Mr. Vice-President and gentlemen,—Our method of experimentation on the Hyderabad Commission consisted in giving animals

in the first place chloroform in enormous doses, without any particular care to keep the breathing natural and regular, though every change in the respiration was noted on the manometer tracing at the time it occurred. Subsequently we gave chloroform in such a way that the breathing was not interfered with except by narcosis of the respiratory centre, and we discovered the mode of death in uncomplicated chloroform poisoning. Later on it became necessary to confirm and amplify our results by cross-circulation experiments; and we found that whenever we managed to get a cross-circulation established and maintained, we obtained uniform results, which entirely confirmed the previous experiments. It is true that there is a discrepancy between the results obtained in the cross-circulation experiments performed at Hyderabad and those performed at Cambridge by Drs. Gaskell and Shore. This discrepancy is accounted for by certain facts which are brought out by the Cambridge tracings. I have noticed that in typical Cambridge tracings—for instance, in the tracing of Experiment No. 6—the blood-pressure of the feeder was excessively low. It was 50 mm. at the beginning of the observation, and 24 mm. at the end. It seems to me that an animal with a blood-pressure ranging from 50 to 24, and which was evidently in a dying state, could not possibly maintain a cross-circulation with another animal; and that therefore in this experiment, which was intended to demonstrate the effect of chloroform sent to the brain of the fed animal alone, the fed got no chloroform. In the second place, in Experiment No. 9 chloroform administered to the fed direct is said to have stopped its heart, and the animal's respiration then stopped and it died. I put it to the Society whether, if the fed had been receiving a supply of pure blood from the feeder, its brain ought not to have gone on living, as I shall show you later it did in one of the cross-circulation experiments performed at Hyderabad, where the fed's heart was stopped permanently by chloroform. Keeping these facts in view, the question we have to decide to-night is, Is it right or wrong to take the pulse as a guide to the effect of chloroform? The answer to this question depends entirely upon whether chloroform has any direct action upon the heart or not; and the task I have set myself is to prove to you that it has not, and that if the pulse is really taken as a guide to the effect of the anæsthetic, danger and death from indirect cardiac failure must be unavoidable.

Before I ask you to examine the tracings I will only remark that from 1889, when the experiments of the Hyderabad Commission were performed, we have made steady improvements in the method of chloroform administration, which correspond with the gradual completion of the proof that the drug does not affect the heart directly; and we attribute our immunity from deaths solely to our disregard of the heart as a factor in chloroform anæsthesia. I think I shall be able to prove to you that the heart is literally and truly the chloroformist's best and only friend; and that if accidental over-dosing occurs, the heart may not only be safely left to itself, but may confidently be trusted to tide the patient over the period of danger while the overdose is being got rid of, provided this is not large enough to have caused permanent arrest of the respiration.

The first tracing to which I desire to draw your attention is one of a cross-circulation experiment, in which chloroform was sent to the animal's heart alone for three minutes and twenty-four seconds. It produced no effect of any kind whatever: no lowering of the blood-pressure, no anæsthesia, and no arrest of the respiration or of the action of the heart. This is the invariable result if you can get a cross-circulation established and maintained between two animals, and we rely upon it for the final proof that chloroform administered by inhalation has never under any circumstances a direct action upon the heart.

The second tracing is one of an experiment which was performed on March 6th, 1890. This tracing shows you what takes place when chloroform is sent to the brain alone; and it is on tracings of this kind, when chloroform is sent to the brain and not to the heart, that we rely for the proof that the fall of the blood-pressure, which is produced by the direct action of chloroform, is entirely due to vaso-motor narcosis, and is harmless or perhaps beneficial to the heart. The second point about this tracing is, that it shows practically everything which takes place in death from uncomplicated poisoning by chloroform. It is a tracing of an experiment in which the animal's breathing was absolutely regular and natural until it failed and was arrested by narcosis or paralysis of the respiratory centre. It demonstrates that when chloroform is given with regular breathing it produces lowering of the blood-pressure with, first anæsthesia, then stoppage of the respiration, and then failure of the action of the heart and death.

If you consider this tracing you will see that when the respiration

showed that the administration was becoming dangerous, and even when it failed and stopped, there was no sign of failure in the pulse, and the animal might easily have been restored by artificial respiration. You will also see that when the pulse showed signs of danger, which was not till several minutes (three) after the respiration had ceased, the animal was practically in a hopeless state, and recovery was probably impossible.

To put it in another way. You can see from the tracing that the information furnished by the first sign of failure in the pulse amounted to nothing more than an intimation that the breathing had stopped three minutes previously. It is clear, then, that by taking the respiration as a guide the chloroformist would have had early and ample intimation of the approach of danger, and that the animal's life would never have been jeopardised, whereas it is equally clear that he would have perceived no sign of danger in the pulse until it was in all likelihood too late to save the animal's life.

Having contrasted the difference in the results of taking the respiration and the pulse as a guide in chloroform administration with regular breathing, we ought next to examine the difference between the two methods when the breathing is irregular. But, practically speaking, this is superfluous, because, no matter how chloroform is administered, the information furnished by the pulse is always second-hand information, and can only show that something has gone wrong with the breathing, which might have been prevented by attention to that function itself.

The third point which this tracing brings out is our oft-repeated contention that chloroform anæsthesia alone, which is all the chloroformist ever wishes to produce, is free from risk. By examining the tracing you will perceive that if chloroform is not pushed beyond the point where anæsthesia is complete, the patient or animal never comes within the sphere of danger at all. The perfection of chloroform administration consists in maintaining the blood-pressure and the anæsthesia exactly at the level of the dotted line on the diagram during the whole time the surgeon is operating. The question is, Can this always be done, and is it the fault of the chloroformist if it cannot? In a certain class of cases, as for instance in particular operations about the mouth, the inhalation ought not to be pushed as far as this, because the patient, if too thoroughly anæsthetised, may incur risk from choking during the surgeon's manipulations, or from

blood finding its way into the air-passages. In a second class of patients there is no difficulty either in producing normal anæsthesia—with a perfectly regular fall of the blood-pressure up to the point where the anæsthesia is complete—or in maintaining it afterwards at the level of the dotted line in the diagram. But there is a third class of cases, by far the largest, in which irregularity in the fall of the blood-pressure is produced by irregularities of the breathing occurring while the patient is going under. These irregularities can be minimised by skill on the part of the chloroformist, but they frequently occur in spite of his skill, no matter how skilful he may be. Owing to fright or to the pungency of the chloroform vapour, some patients resist and struggle, and breathe irregularly from the commencement of the inhalation. Others become intoxicated before anæsthesia is complete, and struggle violently and hold their breath and otherwise breathe irregularly. In others, again, narcosis of the respiratory centre sets in very early in the administration, and may produce what may be called “automatic asphyxia.”

What do all these facts amount to? Certainly not to an argument in favour of taking the pulse as a guide. The pulse is no guide to anæsthesia; no one can tell by the pulse when anæsthesia is complete, and during the struggling stage, when there is more than at any other time a risk of the patient inhaling an overdose, it cannot be felt at all. What they do amount to is the strongest possible argument in favour of the most unvarying and undivided attention to the respiration—an attention so undivided and intelligent that whatever irregularity may occur in the breathing, an irregular intake of the drug is never allowed to take place.

When once the chloroformist has taken the patient safely through the first stage up to the point where anæsthesia is complete, there ought to be no real difficulty in maintaining the anæsthesia and the blood-pressure at the level of the dotted line in the diagram. It must not be forgotten, however, that no anæsthetist ever meets with two patients who inhale chloroform exactly in the same manner, and that what is an overdose to one individual is a normal or an insufficient dose to another. There is no time to go into this point more deeply. I think I have shown that, while it is possible to give chloroform by proper attention to the respiration with uniform safety, no man can do so, however clever he may be, who dreads heart failure and takes the pulse as a guide. In fact, the safety



of the patient when the pulse is taken as a guide is proportioned to the amount of attention which is bestowed upon the respiration, and upon nothing else.

The last point which we have to consider is the most interesting of all. It relates to the mode of death in uncomplicated chloroform poisoning. Dr. Ferrier lately called my attention to the important bearing which Mr. Horsley's experiments upon bullet wounds of the head have upon the question of death from chloroform; and Mr. Horsley has given me permission to make use of his observations in any way I please. I shall give you them as far as possible in his own words, which cannot be improved upon. The effect of a bullet entering and traversing the cranial cavity at moderate velocity is to produce "complete arrest of the respiration and a slight fall of the central blood-pressure. A little later (five tenths of a second) than the arrest of respiration a remarkable rise in the blood-pressure occurs, this rise continuing until the normal tension is exceeded. These observations prove beyond doubt that the first cause of death is not what it is usually supposed to be, and as taught in the text-books, namely, arrest of the heart and syncope, since, as you see, the heart goes on beating although the respiration has completely stopped. Furthermore, if we quickly perform artificial respiration, we obtain recovery from the otherwise fatal arrest."

Mr. Horsley's remarks about the first cause of death in bullet wounds of the brain apply word for word to death from chloroform. But the resemblance between them is much more complete and remarkable than it appears to be at first sight. We must consider them *seriatim*. (1) In death from chloroform the respiration is arrested, but it is arrested gradually, and not suddenly as in death from bullet wounds. It is quite possible that in death from chloroform the respiration may occasionally, and under certain circumstances, be arrested so rapidly as to amount practically to a sudden stoppage, but I have never seen this sudden stoppage of the breathing take place except when anæsthesia was complete. (2) The second point of resemblance is that in death from chloroform the stoppage of the respiration is followed by gradual failure of the heart—it simply runs down. This is precisely what Mr. Horsley states about death from bullet wounds. (3) In chloroform poisoning the arrested respiration may frequently be restored by artificial respiration; and if death ensues, the post-mortem appearances are those of paralysis of

the respiratory centre and not of asphyxia. This is precisely what Mr. Horsley states with regard to death from bullet wounds of the head. (4) In death from chloroform the respiratory centre is narcotised or paralysed from the commencement, and is never stimulated at first as it is in asphyxia. This remark applies equally to bullet wounds, except that the narcosis of the respiratory centre is gradual in chloroform poisoning, and paralysis of that centre is sudden in bullet wounds of the brain. (5) The proper thing to do in chloroform poisoning is to employ artificial respiration rather than the giving of stimulants. These, again, are Mr. Horsley's own words with regard to bullet wounds of the brain. (6) In chloroform poisoning the narcosis may be complicated by vagus stimulation, which produces marked slowing of the heart. The same thing occurs in bullet wounds of the brain; hæmorrhage may be produced, which stimulates the vagus centre by pressure on the medulla and causes marked slowing of the heart.

In chloroform poisoning we regard this slowing of the heart as a safeguard, in that it tends to diminish the blood supply which is conveying the poison to the brain and producing the fatal narcosis. In bullet wounds of the brain the slowing of the heart may also be a safeguard, in that it tends to arrest the hæmorrhage which is producing the fatal compression.

Lastly, from the year 1879 the opinion has gained ground that death from chloroform is due to primary arrest of the heart and syncope. This belief corresponds closely with the opinion hitherto, however, held universally, that bullet wounds of the brain cause death by arrest of the heart and syncope. No doubt in death from chloroform the process may be complicated by asphyxia, but it is important to bear in mind that the only danger of asphyxia under chloroform is to cause an increased in-take of the anæsthetic and rapid narcosis of the respiratory centre, a narcosis which may be so rapid and permanent as to appear like sudden death.

It has been stated that when the respiration stops in chloroform poisoning the heart may stop suddenly. I have frequently seen reflex stoppage of the heart mistaken for heart failure in the laboratory—dogs removed as dead, because the needle in the heart had stopped moving, which afterwards revived spontaneously. I have recorded one case in the operating theatre, where a child was left for dead because no heart-sounds could be detected after its respiration had

stopped from an overdose of chloroform. This child, like the dog, revived spontaneously.

The only authentic instance I know of in which the heart actually failed within a very short time after cessation of the respiration occurred in a cross-circulation experiment in Hyderabad. Chloroform was administered to the feeder and the fed's breathing stopped. The chloroform to the feeder was stopped, but the fed's heart very quickly (in a few seconds) ceased to beat. The feeder revived, and so also did the brain and head of the fed. The fed's head then made unavailing movements of respiration, and in a few minutes it was fully restored to consciousness. We tried to perform artificial respiration, but the fed's head seized a student's finger and bit it viciously. The fed's thorax was then laid open, and its heart was removed from its body. The head went on living afterwards, as an appendage of the feeder, for twenty minutes, as long as the cross-circulation was maintained. This is what ought to have occurred in Drs. Gaskell and Shore's cross-circulation experiment No. 9, if the fed animal's brain had been properly supplied with pure blood from the feeder at the time its heart stopped—*i.e.* if the cross-circulation had been established and maintained, which their own tracing shows clearly could not have been the case. In this experiment the fed's heart had been profoundly weakened beforehand by phosphorus poisoning. Allowing that the same stoppage of the heart might occur clinically in patients with very weak hearts, this affords no reason for watching the pulse; but, since the stoppage depends on the stoppage of the respiration, it forms an argument in favour of additional care being taken to maintain natural regular breathing in all cases of this kind.

To sum up our position. We claim that the tracing I have shown you of the experiment of March 6th, 1890, demonstrates the fact that the first cause of death in uncomplicated chloroform poisoning is stoppage of the respiration from narcosis of the respiratory centre, and not arrest of the heart and syncope. Unless it can be proved that our experiments and Mr. Horsley's are unreliable, it is clear that the first cause of death from chloroform is on all-fours with the first cause of death from bullet wounds of the brain. The discrepancy between the cross-circulation experiments of Drs. Gaskell and Shore performed at Cambridge and those performed at Hyderabad thus ceases to be a matter of any importance, as the proof that chloroform

only affects the heart by failure and stoppage of the respiration needs no further confirmation.

Dr. GASKELL.—The author of the paper just read claims that his cross-circulation experiments prove that chloroform given by inhalation to the heart alone produces no effect; while Dr. Shore and myself consider that our experiments prove conclusively that chloroform given by inhalation to the heart alone has a most marked weakening effect upon the heart. I would point out that our cross-circulation experiments have been many, and we have made others since the publication of our results, and they one and all without exception show indisputably that chloroform when given to the fed animal in sufficient concentration is able to reduce the action of its heart to zero. Our conclusions are in this respect in accordance with those of all other physiologists who have worked at this question except those of the Second Hyderabad Commission, although here too we see, as we have pointed out in our paper, their curves if rightly interpreted point to a weakening of the heart as the result of the administration of chloroform. It therefore seems to me that where there is a mass of evidence on the one side of a positive nature, if that evidence is to be overthrown, something more is wanted than a solitary experiment in the opposite direction such as Dr. Lawrie has brought forward to-night, especially when, as in this case, this solitary experiment is purely negative. It is an axiom of scientific investigation that when a number of positive results in one direction have been obtained by competent observers, a much larger number of consistent negative results must be produced before they can be discredited, and in addition the fallacy upon which the positive results depend ought to be clearly exposed. Now let us see how far the author of this paper has made good his position to-night. In the first place he has criticised two of our tracings, fig. 6 and fig. 9, in our paper by means of two diagrams purporting to represent our curves. These diagrams are misleading, for they give the impression of a continuous curve, whereas in reality our figures represent a series of small samples of the original long curve taken at intervals, as given in the text of our paper, so that, for instance, the curve of the feeder in fig. 6 is not steadily and continuously falling as represented in Dr. Lawrie's diagram, but the pressure remained constant, or very nearly so, for a period of  $2\frac{1}{2}$  minutes, varying only from 54 to 50 mm. Hg. until it fell rapidly, when chloroform was given to the feeder. His criticism of these two curves is to the effect that the cross-circulation was not properly carried out, and that in reality blood from the heart of the fed was able to reach the brain of the fed in each case, so that the effect of giving chloroform to the fed was in reality due to that chloroform-containing blood reaching the brain of the fed. He bases this criticism on the assertion that in fig. 6 the blood-pressure in the feeder was too weak to efficiently keep alive the brain of the fed, and in fig. 9 that the respiration of the fed ceased simultaneously with the cessation of the heart-beats. Now, in the first place, fig. 6 is only one of a whole series of experiments, in some of which the blood-pressure in the feeder was

low, while in others it was so high that there could be no doubt whatever that it was sufficient to send plenty of blood to the brain of the fed, and yet in all these experiments chloroform given to the fed produces a fall of pressure as in fig. 6; and, indeed, the height of the pressure in the feeder, whether high or low, as long as there is sufficient blood sent to the brain of the fed to keep the respirations going, can make no difference to the action of chloroform inhaled by the fed, provided the brain arteries are properly ligatured. Now the test of the efficiency of the ligature of the four brain arteries is well known by the experiments of Kussmaul and Tenner, for as soon as the last artery is occluded convulsive struggles—known by the name of Kussmaul-Tenner convulsions—set in, and we have always found in our investigations on the effect of ligature of the four brain arteries, as mentioned in our paper, that these convulsions occur when the last artery is closed. Neither in fig. 6 nor any other of the cross-circulation experiments did we get any sign of these convulsions upon closure of the last artery, because at the same moment the cross-circulation was established—evidence in our opinion sufficient to show that in this case, as in the others, a sufficient amount of blood passed from the feeder to the brain of the fed to keep that brain alive, and therefore that Dr. Lawrie's criticism has no force whatever. With respect to his criticism of fig. 9, that after chloroform had been given to the fed the respiration of the fed failed simultaneously with the failure of the heart, it is surprising to me how he could have made such an assertion when we distinctly state that the last respiration of the fed took place four and a half minutes after all sign of heart-beat had ceased. The main point of the cross-circulation method is that it gives proof positive of the absolute cessation of the heart's action in consequence of the administration of a large dose of chloroform while the respiration still continues. In all our experiments this has been invariably the case: the respirations of the fed animal have always continued for a considerable time after its heart-beat has been reduced to zero by the inhalation of a large dose of chloroform. So much, then, for Dr. Lawrie's criticism of our results; and now let us turn to the consideration of this single experiment of his which he actually has the courage to bring forward to-night in disproof of our results and those of other physiologists. You will see from the tracing that the time during which chloroform was given to the fed animal was about three minutes, and at the end of that time the experiment ended without any appreciable fall of pressure having been produced. Well, now, unless chloroform is given very strongly, it is the commonest thing in the world not to find any marked fall in the blood-pressure of the dog until more than three minutes after the commencement of the administration of the chloroform; you must wait a little more patiently, and then you will see the blood-pressure fall and the heart finally cease to beat. Why did this experiment cease so quickly? why are we not privileged to see more than three minutes' length of this important crucial experiment? Because there is an interruption at that point, an interruption in the shape of a clot in the manometer tube. Good heavens! this large meeting has come here to listen to the evidence which is to prove that our series of care-

fully conducted cross-circulation experiments have led to conclusions absolutely contrary to fact, and that evidence resolves itself into a single cross-circulation experiment which ceases at the end of three minutes owing to the formation of a clot in the manometer tube! I appeal to every one here present if it is possible to imagine the slightest chance of making an efficient cross-circulation experiment when the formation of a clot in the manometer tube cannot be prevented for more than three minutes. Consider the number of vessels to be connected together by glass tubes, and think what chance there is of blood which clots so easily being able to circulate through. It is absolutely indispensable that the blood should be prevented from clotting before a cross-circulation experiment can even be attempted, and it almost passes belief that anyone should come here and base a physiological argument upon such evidence as that tracing. For this reason alone these experiments of Dr. Lawrie are absolutely worthless, and he has not brought forward to-night one single atom of evidence to negative the conclusive proof afforded by our cross-circulation experiments that chloroform when inhaled causes a fall of blood-pressure by weakening of the heart's action, and that such weakening may be so great as to produce absolute cessation of the heart's beat.

PROFESSOR VICTOR HORSLEY.—I wish to say a few words simply in reference to the danger to life from chloroform administration, and I will illustrate my meaning by four projections on the screen. My experience of anæsthetising animals with chloroform is very large, and I have also had the misfortune to witness three deaths of human beings from chloroform. I am perfectly persuaded that in the vast majority of instances death is due to arrest of respiration, and if a human being be inverted and artificial respiration established many would be restored to life. Still there is truth probably on both sides, and further experiments will probably settle certain matters at present unsettled. In any case the author's experiments have directed attention to the fact that the respiratory centre is the most sensitive among all the centres of organic life. I wish particularly to call attention to the fact that in all cases of cerebral pressure, of compression, or rise of intra-cranial pressure, precisely the same mode of death occurs. The knowledge of this fact has been the means certainly of saving the lives of three patients, cases in which chloroform had been given and the patients stopped breathing on the table and were apparently dead,—in fact, they were only restored after artificial respiration had been carried on long enough to enable us to open the skull and reduce the intra-cranial pressure. Personally I feel so sure of this that although in some cases the heart may also fail, I should at once practise artificial respiration and inversion of the patient in all cases of chloroform narcosis.

DR. SHORE.—Since we published our cross-circulation experiments in January, 1893, we have waited anxiously for details of the experiments performed by the author in Hyderabad. Soon after we published our results he published tracings of those cross-circulation experiments, and some six months later one more tracing. We looked at them very carefully, but did not consider it necessary to publish

any reply. I came here to-night chiefly to see these particular tracings—and others. The author tells us in the 'Lancet' of February 11th, 1893, that he performed cross-circulation experiments almost daily for a period of time extending, I believe, over several months, so that we ought to have had at least twenty or thirty tracings to examine. He has brought us one—one, too, that he has published before. Of cross-circulation experiments we have done six. All these were published, and since then we have done one more. This one was done because we had a new method of performing the experiment. I much regret that the author has not gone on publishing the work of the Hyderabad Commission as it was published some years ago. That vast number of experiments, upwards of 700, all of which were published, is one of the most valuable pieces of work ever done on this question. It is valuable because the results are there written irrevocably by the animals themselves on the smoked paper. Any physiologist can take these tracings, can read them for himself, and see what was the effect of the chloroform. One does not want to read the Commission's report on the tracings, it is only necessary to look at the tracings themselves. We have looked at these tracings, and we have found no evidence which supports the great proposition which the author has laid down, namely, that chloroform has no direct action on the heart. On the contrary, we have pointed out that many of them indicate the direct opposite. I regret that we have not these twenty or thirty tracings of the author's cross-circulation experiments for us to see for ourselves what they teach. We do not want the author to read his tracings. We will read them for him. Let us then examine the tracings which he has published. The first is published in the 'Lancet' for February 11th, 1893, an experiment in which the brain of the fed animal was supplied with blood from the feeder by one carotid only. This contrasts at once with our experiment (tracing fig. 9 of our publication) in which the author says the cross-circulation was inadequate, in which we supplied the brain of the fed animal with blood from the feeder by two carotids and one vertebral artery. Now this first experiment of the author is one in which he gave chloroform to the feeder, and so presumably sent chloroform to the brain of the fed; after two minutes and fifty-five seconds a clot was recorded in the tubes. So much for his first experiment. The second experiment of the author, the tracing of which is represented by the diagram shown here, is one in which he gave chloroform to the fed animal, and this presumably passed to its heart and general system, but not to its brain. Chloroform was given for three minutes, and then the experiment was stopped and a clot recorded in the tube. If you look at the tracing in the 'Lancet' you will see that some fall in the blood-pressure was produced, although it is not so evident in the diagram. In some of our similar experiments there was only a small fall of pressure in the first three minutes after the chloroform was put on, but after that the pressure fell rapidly. This, the second published experiment of the author, fails because it was not allowed to go on long enough to show the effect well. In the author's third experiment, to which he has not referred this evening, chloroform was also given to the fed, in this case for two minutes fifty seconds, and a marked

fall of blood-pressure was produced. Both these experiments show an action of chloroform on the heart. The fourth experiment of the author, published in the 'Lancet,' July 1st, 1893, is of no value as a cross-circulation experiment, for the brain of the fed is not separated from its vascular system. This is obvious from the fact that after its respiration had stopped recovery of respiration was brought about by artificial respiration. In the author's criticism of our experiments, published in the 'Lancet' for April 1st, 1893, he referred only to two. In these chloroform was given to the fed animal, and the blood-pressure fell. He says that it fell because some of the chloroform reached the brain. Now the author maintains that chloroform kills by its action on respiration. We agree with him that in general it does. We only join issue with him as to whether the heart is directly affected by chloroform or not. We and others have shown that it is; he says it is not. Now let me ask him this question: If chloroform is getting into the brain of these animals as well as in the general vascular system, he must admit that it would act on the respiratory centre more powerfully than on the vascular system; how then does he explain the fact that the respiration was not arrested before the heart stopped? In one of these experiments, and this is the experiment to which the author has referred by a diagram this evening, the animal went on breathing some four minutes after its blood-pressure had fallen from 105 mm. to 15 mm., and all evidence of heart-beat was lost. This is proof that no appreciable amount of chloroform had reached the brain. I think when a great proposition like this, namely, that chloroform has no direct action on the heart, is put forward by anyone, we are entitled to ask for proof of it. The mere assertion of the author will satisfy no one. The author has made the assertion, has made it a great many times, and he brings forward certain experiments, some of which show the direct opposite, while others are valueless for very obvious reasons, while in proof of his proposition he has brought forward nothing.

Dr. LAUDER BRUNTON.—The question before us is one of very great importance. Every one of us is interested directly or indirectly in the question, and we must not lose sight of the main point in the endeavour to settle minor points. The main point before us to-night is really the safety of chloroform as a means of anæsthetising. Whatever our opinions may be in regard to the question that the author has brought before us, I think we must be of one accord in thinking that he has done an enormous service towards the investigation of the question he has taken up, and that he has brought together a body of evidence greater than was ever before available to settle the question whether chloroform acts directly on the heart or not. But with all this we must remember that it is not the question whether chloroform acts or can act on the heart that is before us, but only whether chloroform acts on the heart when administered by inhalation. It was clearly expressed in the report of the Hyderabad Commission that chloroform can act as a protoplasmic poison, and that it will destroy every living thing if present in sufficiently large quantity. It will paralyse respiration, it will destroy the heart, it will render muscle as rigid as a board. We have never questioned the power of chloroform to



destroy the vitality of the heart if brought into contact with it in sufficient quantity. The question before us is whether it can be brought into the heart in sufficient quantity to destroy its vitality when given by inhalation. Our contention is that when given thus it always paralyses the respiratory centre, thus stopping its own introduction into the blood before a sufficient quantity is taken in to paralyse the heart. Now Dr. Gaskell has very rightly insisted upon the necessity for evidence; but we have, in regard to the action of chloroform, an immense body of evidence. When looking over some old numbers of the 'Lancet' for 1848, I saw that Mr. Wakley had brought forward 100 experiments showing that chloroform killed by paralysing the respiration, and not by paralysis of the heart. Since then an immense number of experiments have been made, and the conclusion is the same throughout, viz. that when given by inhalation it kills by stopping the respiration, and that the heart continues to beat after respiration has ceased. This is precisely what occurs when an animal is asphyxiated. However healthy an animal may be, if you arrest the respiration the heart is bound to stop some time afterwards, being poisoned by the venous blood. Now in regard to the action of chloroform upon the blood-pressure: this is a question of completely minor importance. We do not say that alcohol is a dreadful poison because it paralyses the heart, yet alcohol has precisely the same action on the heart as chloroform, though in a less degree. Put a heart into alcohol and it will stop; inject enough alcohol into the jugular vein and the heart will stop beating. Narcotise an animal thoroughly with alcohol and you will get a fall of blood-pressure similar to that produced by chloroform. When we have got before us the result of experiments done by honest men, done thoroughly and recorded conscientiously, and yet apparently entirely antagonistic, it seems to me that we ought to look upon them as facts seen from different points of view, and to inquire whether in reality they do not agree. One point upon which Drs. Gaskell and Shore have laid much stress is that they got no clot in their experiments. How was this? It was, as I understand, because they injected into the veins of the animals a quantity of peptones. Now we know that peptones are a powerful poison. They destroy the coagulability of the blood, and what else they do we do not know, but probably very many other actions are produced over and above the effect on the coagulability of the blood. Dr. Lawrie noted that in one case the heart stopped rapidly because the heart had previously been weakened by the use of phosphorus. The Commission saw that it would be necessary to distinguish between death from chloroform and death under chloroform, the latter being due to the operation of asphyxia or to poisoning of the heart by venous blood, or by the presence of substances not normally found in venous blood. In Dr. Gaskell's experiments it was a very poisonous substance that had been injected into the blood. In Dr. Lawrie's experiment, in which death occurred rapidly as the result of heart failure during the administration of chloroform, we had a case of poisoning, not by venous blood, but by blood rendered more toxic by the previous administration of phosphorus. He has put the question before us very fairly. He has

shown by an immense body of evidence obtained by him and others that the first sign of the action of chloroform is the failure of the respiration, and that if we neglect to watch the respiration and wait for the heart to give us warning, we are like the driver of an express train who neglects the distance signal, and only endeavours to pull up when the home signal gives warning of danger ahead, very often too late to avert an accident. We can watch the circulation, but if we wait for that to give us the danger signal we shall often know it too late. But can we foretell the danger of poisoning in the person himself? At Hyderabad we only began the experiments on the effect of phosphorus in forming poisonous alkaloids, and of alterations in the kidneys induced by cantharides in preventing their excretion in the urine. Since that time the question has been taken up by Professor von Poehl, who told me that he had made a number of experiments, and that by examining the urine of patients beforehand for the amount of alkaloid present he knew whether they were going to get any trouble from chloroform, and when the case was going to be an easy one. If there was much alkaloid in the urine there was a tendency to stoppage of the respiration or the heart, whereas if there was little, then one could proceed with confidence. This seems to me to explain a condition which was before unexplainable, viz. that very frequently the cases which give us trouble are not the poor sick people, but the strong and healthy—the pictures of health—those, in fact, who have been feeding well before being anæsthetised. The risk, probably, is that the healthy man has a larger quantity of alkaloids in his body, whereas the poorly fed is probably comparatively free. In looking over statistics, I think it may be affirmed that there has been a marked increase in the number of deaths from chloroform during the last few years. It occurred to me that there must be some new factor of late years to account for this increase. I could not find any, however, until I thought of the change that has taken place in the foreign meat trade. This must have materially altered the feeding of the population to an enormous extent. There is another point, and that is the geographical distribution of deaths from chloroform. In India, in Egypt, in the Southern States of America, and in hot countries, everybody swears by chloroform, while in the colder countries they rather swear at than by chloroform. In Edinburgh, however, we have a marked exception to this geographical rule, and there is a very great difference between the condition of the people in Edinburgh and in London. The people in Edinburgh used to be comparatively poorly fed, and deaths from chloroform were almost unknown, though more frequent of late years. In Edinburgh, when I was a student, we hardly ever met with a case of gout among the out-patients, and when one cropped up he was kept as a specimen for teaching purposes, while in London one gets any number of them. This only indicates one line of research. It is quite evident that we have got as far as we can go along the ordinary lines, and if we are going to find out why deaths occur during the administration of chloroform we must look out for some other lines to work upon. Whether this be so or not, the conclusion we cannot help drawing from the whole number of experiments from the time of the intro-

duction of chloroform is that chloroform does paralyse the respiratory centre before the heart, and the great thing is to look after the respiration as giving the first indication of danger to the patient.

Dr. GASKELL.—I wish to point out that there is no difference whatever in respect of the peptones injected by the author and ourselves.

Dr. BRUNTON.—Then why did he obtain clots and you not so?

Dr. GASKELL.—Because he did not use the right amount.

Dr. BRUNTON.—Exactly so.

Mr. BAILEY.—These experiments are quite beyond me, so that I must turn to the practical side of the question. I can only say that conclusion No. 5 in the précis, "free from risk," refers to a condition of safety which I consider is impossible to attain. When the patient is reduced to respiration and circulation there must be risk, and the whole tenour of the Hyderabad Commission inquiry has been to show that there is no risk whatever in giving chloroform. Those of us who have given chloroform for several years must know that there is great risk even by the very best of methods. There was probably never a better anæsthetist than Clover. I am sure that he watched the respiration and the pulse, but he did not say that respiration failed before the pulse. The whole tenour and the teaching of the present day has been that chloroform does paralyse the heart, and that ether stimulates the heart. I have never been able to see why an ordinary chloroformist cannot do two or three things at the same time. He can watch the respiration—indeed, he should do so—and he can keep his finger on the pulse; but my own way is to attach great importance to the condition of the pupil. In cases of danger the pupil begins to dilate. The pupil ought not to be dilated with any of the three anæsthetics. Coming to the practical point, I see no reason why we should trouble ourselves to look after the respiration only. Personally I have never lost a case under chloroform. Clover used to give  $4\frac{1}{2}$  per cent. of chloroform in his bag, and I saw with him six or eight patients as near dead as possible. Now I have never given more than  $3\frac{1}{4}$  per cent. of chloroform, and I have never seen any cases approaching those I saw with Mr. Clover. I should, therefore, be rather inclined to think that we must look a little to his method of giving the vapour.

Dr. DUDLEY BUXTON:—Sir, I arise in response to your call and propose to speak more from the side of practice than that of experimental research. There has been a striking variance in the results at which experimentalists have arrived, due no doubt to extreme complexity of the methods of which they have made use, but there can be no variance in the opinion of the Fellows of this Society upon one aspect of the question. There must be absolute accord in our sentiment, when we remember that it is really through the remarkable munificence of an oriental potentate, H.H. the Nizam of Hyderabad, that this question of chloroform has been brought so prominently before us. To his public-spiritedness and generosity it is that the research work was carried out in Hyderabad, in Cambridge by Dr. Gaskell and Dr. Shore, and in Philadelphia by Dr. Hare and Dr. Thornton. Probably no one would hesitate to offer to Dr. Lawrie a tribute for all he has done, for his enthusiasm and acumen, but I

think we should go a step further, and that this Society should take this opportunity of expressing its cordial appreciation of the altruism and philanthropy of H.H. the Nizam of Hyderabad. Passing to the contentions advanced by Dr. Lawrie, I am bound to admit that they are far more convincing than one had previously thought. I have no doubt in my own mind that of the large number of deaths under chloroform by far the largest proportion are due to careless administration, and of the residue the majority are due to failure of respiration, and could be avoided if a more rigorous watch were kept upon the actual intake of air by the patient. But when I say this, I am not really retiring from the position I have always held and maintained alike in speaking and writing. I have always held and taught that the respiration should be the chloroformist's guide, but I am greatly impressed by the more thorough way in which this guide is, in Dr. Lawrie's plan, watched and followed. In this I think we have been taught a lesson. To use a colloquialism, the respiration needs watching as a cat watches a mouse. But when we have said all this, there remains a small residuum of cases with which some of us have had experience, and of which most of us have read, that do not, I think, find ready explanation upon the idea that cardiac failure never takes place under chloroform. I refer to those terrible catastrophes which occur during the inception of anæsthesia, when chloroform is given by a careful man who notes nothing wrong with the respiration, until suddenly, without any violent struggling and without any apparent reason, the patient "snuffs out" even before the operation has been commenced. I think, further, we must bear in mind that from the very inception of anæsthesia the medullary centres are being gradually narcotised, and as the degree of narcotism which proves dangerous must vary for each individual and for each occasion of his taking chloroform, it is not so simple a matter as at first sight appears to lay down definite rules, and certainly we can never say we are working with absolute safety. The degree of anæsthesia also must vary, and I am inclined to think that, rightly or wrongly, surgeons over here require and expect a more profound degree of narcotism than can be obtained unless the drug is pushed to its physiological degree, in other words, until the respiration is in some way affected. It is this profound anæsthesia which induces many surgeons to prefer the use of chloroform. It is, perhaps, worth considering that, although we recognise the power chloroform has of acting as a respiratory poison, yet we habitually employ it as an anæsthetic in all cases in which we expect respiratory difficulties, *e. g.* removal of the tongue, of the jaw, in post-nasal adenoid growths, &c., and it becomes a grave question whether we should not delimit its use in such cases, reserving it rather for abdominal and other surgery in which respiration is not interfered with. With regard to Dr. Lawrie's view, that to watch the pulse is to incur a risk of missing the first signs of danger under chloroform, I think he holds a mistaken view of what is meant by watching the pulse. It has been my custom, and I believe it is the custom of most London chloroformists, not to rely upon the pulse as a guide to the progress or stage of anæsthesia, nor indeed to keep the finger constantly upon

the pulse, but to take the radial pulse from time to time to ascertain the actual state of the patient. It has always been the wont of surgeons and physicians to rely upon the pulse as an indication of the general state of a person's vitality, and the chloroformist I submit only refers to the heart or pulse in precisely this way. I am strongly of opinion that, viewed in this light, no one would feel justified in ignoring the pulse, although he might feel, and probably with great show of reason, that the danger of chloroform in most cases arises from overdosing the more delicate of the medullary centres, viz. that of respiration. If this discussion fails in every other way it will, I hope, enforce upon the minds of all that chloroform can only be given safely by those who give the most sedulous attention to the patient's respiration. It should make us more heedful but not more anxious.

Dr. DAVID NEWMAN.—I have taken a great interest in the action of anæsthetics ever since the Glasgow Committee appointed by the British Medical Association. Our experiments, however, differed materially from those of the author. We simply believed that death under chloroform occurred only as the result of interference with respiration through the nervous system. Of course we know that death may occur from an impediment to respiration, but this cause is preventable, and the only cause that is not preventable is that which attacks respiration through the nervous system. Now it is well known that the respiratory centre is more easily inhibited than the cardiac centre. We admit that. We confessed candidly that respiration is more likely to fall into abeyance than the heart, but there are certain cases where you may have a very sudden arrest of cardiac action. Now I have studied these both in the laboratory and in the operating theatre, and it seems to me that the periods of danger are while the patient is going under chloroform and while he is coming out. While he is deeply under chloroform the danger is not so great. If he is excited beforehand, or is subjected to sudden shock, you may have a sudden intermission, similar to that following stimulation of the vagus nerve. Now this is one point of discussion between us, but the discussion is not as to the tracing or as to the reading of it. We say that this sudden depression is due to the action of the vagus. He says it is from the pulmonary side. Now what are the changes that take place in the lungs during anæsthesia? These changes can only be studied in the very lowest animals, viz. in the frog. Fourteen years ago I attempted some experiments on the frog, to see what result chloroform produced upon the circulation. One preliminary remark, however. A thing that is often and easily forgotten is that the circulation of the blood is not dependent entirely upon the heart; the lungs perform a very important function. During inspiration, as naturally carried on, not only do we inspire air, but we also inspire blood through the big vessels, so that if respiration ceases you have this most important factor shut off. When a frog is placed under chloroform, and then a cannula is introduced into the trachea, artificial respiration being carried on carefully, it will be seen that when the anæsthetic is gradually pushed off the capillaries, then off the arterioles, and ultimately off the larger vessels, this obstruction

to the circulation comes on slowly, and corresponding with this you have an alteration in the heart's action. During the early stage of anæsthesia, paralysis of the heart occurring comparatively rapidly, the number of the contractions gradually diminishes. Then during moderate anæsthesia you have an up-stroke and a down-stroke equally on a straight line, but when the drug is pushed further you have first of all an auricular contraction, a slight curve, and then straight down. In still deeper anæsthesia you have a more distinct amount of the efforts of expiration, then a rise, then a fall, and so on. You have these two or three auricular contractions before the ventricular contractions, and the same thing occurs in the human being. I have had experience in the observation of cases of death on the operation table from cardiac failure. One case was the patient of a colleague who for some reason was anxious about what might happen while under chloroform, though he was not the subject of any obvious cardiac or pulmonary disease. I was deputed to watch the pulse and nothing else, the anæsthetist being directed to attend to the respiration. I did so most carefully, and when the patient was just becoming anæsthetised, when the reflexes were just beginning to fail, I suddenly noticed that the pulse had stopped, and at once cried out. Respiration, however, was still going on. I listened to the heart with a stethoscope, but could hear no sound, though the chest was still moving and air could be heard entering the thorax. The period which existed between the stoppage of the pulse and that of respiration could not have been at most five seconds. My former colleague, Mr. MacEwen, published in the 'British Medical Journal' for June, 1891, a precisely similar case. You all know how careful a clinical observer he is, and I am perfectly willing to accept his statement. There are two periods in which syncope may take place, going off and coming to, and these are the most important periods to watch the pulse; but I think, nevertheless, that the respiration is the most important. In the experiments which I have carried out on large dogs, such as retrievers, first of all the respiration was more rapid; then after a time it became entirely diaphragmatic, the thoracic movements ceasing. The animal was then in great danger; in fact, none of the animals that reached this stage ever recovered,—and as for cats, I defy you to get a cat under chloroform without killing it.

Dr. HEWITT.—I should like to draw attention to a point which may help to harmonise the conflicting views which at present exist as to the advisability of pulse observation during chloroform administration. Whilst on the one hand we have the assertion that the pulse may give very valuable indications as to the effects produced by chloroform, we have on the other the equally definite statement that any attention to the pulse is positively dangerous. Now there is reason to believe that this discrepancy of opinion is in large measure due to the supporters of the opposed views applying their remarks to somewhat different degrees or depths of chloroform anæsthesia. When this anæsthetic is given as recommended by the Hyderabad Commission and by Surgeon Lieutenant-Colonel Lawrie, the anæsthesia produced can hardly be called profound, save perhaps at intervals; and it is quite intelligible that under such circumstances other indi-

cations as to the degree of anæsthesia are sufficient to guide the administrator, and to keep him from overstepping certain bounds beyond which he prefers not to proceed. But in large surgical centres the exigencies of modern surgery demand that, in the absence of certain morbid states of the patient, and with the exception of certain operations, profound chloroform anæsthesia, with perfect quietude, relaxation, and freedom from reflex movement, is absolutely essential; and I submit that it is in this condition, whether brought about by design or accident, that pulse observation is likely to be of value. I fully admit that the respiration should be our first care, and that if this function be not performed satisfactorily no time should be lost in re-establishing it, irrespective of any circulatory phenomena. But given that the patient is deeply anæsthetised, and that air is entering and leaving the lungs, an over-dose of chloroform is more likely to be avoided by watching both the respiration and pulse than the respiration alone. In an anæsthesia of this depth it is obvious that there may be junctures at which some doubt exists as to the precise condition of the patient, and the state of the pulse will aid us. I have seen more than one case in which the administration of chloroform beyond a certain point has led to some marked alteration in the pulse, such as considerable slowing, irregularity, or feebleness, without any obvious change in respiration, and I have noted that these pulse alterations have disappeared when less of the anæsthetic was given. Moreover I have notes of a case in which, owing to the abdominal muscles remaining inconveniently rigid for a much longer time than is usual, it became necessary to push the anæsthetic beyond the customary limits, and a particular juncture arrived at which no pulse could be felt at the wrist, although breathing was continuing. I do not say the breathing was as good as it had been,—it was, in fact, shallow. But the point is that respiration had not ceased—air was entering and leaving the chest—and the colour was not noticeably impaired. A few seconds later the breathing ceased and the colour became pale and dusky. All that was necessary was to compress the chest several times, and the pulse, respiration, and colour returned. I should like to ask Surgeon Lieutenant-Colonel Lawrie if he has ever met with a case in which, during deep chloroform anæsthesia, the wrist pulse has failed although the breathing has continued. I am not referring to cases in which the heart's action has become impaired by loss of blood, vagal influences, or allied causes; such cases belong to a different category. If it be admitted that an over-dose of chloroform may lead to this condition, it is obvious that a close observation of the pulse, in addition to the breathing, will place the administrator on his guard sooner than if breathing alone is watched. It would be wrong to state that in every case impairment or cessation of the pulse precedes respiratory failure. And it would be equally wrong to say that when the peripheral circulation is affected in the way I have described, the interval which elapses before respiration fails is always of sufficient duration to be of use to the administrator. All I contend is that cases occur in which the pulse gives earlier warning than the respiration or colour, a contention which is directly opposed to the doctrine set forth in Surgeon Lieutenant-Colonel Lawrie's paper.

Whatever may be the physiological explanation of the cessation of pulse when chloroform is administered in toxic doses—in other words, whether the effect is produced by vaso-motor depression, cardiac depression, or both—the clinical facts to which I have alluded lose none of their significance. I should like to point out, in conclusion, that the views which we are invited to indorse this evening, namely, that the pulse should be disregarded, is also directly opposed to the careful conclusions of one of the most important committees which this or any other medical society has ever formed, and I trust the Royal Medical and Chirurgical Society will receive with caution those principles in chloroform administration which have been enunciated in Surgeon Lieutenant-Colonel Lawrie's paper.

Dr. LAZARUS-BARLOW.—The tracings which have been shown to-night have all been taken from the blood-pressure. I wish to show a single photograph taken by means of the myocardiograph from the heart itself. This tracing is perfectly typical of many others. Hooks were placed in the wall of the ventricle and auricle after opening the pericardium in a curarized dog. Chloroform was administered by inhalation through a Wolff's bottle in a moderate non-poisonous dose. The tracings show the movements of the auricle and ventricle separately communicated by levers to revolving drums. Immediately upon the inhalation of the chloroform they show that the force of both auricular and ventricular contraction diminishes with great rapidity. From this I conclude that whatever the cause of the fall in blood-pressure which is a constant accompaniment of chloroform anæsthesia, Colonel Lawrie is wrong in stating that the heart is unaffected. This being the case, the next question is, What is to be taken as our guide in the administration of chloroform? In the first place, we have found in the Pathological Laboratory at Cambridge that the respiration is utterly useless as a guide. We therefore, as a matter of routine, take the blood-pressure in the carotid, as a fall in blood-pressure indicates with certainty the degree of danger incurred in the narcosis. In the second place, blood-pressure must not be confounded with the pulse. Two simple examples show that it is very unsafe to draw deductions from the pulse as to the blood-pressure. First, the blood-pressure in renal disease is high, but the pulse is often very indistinct. Secondly, if everything remain constant, except the rate of the heart's beat, and that diminish, three things follow,—(a) a fall in blood-pressure, (b) an increase of the residual blood in the heart (viz. commencing dilatation), and (c) a more distinct pulse on account of the diminished fulness of the arteries. A high blood-pressure may therefore be accompanied by an indistinct pulse, a low blood-pressure with a distinct pulse. Practically, the only information given by the pulse of the heart's condition is confined to frequency and irregularity. All other modifications may be obliterated or controlled by vaso-motor action, concerning which we know practically nothing. Respiration therefore is an utterly useless guide as to the condition of the heart with chloroform, and the pulse is a very unsafe guide. We must go to headquarters and obtain information from the cardiac impulse, and probably also from the degree of pulsation in the epigastrium as indicating the degree of cardiac



dilatation. Practically, however, the administration of a drug which is always dangerous may be rendered least dangerous if the narcosis be pushed the least possible degree necessary for the performance of the operation after the corneal reflex has disappeared, as the heart is rarely affected to a profound extent until after the corneal reflex has disappeared.

Dr. SILK.—There are one or two points to which allusion has not been made either by the author or subsequent speakers. The conditions under which the Hyderabad Commission was appointed were very peculiar, and the preconceived notions with which they started render it more than usually imperative that the evidence brought before us should not only be conclusive in itself, but should be confirmed by other observers. I maintain, from what we have heard this evening, that this is far from being the case; but, on the contrary, that their conclusions have been directly challenged from every point of the compass. I cannot attempt, in such an academic discussion, to express any opinion as to the reading of the tracings; but, on the other hand, I cannot help feeling impressed by the stupendous amount of trouble taken by the various physiological observers. The labours of these observers on both sides will remain for a long time as a perfect monument of care and elaboration. But if these tracings prove anything, they appear to me to prove the very intimate relations which exist between the respiratory and cardio-vascular functions. If it were at all possible clinically to differentiate these functions one from the other—if, for instance, it were possible to adopt on the operating table the cross-circulation methods suggested by Drs. Gaskell and Shore,—it might then be possible to ascribe death to the failure of one function or the other. Until the time has arrived when such a direct application of the physiological experiments to clinical work becomes possible, then, I maintain that these very tracings—as, indeed, all the evidence brought before us by the Commission—go to prove that too much care cannot be exercised in the administration of chloroform, and that it would be most unwise, most rash, to neglect any possible precaution; and therefore I not only disapprove most strongly of their conclusion that it is dangerous to take the pulse as a guide, but I am very much inclined to assert the contrary. Though our primary attention ought to be devoted to the respiration, I have little doubt that one ought also to watch the circulation as seen in the colour of the lips, cheeks, and other exposed parts of the body; and one ought to try to detect any early change in the vaso-motor system by occasionally noting changes in the character of the pulse, watching the pupil, &c. As regards clinical evidence, a good deal might be said in opposition to the clinical evidence brought forward by the Commission. As far as I can make out, the clinical evidence brought forward by them was derived almost entirely from the information given by the author, and consisted in the statement that during forty-three years' experience of his own and of Professor Syme no death had occurred. I would only remark with regard to this, that Syme's experience, though great, was not great according to present standards. Operations in those days were, comparatively speaking, few in number. A

surgeon of five years' standing of to-day would probably have far greater experience in chloroform administration than a surgeon of twenty years' standing in the days of Syme. Dr. Lawrie's clinical experience is mainly that of his Indian work; and, as we all know, the natives of India take chloroform remarkably well; such, at any rate, was the experience of Dr. Crawford at the Madras Hospital, and also, I believe, of Sir Joseph Fayrer, and this fact must be borne in mind in making comparisons. I am also certain that the extraordinary methods adopted by the Commission in the promulgation of their views have indirectly led to disaster. Dr. Lauder Brunton has alluded to the enormous increase in the number of deaths from chloroform during the last few years; he has, it is true, referred it in some way to the foreign meat trade, but this will not explain how it is that the increase has arisen almost entirely since the views of the Hyderabad Commission were so profusely trumpeted abroad. Their report appeared in 1890; in the three preceding years the death rate from chloroform, as furnished by the Registrar-General, averaged thirty-three; during the three years following the issue of the report it averaged forty-eight. What I want to know is, why this increase coincides so absolutely with the appearance of the report. As far as the safe administration of chloroform is concerned, I do not believe that absolute safety is ever or can ever be possible. I think, however, that primary attention should be directed to the respiration, and that attention should also be directed to the circulation and vaso-motor system as observed in the colour of the face, the character and alterations in the pulse and pupil, etc. This is the doctrine which I believe was held and taught by responsible teachers long before the Hyderabad Commission was even heard of.

Surgeon Lt.-Col. LAWRIE (in reply).—It is so late that there is not time for me to do more than notice in the briefest possible manner a few of the questions raised in the discussion. The main points in my paper have not, I contend, been touched upon. For instance, Dr. Gaskell has entirely failed to tell us whether it is possible for a dying animal with a falling blood-pressure ranging from 50 mm. to 24 mm. to maintain a cross-circulation. He has also omitted to state why, when chloroform was administered to the fed animal direct in his experiments, and its body died, its brain did not go on living until the cross-circulation, if there ever was any, was stopped. Dr. Gaskell speaks of his two cross-circulation experiments to which I have called your attention as two "of a whole series"—the which series consisting of six, three of which were on rabbits. He also says that "the respiration" sometimes continued in his experiments for four minutes after "the absolute cessation of all heart-beats." The only proof of the cessation of heart-beats in his experiments is the failure of the heart to make a pulse tracing in the drum. This is no proof of absolute heart failure; it is only a proof that the heart was too weak to move the column of mercury in the manometer and the indicator. But in any case I maintain that it is impossible for "the respiration" to continue after the heart has entirely failed, though ineffective respiratory movements might do so. Dr. Gaskell objects to my experiment, in which chloroform was given direct to the heart

of fed, without any effect whatever, because it was only given for 3 min. 24 sec., a period which he considers too short to show anything. This shows a surprising want of acquaintance with the action of chloroform; and, as a matter of fact, in one of his own experiments the animal died from chloroform in less than three minutes. With regard to Dr. Shore's speculative attempts to throw discredit on my statements, I will only say that the total number of our cross-circulation experiments was fifty-three, and not "at least twenty or thirty," as he makes out. Two enlarged photographs of the most important tracings have been laid before the Society. Each of these cost £10. If I had brought similar photographs of all our tracings the sum-total would have reached a higher figure than we could afford; and, moreover, it appeared to me unnecessary to follow the example of Drs. Gaskell and Shore, and to bring forward, as they did, tracings of cross-circulation experiments in which the cross-circulation was obviously a failure. The rest of the discussion needs no notice from me.