have led us here and there into digressions in which the cell as such has been left far behind. Let me now proceed to gather up these scattered strands. We have seen that, in order to obtain a chemical knowledge of the cell, it is necessary to have, in the first place, an accurate knowledge of its anatomy, but that the histological methods of staining and so forth depend on a chemical basis. The main substance of the cell is proteid, and this proteid, when in the living cell, is intramolecularly different from that in non-living protoplasm. In addition, there are smaller quantities of lecithin, cholesterin, and inorganic salts. As the result of their vital activity, cells form various products; some of these, like carbonic acid and urea, are the result of oxidation; and these products of destructive metabolism leave the cell, and are ultimately got rid of by the excretory organs. Others, like glycogen and fat, may be for a time stored up in the protoplasm, and it has long been known that these can be detected by the use of iodine and osmic acid respectively. More recent micro-chemical methods have taught us that the important elements, iron and phosphorus, can also be localised in the cell or its nucleus, and on account of the newness of these investigations, I have dealt with them at considerable length, pointing out, on the way, what I conceive to be some of the practical bearings of the results of such inquiries.

But, in addition to such facts, there are others, concerning which the present state of our knowledge does not allow us to pronounce positive chemical opinions, and here I would again allude to the important work of Ehrlich on the behaviour of protoplasmic granules to the aniline dyes. The fact that these dyes are some acid, some alkaline, and some neutral would seem to indicate corresponding differences in the reaction of the granules themselves. The reaction of living protoplasm as a whole is alkaline, but there are various acid products formed as the result of protoplasmic activity, such as carbonic acid, lactic acid, uric acid, and, in one well-marked case, that of the gastric cell, hydrochloric acid. It appears to me more than probable that during life the reaction of protoplasm or of parts of the protoplasm is a changing one; the reaction may be in as unstable a condition of equilibrium as the other factors of cell life are. We know that when the circulation ceases and the cells can no longer build themselves up from new material, but are still sufficiently living to continue their retrogressive or katabolic changes, that they become acid from the accumulation of such substances as lactic acid or from the formation of acid phosphates. This tendency to become acid is being constantly corrected by anabolic or assimilative changes during healthy cell life, and it appears quite possible that in Ehrlich's staining processes we have an actual proof of this condition of unstable chemical reaction. This appears to be very strikingly confirmed by a recent experiment of Hardy and Kanthack,² who found that eosinophile cells, after feeding on bacteria, become amphophile.

But, leaving such speculative propositions and coming to our positive knowledge, none can deny the enormous impetus that has been given to chemical physiology by the combination of the use of the microscope with that of chemical reactions. Valuable as micro-chemistry is, however, as a means of research, it cannot go very deeply into the matter, and, happily, we can supplement the knowledge so obtained by the methods of macro-chemistry. We have already made the acquaintance of nuclein, the chief material in the nucleus, and of the various proteids contained in the cell protoplasm, and it will be my duty in my next lecture to develop this aspect of the subject more fully.

² *Proc. Roy. Soc.*, lii, p. 27.

CLINICAL SOCIETY OF MANCHESTER.—At the meeting held on February 21st, 1893, Dr. Dreschfeld read a paper on apyrexial typhoid fever, in which he pointed out that though emaciation was less marked in these cases than in the ordinary forms of typhoid, and convalescence was quicker, the prognosis was not the less serious, as one of the four cases he had met with died, and the two cases recorded by Wendland also terminated fatally. Dr. Wild read a short note upon the local treatment of cancer with pyoktanin, which he had found to yield most unsatisfactory results. Cases of muscular atrophy were shown by Drs. Stallard and R. T. Williamson.

REMARKS ON THE ACTION OF CHLOROFORM.

By T. LAUDER BRUNTON, M.D., D.Sc. EDIN., LL.D. (HON.)
ABERD., F.R.C.P., F.R.S.

HISTORICAL.

WHEN a discussion on any subject is much prolonged there is a general tendency to forget the exact point from which the discussion started, and to take up points which have arisen during its progress. The discussion upon the results of the Hyderabad Commission has been no exception to this rule, and it may be worth while to trace shortly the object and work of the Commissions.

Three series of researches have already been carried out under the auspices of His Highness the Nizam's Government, at the suggestion of Surgeon-Lieutenant-Colonel Lawrie. These might all be termed "Commissions," but perhaps it may be advisable to restrict the term to the first and second series of experiments which were carried out at Hyderabad, and to apply the term "Research" to the third series carried out by Drs. Gaskell and Shore, at Cambridge.¹ Each succeeding series has advanced our knowledge of the action of chloroform, and the latest researches of Gaskell and Shore supplement the work of the previous Commissions. At first sight it would appear as if Gaskell and Shore had not only overthrown the conclusions of the second Hyderabad Commission, but had found its members guilty of gross negligence in not attempting to solve the chief question for which the Commission had been appointed, for they say: "At the very outset it was evident that the Commission did not supply any answer to the most important question connected with the action of chloroform; namely, what is the reason of the fall of blood pressure which always occurs when a large dose of chloroform is administered?"

In the remarks I have to make upon this subject I must speak as an individual, and leave it to Surgeon-Lieutenant-Colonel Lawrie as President of the Commission to speak for it. I am also bound to limit the responsibility for the expressions I use to myself alone, because I believe that I do not take such a decided view regarding the complete immunity of the heart from the action of chloroform under all circumstances as Lawrie does, although I entirely hold with the conclusions of the second Hyderabad Commission, of which I was a member.

THE FALL OF BLOOD PRESSURE.

There can be no doubt that if we drive air saturated with chloroform vapour into an animal's lungs by artificial respiration continuously, we will kill the animal. In doing this we almost certainly paralyse the normal respiration before the heart, but this we cannot see, for the artificial respiratory movements completely take the place of the natural ones, and would conceal them even were they present. The death of the animal under such circumstances can hardly be regarded as anything else than death from the action of chloroform on the heart, and if we examine the blood pressure we shall find it fall at the time the heart is becoming paralysed and stopping. But we must, I think, carefully distinguish between such paralysis of the heart and the gradual fall of blood pressure, which the Hyderabad Commission in their Conclusion No. I remarked upon as a constant result of chloroform inhalation, unless the vapour was excessively diluted.

PRACTICAL QUESTION BEFORE THE COMMISSION.

In looking at the fall of blood pressure caused by chloroform as the most important point in its action I think that Gaskell and Shore are entirely mistaken. The practical question which was to be solved by the Commission was

¹ Unless the first Commission is specially mentioned, the second Commission is usually understood when the name "Hyderabad Commission" alone is used, as it is much better known than the first, and its work was more important.

whether death during chloroform narcosis arises from stoppage of the heart—an entirely different thing from a simple fall in blood pressure—or from stoppage of the respiration, and the efforts of the second Commission were consequently directed to the elucidation of this point. We satisfied ourselves that a gradual fall of blood pressure occurred from the administration of chloroform in all cases, even in those where the narcotic was administered with perfect safety (Conclusion 1 of the Report, page 17). We also found that reduction of the blood pressure by irritation of the vagus during the administration of chloroform seemed to have a preservative rather than an injurious action, prolonging rather than destroying life (Conclusion 16, page 20), and a similar effect appeared to follow lowering of blood pressure by division of the splanchnics (Experiment 3, Conclusion 36). The fall of blood pressure produced by chloroform having thus been found not to have much, if any, practical importance in regard to the occurrence of death during anaesthesia, the investigation of its causation was set aside during the greater part of the working of the Commission in favour of experiments relating more directly to the cause of death, and only a few experiments were made with the direct object of ascertaining the *modus operandi* of chloroform in lowering blood pressure.

THE INVESTIGATIONS OF THE HYDERABAD COMMISSION.

Before proceeding further to discuss the paper of Drs. Gaskell and Shore, I will shortly state what I understood to be the position of the question at the time they took it up, and I do not know that I can do this better than by quoting what I said in the discussion in the Section on Therapeutics at the British Medical Association meeting in August, 1891—a discussion in which Dr. Shore took part, and gave a short summary of some of the experiments which Dr. Gaskell and he have now published in full. I then said: “The object of the Hyderabad Chloroform Commission was essentially a practical one—namely, ‘It was to save people’s lives,’ to use the words of H.H. the Nizam himself, to whose generous and enlightened liberality the Commission owed its existence. In making their experiments, the Commission had before them the question how far death during the administration of anaesthetics was likely to be due to the anaesthetic itself, and how far to the effect of shock from the operation. The question regarding the action of the anaesthetic also divided itself into two—namely, how far the lethal effect might be due to affection of the heart, how far to affection of the respiration, and how far to both.”

“Before we attempt further to describe the experiments made by the Commission, I think it may be well to clear the ground by mentioning that the time was too short to allow of the general action of chloroform or ether upon the tissues generally being investigated, and that we had to confine our attention to the methods in which death was likely to occur during surgical operations as usually performed. Previous researches had pretty well established that chloroform is a universal protoplasmic poison, and will destroy the contractile power of individual cells, of cilia, and of muscular fibres, and, when injected into the artery of a limb, will produce rigor mortis in it, and make it as stiff as a board.² There was no question, therefore, of the power of chloroform to destroy any structure of the body if applied to it in sufficient concentration; nor do we attempt to deny that chloroform will destroy the contractility of the heart, just as it would that of a voluntary muscle, provided always it reaches the heart in sufficient concentration. *But this was just the point at issue.* If we drive chloroform into the trachea, or air very heavily loaded with chloroform vapour into the lungs by artificial respiration, it will be absorbed in sufficient quantities to paralyse the heart; but the question is an entirely different one if the chloroform be administered in the usual way—by inhalation. Our contention is that when chloroform is administered in this way, it acts more readily upon the respiration than it does upon the heart, and, from the respiration failing first, a sufficient quantity to paralyse the heart is never conveyed to it, and that, therefore, death from chloroform inhalation is respiratory death—death beginning with the respiration, and not with the heart. It will be convenient to take up first, more fully, the question of death from the

anaesthetic, by which I mean death due to the action of the anaesthetic itself, and, later on, to discuss the question of death during anaesthesia—that is, death from the operation or other causes than the anaesthetic; sometimes in spite of its action and at other times, perhaps, aided by its action. The experiments made by the first Chloroform Commission showed that in dogs subjected to its action the respiration invariably failed before the circulation. Those made by the second Commission confirmed this, but they brought out a new point, namely, the rapidity with which the heart fails from the combined action of asphyxia and chloroform. This action is of two kinds: First, asphyxia during chloroform inhalation stops the heart’s action through the vagus nerve. This is the action which was looked upon by the Glasgow Committee as so dangerous, but which, as Dr. Bomford pointed out, is rather a safeguard, tending to prevent the too rapid conveyance of chloroform vapour from the lungs to the medulla. The second action of asphyxia and chloroform combined is a paralysing one upon the heart itself. If an animal inhales pure chloroform vapour with free admixture of air its heart will go on for a long time; in fact we might say almost indefinitely. If an animal is asphyxiated, either by stoppage of the respiratory movements or by obstruction to the free entrance of air into the lungs notwithstanding the continuance of respiration, the heart will go on for a certain time, but in the course of a few minutes will stop. This stoppage, however, occurs very much more quickly if chloroform be administered at the same time as an animal is asphyxiated, so that we may say that the great risk of death from the action of chloroform lies in the occurrence of asphyxia during its administration.”³

THREE KINDS OF FALL IN BLOOD PRESSURE RECOGNISED BY THE COMMISSION.

The Hyderabad Commission distinguishes between three kinds of fall in blood pressure caused by chloroform, the first of which is harmless, and occurs almost invariably when chloroform is given. Regarding its causation, the Commission, as already stated, came to no conclusion. The second kind is due to slowing of the heart by irritation of the vagus and is also harmless. The third is due to an overdose of chloroform, and is dangerous. The report says (Conclusion 8, page 18):—“The effect of involuntarily holding the breath, which, as anybody can prove by experiment upon himself, must happen when an inhaler saturated with chloroform is first applied to the face, is much more remarkable, the pressure often falling with great suddenness, while the heart’s action is markedly slowed. As soon as the animal draws breath again the pressure rises as suddenly as it fell; but the gasping respiration which succeeds then causes very rapid inhalation of chloroform, with immediate insensibility and a rapid fall of blood pressure, which quickly becomes dangerous.”

PRACTICAL RULES OF THE COMMISSION.

The practical Rules 5 and 6 were framed to avoid this danger. They run as follows: “5. At the commencement of inhalation care should be taken, by not holding the cap too close over the mouth and nose, to avoid exciting struggling, or holding the breath. If struggling or holding the breath do occur, great care is necessary to avoid an overdose during the deep inspirations which follow. When quiet breathing is ensured as the patient begins to go over, there is no reason why the inhaler should not be applied close to the face; and all that is then necessary is to watch the cornea and to see that the respiration is not interfered with. 6. In children, crying ensures free admission of chloroform into the lungs; but as struggling and holding the breath can hardly be avoided, and one or two whiffs of chloroform may be sufficient to produce complete insensibility, they should always be allowed to inhale a little fresh air during the first deep inspirations which follow. In any struggling person, but especially in children, it is essential to remove the inhaler after the first or second deep inspiration, as enough chloroform may have been inhaled to produce deep anaesthesia, and this may only appear or may deepen after the chloroform is stopped. (*Vide supra* sub-paragraphs 2 and 9 of conclusions in paragraph 30).

² This last experiment was repeated by the Commission, Report, Experiment 135.

³ BRITISH MEDICAL JOURNAL, August, 1891

Struggling is best avoided in adults by making them blow out hard after each inspiration during the inhalation."

OPINION REGARDING CAUSE OF FALL IN BLOOD PRESSURE.

My own belief regarding the rapid fall of blood pressure mentioned in Conclusion 8 and regarding the general nature of the danger arising from an excessive dose of chloroform was simply that chloroform, being a protoplasmic poison, more or less paralysed the functions of all the tissues, that it affected the respiratory centre before the heart, but, when combined with asphyxia, paralysed the heart so soon after the respiration that all measures for recovery might prove useless. We regarded this paralysis of the heart and respiratory centre as rather due to asphyxia than to chloroform, for we found, in Experiment 161, that the respiration of pure carbonic acid not only stopped the heart, but appeared to paralyse it even more quickly than the respiratory centre. For in this experiment artificial respiration in an animal poisoned by carbonic acid until normal breathing had ceased succeeded in restoring a few gasping inspirations, but had no effect upon the blood pressure, and did not bring about the recovery of the animal. In both cases, in death from chloroform and death from carbonic acid, the heart and the respiration both became paralysed. In such a process as this, whether it be caused by asphyxia alone, by asphyxia and chloroform combined, or by chloroform alone, it is quite evident that the respiration will affect the heart, and the heart the respiration so closely, that it is very difficult to assign to the heart and respiratory centre the exact proportion which their respective failures contributed towards death. The Hyderabad Commission looked upon this question as coming rather within the scope of pure scientific investigation than of immediate practical utility, and therefore did not make many experiments upon the subject.

AGREEMENT OF DRs. GASKELL AND SHORE WITH THE PRACTICAL CONCLUSIONS.

It is very satisfactory to find that the further researches of Gaskell and Shore have not only confirmed the statement of the Hyderabad Commission that the respiration stops before the heart actually ceases to beat, and shown that local asphyxia greatly increases the paralyzing action of chloroform, but that they also entirely agree with the practical conclusions derived from the experiments recorded in the *Report*.

CRITICISM OF THE HYDERABAD COMMISSION BY DRs. GASKELL AND SHORE.

The two chief subjects which they discuss in their paper are the cause of the fall in the blood pressure which invariably occurs during the inhalation of chloroform if sufficiently strong, and the precise factors which occasion death from an overdose.

As I have shown, these are questions which the Hyderabad Commission considered to be not of immediate practical utility, but rather of scientific interest, and their decision is borne out by the fact that Drs. Gaskell and Shore thoroughly agree with all the practical conclusions of the Commission. They bring forward a large amount of most interesting experimental work in regard to the comparative action on the heart and medulla, but this simply supplements and does not in the least overturn the work of the Hyderabad Commission. Indeed, one of the chief advantages which this Commission owed to the great liberality of the Nizam was the reproduction of all the curves they obtained by means of photography, so that the immense number of experiments which they performed have become as available for the examination and criticism of workers on the subject all over the world as they were for the original experimenters, the only disadvantage being that the use of a more powerful lens is required for the photographs than for the somewhat larger original tracings. This arrangement has given permanent value to the work, for whether the conclusions of the Commission be overthrown or stand, the self-recorded experiments remain, and will be as readily available for deciding disputed points a hundred years hence as they were on the day they were made. Drs. Gaskell and Shore are, therefore, quite as able to draw their conclusions from the experiments of the Hyderabad Commission as the members of the Commission itself, but I wish to point out that in their criticism they have confounded

two things, namely, the *conclusions* at which the Commission arrived after careful consideration, and the *comments* upon individual experiments which were made at the time chiefly for the purpose of drawing attention to the curves afterwards, so that we might be able finally to form an opinion about them.

It is to those comments and not to the conclusions of the Commission that the criticism of Gaskell and Shore is almost entirely directed. Considering the care with which I pointed out in the discussion at Bournemouth that chloroform is a protoplasmic poison, and will destroy any living tissue, the heart included, if brought into contact with it in sufficient concentration, I think that Gaskell and Shore should have somewhat qualified their statement that I considered that chloroform only affects the heart in combination with an asphyxial condition of the blood. At the same time it is quite true that I do hold that while it is possible to paralyse the heart by driving concentrated chloroform vapour into the lungs, it is exceedingly difficult to do so unless you get a more or less asphyxial condition combined with the action of chloroform, and that you may produce almost similar results may be obtained by driving in carbonic acid un-mixed with air but without any chloroform whatever.

THE DANGER OF THE ASPHYXIAL CONDITION.

One of the most important parts of the work of the Hyderabad Commission is, I think, that it pointed out so very clearly the great danger of an asphyxial condition combined with chloroform in producing death, and in drawing attention to the absolute necessity for continuous free respiration if danger is to be avoided.

THE QUESTIONS DISCUSSED BY DRs. GASKELL AND SHORE WERE LEFT BY THE HYDERABAD COMMISSION FOR FURTHER RESEARCH.

Drs. Gaskell and Shore then say: "It is difficult to understand how the Hyderabad Commission could have arrived at the conclusions they have published, for their experiments and their large number of curves confirm again and again the observations of others, and point to heart failure as the cause of the fall of blood pressure." Now, as they themselves say near the beginning of their paper, the Hyderabad Commission does not attempt to explain the reason of the fall of blood pressure, and, as Lawrie says in the quotation they give, "The Commission has given no opinion as to the cause of the fall produced by diluted chloroform alone." It was precisely because the second Hyderabad Commission, when going over their experiments, thought that although their results indicated paralysis of the vasomotor centre as a cause of the fall in blood pressure, yet the evidence before them was not conclusive, that they did not formulate any conclusion on the subject. They left it as a point to be decided by further research, and, if I am not mistaken, it was in great measure on this account that Drs. Gaskell and Shore were asked by Surgeon-Lieutenant-Colonel Lawrie to undertake the research which they have just published.

THE INTERPRETATION OF CURVES.

Drs. Gaskell and Shore have to a certain extent made me responsible for the interpretation of the curves, by saying in the original report they sent to Lawrie, and quoted by him in the *Lancet*, February 11th, p. 289: "It is difficult to understand how the Hyderabad Commission, with Brunton to assist them in the interpretation of their curves, could have arrived at the conclusions they have published." They discuss the factors regulating the size of the pulse excursions in blood pressure tracings, and afterwards say that "the physiology of the Commission is wrong." I do not think in the very least that Drs. Gaskell and Shore meant to import any personal element into the discussion of a purely scientific question; but as my name has appeared, I am necessarily put on my defence. I think it is hardly possible for the readers of Gaskell and Shore's paper and the quotation from them in the *Lancet* to avoid the conclusion that the Commission came to erroneous conclusions in consequence either of my want of knowledge of elementary physiology or failure to apply it to the problems in hand. In regard to ignorance, I may say that I discussed the factors which regulate the size

of the pulse wave with much fulness in my thesis on Digitalis, presented to the Edinburgh University in 1866, and published in 1868,⁵ and have been working at this subject at intervals ever since, so that it is hardly possible that I can be entirely ignorant of this subject. In regard to its non-application, I think that even if Drs. Gaskell and Shore had not learned the distinction between *comments* incidentally made on the experiments and the *conclusions* drawn by the Commission, they might have inferred it from the fact that any evidence from pulse curves regarding the action of chloroform on the heart was carefully excluded from the conclusions.

EVIDENCE FROM INJECTION OF CHLOROFORM INTO VEINS.

In Conclusion 19 we said: "It seems clear from Experiment 92 that the direct action of chloroform on the heart's substance is not the cause of the fall of blood pressure when it is inhaled. In this experiment repeated injections of 20 minims of chloroform were made into the jugular vein, and its effect was not to paralyse the heart but to produce anaesthesia and a gradual fall of the blood pressure, exactly as if the chloroform had been inhaled."

Knowing as I did that the injection of chloroform into the femoral artery will almost instantaneously stiffen the leg until it resembles a bit of board, I was much astonished to find so little action on the heart.

We, therefore, took into consideration at the time the very possibility that Drs. Gaskell and Shore have discussed, namely, that the chloroform had remained in the jugular vein, but dismissed it on the ground that we had injected the chloroform in such a way that it must almost certainly have reached the heart. If this were so we could hardly come to any other conclusion than the one I have already quoted (No. 19). It is no doubt possible that we were mistaken, but we were perfectly aware of the possible fallacy in our experiment which Drs. Gaskell and Shore pointed out, and it is to be regretted that they have contented themselves with simple criticism of our experiments instead of repeating them.

LIMITATION OF EXPERIMENTS.

It may no doubt be said that the Commission ought to have repeated these experiments more often, but it is to be borne in mind that between October 23rd and December 18th, a period of very little over eight weeks, not only 588 experiments, of which about 150 were blood-pressure experiments, were made and fully recorded, but a physiological laboratory in which to make them had to be fitted up. It is probable that such an enormous number of experiments on the action of any drug or series of drugs has never been made before in the same time, and only the great liberality of the Nizam's Government and the unlimited supply of material made it possible. But time was limited, and the number of questions to be solved was great, so that more than a certain number of repetitions was impossible, and when we got results which were not only contrary to what we expected, but were obtained from experiments in which we did not find a source of fallacy for which we were on the watch, there was nothing to be done but to formulate the conclusions to which they led.

OBJECTIONS TO DRs. GASKELL AND SHORE'S CRITICISM.

To sum up what I have said, then: (a) In regard to the conclusions of the Commission regarding the immunity of the muscular tissue of the heart, Drs. Gaskell and Shore have only brought forward a theoretical objection, which had already been considered and discarded by the Commission. (b) They have criticised severely the *comments* made on some of the experiments, which were simply of the nature of notes for after-consideration. This criticism has nothing whatever to do with the *conclusions* of the Commission, inasmuch as the tracings and comments thus criticised were not used by the Commission as a basis for its conclusions.

FALLACIES OF THEIR CROSS CIRCULATION EXPERIMENTS.

But Gaskell and Shore's paper does not consist only of destructive criticism; it contains also a great deal of most interesting original work. Their most ingenious

experiments on cross circulation might almost have been accepted as proof that chloroform weakens the heart much before the vasomotor centre, were it not that Lawrie has performed experiments in which a diametrically opposite result is obtained. How far the discrepancy between these results is due to different methods of experimentation it is impossible at present to say. It took many experiments to discover the cause of the so-called capricious action of chloroform on the heart described by the Glasgow Committee. But I should like to point out that the experiments of the Hyderabad Commission were made with chloroform alone as an anæsthetic, and that other drugs which might possibly interfere with or modify the action of the chloroform were not injected, except in special cases. The experiments of Drs. Gaskell and Shore were made on dogs previously narcotised with morphine, and it is just possible that this may have something to do with the discrepancy between their experiments and those of Lawrie. I should hardly have expected morphine to prejudice greatly the result of such an experiment, but in the case of mixed poisons it is impossible to tell what the effect of the mixture may be. The experiments of Gaskell and Shore upon rabbits are much more liable to be fallacious, because they used chloral in order to narcotise them, and chloral is a powerful cardiac and vasomotor depressant. I think it was unfortunate that in their investigation they did not use chloroform alone, because in a pharmacological investigation of this kind one is never sure that the results of experiments with a single drug, like chloroform, can be safely compared with experiments on the combined action of two drugs like chloroform and chloral.

Moreover, it is unfortunate that most of their experiments were done on rabbits, for these animals react to chloroform in a somewhat different way from dogs, monkeys, and men, and I think their hearts are more readily paralysed.

CONCLUSIONS.

In conclusion, I may sum up by saying that in this paper I have tried to show:

1. That Drs. Gaskell and Shore are mistaken in regarding the fall in blood pressure produced by chloroform as the most important question connected with its action.

2. Secondly, that the conclusions of the Hyderabad Commission are entirely unaffected by their criticism, which is directed to two things: (a) the effect of chloroform injected into the jugular vein, and (b) the pulse tracings obtained during chloroform narcosis. In regard to (a), the error into which Gaskell and Shore suppose the Commission to have fallen was suspected by the Commission, and, as they believe, was avoided. In regard to (b), the criticism of Drs. Gaskell and Shore is not directed against the *conclusions* of the Commission but against *comments* made in the course of describing individual experiments. The tracings thus criticised were not considered as satisfactory evidence by the Commission, and were consequently not employed by them as a basis for their *conclusions*.

3. That they have devised a most ingenious plan of experiment by cross circulation and have obtained many interesting results. While these experiments may afford much valuable information, they are not entirely free from fallacy, as they were not performed on animals under the influence of chloroform alone, but under the influence of chloroform along with either morphine or chloral.

4. That they entirely confirm the conclusions of the Hyderabad Commission that the chief danger from chloroform is the concurrence of asphyxia, and that the most important practical point is attention to respiration, as maintained by Syme and Lister.

BEQUESTS.—The late Sir A. B. Walker, of Gateacre, near Liverpool, has by his will bequeathed £1,000 each to the Royal Infirmary, Royal Southern Hospital, Northern Hospital, and Bluecoat Hospital; £500 each to the Stanley Hospital, Hospital for Consumption and Diseases of the Chest, Infirmary for Children, and Liverpool Home for Incurables; £300 each to the Eye and Ear Infirmary and the Magdalen Institution. He has also left £1,000 for the Ayr Infirmary for Children, and £300 each to the Nurses' Institution and Children's Hospital, Derby.

⁵ On Digitalis, London: Churchill, 1863, p. 26.