ART. XI.

On the Theory and Practice of Midwifery. By FLEETWOOD CHURCHILL, M.D. M.R.I.A., &c. &c. With 105 Wood Engravings. Second Edition, corrected and improved.—London, 1850. Fcap. 8vo., pp. 496.

THIS Manual has been sufficiently long before the public, to allow its merits to be adequately considered; and we believe that we state the general opinion of the profession, as well as our own, when we say, that it may justly lay claim to the merit of laying before the student a condensed view of the present state of the science and art of Obstetrics; whilst its chief fault, like that of the ‘Treatise on the Diseases of Children,’ by the same author, reviewed in our last Number, is that in bringing together a valuable collection of the opinions of various writers, more or less distinguished, upon important points of physiology and practice, it too frequently leaves the student (for whom it is avowedly intended) insufficiently informed as to the author’s own views, and the line of conduct he would himself pursue in a given emergency. We shall adduce an illustration of our meaning, from one of the novelties of the present edition, namely, the discussion of the merits of the treatment of *placenta prævia* recommended by Drs. Simpson and Radford. Nearly five pages are occupied with a summary of the controversy which has taken place on this point, and with an examination of Dr. Simpson’s statistics; and the following is Dr. Churchill’s conclusion:

“I have thus examined with care this very difficult subject; and although I would be very far from pronouncing dogmatically upon it, I feel bound in duty to state that, except in the cases I have mentioned, I could not consent to substitute the new method of treatment for the old, and even in those cases I would recommend the utmost caution.”—(p. 412.)

Nine classes of cases are mentioned by Dr. Churchill; and we cannot but believe that the student would be sorely puzzled to make out precisely what are those in which Dr. Churchill would “consent to substitute the new method of treatment for the old,” and what are the “cautions” which he would interpose. Believing, as we do, that there is a class of cases in which the new method is greatly to be preferred to the old, and that the prompt and judicious adoption of the practice in question has saved many lives, and is likely to save many more, we feel the greater anxiety that such knowledge on the subject as it is thought desirable to communicate to the obstetric student should be definite in its character. And we are sure that if Dr. Churchill had kept his analyses and estimates of the data obtained by Dr. Simpson and others, for his own private consideration, and had furnished his readers with his conclusions thereon, in a form suitable to their guidance through the difficulties of practice, he would have conferred a much greater benefit on them than he has done by the introduction of a summary, concise and well-executed as it is, of the leading points of the controversy. The same remark we would make on the statistics which are introduced in various parts of the book. We fully coincide with Dr. Churchill as to the estimate of their value which he gives in his preface; but we think that the Critical Dissertation on the comparative merits of different modes of practice is rather the place for them, than the Student’s Manual, in which the learner has a reasonable expectation that he shall

find rules laid down for his guidance, rather than the data on which his teacher thinks that such rules may be constructed.

Since the publication of the first edition of this work, great advances have been made in our knowledge of the physiology of the female generative system; and of these Dr. Churchill has in many instances availed himself. Several time-honoured errors, however, are retained; such as the statement that the menstrual secretion “differs from blood in containing no fibrine” (p. 50); and that “not only does each impregnated vesicle give rise to a corpus luteum, but nothing else does, at least in the human subject, so that the presence of a corpus luteum is a proof of impregnation.” (p. 73.) On the former of these points, we would refer our readers to the researches of Dr. Letheby and Mr. Whitehead, and our own remarks thereon (Vol. I, p. 216); and on the latter to almost any recent physiological treatise,*—it being now generally admitted, that corpora lutea differing in no respect but size from those consequent on impregnation, may be formed after the ordinary maturation and discharge of an ovum.

In the description of the decidua, again, the Hunterian doctrine as to the formation of the decidua reflexa is still retained, as if it had never been rendered doubtful by the microscopic researches of Professor Goodsir, as well as by other considerations; and we do not find the least allusion to Professor Goodsir’s researches on the structure of the placenta, whilst Dr. R. Lee’s exploded doctrine of the entirely fœtal nature of that organ, and of the absence of any direct communication between the placenta and the uterine blood-vessels, is still retained, as if for the express purpose of puzzling the student, who cannot be supposed to know that Dr. Lee’s views, confirmed as they seem to be by the authority of eight names (not to mention an *et-cetera*) cited in their support, are no longer held by a single physiologist of repute. It is well known that the researches of Dr. J. Reid, Professor Weber, and others, have *entirely settled* this question, as far as the *venous* communication is concerned; why, then, is the student dragged through the mazes of the whole previous discussion, and left in doubt at last? The whole treatment of this subject shows no evidence of any new information gathered upon it by Dr. Churchill since he penned his first edition; and yet the work of Messrs. Baly and Kirkes, to which we have already referred, would have enabled him to supply very recent and accurate information upon the whole of this department, without taking the trouble to look elsewhere. We may mention that we have lately learned that Professor Schrœder Van der Kolk availed himself of the many opportunities of examining into the connection of the placental and uterine vessels, afforded by the peculiar fatality of cholera among women far advanced in pregnancy; and that he has not only confirmed the accuracy of the statements of Reid, Weber, &c., but has clearly made out the *arterial* connection to be very much what the Hunters had described it.

In the chapter on Parturition, Dr. Churchill introduces a notice of Dr. Tyler Smith’s hypothesis of ovarian excitement as the determining cause of ovi-expulsion, on which he thus remarks: “Admitting that ovarian excitement thus excites uterine action, I do not think Dr. Smith

* See, for example, Messrs. Baly and Kirkes’s ‘Recent Advances in Physiology,’ in which it is laid down (p. 57) that a body possessing all the essential characters of a corpus luteum, but not larger than a small pea, does not afford any evidence of impregnation.

has satisfactorily explained the cause of that excitement occurring regularly at the tenth menstrual period rather than any other." We are glad to see even this degree of caution in the acceptance of Dr. T. Smith's doctrines, which seem to have gained a considerable degree of currency among obstetricians who are not accustomed to weigh the value of physiological evidence; but we should have been glad to learn from Dr. Churchill, why he is so ready to admit that "ovarian excitement thus excites uterine action." So far from this doctrine being supported by further inquiry, it has been, we understand, entirely disproved, and the validity of all our objections to it fully sustained, by the experiments of Professor Simpson, who has removed the ovaries from sows already pregnant, and found that parturition came on at the usual time, and in the accustomed manner. We hope that the detailed results of these experiments will be soon made public.

We have a great confidence in Dr. Churchill's good sense, when he fairly sets himself to master a subject; and as we have had so much occasion to find fault with him for not sufficiently individualising his instructions, we gladly present a favorable specimen of an opposite kind, from his observations on the administration of Chloroform. He first gives the inferences which appear to him fairly deducible from the *facts* recorded by others, irrespective of the *opinions* of the various writers who have engaged in the controversy; and then offers the practical conclusions which may be regarded as his own opinion, "formed after much thought and reading, and after some slight personal experience." "I would not wish," he continues, "to put them forth dogmatically, for I do believe that we are not yet in a condition to define accurately, or to speak positively on the subject. I confess that I can neither agree with those who think that chloroform can do no evil, and therefore ought to be used in every case, nor yet with those who regard it as in all cases injurious, and therefore to be reprobated:"

"1. In most *obstetric operations*, anæsthesia appears to me to be of great use, not so much because it is supposed to relax the soft parts, or to moderate uterine action, as because it enables the patient to bear the additional pain we inflict without outcry or movement. It surely must be a great advantage in performing a dangerous operation, that the patient should lie still, and not by her struggles increase our difficulty, and the risk of injury to herself. If the tissues be relaxed, which is doubtful in many cases, it is of course an additional advantage; and if it happened in a case of turning that the uterine action were suspended, of course the operation would be all the more easily completed; but these are rather accidental advantages than essential consequences. In operative midwifery, therefore, chloroform may be given until anæsthesia is produced, before commencing, and its effects may be kept up during the operation, *provided* that there be no counter-indication to its use, and that no unpleasant symptom arise; in either case it should be given up altogether. In any operation for terminating labour in a case of convulsions, I should be unwilling to use chloroform, on account of the nervous excitement it occasionally produces, notwithstanding that it is said to have been employed beneficially in the treatment of that disease: in like manner I should fear to use it in cases of alarming hemorrhage, lest it should give rise to severe collapse. I mention these cases as illustrative of the caution which appears to me necessary in the present state of our knowledge. Further experience may prove this reserve to be unnecessary, or may confirm its propriety.

"2. As to its exhibition in *natural labour*: as I do not believe that in the large majority of cases, convalescence is at all impeded by the suffering, I cannot see the

necessity, or even the propriety of urging the employment of anæsthesia in every case; and I do feel that even greater caution ought to be used than in operative midwifery. We may be justified in running some risk where an important point is to be gained, such as perfect quietness during an operation, which we should not be justified in incurring merely to relieve pain. Thus in hysterical or nervous patients, in those labouring under nervous affections, or organic disease of the lungs or heart, &c., I do not think we ought to employ it.

"But on the other hand, as pain is undoubtedly an evil in itself, if there be no counter-indication, and if the suffering be either great or prolonged, I cannot see that we are prohibited from the employment of anæsthetics, more especially as it is not necessary in such cases to produce insensibility. It is quite possible to afford immense relief, to 'render the pains quite bearable,' as a patient of mine observed, by a dose which does not produce sleep or impair the mental condition of the patient, and which all our experience would show is absolutely free from danger.

"In my own practice I have never urged a patient to use chloroform in natural labour, and, on the other hand, I have not felt justified in refusing a moderate dose of it when the patient urgently desired it, and none of the conditions were present which seemed to me to counter-indicate it.

"The period at which it has been administered varies with different practitioners; some commence before the os uteri is dilated, others about the time the head escapes through it. There can seldom be any necessity for its use, I think, before the os uteri is fully dilatable, and it is more likely to interfere with the uterine action at an early than a later period. At the commencement of the second stage would, I should think, be soon enough, and this seems to be Dr. Simpson's practice.

"There is a difference of opinion as to the extent to which the anæsthesia should be carried. Professor Simpson prefers inducing complete insensibility at first, and then keeping up just so much of the effect as he deems advisable. Dr. Rigby prefers commencing with smaller doses in natural labour, and increasing them if necessary; and the Obstetric Committee of the American Med. Association, in their Report, agree with this view. Of course, if we are to operate, the patient should be placed thoroughly under the influence of the chloroform before we commence, and its effects kept up by occasional inhalation. But in ordinary cases, as I have said, I prefer beginning with a moderate dose and watching its effects, and if necessary, increasing the anæsthesia.

"The dose should be administered at the beginning of each pain, and increased when the head is passing over the perineum. The anæsthetic state may be kept up for hours without mischief, especially when complete insensibility is not required.

"I have tried various modes of administration, instruments specially contrived for the purpose, sponge, lint, &c., and I believe that by far the best is the one originally proposed by Dr. Simpson, viz. a clean white pocket-handkerchief folded funnel-shape; into which half a drachm or a drachm of the chloroform is to be poured, and which may then be placed near the mouth of the patient, and after a few respirations, over both mouth and nose. It is a good plan to allow the patient to hold the handkerchief herself, unless we wish to produce deep anæsthesia, as it will fall from her hand when sleep commences." (p. 253-4.)

We are strongly inclined to the belief that, on the whole, Dr. Churchill's view is among the most sensible that has been put forth on this subject: to which, however, we shall take an early opportunity of recurring more in detail.