Practical Observations

ON

"SUPPORTING THE PERINÆUM."

BY

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The Causes of Laceration of the Perinæum in Labour (continued).

The other element now remaining for consideration; viz., *the condition of the perinæum itself*, as a cause of laceration, merits our very best attention, for the reason that a great degree of importance has been given to it—whether deservedly or not, it is now my purpose to inquire.

As a cause of laceration, we frequently hear "*rigidity of the perinæum*" spoken of. What does this mean?

In the phrase "*rigidity of the perinæum*," as it appears to me, we find an illustration of the manner in which the true knowledge of a subject is often impeded and obscured by the use of an apparently simple but worthless expression. The phrase "*supporting the perinæum*" I have had occasion to find fault with; and for a like reason the phrase now under consideration seems to me to be objectionable, and obstructive to all true advance in the knowledge of the subject. What is "*rigidity*"? As concerns the perinæum, the physical condition of the parts to which it is applied is a hardness, a resistance, and, it is asserted, a want of dilatability of the soft parts. Now, if the perinæum be examined before labour has commenced, it will be found that the physical condition is precisely that characterised by the foregoing expressions. It is hard, dense, generally thick, and, above all, resistant. What is "*rigidity*" of the perinæum, more than a persistence of this condition? From the manner in which it has been constantly spoken of by obstetric authorities, it has come to be considered, however, as a positive entity, as a kind of disease with which the parturient process is liable to be affected in certain cases, for which this or that remedy should be used, and its removal thus effected.

The results of my own observation have led me to believe that most of the so-called cases of "*rigidity*" are cases in which the perinæal structures are rigid, simply because they have not yet been put on the stretch. Cases of "*rigidity*," it will be recollected, are most common in primiparæ. In such cases, the expulsion of the head through the pelvis and vagina occupies a long time. A period arrives when the head is low down; the attendant imagines that the further descent is arrested by the perinæum; and, finding this dense, thick, and resisting, as the perinæum naturally is, the case is forthwith called one of rigid perinæum, and means are in the next place devised for the purpose of overcoming the rigidity, and of preventing the anticipated laceration; the fact being, that the period of liability to laceration has not even arrived.

We hear of the labour being arrested in its pro-

gress for hours together, owing to the rigidity of the perinæum; but the mere statement to this effect is insufficient to substantiate it. In my own experience, limited as it has been, I can recall cases in which the delay would have been attributed, on superficial examination, to the cause here alluded to—"rigidity of the perinæum". The event of such cases, however, generally convinced me that the real cause of the delay was not really the resistance of the perinæum, as it at first appeared to be; and from these and other considerations, it seems most reasonable to conclude that most of the so-called cases of rigidity are really cases of impeded labour, due to deficient power in the contractions of the uterus; or to the circumstance that the presenting part of the child is angular, irregular, or so placed as not to press in the proper manner on the perinæum; or, lastly, to difficulties encountered in the passage of the child through the pelvis itself.

The term "*rigidity*" ought evidently to be restricted to those cases in which the head of the child, having had time and opportunity, so to speak, for producing an effect on the perinæum, has failed to do so. I have already pointed out how necessary for the production of expansion of the perinæum it is that the stretching force should be applied equably and gradually. Unless it be so applied, the perinæal structures cannot be said to have a proper opportunity for dilatation. It is only when the head has begun to emerge, and has actually separated the labia so as to be visible from the outside, that the edge of the perinæum can be said to be really subjected to the dilating action of the head's pressure—at least, in ordinary cases. Now, where the head is delayed for a long time at the aperture itself, and has advanced so far that any difficulties in the passage of the pelvis itself must have been overcome, supposing them to have existed; where the position and shape of the head are natural, the uterine contractions strong; where the perinæal structures continue dense, firm, and unexpanded,—it is only in such cases, and under such circumstances, that "*rigidity*" of the perinæum can be properly said to be present. Such cases are extremely rare; but it is easy to see how ordinary cases of delay might be mistaken for them.

On this view of the case, the perinæum would be considered as a passive agent in the matter, and its "*rigidity*" or dilatability a matter of accident, or determined at least by conditions of other elements and agencies concerned in parturition—the uterus and the foetus itself.

Some writers—Cazeaux, for instance—attempt to explain the so-called "*rigidity of the perinæum*" by saying that the muscles of the perinæum are in a state of spasmodic contraction. That the perinæum is provided with muscles is certain; but it is not so evident how spasmodic contraction of these could continue so persistently, so undeviatingly, if we may use such a term, as to constitute the condition actually met with in practice, and to which the explanation professes to apply. But admitting the possibility of such spasmodic action of the perinæum, it would surely be soon overpowered if the force of the uterine contractions were allowed to play upon it continuously; the persistence of such spasmodic action would seem to be indicative of *absence* of uterine contraction.

Again, we hear congestion and inflammation of

the perinæum spoken of as conditions liable to arise, and which, interfering with dilatation, justify "support" of the perinæum. This appears to be Dr. Murphy's view of the subject (see his letter in *BRIT. MED. JOURN.*, Ap. 20, 1861). With all due deference to such an eminent authority, I would submit that there is no evidence of the existence of such conditions in the cases where laceration has been known to occur. The labour in such cases has generally been too rapid for anything that could be properly called "inflammation" of the perinæum to have supervened. Neither can I allow "congestion" a place in the list of causes of laceration, or that it can be considered, properly speaking, as capable of interfering with the process of dilatation. If the position of the head were such as to allow of equable pressure on the perinæal structures, any congestion which existed would be speedily removed by the mere pressure from above. The other elements involved in the question at issue, and alluded to by Dr. Murphy, I have already fully considered in the first three papers of this series.

There are a few cases in which the condition of the perinæum and of the vaginal outlet is such that laceration may be considered to be almost inevitable. I refer to cases where the vaginal outlet is *congenitally very small*. Remarkable as are the stretching capabilities of the skin and tissues of this part of the body, they have their limit; and if the ostium vaginæ be really very small, laceration may occur when this limit has been reached. These cannot be considered as cases of "rigidity". Also, if the outlet have been *artificially narrowed*, the result may be that laceration will occur. In both these cases, laceration would more certainly occur if the uterine contractions were unusually powerful or rapid in their succession.

It has been said that, in some cases, the perinæum has given way in consequence of the textures of which it is composed having undergone *fatty degeneration*, or become weakened by disease. There is no evidence that this can be considered as actually constituting a cause of laceration; and laceration occurs, it must be remembered, more frequently in primiparæ, in whom the perinæum might be reasonably expected to be particularly sound.

Extraneous Causes. There are a few causes of perinæal laceration which require a place for themselves, and are not capable of being easily brought under any of the previous categories. Thus we have, in the first place, those cases in which the perinæum is torn in a *rapid labour*, because the head is *not allowed to move forwards*. If the legs be forcibly kept in apposition, the thighs being well rounded and well covered with flesh, while, at the same time, the legs are kept in a straight line with the trunk, the head of the child, at the same moment emerging from the vulva, would find its natural course impeded; the consequence would be that an undue stress would be laid on the perinæum. Such an occurrence is related by Dr. Archibald Hall, in his late *Report of the Lying-in Hospital, Montreal*. There was spasmodic rigidity of the lower extremities; and the patient "closed the thighs on the head of the fœtus when it was passing the perinæum"; a considerable laceration followed. (*Brit. Amer. Jour.*, May 1860, p. 197.) This case is analogous to those in which artificial delivery by the forceps, or by extraction after version, is performed, and in which, the

operator not taking sufficient care to exercise traction in the proper direction, the perinæum is lacerated almost as a matter of course. The same result may follow in ordinary labours during the extraction of the shoulders, if the operator do not take care to exercise his traction in the proper direction.

Another extraneous cause of laceration is, according to Dr. Leishman, *the artificial support given to the perinæum*. The arguments in favour of this view of the question have been already alluded to, and need not be, therefore, repeated. I do not think that, practically speaking, many cases of laceration really do proceed from this cause, for the reason that the support, as ordinarily administered, is not generally sufficient in degree to produce all the bad effects which might be produced if more force were applied; but I quite believe that, were the practice recommended by Dr. Ramsbotham carried out universally, we should hear much more frequently of laceration taking place. Those in favour of supporting the perinæum adduce the fact that severe laceration has sometimes occurred at the last moment, *owing to the temporary or accidental discontinuance of the support*. Of the actual occurrence of laceration under these circumstances, there can be no doubt. Smellie records a case of this kind in his own practice. In his letter in this *JOURNAL* of April 20th, Dr. Murphy defends anew the practice of "supporting the perinæum," on the foregoing argument. To this argument, it has been well replied by Dr. Leishman that it is the artificial treatment of the perinæum which produced the laceration in such cases. The equable and uniform dilatation of the perinæal structures having been interfered with, the laceration occurs because the expulsive action of the uterus, being no longer opposed, effects in a moment what naturally it would have occupied some considerable time in accomplishing. To the force exercised by gradual dilatation, the perinæum can accommodate itself: how and why, I have already explained.

The use of instruments. That laceration of the perinæum is very frequently due to the use of instruments, such as the forceps, there can be no question. Incidentally, I have already had occasion to remark upon this fact. I have stated my belief that the use of the forceps and the laceration are not, however, by any means necessarily connected. With proper care, the use of the forceps need not be dangerous to the perinæum. The danger is in exercising traction in the wrong direction. If traction be exercised downwards, laceration is and must be, unless the perinæal structures be most unusually distensible, almost inevitable. If the perinæum is to be maintained in a state of integrity, the forceps must only be used by one well acquainted with the mechanism of parturition, and who has studied the operations of nature closely enough to imitate them. The accidents accruing to patients from the use of the forceps by individuals who have never taken the pains to master the rudiments of the obstetric art, have brought a discredit to the instrument which ought, in many cases, to attach to the operator himself.

The chief conclusions to be drawn from the foregoing account of the causes of laceration of the perinæum, are—that laceration is more likely to occur in primiparæ, especially when advanced in

life: and that a not unfrequent cause is rapidity of labour, especially when associated either with want of proper relation between the child and the maternal passages, or with malpresentation on the part of the child.

It is perfectly useless to attempt to generalise more completely respecting the causes of laceration, for the reasons already stated. Laceration of the perinæum is not constantly produced by the occurrence of the same circumstances. In one case, one combination of circumstances produces it; in another case, the same combination being present, no laceration occurs. Unquestionably, the causes of laceration are many and various; and this is one of those cases, not so uncommon as is usually supposed, in which it is not in the nature of things that "simplicity" should be attainable, however much it may be desired. In such a complex process as is involved in the expansion and dilatation of the perinæum, not one, but every element in the case must be duly weighed and allowed for, in arriving at any conclusion as to the cause of the laceration.

[To be concluded.]

Clinical Observations

ON A

CASE OF DISEASE OF THE CORPUS STRIATUM.

OCCURRING AT THE LIVERPOOL NORTHERN HOSPITAL.

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THE uncertainty which still exists in our knowledge of the functions of different portions of the nervous centres, and more especially of the chain of ganglia which are placed at the base of the brain, and the conflicting nature of the symptoms that are met with in connexion with the pathological conditions of these structures, render it desirable that all cases possessing more than ordinary interest, and which may eventually serve to illustrate some of the more abstruse points of neuro-physiology, should not be lost sight of. The following case appears to me of sufficient importance to deserve being placed on record.

CASE. George Gibson, aged 43, a man of colour, cook on board a vessel, was admitted into the Hospital on April 11th, 1860. He had previously been a patient in the Hospital, suffering from an attack of pleurisy, from which he recovered, and had been discharged from the house six weeks before the time of admission mentioned above. It appeared that, whilst under treatment for the pleurisy, he was profusely salivated with a single small dose of mercury. After his discharge, he did not follow his ordinary occupation, but remained unemployed in lodgings in the town. The only symptoms he was troubled with, he said, were those of general weakness. About eight or nine days before admission, he began to feel a want of power in the left arm and leg. This symptom gradually increased, and was attended with pain in the head.

On admission, there was no hesitation in his manner; he was able to speak distinctly, and to answer questions readily and well. He gave a clear and intelligible account of the accession and progress of his symptoms, of

which he seemed to think very lightly. There was almost complete paralysis of motion of the left arm and leg; the tongue was protruded slightly to the left side, and the mouth slightly drawn to the right. He could close both eyes, but was unable to open the right one fully. Sensation was perfect on both sides, as well on the side where there was loss of power as on the other. He complained of pain all over the right side of the head. The pulse was 50, and very laboured. He was ordered some decoction of aloes, a turpentine enema, and a little brandy. He continued in the condition above described up to the 14th, early in the morning of which day he was found in a perfectly comatose state. At midday, the pulse was 54, irregular and laboured. He could not be roused. He was breathing noisefully; but, when disturbed, stertor came on. He died at 3 P.M. of the same day.

AUTOPSY. On removing the calvarium, the surface of the brain was seen to be somewhat pale. There was no fluid in the arachnoid. The brain-substance, with the exception of the parts mentioned below, was firm and apparently healthy. The right lateral ventricle contained a quantity of semifluid matter, consisting of disorganised brain-substance mixed with blood. The walls of the ventricle were rough and flocculent, and marked with numerous vascular spots. The substance of the hemisphere around the ventricle, to the extent of a quarter of an inch in thickness, was softened so as to be readily washed away with a gentle stream of water. The right corpus striatum was softened and disorganised throughout, and effused blood was mixed with the broken up brain-substance. The rest of the brain, including the substance of the optic thalami, was healthy.

It has been the opinion of some physiologists, that the corpora striata constitute the centres from which motor influence of a voluntary kind is derived; that the will, generated in the grey matter of the hemispheres, acts through the medium of these centres. Supposing this theory to be true, anything interfering with the transmission of the mandates of the will to the corpora striata ought to be followed with more or less paralysis of motion, even supposing those bodies to remain healthy. On the other hand, disease of the corpora striata, leaving untouched the remaining portions of the brain, ought to produce the same result, although from a different cause. Physiologists who have attributed the above mentioned function to the corpora striata, have generally considered the optic thalami as the centres of sensation. Were all cases as definite in their symptoms and *post mortem* appearances as the one detailed, some ground would be afforded for our belief in a part at least of the theory; for we had here destruction by a gradual process of one corpus striatum, the thalamus remaining intact, with paralysis of motion, sensation being perfect. Unfortunately, however, for the theory, such results are not constant; disease of the thalamus being sometimes attended with loss of motion, and disease of the corpus striatum with loss of sensation. Further, it is very difficult to judge in any given case whether the symptoms produced, either of loss of motion or of sensation, may not be the result of reflex action, rather than directly caused by the disease itself; for, as has been pointed out by Dr. Brown-Séquard, when we consider the variety of symptoms, or the total absence of symptoms, which may accompany diseases of the brain apparently of a similar character and having a similar seat, it is impossible to conclude that all the symptoms which arise are directly produced by the