From 1 to 2 hours	18	cases.
2 to 3 "	5	"
3 to 4 "	2	"
5 hours	4	"
6 "	1	"
7 "	1	"

Presentation in 582 cases:—

In 545 it was natural.
5 the hand descended with the head.
14 the foot presented, 5 were lost.
11 the breech, 4 were lost.
4 the arm, 3 were lost.
2 the funis, 2 were lost.
1 the placenta.

Six cases of *turning* (1 in 100). One child and all the mothers saved.
One *forceps* case (1 in 605). Mother and child recovered.
Four *crotchet* cases (1 in 151). All the women recovered.
Seven cases of *hemorrhage.*
Four women died (1 in 151).

42. *On the Position of the Placenta in the Womb.*—In our Number for May, 1839, (p. 242,) we gave an analysis of an ingenious paper by Mr. Carmichael, in which the author maintains that in natural pregnancies, the placenta is always placed low down on the posterior wall, and that its being implanted in any other situation must *ex necessitate* during the growth of the uterus, or at least during its contractions to expel the fœtus, cause a premature detachment and consequent hemorrhage. These propositions are controverted by Dr. Richard Doherty in an interesting paper in the Number of the *Dublin Journal of Med. Science* for July last.

Dr. D. quotes several cases which seem to entirely overthrow Mr. Carmichael's theory. The most striking of these are the following:

"I. The first case I shall bring forward is one to which I was called on the 5th of June, 1838, in my capacity of Physician to St. Thomas's Dispensary. I was informed the woman was dying in consequence of loss of blood after delivery. On my arrival, I found her pulseless, her features sunken, extremities cold, uterus large and hard. Having given her stimulants, and in vain tried by the usual means to cause the womb to expel the placenta, I prepared to extract, and introducing my hand separated it with little difficulty from the *lower part of the anterior wall.* Its surface extensively presented that gritty degeneration, so frequently seen in such cases. On inquiry into the history of this patient, I was informed by the midwife and other attendants, that her labour had been short and favourable, and no unnatural loss took place, until about half an hour after the birth of the child, which was alive and healthy.

"Here then is a case in which strong uterine contraction existed for four hours, without detaching the placenta, although it did not adhere to the posterior wall; but, as soon as the uterus had rested after the fatigue of labour, and established the peculiar action by which it throws off the after-birth, it succeeded in, at least partially, separating it, and hemorrhage ensued. Why, I would ask, were the strong expulsive efforts unable to effect as much, (particularly as so little assistance was required to peel it from the uterus,) although this placenta was situated where, it is asserted, uterine action principally, nay almost exclusively, resides? I may add, that as soon as the after-birth was detached, the uterus acted naturally, and expelled both it and the hand together, yet I did not perceive any such partial or rotary contraction as is described.

"II. Bridget Nicholson, ætat. 25, a plethoric countrywoman of rigid fibre, was admitted into the Lying-in Hospital, Rutland-square, on the 8th of December, 1838, in labour of her second child. The pelvis was rather undersized

in its dimensions. For some time labour appeared to go on favourably, though slowly; but after several hours had elapsed, and it was evident the head was not advancing in consequence of the want of a good tonic contraction of the uterus, borax was first tried, but this being found useless, three half drachm doses of ergot were administered with intervals of half an hour. The first two doses produced powerful uterine action the last none. It was ultimately considered necessary to resort to instrumental delivery. After the removal of the child, the uterus remained large and flat, by no means an uncommon sequel to the use of ergot. The placenta could not be moved by pressure, and at last it was requisite to pass the hand to detach it, which was accordingly done by Dr. Dwyer, the Senior Assistant Physician of the Hospital, who stated, it was placed on the *anterior and upper part* of the uterus.

"Here then is an instance in which labour was prolonged for *many* hours, during a great part of which strong uterine efforts existed, nay even the uninterrupted tonic contractions produced by ergot were called into action, and yet no hemorrhage occurred, no separation of the placenta was effected, although it was exposed to their violence, and placed in the very situation in which they are supposed by the author to be the strongest and most efficacious, and where, to arrive at its then position, it must, according to his views, have undergone a considerable degree of rotation."

"VI. The last case I shall cite is that of Mary Heron, ætat. 24. pregnant for the third time, who was admitted into the hospital on the 13th of February last. Labour set in regularly on the following day, about nine o'clock, A. M. On examining this woman, I found the placental souffle distinct and sonorous, as if situated immediately under the stethoscope, in the right and upper angle of the uterine tumour. In the opposite angle, the souffle could likewise be heard, but not at all so distinctly. It was also faintly audible across the fundus of the womb. In tracing it downwards from the right angle of the uterus, it gradually grew weaker, until, at last, it was entirely lost about an inch below the umbilicus;—not the slightest murmur could be distinguished in either iliac fossa.

"To these facts I not only directed the attention of several pupils, who happened to be in the ward, but I also pointed them out to Dr. Herdman, the Assistant Physician on duty, who satisfied himself of their correctness. Labour proceeded steadily from nine, A. M. till five, P, M., when the membranes ruptured, and, in an hour after, the patient brought forth a living female child. The placenta was expelled by a renewal of uterine action twenty-five minutes afterwards.

"If now we analyse this perfectly natural case, according to Mr. Carmichael's views, we should expect several circumstances to exist. In the first place, it would be reasonable to infer, as indeed was afterwards verified by examination of the secundines, that the sound indicative of the presence of the placenta, having been heard at the fundus, more plainly at the right angle and feebly at the left, that substance actually was affixed in the situation thus pointed out. And yet how contrary is this to the author's assertion, that the placental murmur is never heard at the fundus, nor is the placenta ever situated there. Such being the case, then, in the second place it was to be supposed, that the uterine contractions would constringe the vessels, interrupt the function of the placenta, (and consequently destroy the life of the child,) and most probably detach it altogether, and that too 'very early in the process of parturition.' No such effects, however, were produced: the labour proceeded naturally, the infant was born alive, and no hemorrhage at any period took place.

"But supposing that the uterus could contract in the way described, (namely, by the anterior wall shrinking within itself, and making the upper part of the posterior wall first become the fundus, and afterwards amalgamate itself with the anterior paries,) without producing the ill effects anticipated, still another objection, founded upon the foregoing case, may be urged against this theory. If the uterine contractions were thus effected, it should necessarily have happened that the bruit, which was faintly heard across the fundus about an inch below its highest point, should gradually have mounted upwards; and, as labour

proceeded, and that portion of the posterior wall, to which the placenta was attached, at last assumed an anterior position, it should have become louder and louder, until it developed itself in full intensity under the instrument placed in the centre of the uterine tumour below the fundus. And the situation of this distinct murmur should from that period have descended, according as the capacity of the uterus diminished, until at length it almost arrived at the pubis. This appears a fair deduction from the author's observations. But instead of such being the facts, I most explicitly declare, that no change whatever was observable in the position of the placental souffle, nor was there any alteration in its relative intensity in the region of the uterus.

"Again, if such a revolving movement took place, as the membranes remained uninjured, until the fœtal head had been impelled deeply into the pelvis, I suspect the orifice, through which the child passed, should have shown, that at the time of their rupture, the placenta was situated on the *anterior* wall. But on the contrary, the membranes at the anterior edge of the placental mass were rather longer than at the posterior edge, and the pouch formed for the fundus was somewhat anterior to that organ; thus pointing out its situation to have been, all through labour, the same as stethoscopic examination already proved it to be."

From the foregoing cases Dr. D., thinks it obvious, that to avoid the early separation of the placenta, and its attendant consequences, it is *not* necessary that that substance should adhere to the back part of the uterus, or even to any region, in which contractions do not take place, save for the purpose of detaching its own connection.

"But, furthermore," he maintains, "that no such contraction, as that for which Mr. Carmichael contends, could by any possibility be effected by such structures as, anatomy shows us, alone exist in the genital organs. The only resemblance in the body to such a rotatory movement, is the pulley-like contraction of certain muscles, such as the digastric, the obliquus superior oculi, the circumflexus palati, &c. In all these, there are necessarily present, at least, one strong attachment to a bony structure, which, during the action of the muscle, acts as a fixed point, and a collar in which the muscle plays, and which serves to retain it in its proper place. Where then are we to find such an arrangement in connection with the generative organs?

"The vagina, to which the uterus is attached below, during parturition, dilates and becomes more relaxed in its tissues, and could not act the part of such a firm bond of union; nor could any of the ligaments by which the womb is supported, but not fixed in its natural situation. Even the round ligaments which have been supposed, erroneously, I think, to perform the office of tendons to certain of the uterine fibres, do not take a direction that would enable them to be of any service in the newly proposed action. On the contrary, the uterine contraction must, by bringing their points of attachment nearer to each other, prevent them from giving any fixity to the organ. Where then are we to seek the point around which the fundus turns, and without which, such a partial contraction of the uterus must, instead of producing a revolving movement in that part, draw it directly downwards, and cause the convex fundus to assume a flattened form?

"The author attempts to supply this deficiency, by assigning to the fœtal body the office of a fulcrum. I am not prepared to deny that such perhaps might be the case, if the remainder of his theory were correct; but I would inquire, what fulcrum can there be, where the uterus, having expelled the child, again 'relaxes completely,' as in the case which that author brings forward, as the fifth instance where he found the placenta on the posterior wall? What prevents us in such a case, when causing contraction by external pressure or the application of cold, from feeling the fundus grow flat under our hand. How is the rounded prominent appearance of the fundus maintained?

"Such are the considerations which, to my mind, throw a doubt upon the validity of the theory proposed by Mr. Carmichael. The subject of the contraction of the uterus, and the mode in which the placenta maintains its adhesions

undisturbed, and its functions unextinguished, during the uterine efforts, are certainly involved in great obscurity, and the explanation offered by that gentleman (obviously the result of deep thought and extensive research on this curious subject) carries with it such apparent truth, that it *deservedly* excited great attention in the profession. But I trust I have demonstrated the incorrectness of his premises, and the fallaciousness of his conclusions, with respect to these points. If I have succeeded in doing so, I need scarcely allude to his theory of the development of the uterus. If the assumed mode of contraction be not the true one, then there is no necessity for imagining its growth to be confined almost entirely to the anterior wall. The old doctrine, indeed, that all the parts of the womb enlarge, holding the same relative position to each other, but being allowed a certain latitude in the degree and period of their expansion, affords, I think, a much more ready solution of well known facts connected with gestation. It accords with the different forms the uterus assumes at the several stages of pregnancy; its being first pyriform, then oval in consequence of the increase of its transverse diameter at its central and lower part, and becoming at last globular when the cervix has also dilated. It explains too the phenomena observable in placental presentations, in which the hemorrhage, consequent on expansion of the uterine parietes, occurring where the placenta has not the power of accommodating itself to the change, in general takes place almost entirely in the three last months."

43. *Case of complete Detachment of the Os Uteri.* By Hugh Carmichael, Esq.—"Late one evening in the course of last autumn, I was requested to visit a young unmarried female, who, I was informed, was about to be confined of her first child. On my arrival I learned that about an hour previous to my being sent for, she was from home, and when at some distance from it, the waters, as it is termed, broke, and that before she could reach her residence they had been almost all discharged. On making an examination, I found the os uteri sufficiently dilated to admit the point of my finger, but thin and hard; the pains slight, but regular. She continued in this state the entire of the next day, the following night, and a part of the ensuing day; the pains at no time increasing beyond those of the first stage of parturition. During this period, though the pains were insufficient, nevertheless, the head progressed, the os dilating but very slowly, until the dilatation became about the size of a crown piece, beyond which it did not extend, its edges still continuing hard and rimmy. There was no deformity of the pelvis. Considering that the obstinacy which the os exhibited might probably result from the insufficiency of the pains, I determined on inducing them, if possible, to a certain extent, and with that view, on the second day, administered the *ergot of rye* in such doses as to throw the uterus rather upon the tension, than induce the strong uterine contractions that follow its full doses: I gave five grains of the ergot, and in about ten minutes afterwards evidently perceived the uterus slightly ergotised. Considering the obstinacy of the os, I contented myself with carefully watching the continuation of the action of the ergot upon the uterus, and when it began to abate, repeated it in the same dose; this interval was in or about half an hour. In this way *three* doses of the ergot were given, and although I had the uterus so ergotised, that under ordinary circumstances, the os must have given way, (dilated), it still continued to resist the contractions of that viscus. I should have observed, that by this time, in consequence of the very protracted state of the labour, the patient had been much exhausted, so that interference was evidently called for. It may be said that bleeding, tartar emetic, and other relaxants should have been tried; I can only say I gave them the fullest consideration, and determined on the ergot in the way administered in preference; and I would here submit, that there are peculiarities attending sometimes particular cases, so devious from what are usually to be observed, that the treatment must be modified accordingly, in proof of which I believe I could not refer to any one more competent to give judgment on than yourself, from the very extensive practice afforded at the Coombe Lying-in Hospital. However, to resume, the os did not yield, but the head