REMARKS ON ADHESION OF THE PLACENTA.

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ADHESION of the placenta is one of those complications of labour which are especially dangerous in the hands of an unskilful accoucheur. Both the diagnosis and the treatment of it demand from the obstetric practitioner the exercise of the *tactus eruditus*; and, if this be wanting, the most disastrous consequences may ensue. The patient may die either quickly from hæmorrhage, or more slowly, but not less surely, from septicæmia. Moreover, adhesion of the placenta is not unfrequently the cause of those frightful cases of malapraxis in midwifery which lead to coroners' inquests. For instance, an incompetent midwife, either male or female, sets about removing what is supposed to be a retained placenta, and, in the rashness of ignorance, tears away the entire uterus, and perhaps a considerable portion of the intestines. It is most desirable, therefore, that we should ascertain the causes of a complication that may be so hazardous, and use every means in our power to prevent it. But here, it must be confessed, our knowledge is very much at fault; the diseases, both of the fœtus and of the placenta and membranes, are at present veiled in the greatest obscurity. Nor is our knowledge of the pathology of the gravid uterus much more advanced. Most obstetric authorities who have gone much into the etiology of morbid adhesion of the placenta have been disposed to attribute it to a subacute inflammatory affection of the lining membrane of the uterus, most probably of a rheumatic character. Others, again, such as the late Sir James Simpson (who has written a very able memoir on "Congestion and Inflammation of the Placenta"), are of opinion that placental adhesion is a result of inflammation of the placenta, or placentitis. Sir J. Simpson remarks: "The effusion or secretion of coagulable lymph as a termination of placental inflammation is best known by the effect to which it not unfrequently gives rise, of producing more or less intimate and extensive adhesion between the uterine surface of the placenta and the inner corresponding surface of the uterus, constituting a morbid state of the after-birth that frequently forms a most formidable and dangerous cause of uterine hæmorrhage after the expulsion of the child."

I have never had an opportunity of making a *post mortem* examination of an uterus in a case of adherent placenta either before or after delivery, and therefore I am unable to say from my own observation how far the lining membrane of the uterus is implicated in the formation of the adhesions in question. My own experience of these cases, however, inclines me to believe that Sir James Simpson's view of the origin of the adhesions is the more correct one. I have met with a good many cases of adherent placenta since I have been in practice, but yet I cannot call to mind one in which this complication was preceded by very definite symptoms of uterine inflammation, or by anything beyond vague and uncertain pains in various parts of the abdomen, which the patient, on being questioned, stated that she had felt some time or other during her pregnancy, but yet which were not sufficiently severe to cause her to seek medical relief. I have always made a point of carefully examining all the adherent placenta which I have at different times removed, and I find the following morbid appearances very constantly present. In slighter cases, the whole of the placenta on its amniotic surface is studded with spots having some resemblance to tubercles. They are usually more abundant near the circumference of the placenta, are very slightly elevated above the surface, and appear to consist on examination of small patches of lymph effused between the placenta and its amniotic covering. In the more severe cases, the effused lymph forms a tolerably firm consistent layer beneath the amniotic surface. This layer is much thicker and more marked towards the circumference of the placenta. It is of a pale yellow colour, and in this respect presents a remarkable contrast to the deep red colour of the placenta beneath. This contrast in colour is shown in the accompanying drawing, which I took from a very well marked specimen of adherent placenta. The specimen has been preserved in spirit, and has, in consequence, quite lost the characteristic differences in colour which are represented in the drawing. In strongly marked specimens, the whole placenta is much reduced in

bulk; but this shrinking is most evident on the amniotic surface, near the circumference; and this causes the placenta to appear concave or cupped, so that it has a very striking resemblance, both in shape and colour, to a highly buffed and cupped coagulum, such as we used so often to observe in the days when it was customary to bleed for acute rheumatism. I have seen but few specimens of retained placenta in which this yellow layer seemed to dip down for any distance between the inner surface of the placenta and the inner surface of the uterus; nor have I seen any in which it presented at its circumference a torn and jagged edge, as if it had been continuous with a similar layer on the free surface of the uterus. As far as I have observed, a contracted placenta of this kind does not appear to impair the nutrition of the child. This remark applies especially to the case of one of my patients who had placental adhesion in several consecutive labours. Although this was always accompanied with very marked contraction of the placenta, yet the children were all of fair size and well nourished. I should mention, also, that the yellow layer I have just described may be present without producing adhesions. This I once observed in the case of a patient of mine who had had adherent placenta on two previous occasions. Although on these occasions I had considerable difficulty in detaching the placenta with my hand, yet, in the next labour, it was expelled, apparently with ease, by the natural efforts, but presented in a very marked manner the characteristic morbid appearances which I have described.

The microscopical appearances of this yellow membranous layer have been well noted by my friend and colleague Dr. Martyn, who kindly examined for me a portion taken from the specimen which I have exhibited. He found it to be "a layer of fibrinous lymph of uniform texture, firm and white, and tearing with difficulty into fasciculi. A section shows fibrinous lymph very granular, and interspersed with the usual corpuscular elements, which are here and there in a state of advanced fatty degeneration, showing that the deposit is of considerable standing."

The frequency with which adhesion of the placenta occurs is a matter which is yet somewhat undecided, and upon which the experience of various practitioners yields somewhat opposite results. The more general opinion seems to be that it is a rare complication of labour. Dr. Barnes, in his *Lectures on Obstetric Operations*, and Dr. Leishman in his *System of Midwifery*, both express this opinion. The records of obstetric practice in Dublin also seem to point to the same conclusion. Dr. Churchill, in his very interesting and valuable "Report of Private Obstetric Practice for thirty-nine years" (*Dublin Journal of Medical Science*, June 1872), states that, out of a total of 2,547 cases attended by himself, there were only ten "in which extraction was necessary for different reasons, such as flooding, irregular contraction, and morbid adhesion." The number of cases of morbid adhesion is not stated; but if we take them at a third of 10, or $3\frac{1}{3}$, this would not give a percentage of 2 in 1,000. In Dr. George Johnston's very careful and elaborate report of the Rotundo Lying-in Hospital for the year 1874 (*Dublin Journal of Medical Science*, February 1875), I find that, out of a total of 1,236 deliveries, there were only two instances of retention of the placenta, or not 2 in 1,000.

The result of my own experience is very different, and, in some respects, so curious, that I must refer to it somewhat in detail. Before doing this, I must premise that, ever since I have been engaged in midwifery practice, it has been my habit to write short notes of every case I have attended immediately after it was over, and not to trust to my memory some time afterwards for details. I must also state that I have considered no case to be one of adherent placenta in which it was not necessary to introduce the hand into the uterus, and separate the placenta from it by a troublesome process of peeling. Since I have been engaged in obstetric practice, I have met with no fewer than forty-three cases of morbid adhesion of the placenta; but, singular to relate, none of these occurred during the early years of my practice. For instance, during the first ten years, from July 1838, when I attended my first midwifery case as a student, up to July 1848, when I had been six years a qualified practitioner, I never met with a single case. During that period, I attended myself 264 labours, besides having the superintendence of several hundreds more, which were attended by the students of the Bristol Medical School. I had many times during this period been sent for by students to remove detached placenta from the vagina, but never to take away an adherent placenta. In fact, the first case of this kind that I met with was in 1849. From 1849 to 1865, I have only noted six cases of placental adhesion. But in the last ten years (commencing in April 1865, and ending in April 1875), I have met with not fewer than 37 out of a total of 576 cases attended in the same period. Thus, in the first ten years, not a single case is recorded; and, in the last ten years, a percentage of rather less than seven. This is certainly a most startling difference; and I must now try to account for it. The most natural solution of the difficulty would be this: that, during the last ten years, when a medical man has acquired age and experience, he

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is, of course, called in to a number of difficult cases by other practitioners, but that he would not be so called in during the first ten years of his obstetric career. To some extent this is true; yet it will not account for the great difference just spoken of. In the first place, although I was scarcely ever consulted by other practitioners in my first ten years, yet I was called in a great deal by medical students, whose cases I superintended; and in the next place, I find that only eight of the thirty-seven cases met with in my last ten years were consultation cases. We may endeavour to account for the discrepancy by supposing that, as every twenty or thirty years there appears to be a change in the type of many other diseases, so adhesion of the placenta, after being little heard of for a time, has reappeared with great frequency of late years. But this theory is upset by the statistics which I have quoted from Dublin which show that there, at all events, this complication has not become more common. Here, however, it is common, as far as my experience goes; and I am sure that many present, who have had much experience in midwifery, will bear the same testimony. My own statistics disprove the idea that the disease is in any way endemic in Bristol, or, at all events, they show that it can only have become so very lately. Is there anything, then, in the station and habits of life of the patients to explain so great a difference? Here I believe we shall find the solution of the problem. The 264 patients attended by me in the first ten years of obstetric practice were nearly all very poor, or ought to have been very poor, for I attended them gratuitously in connection with various lying-in charities; only thirty-seven out of the number being private patients, who paid me for my services. During the last ten years, the 576 cases recorded, with very few exceptions, belonged to the upper and middle ranks of society. Moreover, I have carefully noted the names and addresses of the forty-three cases of placental adhesion, and I do not find one that could be called poor. All were in easy or even affluent circumstances.

The facts just mentioned lead me to believe that placental adhesion arises from a depraved condition of the blood of the mother, analogous to what is considered to exist in a rheumatic or rather gouty diathesis; and that this peculiar state of the blood is induced by a too stimulating and nutritious diet, combined, perhaps, with too little exercise. I do not think that the abuse of fermented liquors, or syphilitic taint, has anything to do with it; for those causes would be more likely to be in operation amongst the lower classes; and in nearly all the forty-three cases I have recorded, there was not the least reason to suspect that either of these causes existed. I believe that too many meals in the day, and too much animal food, are the real causes; and what we know of the habits of society in the present day tends to favour this idea. We most of us eat too much and too often. Instead of three meals a day, which ought to be enough for any one in ordinary health, most people take four, or even five; and instead of eating meat at most twice a day, many will eat it three or even four times, and say that they cannot keep up their strength on less; and this idea is being continually impressed on the minds of ladies either nursing or pregnant. These habits, combined with the small amount of active muscular exercise which married ladies usually take, are quite sufficient to produce the peculiar diathesis to which I have alluded.

For the reasons I have already stated, I have come to the conclusion that adhesion is rather due to disease of the placenta than of the uterus, and that it is mostly met with in the rich and well-to-do class. In this respect, I must differ *in toto* from Dr. F. Ramsbotham, who states that "adhesion of the placenta is more frequent in the lower classes than in the higher circles; and this is easily explained by the greater liability of the poor to such accidents during pregnancy as are likely to induce inflammation in the uterine structure, and which may terminate in the agglutination of the two surfaces together".

In accordance with the theory I have put forth as to the effect of high living in the production of adherent placenta, I can easily explain why there should be only two cases of it in the 1,236 deliveries at the Rotundo Hospital, Dublin; but I cannot account for the small number met with by Dr. Churchill in private practice. It is very possible that the upper and middle classes in Ireland do not, as a rule, take so much animal food as they do in England; or there may be some other important difference in their habits of life. The labouring class certainly take much less than the same class in England.

There are a few other points of interest in my record of cases which I must speak of very briefly. Thus, twelve of the forty-three cases were primiparæ, and thirty-one multiparæ. Amongst the latter, there were five cases in which the placenta was adherent in more than one confinement. Thus I had to remove it twice in four patients, and five times in one. In this case, I had to remove the placenta for adhesion only in the third and fourth confinements, for hour-glass contraction only in the fifth, and for both hour-glass contraction and adhesion in the sixth and seventh. The placenta in some of these instances were

more diseased than any others I have examined. The patient to whom this occurred was the wife of a shopkeeper who was doing a very large and respectable business, and was in very comfortable circumstances. For some months before her confinement, her countenance wore a very sallow cachectic aspect; but I do not think she ever had any decided symptoms of gout or rheumatism. This and the other cases I have just given show (what has often been remarked before) the great tendency which adhesion of the placenta has to recur.

In eighteen cases out of the forty-three, the adhesion of the placenta was complicated with hour-glass or irregular contraction of the uterus, and in twenty-five it was uncomplicated.

There were two deaths in the forty-three cases; but in neither of these did the fatal result seem to be directly traceable to the hæmorrhage or septicæmia, which are the usual sources of danger from adherent placenta. In one case, the placenta was enormously enlarged, and apparently in a state of fatty degeneration. The patient was suffering from jaundice and albuminuria, and in such a precarious condition that it was necessary to induce premature labour at the seventh month. She died from shock within two days after delivery. In the other case, the patient had symptoms of ruptured uterus. She had given birth to a child with hydrocephalus, which had been expelled by three or four violent pains. From the time of the child's birth, she suffered the most acute pain, and was in a state of great prostration; and this continued until her death a few hours afterwards. There was no hæmorrhage of consequence; and, when I introduced my hand to remove the placenta, I could detect no rent in the uterus. Unfortunately, I could not succeed in obtaining a *post mortem* examination.

The *diagnosis* of placental adhesion is a matter of considerable uncertainty, unless the hand be introduced into the uterus, so as actually to ascertain by the touch the nature and extent of the adhesion. Before this is done, we can only form probable conjectures. We may suppose that there is morbid adhesion, if, after the child is born, on placing the hand over the fundus uteri, the uterus can be felt contracting repeatedly and even energetically without any apparent result; and this presumption is strengthened if the cord, on gently pulling it and then letting it go, be observed to spring back as if the placenta were tightly held by the uterus, especially if we can feel that there is no constriction of the circular fibres of the uterus to account for this. It should be borne in mind, however, that the presence of irregular contraction of the uterus is no proof of the non-existence of placental adhesion; for in some of the worst cases these conditions coexist. The introduction of the hand is equally necessary in either case, and soon removes all doubt as to the cause of retention. The existence of hæmorrhage proves nothing, as this may accompany retention of the placenta from any cause.

The *treatment* of adherent placenta should be prompt, because this complication is generally accompanied with hæmorrhage, which may speedily become dangerous. It is never safe to leave such cases long to the unaided powers of nature, or to rely on ergot, styptics, or any kind of drug. The hand of the accoucheur supplies the only safe and effectual remedy. To employ this remedy properly, some coolness and dexterity and a considerable amount of patience are required. The hand has sometimes to be kept in the uterus for half an hour or more before the operation can be completed. In the ordinary obstetric position, the woman lying on her left side, the left hand will be found to be much the most convenient for this purpose; but, to perform the operation properly, both hands should be used. The right hand applied externally over the fundus uteri steadies the uterus, and gives important help to the other hand during these manipulations. If, in addition to placental adhesion, there should be hour-glass contraction of the uterus, the case is rendered more tedious and difficult; for the circular constriction has first to be overcome before the hand can be passed far enough to separate the adhesions. In such a case, the vagina will probably be found to be loose and capacious, and perhaps filled with clots; whilst the upper portion of it is occupied by a firm fleshy mass, into which the cord is apparently inserted. On making a careful examination, however, by passing up the fingers of the left hand along the cord, which should serve as a guide, this mass is found to be the uterus, tightly contracted around the retained placenta; the circular fibres of the os uteri internum surrounding the cord very closely. This is precisely the condition of things which leads to those horrible blunders which are sometimes committed by ignorant and incompetent practitioners. A practitioner of this sort, for instance, having waited a reasonable time after the birth of child for the expulsion of the placenta, and finding that this does not take place, and that, moreover, there is some hæmorrhage, endeavours to remove the after-birth by tugging at the cord. The only result of this proceeding is, that the cord breaks short off, and the hæmorrhage is increased. By the breaking of the cord, he loses his guide; and, becoming still

more hurried from observing the fresh hæmorrhage, he passes up his hand, and feels nothing but the uterus tightly contracted around the placenta. Mistaking this for the latter, he gets his fingers into the hollow of the sacrum behind it, with the object of scooping out the after-birth, which he supposes to be (what the old nurses call) "grown to the side". At last, by means of his nails, he succeeds in separating the posterior wall of the vagina from its attachment to the uterus, and gets his hand into the pouch of Douglas, and of course, into the peritoneal cavity. Still unaware of his mistake, he grasps the fundus uteri from behind; and, thus obtaining a good purchase, he tears away the entire organ; and then at last, with the appearance of the intestines, the horrible conviction of his blunder dawns upon him.

Such is the manner in which we may reasonably suppose that many of these awful cases of malapraxis take place. I need scarcely remark that such accidents ought to be impossible to any one possessed of the smallest amount of skill and anatomical knowledge.

To pass on, however, to the manipulation necessary in these cases: if the cord be tightly encircled by the os uteri, the constriction should be overcome by insinuating the tips of the fingers into the os in a conical form; whilst the right hand all this time is making counter-pressure upon the fundus uteri, so as to steady that organ. Should these precautions be neglected, the connections between the vagina and the uterus may be put very injuriously on the stretch, especially if the circular fibres of the os oppose much resistance to the introduction of the hand. As the tips of the fingers pass through the os, they should be gradually expanded and separated from one another, until, by sheer fatigue, they overcome the contraction of the uterine fibres, so as to allow the passage of the entire hand into the uterus. When this is accomplished, the next step is to pass up the hand sufficiently high to reach the placenta. The distance which it has to pass before this can be felt will depend very much upon the position of the placenta and the degree of contraction of the uterus. If the placenta be attached, as it usually is, to the fundus uteri, or if the uterus be in a flaccid condition, it will be necessary to pass the hand much further than when the placenta is attached lower down, or when the uterus is well contracted. I have sometimes had to pass the hand quite into the epigastric region in search of a retained placenta. As soon as the placenta is arrived at, the fingers should be spread out, taking care not to entangle them in the membranes, until the circumference of the placenta can be felt. If any portion of the circumference be already detached, the tips of the fingers should be cautiously inserted between this portion and the inner surface of the uterus, and the placenta gradually peeled off. All this time, the right hand, externally applied, steadies the portion of the uterus from which the left hand is detaching the placenta, and enables the accoucheur to estimate the exact thickness of the uterine walls included between the hands, so that he can avoid digging his nails into the substance of the uterus. There is sometimes considerable danger of such an accident when the adhesions are very firm and close. There is also considerable danger of leaving portions of placenta behind: a risk that one can readily comprehend in such cases as those described by Dr. Ramsbotham, who states: "I have opened more than one body where a part was left adherent to the uterus, and where, on making a longitudinal section of the organs, and examining the cut edges, I could not determine the boundary line between the uterus and the placenta, so intimate an union had taken place between them." In all such difficult cases, it will be necessary to sever the adhesions by using the finger-nails with a kind of sawing motion from side to side. The tips of the fingers are placed in a line like the edge of a saw, keeping the palm towards the placenta and the knuckles towards the uterus, and the sawing motion is continued very slowly and gradually, until the entire placenta is separated and falls into the hollow of the hand. This proceeding sometimes requires a great deal of patience, and is exceedingly tiring; but the accoucheur should take his time about it, working with both hands, and making his ground sure as he goes on, and not withdrawing his hand with the placenta until he is certain that he has brought away every part of it that can be safely separated. It is very seldom, comparatively, that the adhesions are so firm that this cannot be done. Should this, however, be the case, we have a choice of evils: either to run the risk of causing secondary hæmorrhage and septicæmia by leaving portions behind, or of causing metritis from injury to the uterus in bringing them away. For my own part, I think that the last of these two courses is the least dangerous, except in very unusual cases. I have notes of only two instances in which it was necessary to leave any portion of consequence behind. Fortunately, in both, the pieces were expelled on the third day, without having caused any untoward symptoms, although in one the piece expelled was as large as a hen's egg. Of course, in all such instances, the dangers of septicæmia should be guarded against as much as possible by the frequent use of vaginal injections containing Condly's or other disinfectant fluids.

A REPORT ON INFECTION.

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IN 1872, at the annual meeting of the Lancashire and Cheshire Branch of the British Medical Association, on the suggestion of Dr. Ransome, a committee was formed to inquire into the subject of infection, with a view to rendering our knowledge, if possible, more exact.

The committee, to this end, drew up a letter, asking assistance in obtaining facts; and with this letter a form, containing such questions as we wished answered, was sent to a large number of the profession in Great Britain and Ireland.

The answers received up to this date are not so numerous as could have been desired; nevertheless, they contain some important facts, and, at the request of the committee, I have drawn up the following short report for publication in the JOURNAL, hoping that it may direct attention to the subject, and induce the whole body of members to assist in the undertaking.

By "infectious" is generally understood the power of communicating disease by some emanations from the body of the individual affected. The "infection" is the something which affects the healthy body and produces in it disease. Of the real essence of infections very little is known; but there can be little doubt that in a few years our knowledge in this direction will be materially advanced; and we may yet hope to be able to separate, collect, and bottle up the various infective agents, which are so prolific in our large towns, and so deadly in their action on the human body. For such progress, we look to physiologists, who have the wards of a hospital as a field of observation, and the laboratory, with all modern means of experimental research, in which to unravel the physiology of disease. There are, however, many facts connected with the natural history of infection which can be best observed by the general practitioner, more especially in thinly populated country districts; and it is through the busy practitioner, and not through the experimental physiologist, that the aim of the committee is to be attained.

The diseases selected for investigation are small-pox, measles, scarlet fever, diphtheria, typhus, typhoid, whooping-cough, and mumps. These ailments are seldom entirely absent from a town of any size in this country, and the general practitioner is frequently asked questions about infection with regard to them, which he has to answer in a way that reflects little credit on the medical profession, and makes one wonder that no investigation has been undertaken by the profession as a body, with the view of rendering our knowledge more definite, and the information very naturally required of us more precise and reliable. We have a few examples of an energetic and observant physician investigating the incubation period of some particular disease or class of diseases; but, so far as I am aware, the Lancashire and Cheshire Branch of the British Medical Association affords the first example of a large number of members of the medical profession agreeing to collect facts which may help towards the attainment of more exact knowledge regarding infection; and, since the subject is of such importance to mankind over the whole world, it is to be hoped that its efforts will be appreciated, and the work helped on by the profession abroad as well as at home.

Text-books on medicine teach that a healthy individual associating with a person suffering from any infectious disease is liable to receive the infection into his system through some channel; that, after a certain period known as the "period of incubation", during which time it is supposed that the infection in the blood has been undergoing a process somewhat similar to fermentation, it is rendered able to affect the equilibrium of health. The infected individual then feels what are called the "premonitory" symptoms; and, after a certain time, varying with the particular disease, the sign or symptom which determines the species is developed.

The "period of incubation" then first attracts attention, and what we have to determine is its duration. Now, the text-books do not fail to state the duration of incubation in each of the diseases selected; but in every one it is indefinite, so that it is stated as varying from so many to so many days. Thus, in small-pox, it is from twelve to fourteen days; in measles, ten to sixteen days; in diphtheria, three to twenty-two days; in typhus, a few minutes to twenty-one days; in typhoid, ditto; in whooping-cough, five to six days; in mumps, eight to twenty-two days. That may be accepted as the present teaching with regard to the incubation in each of these diseases, and no one will deny that it is far from definite or satisfactory. What the committee desires is, if possible, to fix the precise duration, since, from all we know, under the