

ART. XXIII.—*On the Position of the Placenta or Afterbirth in the Womb, during certain Stages of Gestation, as also on the Expansion of the latter Organ therein, and of its subsequent Contractions in the Process of Parturition.* By HUGH CARMICHAEL, A.M., Member of the Royal College of Surgeons in Ireland, and one of the Surgeons of the Coombe Lying-in Hospital.

HAD I been aware that my unfortunate placing of the placenta or afterbirth in the womb, would have been a source of such annoyance to any person connected with the obstetric branch of surgical knowledge, I should have quietly left the mass where I found it at the fundus or superior part of the organ, and not have insisted, in these times of reform, on an alteration of its position to the lower and posterior part thereof in the vicinity of its mouth or outlet.

Having embarked, however, in support of this new doctrine, and advocated its cause on grounds which appear to me feasible, I believe too much so for some, I confess I am unwilling to abandon my opinion, unless it be from the pressure of substantial and rational arguments; and as nothing has as yet in that way appeared, which in my mind shakes what I have put forward upon the subject, although much angry matter has been certainly written against me for so doing, I must still maintain, however at variance it may be with the wishes of others, whose equanimity I regret it evidently so much disturbs, what I have before expressed—namely, that the received opinion upon the subject, which maintains the placenta on its formation to be attached at or near the fundus of the womb, and to continue in that position throughout pregnancy *with occasional exceptions*, though it be correct in the first part of it, viz. its attachment at its first formation, nevertheless is wrong as to the other, the continuation in that locality; there being, as I stated in my first

paper, strong grounds to question the validity of this latter supposition.*

My present excuse for again obtruding on public attention, upon this already fully discussed subject, is for the purpose of shewing the untenableness of some objections that have been lately raised against it, and which appeared in the number of this Journal for March last, in the shape of a second review; I have before replied to former objections advanced in a previous number of it from the same quarter, and unfortunately so fully, that it appears to have excited a considerable degree of exasperation against me; and I think now I shall be able also to prove these additional arguments to be equally inapplicable to the matter as those already set aside. Like them, they consist partly in persisting in the misrepresentation of passages of mine, which I have already fully shewn in my last paper admitted of no such construction, as that put upon them by the first review; insisting that I have said what has been stated in it, and that the objections, based upon such explanations of my words, are perfectly justifiable; while others go to controvert the grounds upon which I rested my line of argument, and are attempted to be sustained by abstracts from the writings of those, who have heretofore treated upon these matters, but which I think will be found in no way to support the reasoning for which they are brought forward.

This second review commences by giving me *the lie*; somewhat covertly, but still giving *the lie*.

It begins by informing us that its writer is an unequivocal professional man and gentleman, and the word of honour is solemnly pledged in the declaration of it. I was glad of this, because where *the pledge* was taken, we might now-a-days expect *temper-ance*. However, it would seem that lest any doubt might exist respecting it, and in order to

* Dublin Medical Journal, vol. xiv. p. 452.

insure belief, its truth is at once vouched for, by cautiously bandying *the lie*.

This gentleman further informs us, that it is not the first time *the lie* has been so given in this controversy, which is certainly a fact—indeed it would seem to be a favourite phrase, and probably often resorted to ; with such credentials, therefore, before us, we shall not dispute the character laid claim to in this paragraph, by any individual who could encourage or sanction such an elegant style of defence ?

I shall be very concise and moderate in my notice of this gentleman's phrase. Of all things I shall avoid personalities ; they are low and coarse ; the resource only of the intemperate and the vulgar ; or sometimes resorted to by inconsiderate aspirants, who, having rushed into a subject wherein it is found insufficiency alone has been exhibited ; hope, by such little tact, to turn the disputation into personal offensiveness, and thus escape from a further display of an already too much exposed ignorance. I shall therefore dismiss the matter, lest by any chance they might in some shape here intrude ; they should be left altogether to those to whose taste they may be congenial, and who may boast of them as weapons they are in the habit of resorting to when argument or temper fails them.

The excuse for again calling in the seasonable aid of this language in the second review is, that, in my last paper, I used words and made charges against the Master and officers of the Britain-street Hospital, calculated to convey impressions injurious to them ; it is not at all in the quarrel of the gentleman himself, but as the champion of these individuals.

Now, what are the phrases injurious to them, and conveying the idea that I attributed to them a participation in the framing of these reviews, which is another charge ? Why, in the first instance, it appears, I did not choose to print one person's name, or indeed notice it at all in my last paper ; and next, I used the words, " a review from the Britain-street Hospital,"

and also the words, "the Britain-street Hospital critics:" this is the amount of the charges.

With respect to the first of them, then, I have only to say, that if I be so dull or insensible to my own fame, as not to couple the name of any person with whatever little tract I might obtrude on public attention; the great loss must be all my own; and I do not think I am to be reprimanded for it, but particularly in the way adopted, and the more so by the person who is so angry with me for not printing his name. Next, as to the charges respecting the Britain-street Hospital; I should like to know, where *did* the first review come from; yes, and the second one too, if not from that hospital? Did not the *first* review, in the heading of it, state the writer to be the Clinical Clerk of the Britain-street Hospital, and also the Secretary to the Obstetric Society belonging to that Hospital? Did it not declare in the very commencement of it, that on the appearance of my paper, the vast field that hospital afforded, was availed of for the purpose of testing the views I then put forward? Were not all the cases, with the exception of one, cases from the Britain-street Hospital? Is not the celebrated table of statistics, that appears, in this last review, got up at the Britain-street Hospital? And lastly, though this grave charge is laid against me, of offering such offence to that hospital and its officers, as that of attributing these reviews to it, as an excuse for the neat specimen of breeding that called forth these remarks; was not the philippic in the Medical Press, where *the lie* was bandied before, absolutely dated from *the Dublin Lying-in Hospital*.

All these facts, with the exception of the statistics, were before me when I wrote my last paper; were they or were they not sufficient to warrant me in concluding the review came from the Britain-street Hospital, particularly the last, which was dated from that hospital; or under the circumstances of the case, even admitting it to be injurious to the character of that hospital to suppose reviews like these came from it; was the state of

the question such as to warrant such a departure from (I shall, warned by the example before me, only call it) the courtesy usual in a matter of this description, or justify the handsome expression *false*, in the manner it was resorted to?

If there be anything necessary to establish the justness of this reasoning on my part, and the conviction on that of the review, that there were every grounds for my so thinking and so expressing myself, it is the clever little alteration which has taken place in the heading of the *last* review; for while in the *first*, the Clinical Clerk of the Dublin Lying-in Hospital and the Secretary to the Obstetric Society of that hospital was coupled with its heading; all these titles are now studiously laid aside in the *second*, and it is brought forward in the plain capacity of coming from an M.D. Why have all these titles been laid aside in the *last* review? the reason is obvious; they, with other matters stated, fixed these papers as coming from that hospital, and warranted my expression in the above particular; and they must be omitted here in order to qualify the disclaimer on the part of it in the very elegant style in which it has been done—the more particularly when it is declared, that some of its officers were altogether ignorant of my paper, and also ignorant of the nature of the criticism brought forward against it in the Obstetric Society; although I believe that *all its officers* were present the night that critique was read in that society; and one of them was so attentive on the occasion, that he absolutely was the person who adjusted a placenta brought forward there by a gentleman for the purpose of elucidating my opinion upon the subject.—I should like to know which of them that person was?

Now with respect to my using the word “critics” instead of “critic,” I shall just shortly call attention to the circumstances of the sixth case, mentioned in the first review, which, by the way, was reserved in that review for the last, the conclusive settler of my theory; but which I think has been quite settled in my last paper, so much so, that although triumphantly reiterated in

the reply against me in the Medical Press *as unanswerable*, it has been very wisely let drop in this second review, and nothing more said about it. In this case we are told, that the placental murmur first faintly existed at the fundus, and the observers at the Britain-street Hospital appear to have been quite certain an unanswerable argument against me was just about to get birth in it. The impression with them was, that as labour advanced (if my theory was correct) the murmur must have risen to the fundus; have existed there in full intensity, for a time, and then have descended down to very near the pubis, carrying the fundus throughout with it, and that before the child's head was born; inasmuch as it is then only the soufflé ceases; whereas the review positively averred, no such change occurred.* Now who, not was, but were, around this case, *confessedly* for the purpose of criticising my theory by it? why, the reviewer, the Assistant Physician of the Hospital, and a parcel of its pupils; all were engaged watching it with this view, and on the very score of criticism. At least, so the copy of this Journal for July last (page 359) which fell into my hands has it, and which contains the review in question;† need I go farther then, in order to shew, that I was warranted in saying, first “a review from the Britain-street Hospital,” and next, “the Britain-street

* See the Number of this Journal for July 1839, p. 359.

† The sad mistake these gentlemen committed, for I *must* speak of them in the plural number, and which led to the great waste of their time over this case, was, their being unaware of a fact I informed them upon in my last paper; namely, that the fundus, in labour, so far from going down to near the pubis, does not descend one inch, until the head of the child is being born, when the placental murmur then usually ceases; with all the feeling which pervades the last review, this piece of information to them has been alluded to, stating it as, “a well known fact which Mr. Carmichael has just discovered.” I can only say, if it be “a well known fact,” so much the more it shews the little degree of information respecting it, possessed by these critics at the Britain-street Hospital, before I told it to them; and so much the more astray must they appear, watching over this case, for the occurrence of a phenomenon which is now admitted to be totally at variance with a generally known circumstance.

Hospital critics." I cannot see any thing in the expressions, under the above circumstances, which should excite such sensibility upon this point, morbid as that sensibility evidently is, unless that it gave a pretext for reiterating the expression, which appears to be so peculiarly a favourite in this discussion, and particularly when it was to be applied to a quarter, that has pointed out such little mistakes, both practically and physiologically, on the part of papers intended as no less *than reviews*, and which certainly it would be just as well for their character, as authority, in that respect, that they had not been brought to light.

Having thus, I hope satisfactorily, explained the circumstances connected with the use of the handsome prefatory paragraph, with which the second review commences, I shall now proceed to the consideration of its merits.

Were I to follow the objections contained in it, and answer *seriatim*, such of them as at once admitted of being so, I should be compelled to go through, and reply to every page of it ; it is only necessary to read it over, in order to see, by its style, its object, and the feeling with which it is constructed, displeasure and irritation are breathed throughout ; and we must expect therefore, like most things of the kind, where feeling predominates so much, that sober discussion with candid and fair investigation, will be the least of its ingredients ; I shall confine myself, however, to the most important points connected with my subject, and do it in as concise a manner as possible.

It commences, as might be expected, with an endeavour to exonerate the first review from the discredit of the misrepresentations it contained, and upon which it was based ; but in its solicitude to effect this, it only gets deeper into mire it labours to be freed from.

In order to this, it states at the outset, that in my reply to the first review, I rested my defence solely on *words*, not on their *meaning* ; that what that review attributed to me as the *meaning* of certain passages in my paper, I did not deny ; only

said, such *words* were not distinctly *written* by me ; that I did not dare to dispute it.

First then as to this, I have to remark, that so very palpable were the misrepresentations contained in that review, it would be only necessary to state them and the passages they purported to interpret, in order to point out their unfairness ; and in support thereof, I beg to refer to my last paper pages 219, 20, 21, 22, Dublin Medical Journal, November, 1839, where the original passages, and those distorting them, are set out, and placed together in juxta position ; but independent of this, this second review, so sensitive in the cause of truth that it does not hesitate in the quarrel of others, to tell me my statements respecting *them* are *false*, is certainly very oblivious in making this assertion about me ; as I think I can shew.

The chief thing the first review laboured to fix upon me, and which also the second one is now equally anxious to establish, and says is no misrepresentation, was, that I asserted, if the placenta were affixed to *any other part* of the womb but the lower and back part, (as the anterior wall,) that detachment and *hæmorrhage* would occur during labour, and that, *before the birth of the child* ; all its cases went to this ; indeed we are now informed by the second review, that it only intended such a meaning could be taken out of my words ; even so, let us see whether I did not argue this, not as to the *words* but *meaning*.

First, however, I must quote the passage from the first review charging me with this statement ; “ but laying aside theory,” it says, “ let us inquire is Mr. Carmichael correct in his position, that the placenta in natural pregnancies 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 foetus, cause a *premature detachment* and *consequent hæmorrhage*. These propositions I shall consider together, and I submit that if I succeed in bringing forward cases in which the placenta was *otherwise* affixed, and yet *no hæmorrhage* oc-

curred *before the birth of the child*, I shall have gone a great way in sapping the foundation of the author's interesting but fallacious theory," &c.* A number of cases were then brought forward where the placenta was on the *anterior* wall, and which ended favourably.

Now, whether I replied to this as to its *words* only, and not against its *meaning*, the following passage in my last paper will point out, and to which the second review was a reply. After showing what moonshine the gentlemen round the sixth case were looking for, from their being unaware of the fact that the fundus does not descend in labour until the head of the child is being born, I thus continue: "How then could detachment with hæmorrhage occur up to the resting of the head upon the perineum, if the superficies of the organ diminish in no remarkable degree? and after that, if detachment did take place, it must be by permanent uterine contraction; and how could hæmorrhage then occur; or how could any person aware of this fact (the well known one) ever dream, much less seriously speak of it (*hæmorrhage*)? Yet this is one of the questions gravely asked in these cases by this review from the Britain-street Hospital, and read before the Obstetric Society."†

I believe this was arguing as to meaning, and very decidedly so too; yes, and probably far beyond what was agreeable, inasmuch as it showed what want of knowledge existed in the wards at Britain-street, of what the review now tells us is "a well known fact." So far then as to detachment and hæmorrhage, if the placenta be on the anterior wall.

Another thing the review charged me with was, that I maintained, extinction of the placental function early in the labour, and ultimately a still birth, must ensue, if the placenta be otherwise affixed (on the anterior wall) than as I placed it;

* Dublin Journal of Medical Science, July, 1839, p. 353.

† Dublin Journal of Medical Science, November, 1839, p. 231.

on the grounds, as it now turns out from the second review, that if the expansions and contractions be greatest on the anterior wall, what I contended for would happen at the fundus, must doubly so there ; without, however, taking into consideration the supposed different nature of the contractions under dispute.

In the Medical Press, however, No. 32, p. 90, I argued against this, and likewise against hæmorrhage as above, and showed there, that the condition of the placenta upon the anterior wall was quite different from it on the fundus, regard being had to the two theories of contraction ; so that what could take place (extinction of the placental function) at the fundus, would be avoided on the anterior wall. I there showed, that if two unequal circular surfaces (the placental and the fundal) were in juxta position ; and if while the smaller of these (the placental) remained fixed, the larger (the fundal) shrunk in and lessened from all points of its circumference to its centre, as is the present doctrine of the fundal contraction ; that the change between these surfaces would then be decidedly effective, and so as to detach each other ; whereas if these surfaces were so circumstanced, that the shrinking in of the larger (now the anterior wall) only took place at one end of it, the other end remaining at rest, the entire of the smaller surface would thereby be moveable on it, the larger, which would not be so in the case of the first state of things ; would rise and fall with the motions of the larger surface, and thus escape from the effects inevitable in its first or fixed condition ; the one, in fact, may be denominated centripetal, the other rectilinear contractions. This was my line of argument in the Press, to show how differently the placenta would be affected by the supposed contractions of the fundus and those I advocated on the anterior wall, and I elucidated it by a diagram. Thus I submit the statement of this truth-loving review, averring that I took up my stand only upon the *words*, not their *meaning*, was certainly imbued, very much indeed, with obliviousness.

So far with respect to the two first parts of this explanation of my meaning, viz., hæmorrhage during labour, or extinction of its function when the placenta was any where except where I put it; and now to that which relates to hæmorrhage and detachment of it, *the child fully in the womb*. That I argued against this as to meaning, not words, I can best and most briefly show by my remarks in the Press, No. 32, p. 90, on the second case of the first review.—“ I have only to avow (I there said) that in all the cases, not one had the placenta at the fundus. I shall select the second as a specimen of all the rest,—Bridget Nicholson, a patient in the Britain-street Hospital, whose pelvis was undersized, and, as I suppose, considerably so, inasmuch as she was obliged afterwards to be delivered with instruments, no doubt the crotchet, because three doses of ergot of rye (thirty grains each) were given her, and by its influence, powerful, uninterrupted uterine action was brought on without inducing delivery. The conclusive question, however, is asked; why, when such powerful uterine action was induced by the ergot, was not the placenta (on the anterior wall) *detached with hæmorrhage*? The answer is simple,—simple as possible. Let us recollect that this powerful uterine action was induced by large doses of the ergot in a case of undersized pelvis, and the answer is, in order to detach a placenta in that situation, by uterine contraction, the uterus must diminish in surface to a certain extent; but when the pelvis was so undersized, that powerful uninterrupted contraction, induced by the ergot, could not overcome it, the presence of the child in the womb mechanically prevented the necessary superficial diminution for that purpose. In my mind, the only chance of bleeding in such a case would be, from rupture of the womb, in consequence of powerful, unavailing uterine action being induced over a misshapen pelvis.”

I believe these remarks sufficiently shew, that I not only denied the passages attributed to me, to be mine, but also argued on the unfairness of giving them, by way of explanation of those of mine they were intended for. I could give several others,

in support of this assertion, and which are to be found throughout my defence, but it would be only unnecessarily encroaching on the pages of this Journal; these are sufficient to correct this little mistake, on the part of the second review; but I cannot help, however, remarking, that it is certainly rather strange, these reviews, so sensitive in the cause of truth, as to be so frequently ready with the word "*false*," should absolutely start with an assertion of this kind in the face of the above already printed documents. Such random, wildness of argument, is, as I observed, only plunging deeper into the misrepresentation it was so anxiously endeavouring to escape out of.

The most extraordinary part of the above passage from the review is, the looking, at labour, for detachment of the placenta from the anterior wall of the womb, *while the child fully occupied it*; nevertheless so decidedly impressed with that opinion does it appear to have been, before entering upon this discussion, that it had recourse in its answer in the Medical Press, No. 32, p. 123, to a piece of sarcasm, by way of ridiculing me for thinking such a thing was not quite usual, and rational, and sound doctrine. It there observes: "He (Mr. Carmichael) adds, that a nine months' child is no trifle in a womb, and always manifests the greatest unwillingness *whilst here*, to allow the lateral walls of its chamber, anterior or otherwise, to lessen their superficial extent sufficiently for that purpose, [to admit of the detachment of the placenta,] or give way before the contractile tendency. I admit—I freely admit, that a full grown child, particularly in Ireland, is no trifle in a womb; but though I have heard of 'mares' nests' and other curious phenomena, *I never till now 'knew'* that a nine months' child owed to its shoulder or its bottom, the maintenance of its vitality *in utero*. Pray at what period of gestation is it gifted with such precociousness?" This certainly is very strange doctrine coming from—a REVIEW !!!

The above, I believe, is quite sufficient, as to the quantum of credit to be attached to the opening argument of the second review; and now to the next succeeding one.

It next goes on to a matter upon which it is quite satisfied, sure of defeating me, and introduces it to notice, with the expression, that it is one which Mr. Carmichael will be *constrained* to admit :*—confident language this. Now what would the review think, if the *constraint* should turn out to be all on the other side, and that I should force it, on those future occasions with which I am threatened, to observe the same very prudent forbearance with regard to alluding to, or even hinting at this matter, which is now so sensibly done, respecting the formerly destructive sixth case—drop it and say no more about it ?

This point on which I am so decidedly to be vanquished, is nothing less than to prove, and on my own very shewing, that even if the fundus be eleven inches, in its cross measurement, and the placenta but six, the latter will outgrow the former, and thereby how wrong I was in saying, the fundus would outgrow it.

Before, however, I proceed with its consideration, I must first trespass a little on attention, in order to shew the manner this point was handled in the first review, and the manœuvre with which it was tried to be settled against me ; and I think the admission made respecting it, in the second review, when explained, will tend very much towards pointing out the object of these papers, and the quantum of dependance that is to be placed generally upon them.

It is quite obvious, that when I spoke of the comparative growth of the placenta and fundus, in my original paper, I meant their superficial growths, as a reference to my statement will shew : “ now if the placenta be situated, where such expansion is going on, (the fundus,) a change of *surface* must to a greater or less extent be constantly occurring between the two. The effect is, as the uterus increases in *superficies*, the connexion between it and the placenta must be disturbed, and probably to some extent torn, &c.†

* Dublin Journal of Medical Science for March, 1840, p. 6.

† Dublin Journal of Medical Science, p. 453, January, 1839.

The first review, however, undertook to disprove this assertion of mine, and entered into a calculation to shew, that so far from my statement being the fact, the placenta would outgrow the fundus.

In order to this it stated, that the fundus increased *but five inches*; whereas the placenta being admitted to be six, the consequence obviously was, that I was quite wrong in my position; and in support of the five-inch fundal measurement, Burns was quoted as an authority, though I could find nothing of the kind in that work, at least at the page (49) to which a reference was made.

In my last paper I of course noticed this misstatement as to Burns, and, as I thought, error in the *superficial* increase of the fundus, and shewed that instead of five inches, its measurement across is eleven. What does it turn out then that the first review had recourse to, in order to put down my statement as to these comparative measurements, and which in this second one it is *constrained* to admit? why that the review, in its argument upon it, endeavoured to slip in or substitute the *height* of the fundus (five inches) for its *superficial* measurement (eleven inches) in the calculation, and thus at all events prove Mr. Carmichael was (*ought to be*?) wrong. This confession, so highly creditable to its candour, comes out in the second review, p. 6: "This calculation (that of the first review) Mr. Carmichael objects to, and confounding what I have said of the increased *altitude* of this part, (the fundus,) with its increase in *breadth*, states the proportions, &c."

Now I would beg to ask, how is such a manœuvre as this to be designated? Is this the candid, the *veracious* review; so sensitive in the cause of truth, as to call forth from it the term "false" so repeatedly and so handsomely in these its criticisms? Reviews "that would be so much pained if any gentleman would suppose them guilty of intentionally misrepresenting him; that declare themselves *utterly incapable* of distorting the sentiments of any writer to cast ridicule and reproach on him; that would not *wittingly* seek out a *devious*

path to *strain* the author's meaning, but follow the *straight* and open course which even the *most cautious* would probably pursue."* Is this fine character, I say, the second review gives us of itself and its consort, thus sustained by an endeavour to substitute the *depth* of a part for its *superficial* measurement, in comparing that part with the superficial measurement of another, and where the superficial measurement of both was absolutely stated by the author it volunteers to criticize and purports to hold up to ridicule? This certainly is rather at variance with what might be expected from such a quarter; such highness of mind, of sentiment; and looks *rather* like a desire for victory on the old principle, that *all is fair in war*, than candid investigation of what it undertook so *cautiously* and *unwittingly to review*; an attempt to accommodate matters, not to meet the justice of the case, but to answer an object to be attained.

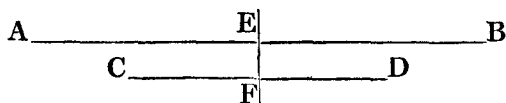
Let us now, however, consider the new grounds taken up by the second review to disprove my original statement in this particular; where I am to be *constrained* into an admission; and I am very much disposed to think, we shall silence it as effectually upon this *constraining* calculation; shew it entitled to just as much credit as in either the celebrated sixth case; the statement that I disputed about words not meaning; or its late *compound-alto-superficial-fundal-measurement attempt*.

The review now admits the measurement across the fundus to be eleven inches, and goes into a comparative calculation between the parts upon this admission. It states I committed a great mistake in not taking into consideration the extent of the fundus between the tubes before the placenta was formed first; this it states to be two inches, therefore the increase of the fundus is as 2 to 11, which being reduced to the one-inch calculation (one-half?) is as 1 to $5\frac{1}{2}$; but as the placenta (the two-halves?) is as 1 to 6, the latter is thus *constrained* to

* Second Review, Dublin Journal, March, 1840, pp. 40, 41.

outgrow the former in the proportion that 1 to $5\frac{1}{2}$, is to 1 to 6. This is *mighty* clever indeed!

The calculation here is very ingenious no doubt, and might, to a careless reader, mystify the matter so completely as to set him altogether astray in it. But it happens to be all on one side, and therefore it wont stand; the best way to elucidate both the error and the real state of the case is by a little diagram; we are here supposing the placenta to be at the fundus of the womb, and the expansion to be going on there, not as I contend, but as is contended for in the review, for the review promises to *constrain* me even on my own shewing.



Let A, B, then, represent the surface of the *fully* expanded fundus; C, D that of the *fully* expanded placenta; E, F the centre of each; now A, E, the *half* of the fully expanded fundus, no doubt has increased in a proportion of 1 to $5\frac{1}{2}$, being the *half* of 2 to 11, which A, B represents; but as that A, E, is only applied to so much of the placenta as will be contained under C, F, it should be compared in the calculation only with the latter; and the comparison would then stand thus: 1 to $5\frac{1}{2}$, for the increase of half the fundus; and 1 to 3, for that of half the placenta; or 2 to 11 for the entire of the fundus; and 2 to 6 for the entire of the placenta; or if the review likes, all 6 for the placenta. The truth is, the first review being foiled upon the plan adopted in it on this important part of the subject, now tries another, equally untenable, though not so palpably unfair; this is too serious a matter to be easily given up; and that is, by a little dexterity in figuring, to compare *half* the expansion of the fundus with the *entire* of that of the placenta; this, however, would be, as I have said,

to have the matter all on one side ; I object to it, therefore, as I wish things to be *straight* forward, however this may please. What anxiety these plans evince to crush my paper, or even the consideration or investigation of it !

But let us see how these *surfaces* in truth stand, relatively, with respect to each other, and which has not yet been done. The true way to do so is, to view them in their total superficial measurement, not rectilinearly, as has been heretofore the plan. Let us then take eleven inches as the measurement between the tubes, and say ten for the antero-posterior ; these multiplied into each other give the square superficial measurement, or extent to which the fundus has expanded, viz. 120 square inches ; let the review take from this its two inches both ways, which will give four square inches, and we have then 116 square inches to represent the fully expanded fundus. The placenta being averaged at six inches both ways, gives thirty-six square inches for it, all of which we may give to the review without any drawback ;—thus then we have 116 to represent the fundus, and 36 the placenta. I hope the review is now satisfied with its calculations ; in future it might be prudent to observe the same sensible forbearance as regards them, as it does now about the destructive sixth case—say no more about it.

From this point the review goes on to one, upon which it is as confident of vanquishing me,—at least it argues so,—as it once was on those we have already put at the back of its books ; sixth case, compound calculation, and all : I think, however, we may show it in a different point of view from that which it is now placed in, and may even require another page of that part of its ledger for the registration of it also in.

The point is, that inasmuch as I stated the expansion goes on chiefly on the anterior wall, and to such an extent, that it not only supplies that wall, but also the fundus, and likewise so much of the posterior wall as is above the tubes ; that where such expansion takes place, not only the disturbance which I argue would happen at the fundus, according to the present

doctrine during gestation, must necessarily occur if the placenta be upon it; but also at birth, the *hæmorrhage* the review labours so much to fasten on, and if possible, to convict me of having said, or as it now turns out, implied in my paper. It states the increase of that wall, according to me, cannot be less than seventeen inches, and (p. 7) “the placenta, therefore, *ought* not accordingly to be able to grow there equally with the wall of the womb—let us consider this, however.”

In order to it then, we must view the placenta as to how it may be affected in two ways on this expanding anterior wall; first, as to its increase in breadth, and second, in its altitude. With respect to the first then, if we say the anterior wall of the uterus expands laterally, or widens to the extent of ten inches, we may probably be near the mark, if not beyond it; now, the breadth of the unimpregnated womb is supposed to be about two inches; this it has *at least* doubled at the end of the second month, (see first review, p. 352 :) “until the middle of the second month, the vessels do not implant themselves into the decidua to form the placenta. Before this period the uterus has *at least doubled* its original size;” it is therefore four inches before the placenta first forms, (at the second month;) *if* then it grows to ten inches afterwards, across or laterally, it has expanded laterally but six since the placenta was first formed, and thus the placenta in that direction can be accommodated so as to grow without disturbance, keeping pace exactly with the growth of the womb. So much for the growth in the lateral direction.

But the principal point the review relies on for defeating me on this question is, the expansion of the womb in its *altitudinal* direction, and which being seventeen inches, as it states, according to my view of the matter, must thus render it impossible for a placenta, growing but to six inches, to be free from disturbance there during gestation, and at birth to be uncomplicated with its favourite *hæmorrhage*. The latter part of this we have already despatched, even with the assistance of *the child*

in utero, in a foregoing part of our inquiry; it is therefore unnecessary to repeat it here, and I think I can very briefly explain how the former also can be avoided, namely, the placenta grow to maturity without disturbance, though on a surface expanding to seventeen inches. Three lines will be sufficient for that purpose. Well; whatever part of the anterior wall the placenta forms upon, must, as it rises, carry up the placenta along with it, the mass growing upon it as it rises. If, indeed, the placenta were formed at or upon a given point of the anterior wall, and that that point were fixed, the growth expanding from it upwards and downwards to the extent of seventeen inches, eight and a half on each side, then truly might the review defeat me altogether on the matter. It would be just the description of growth I contend against, as that which is supposed to go on at the fundus,—just as absurd, just as irreconcilable to reason; and one or two cases with the placenta anteriorly, terminating favourably, must be conclusive against me; but such an expansion is clearly one thing, and its rising only another; in the former, a manifest absurdity must attach to my doctrine, but in the latter, it is perfectly rational and compatible. No doubt, *occasionally*, the placenta may not be accommodated where this great expansion goes on, though generally it can; and such an occurrence may and can “still further support the hint Mr. Carmichael has lately thrown out, that some of the cases of abortion which occur without any assignable cause, at certain periods of gestation, may owe their cause to some such malposition.”*

In fact, all these points were substantially and fully answered before; they, with others, were not only such an unfair representation either of my statements, or of any meaning that could be put upon them, but involved, as I have said, such inconsistency on the part of those who could seriously adopt them, that they led to the ridiculing of them in the Medical Press,

* Review the second, p. 7.

which I can well conceive to have been so very disagreeable to reviews, whose great obstetric mistakes were there so fully and ludicrously pointed out.

In the argument on these points in the second review, the great difference between radiating expansion and that in a simple rectilinear direction ; and centripetal contraction, and that where one part is fixed, the other alone moveable, and which I particularly dwelled upon in my former replies, is very prudently kept out of view ; although from the use of the term "rectilinear contractions," in one place, p. 9, the difference was well understood.

During the discussion of this topic, we are informed, "one of the strongest objections the review has to my paper is, that I not only pass over the *possibility* of the placenta being *otherwise* affixed than at the posterior wall, but 'that I positively assert' it is no where else." In my original paper, however, I stated two cases where it was *otherwise affixed*, Denman's and Mr. Porter's ; and in my last, which the second review purports to answer, I speak of where it was found, in hour-glass contractions, gritty placentas, retained placentas, and such like ; the *hint* also respecting abortions, so convenient in other places as it was thought, is here quite forgotten. When reviews, however, are strongly actuated with the desire of extinguishing a writer, it is extraordinary how very insufficient the memory becomes, notwithstanding high and exalted feeling.

As a proof, however, of my opinion being, that the placenta was *positively no where* to be met but low on the posterior wall, the following passage is quoted from my paper : "In what way can we possibly account for the uterine contractions taking place up to almost the very last act of labour *without the circulation being impeded*, so essential to the life of the child ; clearly on the grounds alone, that the part where the placenta is attached, the back of the womb, not only is not submitted to *these* contractions during labour until after the birth of the child, but is likewise by some means prevented, from the effects *which the contractions in*

other parts would have on it, ('compression,') without some such means being in question."

If the passage, however, *immediately* preceding this and in connexion with it, be read in my paper, it will give it a meaning different from that which it bears according to this second review, (p. 473.) It will be seen by it, that when I so spoke, with reference to *the contractions in other parts*, it was with reference to those supposed to exist at the fundus, and to the opinion of certain French authors as to the placental function being, in consequence of them, extinguished (the compression of the above passage) long before the birth of the child. That immediately preceding passage is as follows:—"From these facts, then, I conclude, that the statements of these authors (the French) are erroneous, and consequently that it is incorrect to imagine the contractions of the womb go on, *as at present supposed*, (the fundal,) upon the authority of which these statements (about compression) are made." Garbled and partial abstracts are within the reach of all. "Such statements, however, are unfair, and bad indeed must be the cause that requires their aid."

From this the review proceeds, with an endeavour to escape from the sad want of obstetric information evinced in the first one, by looking for detachment and hæmorrhage from a placenta on the anterior wall, *the child still fully in the womb*; and with which I charged it in my replies, and particularly that in the Medical Press. The passage quoted, however, above, from that Journal, in answer to my first remonstrance on this point, is too explanatory upon that part of its doctrine to admit of any misunderstanding about it; and shows too clearly how fully impressed with the supposed truth of it the reviewer then was, to be got rid of now in any other way except by a candid declaration, that "the well known fact" respecting it was altogether unknown *then* in the quarter from whence the review came. The dilemma—is too difficult to be escaped from by what is here resorted to. The second review indeed states, (p. 9,)

that *I* asserted in my last paper, *I* never meant hæmorrhage while the child was *fully in utero*. If *I* did not mean it, I know *who did*; and detachment of the placenta too; and who I told was quite astray in so doing; yes, and who took a long time to think of it before the glaring absurdity was discovered. By way of fastening this mistake in some way also on me—getting company in its errors—the passage from my paper is quoted where I spoke of hæmorrhage, “when the placenta was high on the posterior wall;” but though I spoke then of *early contractions*, they were not while the child was in the womb, because (again the review *forgets*) the contractions are all then *on the anterior wall*. The mistake, when discovered, must certainly have been most vexatious, and unquestionably nothing should be left untried to shift it.

Here ends the defence of the first review; the endeavour to exonerate it from its misrepresentations; with what success, however, general opinion will decide; and now the second review goes to prove, that the two propositions upon which I base my theory of expansion and contraction of the womb are both wrong.

The readers of this Journal will recollect the first of these propositions to be; that the placenta, on its formation, is affixed at the fundus *or somewhere near it*; and the other, that at the close of pregnancy, it is low down on the posterior wall. The remark in the *second* review as regards the first is as follows, page 11.

“Now with respect to its (the placenta’s) early attachment, I know not upon what he (Mr. Carmichael) founds his opinion, but that it is *erroneous*, reference to plates and preparations will fully prove.” Thus this review announces the doctrine; that the placenta is not first formed at, or near the fundus, in the vicinity of the uterine opening of the fallopian tube; that this opening is when the placenta is formed on the body of the womb, not at or near the fundus.

First, however, let us understand what *I have* said about its

early attachment, and then we shall refer to plates, and authority also. In order to this, then, I shall quote from the first review, its report of what I have said on this point, and it will serve the double purpose of both stating it, and likewise of contrasting its *then* opinion respecting it, with its angry, late declaration on the matter in the second review as just mentioned.

“ Mr. Carmichael having proved it essential to the life of the child, that an uninterrupted communication be maintained between it and the placenta; details the opinions *generally entertained* on the subject of his investigation; namely, that the placenta is, *on its formation*, attached to or near the fundus with occasional deviations.”*

Here is a statement of what I said, and also an admission that it *is the opinion generally entertained*, yet now the review is at a loss to know what in the name of wonder induced me to make such a statement; that it is altogether *erroneous*. To be sure there is a little displeasure exhibited lately; however it is only necessary for me now to shew, where in the neighbourhood of the fundus I fixed its first formation, and which the following passages, among others, from my paper will.

“ When the ovum descends into the uterus from its ovarium, it is shortly after implanted thereon, as already mentioned; and it is of course obvious, that this attaching of it must take place *nearer* to that Fallopian tube, which it traversed, than the other;”† again, “ I have stated that on its formation, the placenta is affixed at the fundus, or close to it; but it may probably be said, that it is not exactly so placed; still it must be very nearly so, near the fallopian tube.” p. 470.

This is, I believe, sufficient to mark, where I assigned its first place to be, and also that the first review (but before

* Dublin Journal of Medical Science, July 1839, p. 344.

† Dublin Journal of Medical Science, January, 1839, p. 461.

the feeling arose, which might be expected from the exposure of certain little mistakes upon obstetric subjects,) absolutely declared, *I was right in so doing* ; now then to the plates, &c.

We are referred to Velpeau ; we are not, however, told whereabouts in his work “ the many ” representations spoken of are to be seen, which controverts my first proposition ; I have only to say, I have looked carefully for them, but could find none in his “ *Ovologie Humain* : ” I beg, however, to refer to that work myself, in proof of my assertion, and to particularize the plates ; that is, as to the placenta being first *near* the fundus ; plate 2, figures 3, 4, 7 ; plate 3, figures 3, 4, the position of the heads here, in these last views, shews the ova to be reversed in them ; plate 8, figure 7 ; plate 11, figure 2 ; plate 14, plate 15. Again I refer to Cloquet’s “ *Anatomie de l’Homme et Figures*, ” plate 296, figure 1, *foetus* nine weeks ; figure 2, ditto ; figure 3, seven weeks : these plates are the same as in Sœmmerring.

Now let us hear the testimony of Velpeau as to its formation : “ *voici ce qui me semble exister ; apres avoir glissé entre la surface interne de l’utérus et la caduque, après s’être fixée sur l’organe qui doit la renfermer jusqu’à l’accouchement, la vésicule villeuse reste en contact avec lui par une de ses moitiés, tandis que l’autre déprime la membrane anhiste ; on conçoit ainsi un disque de l’ovule, qui n’est pas séparé des surfaces vivantes par une couche couenneuse, et c’est là que le placenta se développe.* ” *

Here then we have the authority of Velpeau upon the subject, who states, that when the ovum descends into the womb, one-half of it is applied to its (the womb’s) surface, while the other depresses the reflexa, and that *the former* attaches to the womb, and there forms the placenta : where, however, is that surface ? near the fallopian tube.

* *Ovologie Humain*, pp. 68, 69.

Let us now see what Burns says: "The placenta *may* be formed at any part of the uterus, but in *general* it is found *near* the fundus:" again, Meigs: "*For the most part* the ovule attaches itself to the naked surface of the womb *in the vicinity of the orifices of the fallopian tube through which it entered*, but it certainly does, in a good many instances, *move to the fundus*, or to the anterior or posterior surface of the organ, or it *may* even fall downwards into the vicinity of the upper opening of the cervix, and attach itself there."

These two last passages are absolutely quoted in the review as against me; why, I cannot say, inasmuch as they most decidedly make for my doctrine; the amount of their evidence is, that the place of attachment for the placenta is the fundus or its vicinity, and that exceptions (*occasional*) to this rule, occur sometimes anteriorly, posteriorly, or at the cervix. Will the review say the attachment at the cervix is common? yet this is one of the *exceptions* stated by Meigs.

Let us now see what the *first* review itself says upon the subject, corrected by a passage from Gardien.

"A wise arrangement must exist to prevent the ovum" (just entered the uterus) "from falling towards the mouth of the womb. *The reflexa* which does not exist in quadrupeds, in whom no such danger could arise, must have the effect of *maintaining it at the upper part of the womb*."* Again, "the hydroperione *supports the reflexa and its contents*, and *must float the embryo towards the highest point of the uterus*."† The first review then goes on to say, that the highest point is the *posterior* wall, but admits it is *in the neighbourhood of the fallopian tubes*. The words are, (p. 351 :) "In other words it (the ovum) must thus be buoyed *up to*, and kept in contact *with*, the *posterior* wall, *in the neighbourhood of the fallopian tube*, which from the position of the uterus, even at this period,

* Dublin Journal of Medical Science, July, 1839, p. 350.

† Ibid.

namely with its axis running downwards and backwards, must form its superior part." This last part, as to the effect of the obliquity of the uterus influencing its *posterior* attachment, is all conjecture on the part of the review; let us see, however, what Gardien says respecting this self-same obliquity; a good authority, I think, on obstetric subjects: "*L'obliquité de la matrice, les positions defectueuses, que prend l'enfant dans la matrice, ne reconnoissent jamais pour cause l'adhérence du placenta, sur telle ou telle région de ce viscère;*"* the obliquity of the womb can in no way influence its (the placenta's) position therein. I believe these facts shew, authority and plates also are in support of my doctrine; and that the first review likewise, in its cool moments, but before its laconic contradictions were introduced into the discussion so repeatedly, also fully agreed with me; it stated the place of its attachment to be the superior part of the womb, in the vicinity of the fallopian tube; (what else did I say?) and its mistake as to *obliquity*, Gardien fully corrects; this then is all I want. Suppose, however, the ovum first attached itself to the *posterior* part of the *upper* part of the womb, as is stated in the first review; to which of the divisions of the womb does it belong? is it the posterior part of the body? or of the neck? or that of the fundus? I have been accused of wrangling about *words*; I hope *I* am not doing it here.

The explanation given in this part of the review as to how the placenta can be found low down *on the posterior wall*, without the theory of my expansion to put it there, is certainly highly amusing; we have just seen, that according to the first review, the reflexa keeps the germ close up to the neighbourhood of the fallopian tube; (to be sure, according to the second review, that is *not near* the fundus in the second month of pregnancy;) that such in fact is its office, and as that review says, p. 351, "*ac-*

* Gardien, Tom. 11, pp. 167, 168.

cordingly it is here most authors agree, its first attachment is usually formed" in the neighbourhood of the fallopian tubes.

Well, then; it appears, when the reflexa does *not* keep it up there, but yields or grows before it, (when the case is abnormal,)* "the ovum arrives in the *lower regions* of the uterus before the embryo forms an attachment to it, and accordingly *in the lower regions* of the uterus will its attachment be found at a later period." The review then goes on to discard the "*absurd supposition*" arising from my theory: here is the explanation without such "*absurdity*."

Let us now then turn to the rebouted table of statistics with this doctrine in our mind, and try how many cases it contains, out of the hundred, in which the placenta was within four inches of the os uteri *on the posterior wall*, and which I believe must be admitted to be *low* thereon, and we shall find that they amount to forty-seven. Hence, then we must conclude, that, so far as the first part of human procreation is concerned, nature as often fails in her work, as she succeeds—rather a doubtful thing however; that this reflexa, of such importance, according to our first review, to preserve the female from the horrors and dangers of placental presentation, is, according to the second, so bungled a concern, on the part of Providence, that it misses as often as it succeeds; or at least forty-seven times out of the hundred; what a mistake, likewise, most authors have committed, "in agreeing, that at the fallopian tube its first attachment is *usually* formed:" why, Mr. Carmichael himself has not committed a greater. But this is not all; the best is still to come on. The first review tells us the womb is oblique; the *posterior* surface uppermost; "the *posterior* wall in the neighbourhood of the fallopian tubes, from the position of the uterus *even* at this period, namely, its axis running downwards and backwards, must form its *superior* boundary."† Suppose now then, that the reflexa dropped

* Second Review, p. 12.

† First Review, p. 351.

from the roof of the womb, in forty-seven times out of the hundred; where would it strike upon, and create its attachment? According to this obliquity, I should say on the *anterior* part of the *lower regions*, and I think I am pretty right there too: how is it afterwards, then, to make its angle of *reflexion*, to get back again to the posterior and upper wall, which position of it we are now speaking of?—I really am at a loss to inform the reader.

I believe the review must admit the doctrine of Gardien, as to the obliquity of the uterus; or the “*glissé*” of Velpeau, discarded in the first one, in order to explain this unwelcome posterior position; the diseased reflexa will scarcely answer; or perhaps, upon cool, temperate reflection, an explanation *might* be found in “*the absurdity*.” However until the matter is made out somehow, I fancy we may despatch this train of reasoning along with the sixth case,—the argument on words, not their meaning,—the compound alto-superficial-fundal-measurement calculation, all to the *lower regions*; where, before much time elapses, we shall likewise, I suspect, send the celebrated statistical “table of one hundred,” with all the other matters contained in this review.

On this point, then, I shall only say what I did before; namely, that if the placenta be *first* formed where the first review tells us “most authors agree its first attachment usually is,” namely, *near* the fallopian tube it traversed; which is so completely part of, or so near the fundus at that time, that how it can be denied or distinguished from it, I cannot conceive. If, in fact, the reflexa at the mouth of the tube do the office the first review admits nature intended it for, how can it get down on the *posterior* wall, so low, as we can discover it does, by the examination of the membranes? I should like to have this explained; particularly if the womb be oblique. That it does, however, exist there, at the close of gestation, is evidently the opinion of the review; and also that it came there from the fundus; from the necessity it felt of explaining it away, and

which was hazarded *in the very rational and satisfactory mode above stated*; we have an admission on it, to the extent of 47 in the 100. How also do the fallopian tubes themselves, in the unimpregnated womb at the top of it; at the cornua; how do they get down, nearly one-half on the *posterior* aspect of the fully gravid womb? * I am at a loss to know, unless my doctrine be resorted to for an explanation. Hear what Velpeau says, as to the *time* the placenta is *first* formed: “On voit donc que le placenta naît, en quelque sorte, avec l’arrivée de l’ovule dans la matrice, et non pas simplement après les deux ou trois premiers moi de la gestation;” † the placenta begins to be formed in a manner, on the arrival of the ovum in the womb. Where are the openings of the fallopian tubes then? In the *lower regions* I suppose.

But the next objection is better again than this, pp. 11, 12. “If we now look into authorities,” says the review, “they *one and all* are against Mr. C.; for they inform us, the embryo *may* connect itself to any part of the uterus.” In the first instance, who are the authorities, one and all? Why, but one,—Burns; and his opinion is in my favour; I have already given it, tantamount just to what the first review states I said upon the matter: “it is formed at the fundus *with occasional deviations*.” Further; so, because the embryo *may* connect itself to any other part of the womb, no person is to speak of it generally; in the way “*usually*” formed. Menstruation *may* go on during pregnancy, notwith-

* This assertion of mine, in my original paper, is denied by the second review, but see Hunter's plates of the profile views of the gravid womb, plate 2: also Cloquet's plate on same, plate 234, tom. v. In Hunter's plates, the progress, as it were, of the descent of the fallopian tubes, during the growth of the womb, may be traced. Plate second shews it at the full period, the tubes one-half down on the womb, and *posteriorly* considerably; plate 23 at the sixth month, the tubes one-third down; plate 27 the fifth month, the tubes one-quarter down; plate 32 the third month, the tubes nearly on the level of the fundus, the child in the womb in all. In Boivin's first plate their position on the unimpregnated womb may be seen on the level of the fundus.

† *Ovologie*, p. 69.

standing which it is said that it then ceases. What an incorrect account of that function we have then, in consequence of the error about it, in this particular. Does the review really mean, that the placenta is not formed first at or near the fundus; or does it say the fallopian tubes do not enter at or near it? One or other of these it must mean.

But the succeeding passage is still better again than those we have been considering; nay I think the best of all. It appears, in opposition to all preceding writers, I have put forward the doctrine, that the placenta is to be found at the close of gestation, low on the posterior part of the womb. How is this declaration of mine met by the review? why by quoting the statements of those very persons on the subject, whose opinion in this respect I have questioned. This is certainly a very novel way of proving the proposal of a *new* doctrine to be wrong; a most admirable method, when argument fails, and therefore very judicious here; namely, shewing it to be in opposition to *old* opinions. The genuine Irish, no doubt, have been always proverbial for adhering to good old opinions; it is said an Act of Parliament was once obliged to be passed, to compel them to desist from ploughing from horses' tails; and that it was afterwards enforced only by coercive measures; and certainly if the practice in this respect be good, and that no person ought to do or suggest any thing contrary to what is to be found in books—I admit this blow finishes me.

With what severe displeasure, however, I deserve to be visited, for having so inconsiderately, in my original paper, doubted the veracity of the placental function being suspended by compression, a length of time previous to the birth of the child; when I found the contrary opinion *already stated in books*, by such men as Baudelocque, Gardien, Capuron, and the like! how fortunate for me, that this my temerity escaped notice! what a castigation otherwise I must have received for so doing, from the reviewer from the Britain-street Hospital! A host of authors, however, are brought forward in support of

the review, "joining issue with me" for saying, "I believe since the appearance of my paper, attention has been very much drawn to the examination of the secundines, and I need not observe what tale they, *I shall not say in every instance, tell*, inasmuch as *one or two* exceptions may be found in the hundred."*

Unfortunately, however, these authors speak not of the placenta's *general position*, but rather chime in with the above passage of mine, which they are brought forward to annihilate; their testimony is, that it is *not constantly, not uniformly, not always, not invariably* fixed *one* where—what a loss there was for materials, to extinguish my absurd paper upon the placenta with; which among other things states, that the placenta, at the close of gestation, is at the posterior part of the womb, *with occasional deviations*, when only this kind of trifling, could be made out for that purpose. I should wish the reader to turn to the second review, pp. 13, 14, 15, and peruse them, with a view to this reading. To be sure among "*the highest authorities*" there mentioned, "*the brightest luminaries* that ever graced our Profession;" the REVIEW!! together with *its experience and researches on the subject*, is coupled in finishing me, on this point of *my incorrect theory*.

Against all these, however, I do put forward my solitary opinion, if they be brought forward as authority against its low posterior position; and as I know I have succeeded in proving it, and that to the satisfaction of those who have tried it extensively; always of course excepting persons, who would wish I had not been so fortunate, and who are interested in the contrary result. I must acknowledge the high compliment paid me by the review, (p. 13,) namely, that having done so "I stand *alone*, in opposition to an array of the highest authorities, and brightest luminaries of our Profession." The interesting line of argument above alluded to, will be found in review the second, pp. 13, 14, 15, 16.

* Review Second, pp. 13, 14.

Towards the close of this closely, and well maintained argument against me, it is stated, that to this host of *former* authorities on the subject, I oppose my own *four cases only*; “therefore, forsooth, his (Mr. C.’s) *MUST* be its natural position. ALL OTHER OBSERVERS ARE IN ERROR.”*

It would appear here as if the review reached the very height of its displeasure against me; and in it fell into that state of obliviousness that those under the influence of such excitement generally experience; and which we have found it in, on so many former occasions; for while it states my most absurd opinions rested on *four* cases of post mortem examinations *only*, it forgets what, in a subsequent passage, it remarks, viz., that the position of the placenta in the womb may be ascertained by observing the secundines after their delivery, and that these opinions of mine were founded upon some hundreds of such observation, and was so stated in my original paper. By way of being conclusive upon the matter, however; proving to a demonstration I was wrong in the place I allotted to it, Velpeau is quoted, who observed the position of the placenta in thirty-four cases, “*in situ*,” in females who died during pregnancy, *or recently delivered*. This is done to do away with the objection I made in my last paper, as to the relative situation of the womb being so changed *when recently delivered*, after the child is expelled, that the position the placenta occupied in it, when distended fully *before delivery*, could not be then satisfactorily ascertained by the hand in the womb, or post mortem examination. In how many of these thirty-four cases, however, was the placenta observed *before* delivery, and therefore applicable to the “*seasonable aid*,” as it is said, it gave the review? Let Velpeau himself speak: “*huit fois j’ai pu l’observer* (the placenta,) *en place après le quatrieme mois* ;”† eight times *after the fourth month*; but we are not told if any were at the *full* period, when I said it was low down; and therefore this testimony

* Review Second, p. 16.

† Ovologie Humain, p. 64.

of Velpeau's goes for nothing. Cruveilhier somewhere remarks, that women *rarely* die near their full time; we had but five cases these last six years in the practice at the Coombe; and but one could be quoted as having occurred in Britain-street, and that certainly a very unusual one; very rarely indeed met with—a female affected with consumption.

I regret I must here for the present stop, in the further consideration of this second review. To reply, however, to forty pages of a critique containing such valid objections as those we have been discussing, would occupy too much of one number of this Journal, for me to expect such indulgence; I must therefore defer the remainder of it till the next number.

(*To be continued.*)

ART. XXIV.—*Contributions to Midwifery*, No. III.—*A Case of Pregnancy complicated with a Tumour, occupying nearly the entire Pelvis.* By THOMAS EDWARD BEATTY, M.D., M.R.I.A., Consulting Accoucheur to the City of Dublin and New Lying-in Hospitals, Lecturer on Midwifery, &c.

ON the 18th of January last I was requested to visit Mrs. —, a lady of tall, erect figure, well formed, and healthy, thirty-two years of age, and pregnant for the first time. She was then in the eighth month of gestation, having menstruated last on the 17th of the previous May. She stated that up to the time of her pregnancy she had enjoyed good health; but that soon after that occurrence, she began to suffer from distressing symptoms in the pelvic viscera. The bladder became very irritable, with frequent desire to pass water, and often attended with much difficulty in discharging its contents. The expulsion of the contents of the bowels was also impeded, and an unusual degree of constipation was the result. These symptoms increased until the third month, when they had arrived at such a pitch, as to compel her to apply to a physician for assistance. After quickening, which