ON DEFLECTION AND ROTATION OF THE PREGNANT AND PUERPERAL UTERUS.

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It is a well-known fact that the uterus, growing under the influence of pregnancy, tends to incline more frequently to the right than to the left side of the body.

In one group of a hundred cases, Pajot and Dubois found that the uterus lay to the right in eighty; in another group of eighty cases the uterus was found to the right in 85 per cent.; in the remaining 15 per cent. it lay either in the middle or to the left. The same tendency to incline to the right more than to the left seems to persist after labour, until, at least, the uterus disappears below the plane of the brim. Thus, in thirty recent cases observed in private and hospital practice, I found that in twenty-two of them the uterus lay distinctly to the right side of the mesial line during the first five days of the puerperium. This proportion amounts to 73 per cent.

It would thus seem that in from 70 to 80 per cent. of all cases the uterus inclines to the right side, in about 20 to 30 per cent. it inclines to the left, or lies more or less mesially.

A considerable number of explanations has been suggested of this preponderance of right deviation, but, as Barnes remarks, "none appears conclusive." Levret thought that the inclination was determined by the insertion of the placenta in one or other side of the uterus, but Tarnier states that the uterus is sometimes inclined to the right when the placenta is attached to the left side of the uterine cavity. Desormeaux thought that the deviation was produced at the beginning of pregnancy by the pressure of the rectum on the left side of the promontory, pressing the growing uterus to the right, but Dubois points out that this influence of the rectum in pressing the uterus to the right side was probably compensated by the cecum on the left, with which it would soon come into contact.

It may be noted that the influence of the rectum as a cause of the deviation seems to receive the countenance of Playfair as being, of all the causes suggested, the one most likely in operation. Were this explanation more plausible than it really is, it seems to me to entirely break down when we come to consider the 20 per cent. of cases in which the uterus is inclined to the left. If the rectum is to be regarded as the cause in the one set of cases, it would require to be shifted over to the right side in order to account for the others!

It seems to me that any explanation which attributes the deviation to the influence of a structure such as the rectum, which
occupies a fixed position in relation to the uterus, must be unsatisfactory, in so far as it takes no account of those cases in which the inclination is to the left side.

Another view which falls under this line of criticism is one which I have heard advanced, in which the influence of the liver is credited with the deviation. This view assumes that the presence of the liver on the right side prevents the displacement of the intestines to that side. They are thus crowded to the left, and the uterus naturally inclines to the right. But here again, if this is so, why should—the liver always being to the right—20 per cent. of the cases lie to the left?

Madame Boivin attributed the right lateral obliquity to a difference in the length and thickness of the two round ligaments. The right round ligament, according to her, was shorter and more muscular than the left, and consequently pulled the uterus in that direction. Had this been supported by anatomical evidence, it would have been a more or less satisfactory explanation, because it would have been easy to suppose that in a certain percentage of cases the left round ligament took the characters more usually found in the right. Unfortunately for this view, Pajot and Rambaud found no evidence of any such difference in the length of the two ligaments, as had been supposed to exist by Madame Boivin.

We may, I think, safely conclude that none of the explanations advanced of this deviation of the uterus is satisfactory. But it has been taught that the growth of the pregnant uterus is accompanied by another movement which is not less characteristic and constant. According to this view, which is supported by many authorities, the uterus undergoes a rotation of more or less degree about its vertical axis, so that one border comes forward and the other recedes. It has, further, generally been believed that in all cases where the uterus inclines to the right side of the body the rotation is such that the left ovary advances, and conversely that in left-sided inclination the right advances. This statement, so far as I can ascertain, was first made by Velpeau, but it has been confirmed by many observers since. In the Transactions of this Society, 1892-3, the subject of uterus rotation during pregnancy was discussed in an exhaustive paper by Dr. Haig Ferguson. Dr. Ferguson was led to study the subject by his observing the development of shock during the compression of the uterus post-partum.

This shock he attributed to the accidental compression of the ovary—the ovary having been brought under the grasp of the hand by rotation of the uterus. This paper of Ferguson's was followed by one by J. C. Webster, in which he stated views diametrically opposed to those of Ferguson. Webster not only combated the explanation of the phenomena advanced by Ferguson,

1 Trans. Edin. Obst. Soc. vol. xviii.
but practically went the length of saying that there was no occasion for any explanation at all, as the phenomena explained did not exist. It would not be easy to reconcile such positions; and it would be safer to assume that someone was wrong. I am bound to say that I arose from the perusal of Webster’s paper a sadder (I shall not flatter myself by saying a wiser) man. Regarding clinical methods of observation in this instance as misleading or futile, and arguing mainly from the certitude of frozen sectional ones, he had apparently, and with no uncertain aim, laid another time-honoured obstetric ghost—that of the rotation of the pregnant uterus. Those of us who had long associated with this venerable myth could scarcely be expected to turn our backs upon it without a pang of regret. To those of us who were teachers of obstetrics, its loss was specially affecting. Rotation of the uterus was a pleasant thing to talk about over the lecture-table. Not itself, so far as one could see, of any profound practical importance, we could all the more easily discuss it in a pleasant and entertaining way, without any grave sense of responsibility. A few neat diagrams were at hand to illustrate it, and its production and relations could be further elucidated by a few simple, but convincing, sketches on the blackboard.

Was it to be wondered at that we were parting, with a sigh? At the time, indeed, I was fain to count it as one more of the many relics of the exuberant palæozoic period of theory and practice in obstetrics and gynecology, which had been swept away by the stress of what I may call the “Great Ice Age,” which has held the field of these departments for near a quarter of a century. This, too, like many another cherished tenet, had apparently perished in the chill grasp of the frozen section!

Nevertheless, on recovering from the first shock of the destructive criticism of Dr. Webster’s paper, one could not help wondering whether, in the light of one’s own experience and the widely accepted opinions of others, his methods of observation were really so infallible as to be absolutely conclusive. Was it certain that no rotation existed at all—even if the degree supposed by Ferguson and others did not always exist. An observation which I was able to make myself some time before recurred to me, and did much to cheer me under the depressing negativity of Dr. Webster’s conclusions. Some years ago Dr. Croom performed a Porro operation on a dwarf with a deformed pelvis, at which operation I assisted. I remember very well that after the abdomen was opened, and the lips of the incision separated, Dr. Croom drew the attention of those present to the fact that the left ovary was lying under my fingers, which were holding the wound open on the left side. The right ovary was never seen until the left ovary was removed from the cavity. And I then vividly called to mind the importance of the time-honoured instruction to the principal assistant at a Cæsarean operation, to bring the uterus to the middle
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line, and de-rotate it, in order that the incision be made in the mesial plane of the uterus. Had this incision been made under the line of the abdominal wound, it would have come near the lateral vessels in the left side of the uterus. If a "living dog is better than a dead lion," how much better is a living woman than a frozen section! This certainly was an instance of a rotation with which one had been familiar.

I am not quite clear how the rotation was ascertained by many of the observers, but some of them have determined it by palpation of the ovary, which they could feel as an ovoid tender mass on one or other side of the uterus. Among those who have observed this are Tarnier, Budin, Chaignot, and Ferguson. Webster, on the other hand, does not think it often possible to make out the ovaries by palpation against the soft walls of the uterus, and compliments those who think they can on their extraordinary powers of touch! Nevertheless, these observers are not likely to be so mistaken as he would make out; and in the case to which I have referred, and I daresay in many others, we have had the advantage of visual observation, at which he can scarcely cavil.

But there is another consideration which must not be left out of account. No one who sits round this table will venture to deny that the uterus is an extraordinary organ, when he thinks of what it does and what it endures. But it would be even a more extraordinary organ than any of us are prepared to admit if it did expand during the progress of pregnancy without rotating to some degree in the course of its growth. I am the last to unduly exalt a priori reasoning, but there are instances in which it cannot be ignored. If the uterus expands absolutely symmetrically, it is the only hollow viscus in the world which does—the bladder, the stomach, the intestines, the heart, all rotate during distension, and de-rotate when relaxed, and I think we may safely assume that the uterus will do likewise. I shall hope to show presently that the rotation of the uterus round its vertical axis is to some degree a physical necessity of its growth during pregnancy, but, in order to bring my views of the relation of this rotation to the admitted lateral deviation during pregnancy into line, I may state now that I regard the lateral deviation as a direct result of the rotation. I hope to show that the distortion of the mass of the early pregnant uterus, which is the immediate result of the rotation, results in throwing the uterus to one or other side of the mesial plane of the body, and so determining the deflection to the right or to the left. If this is true, it must follow that rotation to the right must mean deviation to the right, and rotation to the left will mean deviation to the left. Accordingly, the relations of these two conditions, observed first by Velpeau, will, on this assumption, be one of cause and effect.

It is obviously to the construction of the muscular wall of the uterus that we must look for the explanation of this tendency to
rotation. The fibres of this wall, as has been demonstrated by Hélié, are arranged in three layers. The outer or superficial layer is largely longitudinal (Fig. 1, after Hélié). Some run down the anterior and posterior aspects of the uterus; others run outward to the ovarian ligaments, Fallopian tubes, and round ligaments. The general arrangement may be regarded as rectangular. The innermost layer is mainly circular (Fig. 2, after Hélié) in arrangement, forming circular sphincter-like rings round the orifices of the cavity—the os internum and the Fallopian tubes. The general arrangement may so far be regarded as symmetrical. This statement is subject to modification, because an examination of some of Hélié's drawings of this coat shows considerable asymmetry of arrangement.

Between these two layers we have the middle layer (Fig. 3, after Hélié). The characteristics of this coat are—First, its
considerable thickness; and, second, the great intricacy of its arrangement. Its fibres lace and interlace in a very complex way. Some are transverse, some almost longitudinal, and others, the most numerous, are inclined at all degrees between these. Some, beginning near the cervix, can be traced upwards in a sort of spiral towards the fundus and outer angles of the uterus. A glance at any of Helicë's or Luschka's figures will show the extreme complexity of their arrangement.

Turning now to consider the mode of expansion of the uterine walls, under any condition in which the smallest increase of internal pressure plays a part, we can readily see that the arrangement of the fibres in these three layers must play a very important part in determining it.

So far as the internal strain is taken up by longitudinal, transverse, or symmetrically oblique fibres, the viscus will expand without any appreciable alteration of the contour or development of distortion. Such a viscus might be assumed to expand with much the same changes in outline as an indiarubber or elastic balloon, whose walls are homogeneous. But when we consider the relations of the middle coat, the conditions are very different. The distribution of strain in such a coat as this must be a highly complex matter, the intricacies of which would baffle analysis. On two points we may be fairly clear—

1. That it is inconceivable that the fibres are so symmetrically arranged that the strain will be symmetrically distributed. In other words, can we imagine that the two halves of the viscus, separated by a vertical mesial plane, can have the fibres of the mesometrium so adjusted, in spite of their complex arrangement, that the resistance of each to internal pressure is identical in amount and distribution?

2. The second point follows from the first. Unless we assume that this symmetrical distribution of strain is possible, it must follow that as increase of internal pressure arises it will produce an alteration of contour and a change in the distribution of the mass of the organ about a vertical axis. In the former case we shall have a certain amount of distortion produced; in the latter, a certain amount of rotation.

Now, though it is practically impossible to analyse the influence of the various groups of fibres in any particular case, it is easy to see in general terms the arrangement of fibres which will tend to a distortion of the organ and that which will tend to rotation.

1. Suppose, for example, we take a group of fibres, whose general arrangement is sensibly transverse. Let us assume that this group is arranged in a fan-shaped manner (Fig. 4). Many such groups can be seen in one of Helicë's drawings. The influence of this group at the region a will be much more powerful in preventing expansion than at the region b, where the fibres are
spread out; and, assuming that expansion occurs, it will result in a tendency to distortion in the organ at $b$.

2. Such a tendency to distortion might, however, conceivably be met by the influence of a similar bundle arranged in an opposite fashion, i.e. with the spread of the fan at $a$ and the apex at $b$, so that expansion might take place without distortion. This may be rather large an assumption, but it may be admitted in the meantime.

3. But now let us suppose that the point $a$ in the first diagram is shifted more towards the cervix, and the region $b$ shifted towards the fundus (Fig. 5), the influence of the band in the behaviour of the wall, though more complex, is still easily intelligible. We can see that while the region $b$ will, as a whole, tend to move away from the point $a$, it will do so to a different extent at its two limits $c$ and $d$. For the fibres at $d$, being inserted more obliquely into the mass of the wall, will have less restraining influence than those at $c$, which are acting at more nearly a right angle, and so at greater disadvantage. Hence the point $d$ will move to a greater extent than $c$. But $d$ being a point in the curved wall of the uterus, its movement will be limited by that surface; and we have here the primary elements of a rotation round a vertical axis (Fig. 6).
The arrangement of a very considerable portion of the middle layer of the uterus must, from the shape of the organ, correspond with this theoretical assumption. Bundles of fibres condensed at the lower end of the organ will expand towards the fundus; and unless we imagine the arrangement to be identical on either side, the behaviour of the whole viscus must follow the conditions determining the movements of the parts c and d. For such a symmetrical arrangement there is no anatomical basis, and it may be said to be physically inconceivable.

Admitting the rotation, we have available an easy explanation of the tendency to deflection or deviation from the middle line which is so universal. For with rotation about the vertical axis there must result a redistribution of the mass of the viscus round that axis. In the unrestrained condition in which the uterus lies in the pelvis, this will result in a displacement to one or other side, depending upon the direction of the rotation. Thus, when the uterus rotates so as to bring the left side forwards and towards the middle, the uterus will incline towards the right, and when the rotation is in the opposite direction the inclination will be to the left.

Such an explanation not only meets the fact pointed out by Velpeau, that this relation between rotation and deflection always occurs, but it also offers an easy explanation as to why certain uteri incline to the right, and others, though a minority, to the left. It is only necessary to suppose that the direction of the spiral is different in the two cases. In the majority of uteri it is inclined so that the uterus turns to the right, in the minority to the left. We may, I think, thus conclude that the influences determining rotation and deviation are essentially internal and not external.

My attention was specially directed to this subject about two years ago by some observations which were forced on me by what was, in my experience, a unique series of cases. During 1894 I attended nine cases of labour in which the vertex presented O.D.P. The curious thing was that eight of these were in unbroken succession, one normal case intervening between the eighth and ninth. With one exception all were primipares. In all, forceps were used. In four of these rotation occurred before the application of forceps, in two it occurred after application, and in three the occiput was delivered persistent. The first case was that of a highly nervous primipara whose powers were vigorous enough, but who was terribly restless during the early part of labour. Rotation began satisfactorily, and after the occiput had come round to O.D.A. I delivered with forceps. The uterus showed some signs of relaxing after the birth of the placenta, though there was no haemorrhage, and I left the binder off. I remained in the house about an hour and a half, and before leaving examined the uterus, when I noted that it was well con-
tracted and quite responsive, and lying well over to the left iliac fossa. I told the nurse to apply the binder lightly. For the next four days the uterus still lay to the left side, but less distinctly so each day. On the fifth it was still well above the pubis, but now mesial. On the following morning I happened to call earlier than usual and found the patient newly awake, having had a restless night, for some reason. On examining the abdomen I found the uterus far up in that region, pushed completely over to the left side, evidently by the distended bladder. When the bladder was emptied, the uterus became mesial and sank to its normal place. This case interested me in the question of uterine deviation, and, as I have stated, I had special opportunities of observing its behaviour in occipito-posteriors, for the next seven cases I had were all of this order.

The interesting point is this, that in every one of these the uterus lay, after labour, to the left side. This deviation was observed over and over again in the early days of the puerperium, both with the bladder full and with it empty. A full bladder always increased the deflection, and even in the latter days of the puerperium, when the uterus tended to become mesial, the left inclination was re-established by a full bladder. After eight successive cases of occipito-posterior I had a normal one, and in her the post-partum uterus lay to the right. My next was an occipito-posterior, in which left deviation again occurred. Since then I have in private and hospital practice noted other fifteen cases of occipito-posterior, making twenty-six cases in all. All these I have examined during labour, so as to assure myself of the actual position. In all but two of these, left deviation of the uterus was absolutely demonstrable. In eighteen, the deflection was quite evident in any condition of the bladder; in four, the uterus lay mesially with the bladder empty and to the left when full. In two cases the uterus seemed to be mesial, and did not show any lateral deviation at all. One of these, a private case, was very fat, and it was difficult to make out the uterine outline at all. The other, a Maternity case, confined during September, had a difficult forceps labour and developed cellulitis on the second day. This possibly fixed the uterus. During the periods in which I have had charge of the Maternity Hospital, in 1895-96, I have over and over again demonstrated to students this relation between uterine deviation and occipito-posterior positions—a relation which I have come to regard as normal.

I think one may take it for granted that, when out of twenty-six cases of occipito-posterior position, eighteen show definite left-sided deviation after labour, there must be some well-defined causal association. Such uniformity of relation is more than can be accounted for by mere coincidence. An association of left-sided deviation with occipito-posteriors has been indicated by Schroeder and Stratz, who gave it as their opinion that the side of the
uterus which corresponds to the child’s back rotates forward, and, presumably, will deviate accordingly. They would seem to imply that the position of the child was the cause of the deviation. But the grounds on which they do so do not seem quite clear.

Chaignot, on the other hand, found that in thirty cases, twenty-one were O.L.A. with right rotation, and three were O.D.P. with right rotation, while two only were O.D.P. with left rotation, i.e. only half his posterior cases rotated left. Ferguson gives it as his opinion that posterior cases do not rotate more frequently to the left than to the right, but does not state the grounds on which he comes to this conclusion.

From what I have said previously, it will be seen that my direct observations point only to the relation between posterior positions of the occiput and deviation of the uterus from the mesial line, and in these cases there seems no ground for doubting, if my observations are correct, that there is a close relation between them. I made no clinical investigation as to the presence or amount of the rotation. It is certainly an interesting fact that there is a fairly close numerical coincidence between the proportion of cases of occipito-posterior position and cases of left lateral deviation—each of these may be put down as representing from 20 to 30 per cent. A numerical value of this sort, derived from observations made quite independently of each other, seems to be of considerable importance.

On the causal relation of these two phenomena, of posterior position and left lateral deviation, I do not at present propose to enter. That such a relation exists goes without saying, but as to whether the position causes the deviation, or the deviation the position, I confess my mind is by no means clear. I am content at present to state the facts, reserving their co-ordination for further discussion and observation.

I may, in conclusion, be permitted to briefly summarise the points to which I would direct attention.

1. Deviation of the post-partum uterus from the mesial plane is the rule.
2. This deviation is most frequently to the right side of the body.
3. Rotation of the uterus in a vertical axis during growth seems a physical necessity of its structure.
4. This rotation may offer a mechanical explanation of deviation.
5. In the occipito-posterior cases observed, the deviation was almost always to the left.
6. There is an approximate numerical coincidence between the proportion of cases of occipito-posterior and left lateral deviation.