ART. XI.—*On the Position of the Placenta or Afterbirth in the Womb, and on the Expansion and Contractions of the latter Organ during Pregnancy and Parturition ; being a Reply to a Review from the Britain-street Lying-in Hospital, upon the Author's Opinions on these Subjects.* 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.

IN the number of the Dublin Medical and Surgical Journal for January last, I submitted a paper to the Profession, containing certain reflections which occurred to me respecting the present received opinion as to the manner the afterbirth was disposed of in the womb during gestation and parturition ; and which reflections, as I then observed, impressed me very much with the idea, that strong grounds existed for questioning the truth of our being correctly informed in these particulars.

I stated in that communication, that with such impressions upon my mind, I was led, in several opportunities afforded me at the Coombe Lying-in Hospital, to investigate this matter ; and that in a series of dissections that presented themselves at that establishment and in its neighbourhood, of females who had died at or near parturition, I found the placenta, not at the fundus, as now described, *but low down on the posterior wall.*

The suggestion that its position in these cases gave, naturally led me to further investigation, and the result, as it appeared to me, went entirely to establish the fact, that the posterior wall of the womb, low down, was the true position of the organ towards the close of gestation.

The museums of Dublin, so far as I could reach them, all, in their preparations on the subject, bore testimony in its favour ; the stethoscope *never*. Here let me observe, that when I said *never*, I said it in that sense which every candid mind would take it in, and I am certain there are not three individuals in

the Profession, would seek to pin upon its literal acceptation the success or downfall of my reasoning upon the subject, or think I meant it strictly in that sense. I shall correct the expression, however, to accommodate all: the stethoscope then, in an average of at least 98 out of 100 cases, testified against the fundal situation, and pointed out its real one to be somewhere in the vicinity of the iliac fossæ; and above all, the examination of the secundines, carefully delivered, in a series of not less than 300 cases, within a period of about three months, at the Coombe Lying-in Hospital, with exceptions not greater than as above stated, told the same tale. Such were the evidences upon which I went, and such were the proofs, or I shall say supposed proofs that led me, perhaps prematurely and unadvisedly, to come to the above conclusion.

I confess, however, I was induced to think I had made out a case, and more than a *prima facie* case, to warrant me in doing so; and being impressed with the opinion, that from these facts, the lower part of the womb, posteriorly, was proved to be its proper position towards the *close* of gestation, and the fundus, or somewhere near it, being its admitted position *at the first formation of the mass*, I was hence led further to hazard an opinion which appeared to me also a rational one, as to how this apparent change of position occurred.

I could not, as I then stated, suppose, that the afterbirth absolutely shifted its position upon the internal surface of the womb, leaving the part it was first implanted on; as in that event there would be an interruption to its being in the constant and steady communication with the organ, which was indispensable for its office. I maintained, therefore, that on whatever part it was first implanted, on that identical part did it remain throughout pregnancy, and it struck me that the only possible way to account for the phenomenon under consideration was, that the enlargement which the womb undergoes during pregnancy, or I believe I would be more correct in saying, subsequent to the afterbirth being implanted on it, was mainly conducted on its

anterior wall, or I shall say, anterior section ; and I confess at this moment, after the most dispassionate consideration of the subject, I am utterly at a loss to imagine, if the two points above stated be correct, how otherwise it can possibly be explained.

I could not suppose or imagine that the womb, in its enlargement, enlarged by a general rising up of the fundus, and a distention of it in all other directions, as I believe, is the present opinion ; because, if that were the case, it must evidently have borne up along with it the after birth, and this latter must, therefore, at every time throughout gestation, up to its very close, be found at the fundus ; but as this is not the case, at least so far as the evidence I resorted to gave testimony on the subject, I came to the conclusion I have stated upon the matter.

But although I contended, that, from these reflections, I was justified in thinking the expansion of the womb went on chiefly on its anterior wall or section, (for otherwise I could not explain the matter, and I believe without such a supposition explanation is difficult,) I by no means said that the greatest expansion was there ; on the contrary, I expressly declared that this, "the greatest expansion," was to be found *at the fundus*, my words are (p. 452) : "at the fundus the womb expands to a much greater extent than at any other part."

Having, as I imagined, laid rational grounds for what I have here spoken of, the question naturally occurred to me, what was the object of nature in this arrangement ; for it is evident that where such an almost universal regularity of its position was to be observed, it must have been with a view to some intended end ? It appeared to me then, that the after-birth or placenta was an organ of the utmost importance to the preservation of the child, and that the peculiar office it performed was such as required rest, or perhaps I should say, would best be performed when at rest, or most out of the reach of disturbance. Now it struck me that if the placenta were placed, and continued throughout gestation, at the fundus of the womb, where the

greatest expansion goes on, and on the supposition that this expansion took place as now imagined, that for reasons to be presently mentioned, this rest could not possibly be there enjoyed by it; whereas in the position I found it in in such an overwhelming proportion of instances, if my method of accounting for how its apparent sinking on the posterior wall of the womb were correct, in that position, and as it were by that manœuvre, would this rest be *best* secured; to the consideration of these two points, and no more, did I go, namely, the placenta at the fundus, under the effect of the present opinion respecting the changes that part of the womb is supposed to undergo—and at the lower and back part of it.

Such is a short summary of the views contained in my paper of January last; and I believe I am safe in saying, the opinions expressed in it met with the approbation of a considerable number of the Profession: there were some, however, it appears who dissented from them; and finally, a reply to it, and intended as a refutation of the doctrines it contained, was published in the Number of this Journal for July last, from the Britain-street Lying-in Hospital, after it had been first read and fully discussed before the Obstetric Society lately established at that Institution, and in the presence of the medical officers of the hospital.

The objections contained in this review against my views on this subject may be resolved into two heads; firstly, conclusions drawn from what have been stated to have been my own expressed premises, laid down by myself in my original paper, but which certainly are not my statements; and secondly, arguments arising generally upon the subject, which are deemed to be fatal to my theory—decisive against it.

Before I proceed, however, to consider the force of these objections, I must first observe what I have already done elsewhere; namely, that it is the duty of every person reviewing the works of another, if he deem it proper to allude to any of the statements of that work, to confine himself, particularly in his quotations

from it, to the real expressed passages which he objects to, or purports to comment upon. If he should consider these passages "admit of a construction" which will be at variance with the conclusions his author has arrived at, it is but fair that he should state he is showing how such can be maintained, and that that is the point he is aiming at; but I am certain I shall be supported by every person, when I say, that there is nothing more to be censured in a reviewer, when alluding to the passages of an author's work, or more calculated to excite, and justly, displeasure in the mind of him reviewed, than to do it in a manner that will lead readers to suppose such are the real written passages contained in it, if they be not, and particularly if grounds be thereby laid for holding his writings up to ridicule and contempt, though the reviewer may afterwards, when the author so treated remonstrates, excuse himself on the plea, that what he said was deducible from it, and that it was in that light he meant it; and I will go farther and say, that the writer who finds himself so misrepresented, and every reader whose opinion is set right upon the matter, have every just reason to think, that he, the writer, has been most unfairly dealt with, and not with the courtesy or respect he was entitled to, or might have expected. That I have reason to complain of this in the review of my paper from the Britain-street Hospital, and Obstetric Society, I think I shall be able fully to show.

My original paper, in January last, was drawn up, as I have already stated, with a view to questioning the present received doctrine of the placenta or afterbirth continuing throughout pregnancy *at the fundus of the womb*; and I endeavoured to sustain my opinions upon this subject, by the observation, that the expansion of the womb during pregnancy was greatest *at the fundus*, and that if the placenta were situated where such expansion was going on, and also that that expansion took place as is now supposed, that the surfaces of the two organs could not *there* keep pace together in their growth, and that the consequence would be, during gestation, "*disturbance*" of the placental func-

tion, and “*probably*” partial detachment. I next went on to consider what the consequences, in my opinion, would be, when the contractions of labour set in, and I stated, as regarded them, that the first series of these contractions must constrict or lessen the uterine vessels going to the placenta, if at the fundus, and that these contractions going on and increasing in force, the placental function must ultimately be extinguished, (obviously in consequence of the obliteration of the above vessels from *permanent* uterine contraction,) if the organ itself were not detached altogether, and that early in the process of parturition. I beg to observe again, that I made these statements on the supposition of the placenta continuing throughout pregnancy *at the fundus* of the womb, and also on the supposition of the fundus enlarging by a general expansive rising up of it, pushing the placenta up before it, and further of the contractions of the womb going on first at the fundus, and then progressing over the organ in all directions, as now believed.

To my opinions, so supported, this review has taken certain exceptions, and argues these exceptions on certainly very infelicitous grounds; it declares, that I have said, if the placenta be *in any other part* of the womb, save the lower and back parts, that the consequences will ensue which I have strictly confined to its being at the fundus, and under the above circumstances; and further, in addition to this, adds, that I asserted *hæmorrhage* would follow *during labour*, when the placenta is so affixed to *any other part of the womb*; and having thus then given these as my statements, or if it be more agreeable, having quoted me so carelessly as to give room to the reader to imagine them to be mine, a series of cases are brought forward to shew the absurdity of my opinions, in none of which was the placenta situated at the fundus, but on the walls of the womb; and because the labour was in them effected without the placenta being detached, and without hæmorrhage, “the child still in utero,” these cases are thought to be destructive to my paper, and are brought forward as such.

Now if I *had* said that the consequences of the placenta being affixed to the anterior part or walls of the womb, would have been detachment, instead of confining this remark, as I did to the fundal position, and that only as probable ; and if in addition to what I did say, I further declared the gross absurdity, that with placental detachment, *hæmorrhage* would accompany labour in these mural positions of it, while the child was fully in the womb ; there is not one question upon the subject but these cases would have upset such doctrine at once, and also have proved on my part, as it must on the part of any person who could be found entertaining such a thing, an utter ignorance of the first principles of midwifery, in stating how such could possibly take place ; but as I never have made any observation of the kind, nor any thing that would bear such a construction, (at least such was never intended,) I trust I will be excused in protesting against these statements being attributed to me, in the strongest possible manner ; and I do say, that inasmuch as they are calculated to exhibit a writer on midwifery, capable of doing so, in the most ridiculous point of view, that it should be well looked into, in the first instance, to see if he has been guilty of such absurdity, before he is brought most unjustifiably before the Profession, on a charge of that description.

I did not in my paper say, that if the placenta were attached on the anterior wall, or laterally, that it could not there be accommodated, or that the expansion there was too great for that purpose ; I said nothing about these parts, my remarks in these respects were confined to the fundus, and that under the circumstances mentioned. The veryest tyro in the art is aware, that in placenta presentation, the placenta is often principally on the anterior wall, and still supplies the child often up to the birth, and that the fatality in such cases to the latter probably arises, among other matters, from the interruption to the function which takes place shortly before the child can be born, in consequence of the *hæmorrhage*. The partial placental presentation is well known to every one, and is often principally on

the anterior wall, and it may and does happen that after the mouth of the womb has dilated somewhat, the membranes will spontaneously break, the head descend, and pressure being so effected, all will go on well, and a living child be sometimes born without interference ; but will any one deny that this is an improper situation, or adduce rare cases of this kind as an argument against those who would point out a better position for the mass, and submit a series of rational grounds to prove it ? The child will sometimes present by the arm or the foot, and by management will be saved ; the breech will also sometimes present, and often without interference the labour goes on well, and the infant is preserved. But will any one say, on that account, these are proper presentations, or bring them forward as a proof to shew the absurdity of Doctor Denman's doctrine, as to the head being the one that is *the best* ? It has been stated, that my paper went to establish the necessity there was for the placenta being in a situation where it would be free from disturbance : it did ; but although it shewed, and I still think satisfactorily, to any unprejudiced mind, that that freedom from it was *greatest* low down on the posterior wall, under the circumstances I mentioned, where every person, even my reviewers, must *almost* invariably find it, still I did not at all say that this was the only place where it could preserve its function by any means ; there are others where it may, but less decidedly so ; and as I confined my remarks as to disturbance amounting to the decided prejudice of its office to the fundus, and that under particular circumstances, it is rather unfair to take me now to the anterior wall or sides of the organ, which I did not speak of, in order to disprove my opinions on that score, and there must indeed have been a great lack of valid cause for complaint against me, when so untenable a trifle is resorted to. Nature is not generally satisfied with where she *can* be accommodated, but where she can be *best* accommodated, and if we sometimes find her in the former position, it is not to be raked up against those who have pointed out the latter, as examples of their failure, or of

how unnecessary their work has been. I believe, since the appearance of my essay, attention has been very much drawn to the examination of the secundines; and I believe 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; but for those that cavil with my doctrines, let them, if they can, deny this assertion, as to the proportion of instances in which it is found low on the posterior wall, and if they be forced to *admit* it, or what is the same thing, be silent on it from an unwillingness to admit it, let them then say, either that Nature had no object in view, in thus almost so invariably placing the placenta in that situation, or if she had, let them give a better solution of it than I did; and further, let them show how the apparent change of position I have above alluded to, can otherwise take place than as I have accounted for it. We know well the great resources of Nature, when deviations take place from her usual ways; I need not here give examples of it; and if any person be disposed to produce instances of this fact, is it to be taken as disproving the general principle? such a thing, I do say, is cavilling, not scientific discussion.

In fact my essay went simply to an exposition of what I considered was the mode Nature proceeds in, when in her usual way; not either professing to explain how any aberration from that way was affected, and the resources she could fall back upon when under such circumstances, or denying the possibility of such things occurring. The writer upon the symmetry and order which Nature so *universally* observes in the formation of the female human pelvis, cannot be questioned upon the truth of his statements, because that very same Nature sometimes, by a process of her own engendering, (rickets,) warps that symmetry upon which he treats, and even still effects her object sometimes in that warped state; nor will public opinion entertain the catching of him upon the word "*universally*," because, sometimes, in disease, departures from it are met with; to say, therefore, that my paper went to establish the *necessity* of the pla-

centa being deposited in the posterior and lower part of the womb, and the attaching to it a meaning that, according to it, in no other place would the object of Nature possibly be accomplished, and that hæmorrhage and a still-birth must necessarily result from its being in any other position, and which has been endeavoured to be done, is obviously taking me in a way never intended by me ; and I am surprized how those who fancied this to be my intention, could think such an essay *deservedly* attracted professional attention, or be found making preliminary observations in any way complimentary to the writer of it.

But having disposed of the question with respect to the effects of such a position of the placenta as regards the child, let us now turn our attention to it in another way not heretofore considered, namely, as it may affect the case of the mother—a new view of the subject. Every obstetrician knows, that the proper and healthy attachment of the placenta is of such importance to the mother, that it often involves her very existence ; it must be detached fully and at once, not partially, or hæmorrhage, frequently fatal, is the result. What is the cause of hæmorrhage when the placenta does not come away ? Partial detachment. And does not Nature seem to have endowed the womb with powers within itself of completely detaching the mass unaided by art ; that is, in ordinary cases ? Now I would ask the question, when the hand is introduced into the womb to take away the placenta in cases of its retention, to effect what Nature seems, contrary to her usual way, here to be unequal to, where is it the placenta is found ? is it not in this very position where my reviewer from the Britain-street Hospital found his placentas in. Where is the placenta in those dangerous cases of hour-glass contraction ? In fact, in all such cases, is it not found in some place different from where I have pointed out *its proper* one to be. Will any person tell me, that has attended to the position of the placenta by the means I have pointed out, that out of every hundred cases, in ninety-seven at least the place at the close of gestation is not where I have located it ; and has

any person who has been obliged at any time to take away a placenta, ever found it there, *low down at the back part*, or where *has* he found it? is it in this latter situation? Where is it that morbid adhesions are found? And what is the inference from all this? why that the placenta is always (now I hope the word *always* may not be fastened on) fully and effectively detached when in my position, but most generally not so when out of it; in fact, that Nature there is astray. It may be said I am now changing my point, explaining matters differently from how I did. No, I am giving additional proofs of the correctness of my doctrine; what I have said as to the quietude such a mode of proceeding, on the part of Nature, insures the organ, must also arise from it, as well as what I now point out: it cannot be otherwise; yes, and if I might hazard a further matter for consideration, one which I think deserves the attention of the obstetric pathologist, it may probably be, that some of those cases of abortion, that occur without any apparent assignable cause, at certain periods of gestation, might owe their cause to some such mal-position of it. I do not retract what I have said in my original paper, I still think it correct; I now along with it give this additional cause for such being the object of Nature.

But let us proceed to the consideration of this review from the Britain-street Hospital; and, first, I shall lay before the reader the passages alluded to from my paper by the reviewer and those which purport to quote these passages, in order that it may be seen whether I am correctly represented or not, and whether cases based upon the data assumed by the reviewer can at all apply to my doctrines; and for that purpose the more clearly to do so, I shall set them out in two contiguous columns.

*Statements from the Coombe
Lying-in Hospital.*

“Admitting, however, that
during gestation there should

*Statements attributed by the
Reviewer from the Bri-
tain-st. Lying-in Hospital.*

“Admitting the original
position of the placenta to be

be a perfect freedom from untoward consequences, when the contractions of labour set in, I cannot see how they will not occur, and in the most decided manner.

“ These contractions, as I have already mentioned, are supposed to commence *at its fundus*, and from thence progress downwards, lessening the surface they act upon as they proceed, and increasing in power with each succeeding pain ; the first series of them, therefore, must, by contracting *that part* to which the placenta is supposed to be affixed, in the first instance *lessen* or *constringe the uterine* vessels upon which its utility chiefly depends, and thereby impede or interfere with its office to a certain extent, and the contractions going on increasing, they must *ultimately extinguish* its functions if not detach it altogether, and that very early in the process of parturition.”—p. 454.

Again, “ Let any person form an opinion as to what the state of the placenta must be *respecting the impeding of its office*, or even of its attachment to the womb, when the

at the fundus, Mr. Carmichael denies that it continues in that relative position throughout gestation, and *endeavours* to prove the result of such an arrangement should be, a partial separation, *followed by hæmorrhage* during the growth, or, *at all events, during the contractions of the womb.*”—p. 344.

Again, “ Mr. Carmichael’s arguments resolve themselves into three heads : 1st. In healthy and natural pregnancies, the placenta is never implanted on any part of the uterus save the inferior part of the posterior wall. 2nd. If the placenta forms its attachment *in any other situation*,” [here I beg attention to the words ‘ *any other situation*,’] “ its growth cannot correspond to that of the womb, and early separation and its attendant circumstances must follow. 3rd. *In the latter case*,” [attachment in any other situation,] “ even though it should

capacity of the organ is lessened to the extent it must be when the head has fully entered the pelvis in a labour.”—pp. 454, 455.

Again, “That such must necessarily be the effect of the contractions upon the placenta, *extinguishing its function* early in labour, and depriving the child of the benefit it derives therefrom is obvious, *if these contractions be performed in the way I mention*, and the placenta be *at the fundus* of the womb. The statement is *by no means overdrawn*, or for the purpose of answering particular views; we find the same thing put forward by some of the French writers, and which I shall beg to set out here.”—p. 455.

Again, “If the placenta then be *at the fundus* of the womb, it is situated at the most expansive part, it is therefore, as well during gestation as labour, exposed to all the evils its unchangeable surface must give rise to.”—p. 478.

Again, “In a word, according to this theory, the placenta, in every stage, from its

able to maintain its adhesions uninjured throughout pregnancy, the consequence of the uterine contraction, when labour begins, must *inevitably* be its detachment, *followed by hæmorrhage*; in fact, according to him, such an untoward occurrence will form as truly an instance of *unavoidable hæmorrhage* as when the placenta is placed over the mouth of the womb.”—p. 346.

Again, “But laying aside theory, 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 contraction to expel the fœtus*, cause premature detachment and consequent hæmorrhage.”—p. 353.

formation to its end, is in a constant state of disturbance, must be exposed to *interruption* during pregnancy, and at labour to suppression by detachment and *compression*, long before it ceases to be required.”—pp. 478, 479.

When the reader inspects the two foregoing columns, and compares what I *have* said in my original paper with what I would *appear* to have said, I think he must admit I have reason to complain, and that even if it were intended only to say that such was *deducible* from my statements, this has not been put forth in a way that would lead one to think so.

Now the two misstatements I complain of here, are 1st, that I am represented to have maintained, if the placenta be implanted *any where* upon the womb but the lower and back part, that its growth cannot correspond there with that of the womb, whereas I have only said that is the case *at the fundus*; and 2nd, that I am stated to have said, that when it is in any other part, the contractions at labour must produce detachment and hæmorrhage, whereas I never spoke of hæmorrhage in any place except when the placenta was *somewhat* high up on the posterior wall; even when at the fundus I have not said it will follow, because if the placenta were detached there, it would be by permanent uterine contractions.

However, these are the two statements the reviewer seizes upon to prove, as he terms it, “the fallaciousness” of my doctrine; and having attributed these sayings to me, he proposes giving cases in support of his opposition, based upon them. I shall beg to quote the following passage to show this:—

“But laying aside theory, 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—*otherwise* affixed, and yet no hæmorrhage occurred before the birth of the child, I shall have gone a great way in *sapping the foundations* of the author's interesting but *fallacious* theory of the mode of contraction of the uterus."—p. 353.

I trust I need go no further to shew the value that is to be set upon the cases, as against my doctrine, which are based upon the supposition of the foregoing statements; there is one thing, however, in them which I wish particularly to direct attention to, namely, looking for hæmorrhage *while the child occupied the womb*, the placenta being on *its sides*; why for hæmorrhage to occur, the placenta must be somehow detached, but the presence of the child necessarily will prevent the diminution of the womb on the wall where the placentas were in these cases; how then could it be detached? and is it not a sad hardship on my part to have such silliness attributed to me, or is it more a matter of wonder that they seriously entertained such a matter at the Britain-street Hospital, instead of at once ridiculing me on it, if they could find me saying so.

But the cases—we must come to the cases: I do say, not one of them are in point. I spoke of the placenta only in two situations, the back and the fundus; in the cases they are all on the wall, chiefly the anterior, and therefore do not apply to any thing I have said, and as looking for hæmorrhage from detachment, partial or otherwise, while the child was in the womb, this I can only say, I never even hinted at, nor can I see how it could possibly occur.

Let it not be said, that if these cases be examined it will be found that every part of the womb (certainly, save the fundus) was shewn to be covered by the placenta from one or another of

them, and therefore that what I said was upset, because no disturbance, no detachment took place in these situations. The latter I have shewn to be absurd, "the child in utero," and will *demonstrate* it when we come to analyze the sixth case ; and with respect to the former, it is only, as I have said, an instance of the cautious providence of Nature, in supplying herself with resources, when by accident or otherwise she is thrown from her usual way ; and it will be also remembered, I never spoke about it there : as well might a physiologist say, that the main artery of a limb was useless, because if it be tied or rendered imperious by disease, the circulation goes on by anastomosis ; this latter, no doubt, is a fact, but is it not better conducted by the system of a main trunk, or will the exception disprove the general rule. I only told a plain story of what I observed from dissections, and the testimony from the stethoscope and the secundines, and what struck me as the cause for such being the case, but I did not want to say I had found out what *alone* would render the entire process of conception successful, or seek to stultify the skilful professional reader, who knows well the placenta is sometimes on the anterior and lateral wall of the womb, into the supposition, that Nature here tied herself up, in a way she is almost never known to do elsewhere, in leaving herself altogether without a resource in case of need ; nor do I think examples of such lateral position can at all bear upon the question, unless it can be shewn that I said what has been attributed to me about hæmorrhage and other positions ; the fundal position, I think, will form an exception, if the expansions and contractions go on as supposed, and if a case be adduced, shewing such a one from secundine observations and length of labour, which I think material, so inconsistent is it with my plain mind, that it would go (regard being had to the three questions I have asked as to place, apparent change, and object of nature) to prove with me that the fundal expansions and contractions do not go on as now supposed.

In two of these cases part of the placenta extended high up,

but where was the rest of it then ? low down certainly ; in one, six inches at least below its top, and in the other it went down to the very os ; now I do say, if a case be brought against my explanation respecting the effect of its being at the fundus, it should be *all* at the fundus, or dome or vaulted arch, if these terms be more expressive, and what is clearly meant by the present opinion as to how it is placed, or supposed to be placed, upon the fundus ; and not to bring forward a case to disprove my assertion in that respect, where it was partly within the expansive dome and partly not ; yet this is what is sought.

Further, I say, there is but one of these cases (the third) which can be relied on as giving any satisfactory evidence upon the matter—that is, as to the true placental position ; the hand introduced into the womb, when the latter has undergone the great diminution it does to expel the child, cannot give admissible testimony as to how the placenta was placed when the viscus was in a full state of expansion. In a disputed point like this, the hand in the womb, when turning, or the secundines, are the only true proof, particularly where the contractions in question are contended for.

Neither can a point, very much relied on by my reviewer, at all apply to the dispute at issue, or be admitted as in any way invalidating my doctrine, viz. that where the placenta was found on the anterior wall by the hand introduced for its removal, that it must have been at one time at least, on the fundus, and so have been detached, &c., in accordance with my statement. If I am to be tried upon my own ground, give me the full benefit of it ; when I spoke of detachment at the fundus, I did it on the supposition of the contractions there going on in the way they are supposed, from all points in the circumference of a circle to its centre ; but if the placenta *passed* the fundus, it must have done it according to my views, and the contractions, therefore, being there similar to those on the anterior wall, or somewhat so, (rectilinear,) and not the true, or supposed true, fundal centripetal contractions spoken of, they must have

influenced the placenta and its attachment only, in the same way they would if in the latter position, or nearly so ; and, therefore, by no means admit of application in the way they are meant to do by the reviewer. But having parried my opponent's thrust in this respect, let me now try one at him : suppose we examine the matter according to his notions of uterine changes. The fundus with him is always the fundus, therefore he cannot read any of his cases against me in support of his views of fundal contractions, for he has not produced one case in which the placenta was fully and decidedly upon the fundus, or dome, or vaulted arch, if these terms be preferred, and which is certainly necessary in order thereto ; as matters are now, it is clear, he wishes first to establish, that according to my doctrine, the placenta in some of his cases was *one time* at the fundus ; and having done that, he then wishes to change the scene, and try by the placenta there, not my theory which brought it there, but his own, which, under the circumstance, it has nothing whatever to say to.

Let us now to the sixth case, and show, how sad the mistake was to consider the points connected with it to be so conclusive against me, in fact, how they vanish under a little consideration. What are these points ? Why, in this case, and it appears it was the first, the only one of the kind that was met with, after several months' search at the Britain-street Hospital, the placental murmur was heard *faintly* across the fundus. This was the case that was to complete my ruin—the one the man at the mast-head was on the look out for for months before—the welcome announcement of breakers a-head, upon which what remained of my fragile bark was mercilessly to be drifted, and triumphantly dashed to pieces on. Well, the first thing here adopted is, that a case was got with the placenta *at* the fundus ; and it appears, the assistant physician of the hospital on duty, and the pupils, were called to witness the expected phenomena, or rather to bear testimony to the fact, that they did not manifest themselves, and, therefore, the triumph against me, and my

eternal disgrace. However, the first thing settled upon was, that a case was got with the placenta *at* the fundus. This I take leave, nevertheless, to be very sceptical upon ; for if the murmur was but *faintly* heard across the fundus, it must have come from the superior border of the mass, which I should only say was *near* the fundus. Now, I would ask, where was the lower border of it ? Why, at least, six inches below the upper, and perhaps more ; therefore, making one inch allowance for the placental murmur at the fundus being but *faintly* heard there, which is a very moderate allowance, the placenta must have extended seven inches, at the very least, downwards posteriorly ; in other words, I should call this not a case where the placenta was at the fundus, but one on the back of the womb completely. Thus then falls to the ground the first series of phenomena looked for—the first of the conclusives against me, namely, all those I stated in my paper *would* occur, if the placenta were situated *at* the fundus of the womb, without even taking into account the description of contractions on the supposition of which I spoke.

The next point against me, arising from this case, I strike under to completely and at once ; I certainly said the placenta was *never* heard at the fundus of the womb, and here unquestionably I am upset, for here is *one* instance of it ; I admit the justness of the charge.

The next against me, is one which is relied on as altogether *conclusive* on the subject. It is said, if my account of uterine contraction be true, it should happen that *as labour proceeded*, the murmur, heard first but faintly across the fundus, should have mounted upwards, be heard then in full intensity at the fundus, and after that have descended, till at length it almost arrived at the pubis ; whereas the observers, physicians, pupils, and all, *most explicitly* declare that no change whatever was observable in the position of the murmur, nor was there any alteration in its relative intensity in the regions of the uterus.—(p. 360.)

In all of these latter parts I most fully and cordially concur ; they are perfectly right ; nay, it could not be otherwise : and yet I can show they do not affect the question one particle.

In the first instance, I beg to state, I never said the placenta passed over the womb, nor could I say so ; I stopped it at the fundus, and even then, *after* the child was expelled, my words are : “ at length by this means the child is expelled, and *by that time*, the placenta arrives at the fundus.”—p. 480. But this apart, I shall proceed to show how impossible it was for what these gentlemen were gravely looking for, to occur in this or any case like it, and how irrelevant it is to our subject. In order to this, I have to state a matter connected generally with labour, which I am disposed to think they were not aware of at Britain-street at the time of making their observations upon this conclusive case ; or, if so, that the facts which I insisted on in my paper respecting the fixidity of the placenta to one spot on the womb, were not on their memory at the time ; and yet I am much surprised, as they were so intent watching every thing connected with this case, that what I am going to say did not then strike them.

The fundus, or uterine tumour, does not in labour descend, I think I may say, more than one inch and a-half, if so much, until the head of the child is passing the outlet. In the transit of the head from its resting on the brim, to its resting on the perineum, the general advance is not more than an inch and a-half, or thereabouts. Now, if we bear this in mind, and also what I have said in my paper, (p. 468,) “ on whatever part of the womb the placenta is first affixed, on that identical part does it remain, &c.,” we shall see how the case is altered as to supposing the bruit or murmur should have here mounted up to the top, passed then over it, and finally go down to the pubis, and which it was insisted on it should, if my theory were correct ; and further, how obvious it must be to any person, aware of this, that such a thing could not possibly occur, and, therefore, how its absence can in no way affect the question

before us. Until the head of the child is about being born, the relative position of the uterine parietes in fact remains nearly stationary; and how then could this change of posture as to the placental sound take place, the absence of which has been deemed at Britain-street so decisive against me, unless it were done by a shifting of the placenta itself upon the inner surface of the womb.

If the fundus descended to or below the umbilicus, during the stage of a labour I have just stated, namely, the transit of the head from the brim to the floor of the pelvis, then might this phenomenon be expected and looked for, because it is during that period that stethoscopic observation can be alone made; but it remains nearly stationary up to that period: it begins to descend as the head is about to escape; and when the head is born, what from the sufferings of the patient, the hurry and confusion of the moment, and the quickness with which the rest of the child is born afterwards, any thing like the use of the stethoscope is out of the question. I believe this is an answer, and a complete one, to this matter which has been thought so *conclusive* against me, and the looking for which occupied so much of attention in the wards of the Britain-street Hospital.

That what I have now said is a fact, every person can satisfy himself of by plain observation, and where such opportunities exist as there do at Britain-street, it can easily be ascertained; but if we consider what takes place in labour up to the time I mention, we shall see that such must be the case—the fact will be easily explained.

On the discharge of the waters the uterus contracts, and compresses the body of the child into the smallest possible space; the chin then sinks on the chest the head now begins to advance, but the most depending part of it then is the occiput; as the head advances, the chin, however, leaves the chest; and when the head is on the floor of the pelvis or perineum, the crown, which was in the slanting position of the brim when on the brim, now has attained a horizontal position, and the forehead is on the level of the occiput. Thus we see the depth

of the sacrum has nothing to say to the matter, in the way of measuring the general advance of the foetal body ; it becomes filled by the face of the child no doubt, but that is in consequence of the backward motion of the head during its advance, by the chin leaving the chest.* The general advance of the body, therefore, is only measured by the anterior depth of the pelvis, which is the depth of the symphysis pubis, or a little more. If now then we take into account the contractions of the womb, no matter how effected, we may readily perceive the diminution of it to propel the child, two, or say three inches, being chiefly in its (the womb's) superficial measurement, that the change in the absolute altitude of it will be less than the extent to which the foetal body is propelled ; that is, say one inch, or an inch and a-half at most. Whether this be a satisfactory explanation of the matter I shall not say ; but certainly the sinking of the fundus until the head is about to pass the outlet of the pelvis, is not even apparent. I believe I need say no more respecting this looking for an excursive movement of the placental murmur over the uterine surface ; it is as much in point as the supposing this to be a case with the placenta *at* the fundus ; the above fact respecting the descent of the fundus in labour, is more observable in first cases, where the tension of the abdominal parietes tends to keep the uterus compressed ; in subsequent ones, their relaxation admits of the advance of the fundus forwards,

* The final escape of the head through the soft parts, is very much accomplished by this backward motion of it, and without it, it would be difficult to suppose how rupture of the perineum could be avoided. If its escape were effected solely by the advance of the head, descending as it were perpendicularly, the perineum, which lies upon the crown like a broad band, must obviously be rent in most instances ; but combined with this throw back of the occiput, such a thing is not only thereby avoided, but a tendency thus given to the thin, expanded, band-like integument, to slip over the face in the direction of the coccyx, and so its integrity is preserved. Thus, then we see the chin is on the chest when the second stage of labour begins, and that from that, the head is impelled to its birth by a double or combined movement ; one, a general advance, and another, a tendency of the occiput to move backwards towards the spine, and which last is not at its maximum until the head is born.

so well known to every one, and which, as I endeavoured to explain in my original paper, resulted, or might result, from anterior uterine action.

The next conclusive point relied on, as arising from the sixth case, is, that as the membranes did not break until the head of the child was *deeply* in the pelvis, the reviewers think, on the supposition that all this time the placenta was revolving round the uterine tumour, and of course the membranes going round also, like planets in their orbits, that the orifice in them should have been different from what the first stethoscopic observation indicated, whereas it *positively* was not. This objection, however, is obviously based upon the opinion, that the fundus descended at the time, I have shown it was nearly stationary; and therefore is neutralized by the fact I have related with respect to that opinion.

I would beg of the reader, after this explanation, now to go back to the conditions under which these cases are brought forward against me, and particularly as to the reviewers at Britain-street looking for placental detachment, from the anterior and lateral walls of the womb, with hæmorrhage, during labour. The phrase here "during labour," we all know, is confined almost up to the birth of the head, the full delivery of the child being so quick after this, as scarcely to deserve notice in it; how then could detachment and hæmorrhage occur up to the resting of the head upon the perineum, if the superficies of the organ diminish in no perceptible degree? and after that, if detachment took place, it must be by permanent contraction; and how could hæmorrhage then occur, or how could any person, aware of the fact just stated, ever dream, much less speak of it? yet this is one of the questions gravely asked upon these cases. With respect to the objection that there is no *fixed point*, from whence contractions could have effect, such as I speak of, making the foetal body a fulcrum to produce their result; I do not think this is at all supported: in order to it, it should be first shown that there is a fixed point which will

prevent what are termed the longitudinal fibres from pulling up the mouth of the womb, and will give their action a tendency downwards, which, I believe, is not yet done : somehow, which we cannot explain, the lower part of the womb is preserved in a fixed state ; and this will apply to the one as well as the other theory. Indeed, with respect to uterine muscular fibres, whose existence are as yet, I believe, under dispute, I am disposed to lean to the negative side of the question ; I have in the several dissections in which I had an opportunity of investigating the matter, looked for these fibres, not with a view to their arrangement, but as to where such muscle existed as that to which we could refer the powerful action of labour, and which, I confess, I have not been able to trace ; and when I consider the excess of intensity with which this action exists, when the womb is under the influence of the ergot, I am disposed to think, that at least something else is in operation besides a few scattered fibres, such as are alone observable, to produce it.

We shall now proceed to consider the objection, as to the placenta and uterine surface not keeping pace together during their growth.

But before I examine the statements of our reviewers on this point against me, there is a matter connected with it which I really feel quite at a loss to reconcile, or indeed to understand.

I have complained of being *misconceived* ; and among other complaints from me on that score, that I have been reported to have said, “ if the placenta form its attachment *at any other situation*, (save the inferior part of the posterior wall,) *its growth cannot correspond to that of the womb*, and early separation and its consequences will follow.”—*Reply*, p. 346.

Now, as what *I have* said on this matter was, “ if the placenta formed its attachments *to the fundus*, and remained there during gestation, that these consequences would follow,” I should be disposed to attribute this incorrectness to mistake, were it not that in *the very page preceding it*, what *I really have said* is stated, and therefore would appear to have been

quite known when the next one was being written, where I have been misstated; the words are, in the page preceding the misstatement :

“ Admitting the original position of the placenta at its formation to be *at the fundus*, Mr. Carmichael denies that it continues *in that relative situation* throughout gestation, and endeavours to prove the result of such arrangement should be separation, &c. ; and the reasons he assigns are twofold for these opinions. In the first instance, the principal growth of the uterus is, *as must be admitted*, at least with respect to a longitudinal direction, in that part of it, situated above the Fallopian tubes, *where* the enlargement, he says, is too extensive *to permit the placenta to keep pace with it, or maintain its connexion unbroken.*”—*Reply*, pp. 344, 345.

I would not for one moment suppose that the mistake was intentional ; it must have resulted from mistake alone ; but the cases that were “ *to sap the foundations of my fallacious theory*,” without it, had as much to say to the matter as they have to the circulation of the blood.

The reader, however, will see that in one place I am represented as quarrelling with the placenta being *at the fundus* ; and in the other, with its being *at any other situation* but the back low down. It is a hard matter for me or the placenta to keep our ground under such a cross fire.

I shall now, however, proceed to consider the arguments brought against me (pp. 351-2-3) to upset my assertion on this point, as to the two surfaces being unable to keep pace together *at the fundus*, for here I am *correctly* quoted on the matter ; “ let us now,” say our reviewers, “ follow up the process of gestation, and see whether there is any true ground for supposing the placenta cannot keep pace with the womb, if attached *even above the Fallopian tubes.*”

The first assertion on this point is certainly very novel to me, though given on the authority of Burns, and a reference made to morbid anatomy, namely, that the increase of the fun-

dus is no more than five inches: the words of the reviewers are ; “the greatest development, in a longitudinal direction, takes place at the fundus, which increases from one-fourth of an inch, its measurement in the virgin state, (Burn’s Principles of Midwifery, p. 49,) to about five inches, as is evident from the relative position of the Fallopian tubes.”—p. 352.

Having assumed this then, a proportional calculation is gone into ; it is said the placenta is six inches in diameter (p. 352) at its full growth, and that at its first formation its diameter is one-half an inch ; thus then the size of the fundus and placenta at its full growth, and of the latter, at its formation, is given ; but as this last is in the half-inch measurement, the two others must be reduced to that measurement for the purpose of demonstrating their comparative sizes in these different states, and so we have 1 to represent the newly formed placenta ; 12 for it at its full growth ; and 10 for the fundus of the womb at its full growth : thus evidently showing the untenableness of my opinion ; and that so far from the fundus outstripping the placenta in its growth, the latter outstrips or encroaches on it in a proportion that would be represented by 2 ; therefore there can be no disturbance to it during gestation.

This certainly is all very good if the proportions given be correct, but there is one most unfortunate mistake in it, viz. *the size of the fundus* between the Fallopian tubes at the full period of gestation, which is the point at issue, and upon which all depends: let me be understood ; I mean the size across the fundus—five inches from one Fallopian tube to another *across* the fundus? Why, its measurement in that direction is about eleven inches, which shakes most materially this fine calculation. In a marginal note, reference is made to Burn’s Principles of Midwifery, p. 49, for authority for this five-inch measurement ; I can only say that I have looked to the reference, and could not find it or anything about it.

In support of *my* statement, however, I shall beg to refer to the Museum of the College of Surgeons, where two prepara-

tions will be found of gravid uteri at full time ; and here it will be seen that I am correct in what I say, the fundal measurement, from one tube to another *across* the fundus, being fully eleven or twelve inches. We have a similar preparation at the Coombe, which bears also testimony to the truth of it. I was lately called to remove a child from a poor woman in the neighbourhood of the Coombe Hospital, who died about a week previous to her expected confinement, and the measurement carefully made on that occasion, from the surprise this statement from the Britain-street Hospital created amongst us there, was eleven inches. Now, if we make allowance for the contractions the womb undergoes after death, we shall see how very wide of the fact our reviewers are upon this matter. I do not, however, wish my statements here to be taken as admitted upon my own authority ; the museums I have alluded to are open to the public, and the preparations may be referred to and seen, to prove which of us is right, or which rather is wrong. I shall take the eleven inches, however, as the true measurement, in which, I believe, I am right ; and let us try this question by the proportions so corrected :—1 for the newly formed placenta ; 12 for it at its full growth ; 22 for the fundus of the womb at its full growth ; showing that the placenta does not increase during its growth upon the fundus, by a proportion which may be represented by 2 ; but that the fact is, the fundus increases on the placenta in a proportion that will be represented by no less a number than 10 ; so that if we take away the 2, contended for by my reviewers on the part of their placentas, and give it to the *masses* at the Coombe, the error in the calculation will be represented by 12, and that 12 will be in favour of my theory.

The next point against me is, “morbid adhesions of the placenta, slightly contracted pelvis, and even arm presentations do not vitiate the arrangements of nature ;” if they do not, I certainly say I do not know what will.

I have already given a reason, however, on my part, for con-

sidering that deviations of this kind *may* "vitate the arrangements of nature," and be "no authority, or standard, or rule in generation." I have done so in my publication of January last; and I shall again state what I then said. I have there urged the fact, that it is an observation made by many, that preternatural presentations occur most frequently where disfigurement of the bony structure exists in some shape; and from this I inferred, and, I think, naturally, that it seemed to require but a very immaterial departure from the healthy state of all parts, to derange the general order of things; and thence I went on to deduce, if deviation from the natural standard of the pelvis could so influence the *position* of the child as to *reverse* it, or throw it into a cross form so frequently, that we might suppose it capable of going farther and affecting the general harmony that should prevail as regards other matters, and so perhaps derange the position of the placenta, as well as those it did; and I shall show that our reviewers think so likewise, and have absolutely stated an example of it.

This is the train of argument, however, I used on that occasion, and to my moderate understanding it does not seem at all irrational; and I am happy to be supported in this opinion by this document coming from the Britain-street Lying-in Hospital, containing a statement to that effect. I shall go even further and say, that if I stood in need of proof of this my train of reasoning, these proofs are most satisfactorily supplied me by those very cases brought against me from the Britain-street Hospital. They have been five or six months observing the placentas at that hospital, with a view to my theory, and they have been enabled to bring forward but six or eight out of the entire that presented themselves during that period; and how are these six or eight circumstanced? They are cases of distorted pelves, gritty, diseased placentas, arm presentations, placental presentations, and the like. Why, this is the very thing I have been contending for, that it is in such cases aberrations of placental position do occur: instead of proofs against my reasoning upon

this matter, these very cases exemplify it. I cannot, however, but regret the mistake in not stating the situation of the hundreds of other placentas which were observed in Britain-street during the above period. If the position of these hundreds of other placentas had been stated, and also how the births went on in them, as to whether they were detached naturally, or by the hand introduced into the womb, and then compared these matters with those as observed in the five or six given, I think, at least, plain ordinary minds might be led to suppose that the testimony generally, very, very largely went in support of my doctrine ; and that the exceptions to it were to be found in gritty placentas, cross births, distorted pelves, placental presentations, *et hoc genus omne*, and therefore probably, that *I was* correct in saying, “any thing met with in such cases cannot be looked on as an authority, or standard, or general rule in generation.”

Our reviewers say, they cannot conceive “in what way an unusual contraction of the pelvis could modify the course the foetal vessels take to attach themselves to the maternal surface ;” and they protest most decidedly against such a doctrine. If they mean as to how it would explain such an occurrence, I cannot either ; neither can I or they explain how a contracted pelvis will modify the disposition of the child in the womb, so as to change its position from the natural one, yet this is a remark which, as I have already said, has been made by many. I think, however, I can show them absolutely adopting the very thing they protested so strongly against.

With all this declaration against a contracted pelvis, or bony disfigurement, possessing the power of influencing the place of the placenta, I beg to refer to their own fifth case, Elizabeth Hoey, one of disfigured pelvis, where the crotchet was used twice in consequence of this disfigurement ; and in this case the placenta was most irregular, so much so, that it was exhibited at the Pathological Society ; now the reviewers absolutely account for the irregular *position* of the placenta here, and how do they account for it ? Why, that it was in consequence of the

bony deformity ; the very thing they object to when I want to use it for that purpose. The words are, (p. 358,) “in this case the placenta had assumed a curious modification, in consequence perhaps of a deficiency of fluids, while the uterus *was thrown very much forward by the projection of the sacrum* ; the embryo had attached itself to the anterior wall :” what does all this mean ?

But the selection of cases are qualified by stating, “that it is only where some extraordinary difficulty exists, that we have an opportunity of satisfying ourselves on the point at issue by the introduction of the hand.” This I think, however, not the case ; I think an examination of the secundines, and an attentive observation of the phenomena that occurred during the birth, will give every information on it, as fully and more satisfactorily than testing natural cases by preternatural and anomalous ones, most of which these are.

I trust I have said sufficient in the foregoing pages to show the value of the arguments that have been brought against me in this review, and the extent to which my opinions are likely to be affected by them. The reader, I think, will also be satisfied, that the passages attributed to me in it are not exactly what are to be found in my original paper of January last, nor perhaps what would admit of being considered even obviously deducible from it ; and as upon these allegations alone, can the cases for one single moment be sustained, the force these latter have in the investigation, must, I should hope, be appreciated accordingly. I think also, the other arguments brought forward as expositions of my supposed fallaciousness, namely, the manner it is shown how the fundus in its growth can keep pace with that of the placenta ; the cases of diseased afterbirths ; of arm and placental presentations ; of distorted pelves, and such like ; now for the first time, I believe, put forward as specimens of natural cases, and intended confessedly to apply to natural pregnancies, together with the conclusive sixth case, and its destructive bearings on the matter : I say, I think, it must appear that all these rest

upon just the same stability, and affect my opinions just in the same degree, and no more.

I shall conclude, therefore, with the hope, that what I have written upon the placenta, &c. is not quite so ridiculous as this review would fain point it out to be ; and further, by observing that I shall not, on my part, be “deterred” at any time hereafter I may be induced to give expression to my opinions and observations, from protesting in the strongest possible language against any incorrect representation of what I shall write upon any subject I may be led to treat of, coming from where it may, or how it may. It is the right and duty of every person who feels himself so aggrieved to do so. I shall never take such a liberty with the writings of any person, neither shall any one or any institution do so with impunity with me, if I can help it ; and I therefore beg to observe, that the philippic lately published against me, and dated from the Lying-in (Britain-street) Hospital, for daring to exercise this right, and published chiefly for the purpose, as it confessedly declared in the commencement of it, “of deterring me and others from ever doing so again,” is altogether thrown away upon me ; I acknowledge no such authority in any institution.

It may be possible, what I have said respecting generation in these particulars is all erroneous ; if so, however, I should expect it to be met by a fair and candid canvass of the matter, which certainly has not here been done ; and if it should, and that it be so shown that I have been in error, I am ready to be the first to declare against it. But if, instead thereof, I am sought to be tried upon statements I never made use of, and so conclusions drawn against me calculated to hold me up to ridicule, I shall (in defiance of this “detering order,” lately pronounced and dated as above stated, and regardless of the “emblazoned shield” and the “high exaltations,” which an individual of so minor an importance as I am, has been sought to be dazzled with) set public opinion and judgment right on the matter, by pointing out, in no very measured language, the mis-

take in the premises from whence such conclusions are drawn ; and I am certain, in so doing, I shall experience, as I have in the instance gone by, the universal approbation and support of the Profession.

ART. XII.—*Contributions to Midwifery*, No. II.—*On Purulent Effusions into the Joints, &c. in Puerperal Women.*
By THOMAS EDWARD BEATTY, M.D., M.R.I.A., Lecturer on Midwifery in the Park-street School, and Consulting Accoucheur to the New Lying-in and City of Dublin Hospitals.

[Read before the Surgical Society, January 25th, 1839.]

IT having fallen to my lot to witness some examples of this rare form of disease in puerperal women, I am induced to make the following communication, more particularly, as although the affection has been treated of in detached papers by different authors, yet it appears not to have received the attention which its importance demands, as I cannot find it noticed in any of our most approved systematic works on midwifery. I am further induced to call attention to this formidable consequence of parturition, because of the obscurity in which it is sometimes veiled ; an obscurity which leads me to imagine that the disease may not be in reality so rare as it would appear to be, and that by a close attention to its nature, a greater number of instances might be discovered.

The affection to which I allude is purulent effusions in several and remote parts of the body, principally about the joints, and often communicating with their cavities. The symptoms which accompany the early stage usually commence within a few days after delivery, and consist of severe pain, tumefaction, and sometimes redness of one or more of the large joints, together with intense fever, loss of appetite and great thirst, and occasionally, but not always, pain and tenderness in the lower part of the abdomen. The absence of the last symptom leaves the case with