

asseveration; but he makes one or two distressing attempts to do so. For instance, at page 437 of *THE LANCET*, he describes Portal's second observation as the first of his series of complete placental presentation. He now, at page 548, doubts if this was a case of placental presentation at all. In Portal's twenty-ninth observation, the child's head presented, and passed *through* the structure of the obstructing placenta, (a pretty conclusive sign that it must have been lying quite across the os uteri.) But even here, so writes Dr. Lee, page 548, "in the history of this case, no fact is stated to prove that it was an example of complete placental presentation."

Still, with all these and similar equivocal means, the alleged number of ten partial placental presentations is not made out by Dr. Lee; and it simply remains one other illustration of his inimitable powers of assertion.

Let me close these remarks in Dr. Lee's own apposite words:—"There can now be no longer any difficulty in determining how much reliance is to be placed on Dr. Lee's statistics, and whether he has been sufficiently impressed with the importance of observing the most rigid accuracy—the most scrupulous regard to truth. . . . In proportion to the imposing effect which figures give to assertions, so ought to be our jealousy with regard to the data on which numerical calculations are founded. If correct, we are furnished with a safe and steady stepping-stone to truth; if incorrect, (as lamentably exemplified in the above instance,) we are confounded by the sudden yielding which takes place, and get involved in a slough of error."

*Dr. Lee's Apology for Concealing his Detection of his Published Errors regarding the Structure of the Placenta.*

In a former communication, (see *THE LANCET*, page 519,) I had occasion to notice incidentally—first, that Dr. Lee had propounded, in 1832, an entirely new view regarding the anatomy of the vascular relations between the uterus and placenta; secondly, that he had discovered in May, 1833, that this supposed great anatomical discovery was simply a great anatomical blunder on his part; and thirdly, that he had, for eight or ten years afterwards, wittingly and improperly concealed from the profession this knowledge of the error into which he had thus led anatomists and accoucheurs. In defence of his conduct, Dr. Lee avers in his last paper, (*THE LANCET*, page 549,) that a month after he discovered his error, it was mentioned in a review of a work of Velpeau's in the *Medical Gazette*. In other words, he would argue, that the casual notice and correction of his error, in the course of this anonymous review, was a sufficient public notice of his mistake. And this is his only apology (?) for continuing for years to mislead his professional brethren on the matter. But I have already shown, that about a twelvemonth after this review, by an unknown hand, had appeared, the editor of the very journal in which the review appeared, announced, as if officially, to his readers, in reference to Dr. Lee's views of the utero-placental circulation,—"We are enabled to state that Dr. R. Lee is still pursuing his inquiries on the subject. We have not learned that there has been any change of opinion."

Can Dr. Lee explain two things—1. Who could possibly have told the Editor of the *Medical Gazette* to make this false and deceitful semi-official announcement to the profession? and 2. Why did not Dr. Lee at once contradict it, and why did he not confess his error, when publicly called upon to do so, a few weeks subsequently, by the duped Editor of the *Gazette*? (See *THE LANCET*, p. 519, for quotations and references in full.)

*Dr. Lee's Argument regarding reliance being formerly placed on his recorded Observations.*

"In spite," says Dr. Lee, "of all the errors that are stated by Dr. Simpson to have been committed by Dr. Lee, Dr. Simpson placed . . . unbounded reliance on Dr. Lee's accuracy for constructing the Old Mortality Table, in 1845." Quite correct. But then I believed that Dr. Lee, like other writers, spoke and wrote the truth. His anatomical facts (?) have been all severally doubted and challenged by competent anatomists. Let him beware lest his late papers do not invoke from accoucheurs a similar judgment upon his obstetric facts.

*Dr. Lee's Objections to the Corrected Statistical Results of the Cases of Dr. Ramsbotham, &c.*

In the table which I first published in *Cormack's Journal*, I entered Dr. J. Ramsbotham as having reported sixteen cases of placenta prævia, with eight deaths, and Dr. F. Ramsbotham as having given forty-four cases with eight deaths; sixty cases in all, with sixteen maternal deaths, or one mother perishing in  $3\frac{1}{10}$  deaths. To this it was objected that the cases were partial and selected, and hence apt to lead to error. Dr. F. Rams-

botham most kindly, and with great trouble, drew out for my second table, the notes and results of *all* his father's placental cases and *all* his own, and has certified their accuracy in a letter to Dr. Lee which Dr. Lee himself has published. (See *THE LANCET*, p. 440.) But in objection to my thus stating the whole of the Drs. Ramsbothams' cases instead of a part, Dr. Lee earnestly argues, "In the old table, J. Ramsbotham reports nineteen cases, eight of which were fatal; in the new, 129, and forty-one fatal. In the old table Dr. F. Ramsbotham reports forty-four cases, eight of which only were fatal; in the new table he reports 189, of which forty-nine were fatal. These tables represent Dr. J. Ramsbotham as having reported nineteen cases in 1845, and 129 in 1847; and Dr. F. Ramsbotham eight fatal cases in 1845, and forty-nine in 1847." After some more precisely similar citations, he adds,—"It would be insulting to the understandings of the enlightened practitioners throughout Great Britain to carry comparison further. . . . I have great difficulty in comprehending how any man endowed with common sense and the ordinary feelings of humanity could have ventured to publish such a statement," &c., &c.

Now I most willingly leave it to my professional brethren to decide what character should be given to such melancholy mutterings as these, and whether they are capable of being explained, except on the ground of the old adage, that *Ira brevis dementia est*.

Dr. Lee goes on to allege that his statements have excited "great wrath" in me. I can most truly and sincerely assure him of the very contrary. They have excited, not anger, but most unfeigned regret and pity on my part—regret and pity that a man of Dr. Lee's previous literary standing and supposed integrity should have so far forgotten the respect which he owed to himself, and the respect which he owed to his profession, as to have indulged in making those numerous and deliberate errors of statement of fact, which it has been my very painful duty to unwind and expose.

Edinburgh, Dec. 1847.

ON THE  
USE OF ETHER AND CHLOROFORM IN  
SURGERY AND MIDWIFERY.

By J. G. LANSDOWN, M.R.C.S.E.

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THE recent discovery, by Professor Simpson, of the anæsthetic effects produced by the inhalation of chloroform, will no doubt prove a great boon, not only to the public generally, but especially to those medical men who may think fit to apply its use practically. For my own part, I consider myself much indebted to the Professor, not merely for the discovery he has made, but also for his great kindness in so early forwarding to me an account of it.

I did not fail to take the first opportunity afforded me of testing its qualities, and I have found that the experience of my own practice fully bears out its properties, as described by Professor Simpson, and the advantages of the chloroform over ether, which he has stated to be the case in his pamphlet, the greater part of which appeared in *THE LANCET* for Nov. 20th.

I have used it several times to relieve pain in natural labour, in operations, and in tooth-drawing; also for the reduction of strangulated hernia, and in a case of delirium tremens. Each time I have used it I have found the effects produced more suddenly than they can be with the ether, and that the state of sleep can be continued for a much longer period without a renewal of the inhalation, should such be desired, as in performing operations about the mouth, when a renewal of the vapour may be impracticable. It is also by many considered exceedingly pleasant in taste. This, added to the peculiar state of quietude or happiness which some experience during the period they are under its influence, renders its inhalation a thing to be desired rather than to be dreaded, or, at the best, to be taken as the alternative against the suffering of pain. One lady, to whom I was administering it in natural labour, having been in a complete state of rest for about two hours, aroused only by each return of the uterine action, when she would make signs for a renewal of the inhalation, remarked, at a time when her friends, thinking she was fast asleep, were indulging in a little talk,—"I enjoy it more when you are quiet,"—thereby implying that she was not suffering from the pains of her labour, but that she was actually enjoying the happy state of repose into which she was thrown by the chloroform. She inhaled two drachms and forty minims in three hours and ten minutes.

The rapidity with which it takes effect renders it peculiarly applicable to obstetric practice. I find in such cases the patient is insensible with the chloroform in ten seconds; while with ether the usual average is twenty-five seconds. Thus it being more speedy in its action, a less quantity is required to be taken previously to the pain coming on with its full force, after it has given the patient the warning of its approach, thereby allowing her to be insensible to pain before the time arrives for her to cry out; whereas, when inhaling the ether, should the action of the uterus assume its full force before insensibility is produced, the patient will sometimes cry out instead of continuing to inhale. In the case of strangulated hernia, an old lady of seventy-two was completely insensible in half a minute, when the tumour, which before had been exceedingly tense, became perfectly soft, and the slightest pressure returned it immediately. The patient in delirium had taken six grains of opium, divided into three doses, in the course of twelve hours. Two of these doses I had myself given, therefore I knew they had been duly administered. Seven hours after the last dose had been taken, no sleep having been procured, I covered his nose and mouth with a cloth, upon which I had thrown forty-five drops: in half a minute he was asleep. Quiet having been once obtained, the opium took effect, and he slept for four hours, awaking perfectly quiet and rational; and now, four days since he took it, he is quite well, and has required no medicine.

The use of ether having been superseded, it would appear now to be almost useless to mention those cases which have occurred in our practice; but as the chloroform is a very similar agent, it may not be amiss to mention some in which I have used it, and the extent to which I have at times carried it amongst my obstetric patients. My practice being of a mixed character, and not paying more attention to the midwifery than to the surgical department, I cannot be expected to have used it so frequently in that class of practice as others who have paid exclusive attention to that branch of the profession. Upon one occasion of a labour, I went on imperceptibly hour after hour, not thinking how time was passing, until my patient had been inhaling the ether for eleven hours and a half, at intervals of about five minutes apart, and had consumed fourteen fluid ounces of ether. Not a bad symptom followed; there was neither headach nor anything else unpleasant; but she was the next day very well, having herself been spared the knowledge, and her system the shock, of what would otherwise have been a very painful and tedious labour of twenty-eight hours' duration. Having mentioned this one, the others in which I have given the ether three or four hours and upwards, which I have done in several instances, must sink into the shade. I have never had occasion to regret using the ether in my midwifery practice, which I have done thirty times. The patients also are themselves much pleased with it, and they have had much more rapid recoveries than is usual upon ordinary occasions. I find the uterus sending out the placenta immediately after the expulsion of the child, and there has scarcely been any hæmorrhage following. Since I commenced practice in 1828, I have seldom gone to a labour without my tincture of ergot in my pocket; not with the intention of using it during the labour,—a mode of practice which, when employed indiscriminately, by way of hurrying the labour, cannot, I think, be too much condemned,—but for the purpose of suppressing hæmorrhage after the expulsion of the placenta, should such occur. I have now quite forsaken my little bottle for the ether, which in its turn must make way for the chloroform. But the state of the perinæum is a thing which has forcibly struck me while using the ether; that has invariably relaxed before the head has come to bear upon it, thereby not requiring the pressure of the head to force it open and to lacerate it, as was frequently the case when left to itself in the old mode of practice. This advantageous quality I have not observed while using the chloroform, the parts under its use continuing unaffected by it; neither have I ever found the action of the uterus, when in a sluggish state, induced by the chloroform as it is by the ether.

In a case of strangulated hernia, in which I used the ether, the result was similar to that I have before mentioned while speaking of the chloroform. The ether was much longer in producing its effect, the man being unwilling to inhale; therefore in such cases the chloroform is to be preferred, for the reason, that should the inhalation be opposed, the effect is so much more readily produced, that the patient becomes narcotic before he is aware of it. I have also freely given the ether in a case of tetanus, accompanied by trismus, which has lasted three weeks: the only thing by which the mouth could be opened was the inhalation of ether. The poor fellow has

struggled through it thus far, and the trismus has now nearly left him. I hope to report this case at length in *THE LANCET*, when I shall have time to do so. Dr. Protheroe Smith, in the perusal of whose letters I have felt much interested, in his paper on chloroform, inserted in *THE LANCET* of Nov. 27th, has so worded his last paragraph but one, that it may by many be understood that he was the first who recorded in the pages of *THE LANCET* his experience of the use of ether in obstetric cases in England. If I am so to read it, I must beg to correct him. I am quite aware that his was the first case in which it was used in England; but mine following immediately after, was recorded in *THE LANCET* of April 24th, one week previous to Dr. P. Smith's announcement; and if there is any honour in being the first to have used it in natural labours in England, I may claim that and its publication also, as my first case of natural labour took place on April 7th—Dr. Smith's, April 27th.

While writing the foregoing, I was called at half-past six P.M., December 2nd, to Mrs. H—, a fine robust woman, aged twenty-eight, in labour with her first child. I found the os uteri fully dilated, the head presenting in proper position, and every probability of a speedy delivery. At five minutes past seven I commenced using the chloroform: the labour proceeded slowly, but naturally. At eleven the head was at the outlet, and I expected the child would soon have been born; but the head continued in the same position, not pressing against the outlet, but remaining steadily there. By half-past four the following morning, having exhausted my entire stock of chloroform, I went home for some ether, which she commenced to inhale at twenty minutes past five, and continued to do so until twenty-five minutes past twelve P.M., when, everything being favourable, with plenty of room for the application of the forceps, I introduced them, and in five minutes from the commencement of the inhalation for that purpose, the child was brought away alive and well. In a few minutes she awoke, and was astonished to hear the child cry, not being aware that she had been delivered by means of instruments. The ether was used seven hours and ten minutes, making a total of sixteen hours and a half, during which she was inhaling the chloroform or ether. In this case I particularly remarked that the chloroform acted merely as an anæsthetic agent; it neither increased the force nor frequency of the action. I will not state positively that it arrested the action, but I sometimes almost fancied it did so. After eleven o'clock, when the head came to the outlet, the vagina continued in the same condition as before, and towards the close of its use the vagina and surrounding parts were hot and dry, as they would have been in protracted labour without the chloroform. No sooner, however, did she commence to inhale the ether, than the action of the uterus was more frequent and lasting, the vagina gradually gave way, and the parts assumed a more favourable condition, moisture taking the place of the dryness and heat, which before were so marked; and when introducing the blades of the forceps, they passed in without any opposition from the sides of the vagina. During the inhalation she was not once sick, neither was she uncomfortable in any way. To-day, December 4th, she is as well as she would have been under the most favourable description of labour.

I may remark, that out of 111 cases in which I have used the ether, only four have been sick, and two of those were persons to whom I exhibited it in natural labour.

St. James's-square, Bristol, December, 1847.

## Reviews.

*Researches into the Pathology and Treatment of the Asiatic or Algide Cholera.* By E. A. PARKES, M.D., &c. London: Churchill, 1847. pp. 250.

(Concluded from page 680, vol. ii. 1847.)

In our former notice of this volume, the details of the morbid appearances found in persons dead of cholera, and of the symptoms presented during life, were discussed at some length. With the view of then confining attention to the leading features of the disease, the symptoms of the premonitory and consecutive stages were not alluded to. It will now be necessary to refer to them briefly. Those of the *premonitory stage* are thus clearly summed up by Dr. Parkes. He says they are—