AN ADDRESS ON THE EXPERIMENTS ON ANÆSTHETICS

CONDUCTED AT HYDERABAD.

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MR. PRESIDENT AND GENTLEMEN,—I thank you very cordially for your kindness in giving up an evening to me. I may also mention that the main part of the work is not the conclusions—not what Dr. Laurie, or Dr. Bomford or I, or any other member of the Commission think—but it is the number of experiments we have done, and which we have recorded automatically, so that the facts upon which we founded our conclusions are as readily available for you as they were for us; and if any false conclusions have been come to by us you will be able at once to correct them and come to the right conclusion, because you will have all the data before you. The way in which we were able to attain this end was by employing a different method of registration from the method formerly used. I can show you, though imperfectly, the apparatus we employed. I should first, however, mention, although it has been mentioned, that this Commission owed its origin to Dr. Laurie's great reverence for his former teacher, Professor Syme. Professor Syme was a man of great ability; he was also a man of very marked temper. He could hate most cordially, and one of his colleagues whom he did hate was Sir James Simpson, and he showed this so markedly on one occasion that when Sir James Simpson wrote a book, Professor Syme brought it into the lecture theatre and said: "Gentlemen, one of my colleagues has written a book," and he then proceeded to tear it in two and to throw it into the waste basket among the sawdust. Every student knew this about Syme, and as we learned from his own lips that he had such a respect for the discovery of his despised colleague, we thought all the more of the chloroform which Simpson had discovered to be an anæsthetic. Syme used to tell us that in all his experience, and in all the experience of his colleagues, in the infirmary there never had been a death, and he said: "The reasons why we never had any death were twofold: first, we used very good chloroform; and, secondly, we gave plenty of it." Now occasionally deaths do occur even although the same chloroform is used as that which Syme employed; and it occurred to me some years ago that possibly one reason of the deaths which did sometimes occur was the neglect of the second part of his advice—not giving enough of the chloroform. I thought that in all probability some of the deaths might be due to shock, and I wrote a paper on this subject. One finds very often that the deaths which are recorded as occurring from chloroform occur just at the very beginning of the operation, and Simpson did record one case of a man who died from the simple opening of an abscess, although he had no anæsthetic whatever. This man appeared to be particularly sensitive and to have died simply from shock, anæsthetics playing no part whatever in it.

On account of the great reverence which Dr. Laurie had for his teacher, he instituted a number of experiments to show that Syme's teaching was entirely right, and that in giving chloroform one should follow the practice which he always adopted, watching the respiration and never minding the pulse; and a number of experiments were made by the Commission in Hyderabad, which led to a confirmation of Syme's conclusions, as the animals to which chloroform was given always died from a failure of respiration, which invariably occurred before the failure of the heart. In the first series of experiments 141 animals were used. The second Commission was intended first of all to repeat the experiments of the first; and we repeated them, using 430 animals, and in all those cases the result was the same. We had in all 571 animals to which chloroform was given, and in all the respiration failed before the heart. There are two distinct theories before the profession as to the action of chloroform. The one that is generally adopted in Edinburgh is that chloroform paralyses the respiration and does not affect the heart, at least to

any serious extent. The second theory is that chloroform does affect the heart, and may paralyse it, and that this is one of the sources of danger. This idea is chiefly held in London. It occurred to me before I went out that possibly there might be a difference between the people to whom the chloroform was administered; that in Edinburgh the comparatively small amount of beer drunk might lessen the tendency to fatty degeneration of the heart; that we might get cases of fatty degeneration more abundant in London and that this might have something to do with the apparent greater mortality from the heart under the influence of chloroform in London than in Edinburgh. This was evidently one of the points that required attention, and it was with this apparatus that we tried to investigate the action of chloroform upon the heart. More especially was the investigation of the effect of chloroform upon the heart by means of such an apparatus as this rendered necessary because it appeared to me that a great deal of the idea that chloroform paralyses the heart was due, not so much to clinical observation, as to laboratory experiments; that the results of laboratory experiments have been taken to explain deaths from chloroform upon the operating table. One of the most valuable of the researches that have been made upon anæsthetics was that made by the Glasgow Committee appointed by the British Medical Association, consisting of Professor McKendrick, Dr. Coats, and Professor Ramsay.¹ They had found, as they said, that chloroform sometimes paralysed the heart, and that this paralysing effect was exerted occasionally in an unforeseen way, so that it could not well be prevented. This, therefore, was a most important point that required our strictest attention. We then set to work with experiments upon blood pressure and upon the heart. In most of the experiments that have hitherto been made upon blood pressure the tracing has been taken upon a revolving drum, which revolved with very considerable speed, so that you got upon the tracing all the pulse beats recorded; but the tracing was so long that it was impossible to reproduce the whole of it; it was many feet or many yards in length, and only pieces could be taken out, engraved on woodblocks, and printed.

In order to obviate this difficulty I thought it would be advisable to take a tracing of the arterial pressure upon a slow drum; and here you have an instrument of this sort. There may be some here, perhaps, who do not know practically the mode of taking the blood pressure. It is simply this: the animal is put under chloroform, then the carotid artery is exposed, the distal end of it is ligatured, and then the proximal end is clamped by a pair of bulldog forceps. A small cannula (such as that which I will pass round) is inserted into the proximal end and tied firmly in; this cannula is then put into communication with a bent tube containing mercury; the part between the mercury and the artery is filled with a solution of bicarbonate of soda, in order to prevent the blood from coagulating within the tube. As soon as connection is made, the bulldog forceps which clamped the artery are removed, and then you have a continuous communication through the carotid right down to the heart with the mercury which is in the tube. At every beat of the heart the mercury oscillates, so that you see it passing up and down. In order to record the movement of the mercury, a light float is placed upon it which writes against a blackened smoked surface, so that we get upon the black surface oscillations like those which I have marked upon the blackboard. But if you use a very slow drum instead of getting a tracing corresponding with the beats of the heart, it will probably correspond only with the respirations, and you lose the beats of the heart. With a slow drum which revolves only once in half an hour you get the whole of the long experiments, lasting perhaps an hour and a half, into three tracings, which you are able to reproduce by photography. You can thus follow the condition of the arterial pressure from the beginning to the end of the experiment. But by taking the tracing on such a slow drum you lose all the advantage of knowing the condition of the heart. We therefore got the pulse tracings of the heart by a second drum revolving ten times as fast as the other, and by having both connected by a Y tube with the artery we could take a tracing by the one drum or the other just as we liked. What we were accustomed to do was every now and then at any interesting moment to clamp off the mercury manometer and to put the other, which took the pulse tracings and worked upon the quick drum in connection with the artery, so that we could take a sample

¹ JOURNAL, December 18th, 1880, p. 957. Towards the expenses of this Committee the Association made a grant of £100. Professor Ramsay was succeeded by Dr. David Newman.

of the condition of the heart at any moment. With the quick drum we have a different form of manometer, called a Fick's manometer, which has the advantage that it does not, like the other, oscillate by itself. The mercury in the ordinary mercurial manometer will continue to oscillate a little while after the original pulse beat which put it into movement has ceased. The Fick's manometer does not, and so it gives a true pulse trace. We had thus two tracings, one giving the ordinary blood pressure throughout the whole experiment, and the other the condition of the pulse at any one moment.

—That is the advantage of this twofold method. By using it we have the great advantage that we are able to bring before you the whole of the work that we did with these instruments, because the Nizam had the whole of our tracings photographed, and is going to present a copy of the photographs to each of the principal medical libraries, so that by-and-bye the whole of this work will be brought before the profession, not only in this country but in other countries, and any error that we may have fallen into can be rectified. The first thing I show is a tracing of the action of chloroform when given steadily. I have also a tracing of ether. As a matter of fact, the difference between chloroform and ether upon the ordinary blood pressure was just what had been found by that exceedingly careful observer Claude Bernard many years ago, that they are similar in their action, but that chloroform is the more powerful of the two. We might compare them, indeed, to a sharp knife and a blunt knife in the hands of a surgeon; the sharp knife is more powerful either for good or for evil; it depends entirely upon how it is used. If a man is afraid of using a sharp knife, he may use a blunt one, and in the hands of some people the blunt knife will be the safer of the two. I will give you a typical tracing of the action of chloroform. You see that as you give the chloroform the pressure gradually sinks very slowly till you get down to the point indicated in the tracing; then you begin artificial respiration, and there are a number of tracings here which will show the effect of it. In one of these tracings you will see a sudden fall, because a great deal of chloroform has been given at once, and the effect of artificial respiration in raising it is also shown. The first tracing in experiment No. 84 shows this. In another tracing the effect is shown where chloroform is given slowly. If in place of chloroform you use ether, you generally get a much slower fall, and this is also shown in the tracing. You see the gradual fall that is obtained with chloroform and ether if you use these anaesthetics with plenty of air. With ether we found that if we gave plenty of air we could hardly get the animals under; in fact, we found it almost impossible. In giving ether we were obliged nearly to suffocate them as well. If you give chloroform, and give very little air, so that the animal is partially asphyxiated, then you are apt to get a sudden fall, as is the case in dogs when you put the inhalers over the muzzles so as to obstruct the respiration, or give the chloroform so strong that it causes the animal to stop breathing. In cases when we took the tracing upon a slow drum we found that at this sudden fall the heart had become very slow.

The next thing to consider was the question of how the chloroform acted. We did not try to go into all the scientific questions about it, but we directed our attention almost entirely to the practical part of it, namely, to find out how chloroform was likely to cause death, or rather how deaths were likely to occur during the administration of chloroform, and how they could be best prevented. The first question was whether chloroform itself was likely to paralyse the heart. Now chloroform is a powerful muscular poison; there can be no doubt of that. If you take chloroform and inject it into the artery of a limb, in a short time, almost immediately, you will find the muscles of the limb become as hard as this board. So it was thought very likely that chloroform might act distinctly upon the heart and cause paralysis of the muscular fibres; but we injected chloroform into the veins, and we did not get this distinct action upon the muscular fibres of the heart in anything like the degree that we expected. We found that its action was like that shown in one of the photographs. With the injection of chloroform and ether into the veins we got a fall of pressure, not so marked as there shown, but more like that which I showed in the first tracing. No. 92 shows the injection of chloroform into the veins, and No. 93 shows the injection of ether into the veins. There is not such a great difference between the two, although even there perhaps you may notice that the ether does not quite pull down the blood pressure so markedly as the chloroform. There is this difference, however: ether

always caused clots in the heart to a greater extent than chloroform. The next thing to do was to inject into the trachea, because if you inject it into the trachea you would expect the chloroform to be absorbed very rapidly—at least if you get it at all into the lungs—from the blood vessels of the lungs, and carried to the heart; you might then get the heart stopping, and we did get very markedly indeed the heart stopping after an injection of chloroform into the trachea. We got very much the same from the injection of ether. No. 64 shows the effect of the injection of chloroform into the trachea. The sudden fall in No. 70 shows the effect of ether injected into the trachea. These two, however, are very much alike. You will notice that the fall from the injection of ether is also very rapid, just as rapid as with chloroform. Now the action of chloroform given in these two ways is not really its therapeutic action; it is its toxic action, and we do not give chloroform for its toxic action. There is a very marked difference between the toxic and the therapeutic action of many drugs. I do not know that I can take a better example than the toxic action of strychnine. If you push strychnine to its utmost effect upon the muscles its effect is to paralyse the ends of the nerve in the muscle, and strychnine may be looked upon therefore as a paralyzing agent. But it is not as a paralyzing agent that we use it; in fact, you cannot give it to a mammal to produce paralysis; you produce tetanus; but it is not even for its tetanic action that we use it; we always stop short of giving it to produce tetanus. When we are using it as a remedy we use it only for its stimulant action. We therefore draw a sharp line between the use of a drug for its poisonous action and for its therapeutic action. What we want is to use chloroform for its therapeutic and not for its toxic action. But it is possible that even when you use things for their therapeutic action, you may get the toxic action beginning. For example, if you give strychnine you may get spasms of the muscles, and you want to stop short of that. We wanted to find what was likely to occur when we were using chloroform or ether in the ordinary way and employing them therapeutically, I mean by inhalation. If you continue to drive chloroform into the trachea after the respiration has stopped, if you use artificial respiration and drive the vapour in, you can paralyse the heart; but what we found was that if you allowed an animal simply to inhale the vapour, the stopping of the respiration prevented the access of enough vapour to the heart to stop it. The chloroform paralysed first the respiratory centre, and then, by stopping the respiration, prevented enough of the chloroform from getting into the circulation to enable it to paralyse the heart.

I have made this statement, but there are certain points about it which want a more clear and a more full consideration, because it is quite evident that if an animal is just on the point of dying from paralysis of the respiration, it may, just as the respiration is ceasing, make one or two violent gasps, and may thus get a large quantity of chloroform into the respiratory passages, possibly enough to stop the heart. Well, we found that there was some risk in this, but the greatest risk which we found was always from the chloroform vapour *plus* asphyxia. Asphyxia itself will produce the symptoms which have been ordinarily ascribed to chloroform. On looking at the tracing which was given by the Committee of the British Medical Association, we found that we were able to reproduce it. I have reproduced it on the board. You observe a sudden drop and a slow action of the pulse. It was thought that chloroform occasionally acted in an unexpected and unforeseen manner. This slow pulse is clearly due to vagus irritation. Tracing 64, showing the effect of chloroform in the trachea, also shows the effect of irritation of the vagus, and you will see the slow pulse due to it. But in this class of experiments the Committee had not irritated the vagus wittingly. We tried hard to find out how they did it, and thought they might have allowed some irritating solution, perhaps bicarbonate of soda, to fall upon the vagus, and thus might have irritated it chemically, but on trying the experiment we failed; we were not able to produce a tracing like that of the Glasgow Committee. Then we thought they might have tied the vagus along with the trachea; we tried that, but it would not do. Then we found that we could produce this tracing at will by simply closing the dog's nostrils, and that this tracing was not due to chloroform but to asphyxia. We could produce it either by stopping the dog's nostrils or by putting a cannula in the trachea and stopping up the end. We also tried the same thing by making the dog breathe pure carbonic acid. We could produce this kind of tracing at will by asphyxia; but however often we gave chloroform and repeated the experiment with

plenty of air and without asphyxia, we never got a tracing like this, though we could produce it again and again by asphyxia. I think therefore we may conclude that this tracing on which the Glasgow Committee laid so much stress was due to accidental asphyxia. I was puzzled for a time in thinking how they could have got it; it was clear that they had not suspected asphyxia. Now there is an accident which is not at all unlikely to occur, and which occurred to me years ago several times. If you are using artificial respiration, and you put a cannula into a dog's trachea, unless you tie it in very firmly, a little oozing of blood may take place from the wound, and the blood may ooze down by the side of the cannula, and get into the lower part of the trachea and into the bronchi. It may there clot, and thus form a plug, which will produce a stoppage of respiration and cause asphyxia, and I am inclined to think that the appearance, the disappearance, and the reappearance of the symptoms of asphyxia shown in the tracing may have been due, in the case of the Glasgow experiment, to the formation of a clot in the trachea or the bronchi, which moved up and down, causing at one time free respiration, then falling back into the bronchus, and then leading to the product of this asphyxial tracing.

We tried also several experiments on the effect of shock, because it seemed that shock was very likely to be a powerful agent in the production of death. The thing that struck us most was that we were not able to get any very marked symptoms of shock. Now, there is one objection that may be brought against our experiments. It may be said: "You never experimented upon any animal which had not a certain quantity of chloroform or ether first." That is quite true. There was a set of experiments which we thought over carefully, as to whether we should do them or not—namely, to fasten the animal down and put the cannula into the carotid without chloroform, and then try shock. But it seemed to us that the cases of shock which you get in men are comparatively rare, and that we should not in the case of an animal look for the production of shock such as occurred in Simpson's case, where the man died, without any anæsthetic, from the mere opening of an abscess. We thought that we were not likely to find that, and that we might probably make a number of experiments, causing the animals a great deal of pain, and not come to any very definite result in the end.

Therefore, on account of the pain that we should have to inflict we did not try the experiments. Many antivivisectionists probably think that it is a pleasant thing for men who are engaged in physiological experiments to inflict pain, but it is not so. Many a time long ago, before we used anæsthetics so much as we do now, I have spent nearly a week in thinking over an experiment, and deciding whether I should do it or not, and putting off the evil day as long as possible. Now, we concluded that these experiments were not absolutely necessary, and that they would not probably give definite results, so that we did not do them. We may have been wrong, but that is the fact of the matter. We tried, however, other experiments. We thought we might possibly gain the same result by putting the animal first of all under chloroform, then connecting the cannula with the trachea and allowing it just to come out, and then, as it was coming out, we first of all tried to produce shock by the extraction of a tooth. This we repeated several times, because it seems that during the extraction of teeth a number of deaths have occurred.

Another class of operations in which deaths have occurred is that of operations upon the anus; we therefore tried laying hold of the skin of the anus, pulling at it, and then snipping it. Deaths have also occurred during operations upon the intestines; we therefore opened the abdomen, drew out the intestine, and ligatured it. Then we thought it very likely that we might get shocks markedly from injury to the testicles; we therefore struck the animal, just after it came out from the chloroform, and while the cornea was sensitive, a blow upon the testicles, and we did sometimes find a slight fall, but not so marked as one would imagine. Then we tried the effect of position, and we noticed that when you put an animal upright, with the head up and the legs down, there was a distinct fall in the blood pressure, which rose again to the normal height when the animal was restored to the recumbent posture. If we elevated the feet, we got a rise of blood pressure. Even by striking the animal upon the abdomen, or by extracting a tooth while it was in the upright position, we were still unable to get any very marked evidence of shock. The next thing we tried was to see how far a mode of dress which was likely to interfere with respiration would cause sudden death. We tried these experiments upon monkeys, because they

were more like men than dogs were; or rather, I should say, we tried them upon female monkeys, because it is women's dress that is more likely to obstruct respiration. We put on a monkey a plaster-of-Paris jacket, so as to imitate stays; we then tied round the abdomen a tight bandage, so as to imitate the band which would sustain the petticoats. The result of these experiments was very marked indeed. In two cases the animals seemed to die very quickly. I need not now go into the particulars, because these are contained in the tracings and in the record of experiments, some of which will be published, so as to be available afterwards. Another point was the effect of phosphorus poisoning, so as to produce fatty degeneration of the heart. The heart, in some of these cases, seemed to stop more quickly after the respiration than in the case of healthy animals, but the effect was not so great as one would imagine. In No. 159 the effect is shown of chloroform given to an animal that has been poisoned with phosphorus, and in which the organs were already more or less fatty.

I have said that in most of our cases the respiration stopped before the heart; this was the case in all those animals where the chloroform was given in the ordinary way, and which were not connected with the manometer. But there were one or two cases in which the animal was connected with the manometer, and where we did not find gasping movements occurring after the heart had stopped. The respiration would get weaker and weaker, and then stop, then the heart stopped, and after that there would be one or two violent gasps which had no effect upon the blood pressure, and which were apparently useless to the animal. It might be said that in those cases the stoppage of the heart must have been due to the chloroform, but we came to the conclusion that this was not the case. One reason why we did this was that in these experiments a number of other things had been done besides administering chloroform. In one of them we found that the animal was going to die in the ordinary way, and that it had, as it were, died. The respiration had ceased; we used artificial respiration and brought the animal round, and then after it had come round we did something else to it. I think we injected atropine, and we got these gasping movements. It might be fair to say that this was due to chloroform; but another reason which led us to say that it was not due to chloroform was that we produced the same effects without chloroform; by simply making the animal inhale pure carbonic acid we produced exactly the same phenomena, as is shown in tracings 160 and 161.

I have told you the general results obtained, namely, that chloroform and ether both act in the same way. Both of them paralyse the respiratory centre before the heart. If you are able to push either one or the other after the respiratory centre has ceased to act, you can then paralyse the heart; but the chloroform will do either the one or the other more quickly than ether. It is an agent much more potent than ether; and it will stop respiration and paralyse the heart in much less time than ether. We found that ether in hot countries cannot be used as an anæsthetic. First of all, I could not get pure ether in Bombay, for the simple reason that it would not keep. You may have it one day, and the next day you look at the bottle, and the pure ether is gone. So that the ether we used was not pure; it was the purest we could get, but we found that we were unable to keep the animals under unless we used the greatest pains to have them thoroughly suffocated at the same time. We found that whereas with ether we had to suffocate them, with chloroform the proper thing was not to suffocate them, but to give them chloroform with plenty of air, because if you do suffocate them with chloroform, the suffocation is a very deadly thing indeed.

The conclusion that we came to was that deaths from chloroform are due to asphyxia, and that in cases where deaths had occurred they had been due to the imperfect observation of the respiration. The most important experiments that we did were our accidental deaths. We had a large proportion of them, much larger than ordinary surgeons have, and we could always trace the mode in which they occurred. We knew that the time of day had something to do with it. The deaths did not occur in the morning when everyone was looking sharply after things, when we were not hurried, when there was plenty of time, but they occurred in the middle of the day when we resumed our experiments after the interval for lunch. An experiment was finished before lunch, then there was an interval, during which we left the apparatus standing, but the drums had to be blacked, and a lot of little odds and ends had to be done. We did not want to lose time, and while things were being got ready, some one would put the animal

under chloroform. The way in which it was done was this: we simply had a big packing case into which we popped the dog, and put in some chloroform on blotting paper; after a time the animal quietly went over, and then he was taken out. Someone looked in perhaps just to see that he was thoroughly over, and if he was not we gave him a little more time, put him back into the box, and put in a little more chloroform. Then, instead of watching, the man attending to the case would go away to black a drum, and when he went back he found the dog was dead. Another thing which is very instructive is the fact that very few deaths occurred upon the table. When a man was giving chloroform who was accustomed to give it at a hospital very few deaths occurred. But one day he was too much intent upon the operation; he had not seen it done before, and instead of watching the animal as he ought to have done, he was watching the other man operate, and the first thing that was noticed by a third person present who was standing by, was that the animal was not breathing. The animal indeed was already dead. Another case of accidental death occurred to myself; I was doing two things at once, giving chloroform and putting a cannula into the trachea. It was a peculiarly-shaped T-tube that I was putting in. Another man was doing the same thing at the same time, and I am sorry to say that we were running a race; I was trying to get my tube in before him, and he was trying to get his in before me, and in doing the thing as quickly as I could I forgot to attend to the respiration of the monkey, and he died. It was simply from the want of attention to the respiration.

Now it might be said: "Why do you want to insist upon attention to respiration so much and neglect the pulse?" The reason is simply this: that as far as experiments went the pulse did not begin to fail until after the respiration, and if we did not attend to the animal until the pulse gave signs of failure we were apt to be too late. But is it not advisable to attend both to the respiration and the pulse, because you may get signs of failure from one or the other? That is true; the only objection is that if a man has his attention distracted, and he is attending both to the respiration and the pulse, he may be perhaps not paying as much attention to either as he would if he were attending to the respiration alone. These are the most important points to be made out in these experiments.

Before closing, I think I ought to say that we owe a great deal to the Nizam for the interest he has taken in this matter, and the generosity he has displayed. It is not merely that he has had the experiments done; anybody might have done that, perhaps, but he has been so wise as to do what a great number of learned societies in this country will not do. After they have got a research they will not publish it, or they cut down the expense of publication so much that the published results are not worth a great deal. The Nizam has decided to have, at great expense, every tracing photographed, to have a hundred or a hundred and fifty copies made, and a complete set sent to all the principal libraries throughout the world. The example which he and his ministers have set is one which is worthy of imitation by our own Government, both in this country and in other parts of the British empire.

ABSTRACT OF THE ERASMUS WILSON LECTURES ON THE RELATION OF MORPHOLOGY & PATHOLOGY. *Delivered at the Royal College of Surgeons of England.*

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LECTURE I.

WALLACE, in his interesting account of the Malay Archipelago has shown clearly enough that a line commencing in the Indian Ocean and passing between the islands of Lombok and Bali, continued between Borneo and Celebes, till it emerges into the Pacific, divides this archipelago into a Western or Indo-Malayan, and Eastern or Austro-Malayan division. The nature of the animals and plants inhabiting the great islands of Borneo, Sumatra, Java, the Philippine Islands and Bali indicate that these truly belong to Asia. In the same way the islands of Lombok, Celebes,

and the Mollucas, with New Guinea, are as clearly Australian. This is the more remarkable seeing that Bali and Lombok are separated by only fifteen miles of sea. In the islands eastward of Wallace's line, kangaroos, opossums, wombats, the duck-billed platypus, brush-turkeys, cockatoos, and honeysuckers abound. These are mammals and birds characteristic of Australia. On passing from Lombok to Bali we find barbets, fruit thrushes and woodpeckers. In Java and Borneo, monkeys, wild cats, deer, civets, otters and squirrels, mammals and birds characteristic of south-eastern Asia. In the Celebes and the Mollucas, the prehensile-tailed cuscus is the only terrestrial mammal, except wild pigs; so that in passing from the western to the eastern islands the naturalist finds himself in a new world. Indeed, this enormous archipelago, though apparently homogeneous is, as a matter of fact, made up of parts derived from two distinct continents.

We have in our bodies, in a morphological sense, more than one such archipelago, and I shall endeavour to show that several apparently homogeneous organs may be subdivided by lines pathologically as characteristic as Wallace's line is justified zoologically. The tongue illustrates this in a striking manner. If a line be drawn transversely through this organ, immediately anterior to the foramen cæcum, it will divide it into an oral and a pharyngeal portion. The oral portion resembles, in the structure of its mucous membrane and in its diseases, the lips and buccal aspect of the cheeks. The pharyngeal division resembles structurally and pathologically the adjacent tonsils and pharynx. The anterior and posterior portions of the tongue have a separate nerve supply, as well as a different origin. The base of the pharyngeal segment arises by bilateral outgrowths from the branchial arches, whilst the tip is formed by an azygous median tuberculum, which arises in the meso-branchial area. The oral portion of the tongue is liable to gummata, nævi, the so-called ichthyosis linguae, warts, epithelioma, and mucous plaques. The pharyngeal portion is prone to adenomata, dermoids, mucous cysts, and lympho-sarcomata, conditions resembling enlarged tonsils and varicose veins. Thus, morphology and pathology insist that it is incorrect to jumble the diseases of these two regions of the tongue under one heading, as is the custom in textbooks of surgery, and even in monographs dealing with lingual diseases. The value of the morphological method as a basis for the classification of pathological conditions, and its admirable convenience for conveying clear and lasting impressions in teaching, is well illustrated in the case of the testis. The embryological history of this organ establishes the fact that it consists of a secreting portion furnished with an excretory apparatus, the vasa efferentia, epididymis, and vas deferens, which originally served as excretory conduits to the primitive kidneys, parts of which become entangled in the compound organ to which the name testis is applied. These entangled remnants of the mesonephros are easily recognised in the foetal testis under the name paradidymis. This structure varies considerably in size in different individuals. Thus neglecting the coverings acquired by the testis in its descent, we have for pathological purposes three distinct parts: (1) the testis proper; (2) the efferent ducts; and (3) the paradidymis. Many of the cystic conditions arising in connection with the testis originate in some functionless tubules, situated near the head of the epididymis, in series with the vasa efferentia. Such cysts are of two kinds; they may be small and resemble the "hydatid" of Morgagni, or, remaining sessile, be described as encysted hydrocele of the testis. These cysts lie quite outside the tunica vaginalis; they must not be confounded with unobliterated portions of the funicular pouch or the sac of an old hernia. The third portion, the paradidymis, gives rise, as Mr. Eve and myself pointed out some years ago, to the tumours described by Curling as "general cystic disease" of the testis. These tumours present very distinct characters, and, as they arise in the remains of a disused gland—the mesonephros—and are glandular in structure, it would be very convenient to refer to them as adenomata. Occasionally the gland substance contains hair. Thus the cystic tumours of the testis proper fall into two very different groups—namely, those belonging to the disused ducts of the mesonephros, and those originating in the glandular remnants of that interesting ancestral organ. The tongue and testis may be described as organs anatomically homogeneous, but morphologically and pathologically heterogeneous.

In order to justify my view as to the relation of morphology and pathology I must compare organs anatomically heterogeneous but morphologically and pathologically homogeneous. A comparison of certain parts of the ovary and testis demonstrate this in a striking and suggestive manner. It is now accepted by com-