ART. XVI.—*Anæsthetics: a Clinical Study.*^a By DUDLEY W. BUXTON, M.D., B.S. Lond.; Administrator and Teacher of Anæsthetising in University College Hospital; Anæsthetist in the Hospital for Paralysis, Queen's-square, London.

I CANNOT but feel greatly honoured and gratified that you have permitted me to address you upon this subject. My theme needs no apology, and whatever deficiencies you detect in the manner in which I may unfold its many sides to you will, I hope, be forgotten in the interest of the subject itself. Anæsthetics is, of course, a thing of old-world lore, but the knowledge of the systematic induction of anæsthesia by the formal use of chloroform, ether, nitrous oxide, and their congeners dates from less than half a century ago. What knowledge, therefore, we have upon this point is well within reach; consists, if I may be permitted to say so, of a large amount of undigested material dealing in the main with the physiology of anæsthesia as studied in the lower animals, and a certain number of records of deaths occurring during the use of one or another anæsthetic. To this catalogue we must add a good allowance of more or less acrimonious discussion between the partisans of one or another school of thought. In the early days of chloroform we met with pietists crying aloud in the wilderness of the lay press against the iniquity of giving parturient women chloroform, and so defeating the aims and mandates of a Providence which had ordained that in sorrow should woman bring forth children; and in these later days we find argument is tempered by personality, while the tedium of science is relieved by wrangling. A careful study of this literature justifies me, I think, in saying that up to the present time no systematic effort has been made to treat the study of anæsthetics as a clinical research, and employ the same acumen, the same rigid methods, and unimpassioned words in obtaining records as those we should insist upon were we about to grapple with, say, tachycardia, Raynaud's or Thomsen's disease; or the action of strophanthus or ulexine upon the organism. It is to urge the necessity for such a clinical study that I am here.

I admit readily enough, and I have no doubt I shall be reminded again and again in the course of the discussion, that many valuable, though brief, monographs dealing with the matter have from time

^a Read before the Section of Obstetrics of the Royal Academy of Medicine in Ireland on Friday, January 8, 1892. [For discussion on this paper, see Vol. XCIII., p. 155.]

to time appeared, but I submit, the general bearing of the profession with regard to anæsthetics has been one which cramped the study, lost its leading lines by running into side issues, and finally dwarfed the significance of the whole matter by accepting the *dicta* of great men, instead of seeking the *facta* of every-day experience. Most men pass through two stages at least—the book-knowledge stage, in which, chrysalis-like, they accept a store of food supplied without initial labour; and secondly, a butterfly stage, when they discard mere book-lore, and test all knowledge by what they see at their patients' bedsides. So is it with anæsthetics; many of us have believed that all that was to be known had by this time been set forth in books, so that no further appeal to the experience of the eye, the hand and the mind was deemed necessary. So assured are many persons of the soundness of their theories that, discarding the usual caution of scientists, they tell us that this *quæstio vexata* is now and for ever settled, and that further research is, besides being a weariness of the flesh, futile, if not impertinent. But such, surely, are not the ways of science. Let us say, *we believe* rather than *we know*; *we conclude* rather than *we have proved*.

The necessity for the clinical investigation into the action of anæsthetics seems to be necessary for many reasons. In the first place, we have already had the benefit of many most careful researches conducted upon the lower animals, with results which are at present, to say the least of them, quite irreconcilable, but which have been conducted so carefully, and by such able experimenters, that we are bound to give them full credit for their work, and might well be "happy with either were the other but away." Thus, in 1855 the Commission appointed by the Society of Emulation in Paris came to the conclusion that in chloroform-poisoning the heart always survived the cessation of the respiration. This view was reversed by that taken by the Committee appointed by the Royal Medico-Chirurgical Society of London in 1864. The finding, again, of the Glasgow Committee of the British Medical Association was adverse to chloroform; it pointed out not only the liability of heart-failure, but laid stress upon the dangerous fall of blood-pressure, and the "capricious" behaviour of the heart under chloroform. The last Commission, that which did its work so well at Hyderabad, and with which all civilised nations must always couple, with gratitude, the name of that high-minded and philanthropic prince, the Nizam of Hyderabad, has taken a strong

line of its own, and has declared that it never met with heart-failure under chloroform, either primary or otherwise, and it commits itself further to the statement that it does not believe heart-failure ever occurs. The arguments brought forward are too weighty to be passed over without careful consideration, but are too long for reproduction here.

The weak points in them appear to me to be that their experiments, and the deductions based upon them, fail wholly to explain the cases of Europeans with which we are constantly meeting, in whom heart-failure does occur. It is asking too much to expect us to refuse the evidence of our senses and our daily experience, because they run counter to the findings of a Commission whose work we all confess was well done, even if its conclusions were fallacious.

The results arrived at by Professor Wood, of Philadelphia, and by Professor MacWilliam, of Aberdeen, and of Professor Gaskell, of Cambridge, are so much out of accord with those referred to above, that we are compelled to admit that the physiological method has not settled the question of how does chloroform kill? It would, perhaps, be premature to say that method never will succeed; but we may, I think, confidently affirm that it must, to be of any real service to us, wait upon the steps of clinical research rather than attempt to settle questions of anæsthesia out of hand, and without reference to our hospital and bedside experience. Further, we have the incontestable fact that deaths under anæsthetics are, if anything, upon the increase, and this in spite of better teaching, the wider ventilation in the journals and in manuals of the most approved means of avoiding fatalities, and a recent propaganda, as far as chloroform is concerned, which seemed to promise immunity from death and danger if certain rules, familiar to most of us, were only kept in their entirety. In spite, however, of all this, deaths go on occurring, and even in the hands of specialists and men imbued to their finger-tips with faith in the innocuousness of chloroform. All this, then, must point to one of two conclusions—that whenever a death occurs it is either the direct result of the anæsthetist's want of skill, or of his lack of conscientiousness—as by allowing his attention to deviate from watching the all-important rhythm of respiration—or that there is something yet in the whole matter of which at present our philosophy has not dreamed. Surely we may reject the former hypothesis, for although Syme in 1855 said that “the differences

existing between the methods in vogue in Edinburgh and in London fully accounted for the fact that deaths were rare in the one place, and comparatively common in the other," yet I think it will be admitted that now-a-days we know every method has supplied its holocaust of victims, and the most skilled and conscientious have lost their patients. If, then, we admit—as I think we must—that there are many points about the practice of the induction of anæsthesia which bristle with difficulties, there remains the question how best to grapple with them, and, if possible, reduce by orderly arrangement and precise investigation their number and formidable aspect. To effect this we may appeal to pathology, the results of necropsies, the clinical study of the effects of anæsthetics in human beings, and the chemistry of the reagents themselves—alike as regards their manufacture and the changes which occur after keeping, exposure to light, &c., &c.

Valuable as no doubt necropsies may be in the future, they have not in the past been of much service to us in elucidating the facts of the question before us. The changes made in the organism by chloroform or ether cannot be dissected by the scalpel, tested by the test-tube, or reckoned in the chemical balance. It may be a satisfaction to the person who gave the chloroform to be able to face the coroner's jury with the announcement that the patient's heart was undoubtedly fatty; but the scientific anæsthetist cannot but remember how many must have been the fatty hearts of patients who passed safely enough the rubicon of chloroform. In the *post-mortem* rooms of any large hospital how many normal hearts do we find? On the other hand, how many are the cases of chloroform-death recorded in the young and vigorous, with hearts healthy, and lungs free from disease? Again, necropsies, to be of any use, should be performed within a few hours of death under chloroform or ether. When possible, other means having failed to restore consciousness in a person poisoned by chloroform, bleeding to many ounces should be used, the blood being replaced by normal saline or milk, and the blood so withdrawn placed at once in a vacuum over mercury, to be reserved for subsequent gaseous analysis.

I do not wish to depreciate the value of *post-mortem* research, but I attach far less importance to it in the present connection than to the study of our cases during the actual process of the anæsthesia. It may, however, be interesting to inquire what facts have been elicited by the study of the cadaver.

Lallemand, Perrin, and Duroy found in the lower animals the lungs were rose-coloured, but free from congestion, unless a delay had been allowed before the necropsy was made, when hypostatic congestion became well marked.

The right cavities of the heart and veins were filled with dark fluid blood; the left were empty or contained clot (Sansom).

Snow, in his careful record of cases of death under chloroform, reports 35 necropsies; 16 showed disease of the heart—*e.g.*, thinning of the ventricular walls, valvular lesions, degenerations.

Turnbull records 44, and of these 20, or nearly 50 per cent., gave evidence of marked degeneration of the myocardium, pericardial, and valvular mischief.

A large number of sporadic cases reported from time to time, but not as yet collected, again afford evidence of the association of heart disease with fatalities under chloroform. It is necessary, however, to accept with caution many of such reports, because in too many cases a preconceived idea existed that such cardiac lesions were sure to be found in any given case.

As regards the other organs of the body, I confess we know little. The state of the lungs and the kidneys found *post mortem* shows no causal relation. As one would expect a certain number, which is by no means large, of cases of very grave disease—such as empyema, phthisis, &c.—have died under the anæsthetic. This forces upon my mind the belief that we must seek elsewhere for the *fons et origo mali* of death under chloroform, at least in many cases—and that is in the central nervous system. Necropsies do not give us aid here—first, because the skill of the neurologist has seldom if ever been called into requisition in making them; and, secondly, because it is very doubtful, indeed, if the *nerve* cells and commissures have any material impress left upon them by the act of chloroforming. In the physiological laboratory we know that the brain and cord undergo great and palpable changes during anæsthesia, and I have verified as far as possible, in the cases of brain and spinal surgery which I have had with my friend and colleague, Mr. Victor Horsley, the facts which I had learned whilst working with the lower animals. Still these changes, being mainly vaso-motorial, were lost after death.

I may, perhaps, quote a table which Dr. Sansom drew up some years ago which gives an analysis of 51 *post mortems*, and which draws attention to some salient points in making these examinations:—

<i>Blood:</i>					CASES
Dark and fluid -	-	-	-	-	24
<i>Lungs:</i>					
Congested	-	-	-	-	18
„ in dependent parts	-	-	-	-	4
Loaded with blood	-	-	-	-	5
Normal	-	-	-	-	10
<i>Heart:</i>					
Accumulation of fat (right ventricles)					27
Distended	-	-	-	-	3
Little or no blood in left ventricles.					
Blood fluid in all but 2 cases.					
In one “air” (<i>sic</i>) was mingled with the blood.					
All cavities empty (in one case “spasm”)					9
All cavities containing blood	-	-	-	-	13
1 case firm coagulum—galvanism has been employed.					
Auricles empty, ventricles not	-	-	-	-	2
<i>Brain:</i>					
Congested	-	-	-	-	9
Normal	-	-	-	-	14
Pale	-	-	-	-	7
Air in vessels	-	-	-	-	1

Let us now pass to the phenomena of anæsthetisation as we meet with them in our daily practice. They are best grouped under the following headings:—

- I. Facts connected with the Patient.
- II. Facts connected with the Operation.
- III. Facts connected with the Anæsthetic.

In the system of recording which the Committee of the British Medical Association now engaged upon the clinical investigation into the action of anæsthetics has put forth, and books embodying which are now in your hands, an attempt has been made to elicit by questions all the important points covered by these three groups. But before considering the system in detail, it will be well to call your attention to the range of the inquiry upon which you are asked to embark. All cases in which an anæsthetic is given may be comprised under three headings:—

(a.) Those in which anæsthesia is attained without let or hindrance.

(b.) Those in which symptoms arise which are foreign to the usual course of anæsthesia—*e.g.*, failure of respiration, failure of heart, vomiting, undue struggling, &c.

(c.) Those in which death arises either during the induction of anæsthesia, or within so short a period after its cessation as to be associated casually with it.

Group (a) is important, in that we must have a sufficiently large number of cases of normal anæsthesia to serve as a basis for comparison, and in order that we may learn with greater exactness than is at present possible what are the variations of normal anæsthesia determined by conditions of age, sex, temperament, habits of life, racial traits, hereditary peculiarity, &c.

In dealing with Group (b) we come upon what I am sure will prove the most valuable source of future information, and it is above all things necessary that our records should be exact without being diffuse, and full without being unnecessarily prolix.

If, as is admitted upon all hands, persons die from paralysis of respiration, we should meet with every stage of respiratory paresis; the symptoms carefully observed should guide us into deciding whether the insufficiency of respiration arises from peripheral or visceral, central or nervous causes. In the one case waterlogging of the lungs from bronchorrhœa, in the other paralysis of the muscles of respiration may have been the cause of trouble. Or to take another example—the watchful anæsthetist might find that respiratory movements were being normally executed, but no air entering the lungs, and here symptoms might decide whether the impediment to inspiration arose from a centric cause—namely, spasm—or was the result of mere mechanical closing of the rima by the falling back of the tongue.

These are but suggestions how a study of respiration during anæsthesia would and does help one; it would be easy, did time permit, to formulate systematically the symptoms which arise and their significance as regards the various anæsthetics. I am accustomed to tell students that were an anæsthetist deprived of hearing, or touch, or smell, yet the remaining sense should be enough to guide him, and should tell him by its use directed upon the patient what anæsthetic was being employed, and what stage of narcosis the patient had reached.

But a close scrutiny of the respiration would soon lead the

observer to notice a very close correlation between its force and rhythm and the action of the heart. To me it is a difficult matter to comprehend the strong stand which many most careful and distinguished observers have taken in asserting that chloroform has no primary effect upon the heart. I have seen in my physiological work heart-muscle as muscle absolutely killed by dosing with chloroform, and I have seen the heart of mammals, while in physiological connection with the central nervous system, become enfeebled to final cessation of its action, even when ample artificial respiration was uniformly maintained by air being regularly pumped in and out of the lungs by special mechanism, permitting of no possible chance of asphyxial processes being at work. Again, we know that under the conditions above recited, the whole heart undergoes dilatation. Such being the case it becomes, not only a matter of interest, but one of the most profound importance, that we should make the most exhaustive clinical investigation of the circulatory system of persons whilst under anæsthetics. Thus surely we should remember that the circulation comprises a double heart, an elastic arterial system, and veins and capillaries; we should mentally estimate by the symptoms the state of affairs in the visceral circulation, the pulmonary system, and last but not least, the vascular system of the brain. This may seem very formidable, and doubtless would be so to the unskilled tyro in precisely the same way that a raw student would find himself perplexed and harried, were he to be directed to work out and record the case of a patient in the manner familiar enough to the careful note-taker in a hospital where clinical work was thoroughly recorded. Still for practical purposes all this can be done by watching, *besides respiration*, the force, regularity, and rhythm of the heart and pulse, the colour of the skin, the dilatation of the vessels in the matrices of the nails, the ears, and mucous membranes. The jugular veins give one readily-accessible veins to study, and the colour, moisture, and warmth of the skin again teach us much.

My own observations, made for some years, and without, I think, any bias or prejudice, induce me to believe I have met with all stages of circulatory enfeeblement short of fatal syncope, and that in persons who, to all evidence, were quite free from respiratory difficulties. In these persons, I believe, we find pallor, cold, clammy skin, sometimes absolute blanching, the radial pulse growing smaller and smaller, and finally ceasing; the cardiac

action becoming feeble and flickering; and the respiration only growing secondarily weakened, and then only showing it by regular but progressive shallowing of the respirations. With these I should contrast those who, suffering from asphyxial complications, evidence venous engorgement, marked by cyanosis, irregular, jerky breaths, with an insufficient air entry and exit, and a heart which beats on until, the right side becoming distended, its action grows hampered, and unless a return to the normal respiration takes place, cardiac action ceases. These two extremes, doubtless, meet in the countless intermediate cases which exist, and it is that we may all know the exact delimitations of such phenomena, that we urge a systematic inquiry into the matter.

These are all practical questions, for upon the recognition of the true nature of the case must depend the method of treatment. What, for example, could be more futile than to administer nitrite of amyl in a case in which respiration has ceased, and the heart is beating well and regularly? Or again, could any proceeding be more likely to remove what chance of recovery a partially asphyxiated patient had than to invert him, and pour more venous blood into a right heart, which was already unable to force its contents through a hampered pulmonary circulation? Would venesection be likely to help a heart which, belonging to an intensely anæmic patient, was, from its feeble resiliency, unable to stand up against the dilating power of the chloroform? But, surely, in a true chloroform toxæmia the abstraction of blood offers a reasonable chance of resuscitating a patient whose death is being compassed by the circulation through his system of a chloroform-laden blood.

We know from Snow's most able researches that 36 minims of chloroform distributed through the circulation of an individual of average weight and health would kill the patient, while 24 would only produce profound narcosis. And while speaking of the possible supervention of syncope, or heart-failure, under chloroform, I cannot refrain from noticing a strange oversight which certain persons have committed, who in stating most positively that cardiac syncope never occurs, assert that they are teaching what Syme and Simpson taught, and upholding the truth of a dogma those masters are supposed to have clung to, and handed down to their students. This error a few quotations will rectify. In a clinical lecture published by Syme in 1855 (which, by the way,

is erroneously quoted as 1885 in the Hyderabad Commission's Report), he says, "All that I have done has been to follow the example of Dr. Simpson." While Sir James Simpson describes in the *Lancet* a case in which he himself gave chloroform when the patient died, and he himself attributes the death to syncope from chloroform; and further, in the remarks he made, says:—"According to all the experimental and clinical observations which have been made, chloroform appears capable of destroying life in two ways—namely, (1) by asphyxia; and (2) by syncope. Death by asphyxia can generally, if not always, be averted by at once arresting the inhalation of the drug whenever the breathing becomes noisy or stertorous—states which, as already mentioned, never occurred with the preceding patient. Death by syncope, or by sudden stoppage of the action of the heart, is undoubtedly far less under control, and has apparently formed the principal cause of the fatal issues in almost all the cases in which the patients have perished when under the use of chloroform."

It would serve no useful purpose for me to go through the problems of life connected with the other visceral systems as regards anæsthetics, but I may state that at present we have no scientific certainty about the behaviour of the kidneys in ether or chloroform patients. The statements that nephritic catarrh follows the exhibition of ether, wants, I think, further proof, and although we have no special column for it in our record-book, yet I am sure it would be a most valuable addition to our knowledge if we got reports of the quantity of urine passed, say, for 48 hours before and 48 hours after each operation, and if an analysis were made both before and after each operation. Again, in my experience glycosuria plays an important part in making our prognosis of the danger of the anæsthetic.

One of the most important issues of the problem before us is involved in the expression, "after-effects." These may be transient and slight, or severe and most serious, since it is not impossible their severity or persistence may jeopardise the success of the operation—*e.g.*, vomiting after a gastro-jejunostomy. Clinical evidence will, I feel convinced, teach us a great deal concerning the so-called ether bronchitis, the causation of uncontrollable retching and vomiting, and other as important matters. My personal experience convinces me that "after-effects" vary directly with the quantity of the anæsthetic given, and are in no slight measure influenced by the way in which it is given.

When we consider Group (*c*) we are met by the statement that so frightful is the loss of a patient under an anæsthetic, that the administrator is the last person from whom any reliable information can be obtained. But I think such a contention need not be seriously considered, for however keenly may the anæsthetist feel his position, he should, if he is a reliable man in his work, remain calm, observant, and ready of resource, and when the danger is passed, or has unhappily culminated in a fatality, his memory should readily enable him to make a clear statement of all that took place during a few minutes of peril.

And we need much carefully-gathered information about deaths which may occur—(i.) in the early stage of narcosis; (ii.) during the deeper stages; (iii.) towards the close or after the completion of the operation. It has been suggested that of the deaths grouped under (i.) some are due to fright; as, for example, some quoted by Sir James Simpson:—

CASE I.—John Hunter mentions an operation for hydrocele, in which the man was thrown into such convulsions that he despaired of his recovery, and also quotes a man dying from castration. Mr. Travers, in Vol. I. of “Constitutional Irritation,” cites several cases of sudden death in surgical patients.

CASE II.—Robust middle-aged man, moderate-sized aneurysm in femoral artery.

CASE III.—A man who was subject of strangulated hernia.

CASE IV.—A man who had been bitten in finger by cat, died three minutes after excision of bitten part.

CASE V.—Brewer's servant, of robust frame; operation for thecal abscess, caused by splinter of wood penetrating beneath nail of thumb.

CASE VI.—Male; operation for strangulated inguinal hernia, by Dr. John Argyle.

CASE VII.—Strangulated hernia, by Prof. Millar.

CASE VIII.—Child; operation for abscess in neck, by Dr. Pattison.

CASE IX.—In a case belonging to Dr. Gilchrist, of Leith, a surgeon tried to treat a sacculated aneurysm at root of neck by a small quantity of hæmostatic injection.

CASE X.—Fracture of radius; adjustment of displaced ends of bone and application of splints and bandages, by Dr. Richard Mackenzie.

Possibly fright accounts for some deaths, not many. The same

fear occurs in persons about to be etherised, and yet death in the initial stage of ether narcosis is quite a clinical curiosity; and probably only one, and that a doubtful one, has been recorded. Children, again, seldom die in the first stage of chloroform. Deaths in the later stages of chloroform narcosis usually arise from hampered respiration, or at least this forms a factor in bringing about the result. Diseased conditions of the lungs, especially emphysema, here play a part. The inability to expire freely prevents due exhalation of chloroform, while inhalation goes on.

Clinical evidence will be of the utmost use in elucidating the phenomena of deaths in the second and third classes—*i.e.*, those occurring in the course of the operation, and those taking place after its completion. The problem is only a simple one before the surgeon takes up his knife, for then the anæsthetic or fear is the only cause of death; afterwards, shock, emergencies such as hæmorrhage, blood entering the lungs, &c., complicate the issues, each probably contributing a factor in bringing about the fatal result.

Most important are the facts which shall serve to elucidate those deaths which take place after the operation is over and the patient is partly or wholly recovered from the influence of the anæsthetic. It is unscientific to put down such casualties to the anæsthetic, speaking vaguely of "ether bronchitis," "heart-failure after chloroform," until we have eliminated concomitant circumstances, such as the chilling effects of prolonged exposure, especially of mucous surfaces, the influence of the spray, loss of blood, &c. The only way to work out these points is by clinical research.

Further, among circumstances affecting the patient, we have yet to determine whether the hour of the day, the state of the weather, and electric potential, and so on, do not materially affect the risks of the patient. We know that the vitality of the individual is lower at certain hours—*e.g.*, 3 a.m., and has reached a maximum at 9 a.m.; while late in the afternoon the system is again jaded.

Of sex and age, except as regards proclivity to certain forms of ailment, we know very little as regards their influence on anæsthetics, and the same applies to race and climate.

As regards the nature of the operation much may be said. We know that posture and the surgical proceeding as regards the degree of shock it produces play no unimportant part in chloro-

form prognosis. Shock, again, in partial anæsthesia—*e.g.*, pulling about of intestines, reducing dislocations, has apparently caused many deaths. It is curious that experiments made upon the lower animals have not been in accord with our experience.

The last points to be briefly considered are those about the anæsthetic:—(i.) purity of drug, its quality, the quantity employed, and the source and method of its make; (ii.) mode of employment—*i.e.*, the method of administration; (iii.) personal knowledge of anæsthetist, and his responsibility.

There can be little doubt, I think, that a certain number of deaths under chloroform must be put down to inexperience or carelessness on the part of the administrator, and probably impure chloroform may account for some others, so that the headings given above cannot be held to be superfluous in the conduct of any clinical investigation into the action of anæsthetics.

In conclusion, I beg to thank you for the courteous hearing you have accorded me.

ART. XVII.—*Detachment of Retina.*^a By JOHN B. STORY, M.B., F.R.C.S.I.; Surgeon to St. Mark's Ophthalmic Hospital.

THE well-recognised fact that, in presence of retinal detachment the most skilful oculists are generally both helpless and hopeless, has induced me to communicate brief notes of some cases of this affection which have so far been exceptions to the general rule, and have either partially or completely recovered. The ordinary course of a case of detached retina is too well known to require description. All are familiar with the sudden and rapidly-increasing blindness, the weeks of futile medication—perhaps combined with equally futile operation—and the final nearly complete blindness, with formation of secondary cataract. I might bring hundreds of such cases before the Academy, but to no good end. The few exceptional cases I am bringing forward are sufficient to show that all cases need not necessarily terminate in blindness.

CASE I.—Mrs. C., aged forty-one, from the County Tipperary, consulted me on April 23rd, 1890, with the complaint of a shade over her right eye of a month's duration, so that now she can only see in a little

^a Read before the Section of Medicine in the Royal Academy of Medicine in Ireland, on Friday, April 22, 1892. [For the discussion on this paper see page 78.]