ticularly where the bridge of skin under which the eyelash has been passed is very thin, I believe the eyelash ulcerates its way through; in others, again, it is possible that the eyelash gradually slips back out of the tunnel, but, having been for some time kept in a proper direction, has a normal curvature given to it, and a proper direction imparted to the follicle which produces it, which direction they retain. I believe, however, that, in the majority of instances, the eyelashes after a time are shed and fall out of the tunnel, and the new ones that are generated grow in the normal plane.

This operation is, unfortunately, only applicable to those cases of partial trichiasis in which the eyelashes inverted are few in number and of considerable size. In such cases it is, I am convinced, much superior to any other method of treatment employed at the present time. Where, however, the trichiasis is general, this method of operating would be excessively tedious and ineffectual, owing to the fineness of some of the inverted lashes. Where, on the other hand, the misdirected hairs are few and fine, the operation described is inapplicable, as the difficulty of getting them entangled in the loop of the ligature is much increased; and, even if successfully accomplished, and the eyelashes drawn into the tunnel, they are very apt to free themselves, not being sufficiently firmly held in their new position.

The reason why the operation in ancient times fell into disuse, is evidently to be found in the fact that it was then employed in all forms of trichiasis; and, as suitable cases form only a comparatively small proportion of the whole, the general results of the operation could not have been satisfactory.

To Dr Snellen great credit is due for several ingenious operations upon the eyelids he has devised, as well as for the re-introduction of this old operation.

ARTICLE IV.—*Report of, and Observations upon, a Case of Twins with Double Placenta Prævia (Partial), and both Children presenting Transversely—Abortion from Typhoid Fever in previous Pregnancy.* By Dr ANGUS MACDONALD, F.R.C.P.E., F.R.S.E., Lecturer on Midwifery and Diseases of Women and Children.

(*Read before the Obstetrical Society of Edinburgh, 25th February.*)

CERTAIN specialties of the following peculiar case have induced me to lay it before the Society in the form of a brief communication. The particulars of it are as follows:—

Mrs A., residing at 3 East M—— Place, Edinburgh, was married, at the age of thirty-one, on the 17th of July 1872. In September of the same year, whilst suffering from an ordinary attack of typhoid fever, she had a miscarriage at the middle of the second month of utero-gestation. She ceased to menstruate on the 14th of January

1873, and all went well, so far as she could judge, with her pregnancy, till the morning of the 22d of July last, when, on rising to pass water about 3 o'clock A.M., nearly a teacupful of blood escaped from her vagina. Alarmed at this occurrence, her husband came for me immediately, and I arrived at my patient's house at 4.30 A.M. I then found her in bed. Pulse regular and quiet, about 70 per minute, and the hæmorrhage entirely stopped. The patient had experienced no pain that morning. On examination, I discovered that her abdomen was much more distended than one would have naturally expected, considering the period of the pregnancy, and I was thereby led to suspect the possibility of the presence of either twin pregnancy or of hydramnios. Examination per vaginam was specially difficult, because the presenting part was uncommonly high, and the vagina was long and narrow. I was able to ascertain, however, that the cavity of the cervix was already obliterated, and the outer os distended to fully the size of a shilling piece. The anterior part of the lower segment of membranes presenting at the os, which part alone could at this time be reached, was *not covered* with any portion of placenta. There were occasionally slight uterine contractions, but so trifling as scarcely to attract the patient's attention. From inability to reach any portion of the placenta, or to feel any definable portion of the child, I was at first inclined to regard the case as most probably one of simple over-distention from hydramnios. The patient was, however, kept quietly in bed for a week, and an astringent mixture administered to her, as I considered it, on the whole, prudent to treat as for placenta prævia. About the same hour, on the morning of the 29th July, the bleeding recurred, while the patient was lying quietly in bed, and this time the hæmorrhage was much more severe. On arriving, in obedience to summons, I found the vagina full of clots, which I removed. I then ascertained that the cervix had become dilated since I examined last time to over the size of half-a-crown piece, but considerably less than that of a crown piece, but that its position was still very high up. Of the surface of the membranes thus laid bare by the dilatation of the cervix, about one-third—the left posterior third—was covered with a mass of placenta, whose free border was distinctly felt, and could be easily traced running from before backwards and inwards. A portion of the right side of the same membranous area was, at the same time, observed to be covered also with what seemed to be a mass of placenta. These masses of placentæ ran into each other posteriorly. Pains were now present, slight in force, but recurring at regular intervals. There was scarcely any hæmorrhage. These pains continued regular, but weak, and accompanied with occasional, but slight, bleeding throughout the day. About half-past 10 o'clock P.M., however, the contractions became much more severe, and, at the same time, the hæmorrhage somewhat increased. At 12.30, as the latter had now become rather severe, and the cervix was toler-

ably widely dilated, the presenting bag of membranes was ruptured, and the right foot of the first child, which presented transversely, was hooked down, and the breech brought to engage in the cervix. The bleeding then ceased almost entirely, whilst the pains became much more severe till the first child was expelled. It was a female, and living. The first placenta now prolapsed, and, becoming entirely separated from the second placenta, was expelled and removed. The bleeding from the remanent placenta being at this moment very serious and severe, the second bag of membranes was quickly ruptured, and a foot of the second child, which also presented transversely, pulled down, and delivery speedily effected. The second placenta quickly separated spontaneously, and there was no post-partum hæmorrhage. Though the patient almost fainted at the moment of completion of the birth of the first child, she rallied very well after the labour, and, with the exception of a somewhat anæmic expression for some weeks, and a rather quick pulse for a few days, her recovery was in every way normal, and she is now quite well. The first child lived for two days. The second made only a few imperfect efforts at breathing. Both, of course, were non-viable on account of prematurity.

As the subject of the bearing of typhoid and typhus fevers on pregnancy and childbed seems to me of great importance, and to have received an insufficient amount of attention from British obstetrical physicians, I mean, at the conclusion of this paper, to offer some observations on that point of this case, but will, in the first instance, take up the main questions involved in the paper.

In the first place, I would remark that the present case is one of very great rarity, and presenting a wonderful concurrence of complexities.

We had here two placentæ presenting both at the internal os, and belonging to twins, both of which, again, presented transversely. Then we had the onset of labour at a comparatively early period of the pregnancy, apparently due to the over-distention occasioned by the twins acting in conjunction with that preternatural tendency to prematurity of onset of labour so peculiar to placenta prævia.

I have never met with a case of twins previously in which the placenta presented, and can scarcely imagine it to be otherwise than exceedingly rare. Indeed, it does not need much mathematical knowledge to prove, from the known expectancy of the individual accidents, that the probability of such an occurrence happening is exceedingly small indeed. For, granting that placenta prævia is just as common in twin as in single conceptions, since twins occur not more frequently than 1 in 89 of all cases of labour, and placenta prævia not oftener than 1 in 500 of all cases of labour, the expectation of the concurrence of twins with placenta prævia would only be 1 in 500 × 89, or 1 in 44,500 cases of labour. But the expectation of the occurrence of twins with one placenta presenting at the internal

os uteri must still be much greater—small though it be—than that of the accident with both placentæ presenting.

Again, taking into account the position of the two children, Kleinwächter[1] arrives at the result, that of all cases of twin births only in ·33 per cent. do both children present transversely, or about 1 in 26,969 of all cases of labour whatever. Then, if placenta prævia occurs in only 1 in 500 of all cases of labour, the concurrence of placenta prævia with the latter peculiarity must be 500 times less frequent, or 1 in 13,484,500 of all cases; and even this must be considerably more frequent than the concurrence of both placentæ presenting with both twins lying transversely.

Leaving this matter, however, which, it must be admitted, is more curious than useful, I may be allowed to observe that the concurrence of complexities in the case led to considerable practical difficulty, as it rendered the diagnosis of the case at first rather obscure and troublesome.

The transverse presentation of the children made them assume a distant position with reference to the cervix uteri, and allowed of the gravitation of fluid to fill the lower uterine segment. Thereby the os was very difficult to reach by the examining finger, and it was much more than usually hard to make out exactly how matters stood. Indeed, the whole abdomen was so largely distended in comparison to the advance of the labour, that although the probability of twins, or hydramnios, or either, with placenta prævia, was before my mind, and duly considered by me, I was, on the whole, inclined, after the first examination, to believe that it might turn out to be a case of simple hydramnios after all. The fact that I felt the anterior portion of the bag of membranes, and could reach that only, and found that it was entirely bare, added to the sources of error, and rather negatived the idea that I had placenta prævia to grapple with. I confess I did not pay much attention to the decision of the question so far as the point twins or no twins was concerned, and was never certain of twins being there till after the birth of the first child. Nor do I look upon such uncertainty as of any serious importance, because it can lead to no practical injury. The accoucheur can quite well afford to wait with patience till the question is solved.

But the question as between hydramnios and placenta prævia was both more important and beset with special difficulty. So far as regarded the mode in which the hæmorrhage occurred in even the first instance, everything pointed towards the greater probability of its being a case of placenta prævia. I accordingly treated the case as though it had been one of presentation of the after-birth. But, then, I had to put over against this fact, that the bleeding was not much, that it stopped at once, that I could feel a portion of the membranes through the partially dilated os the very first morning I saw my patient, and that that portion was entirely bare of placenta. The long narrow vagina, coupled with the high posi-

[1] Lehre von den Zwillingen, S. 119.

tion of the cervix, prevented me from making a full and thorough exploration of the posterior circumference of the os, and of the lower posterior portion of the inferior segment of the uterus, else, I have no doubt, even then, I might have satisfied myself of the presence of placenta prævia. On the whole, however, I was rather inclined to believe that there was at least a chance that the bleeding might, after all, have proceeded from the rupture of some small cervical twigs through the opening up of the cervix from over-distention, probably due to hydramnios.

The second examination, with the cervix somewhat more dilated, however, revealing the well-known spongy masses of placenta on the posterior and lateral aspects of the presenting membranes, removed of course all doubt, and justified the treatment which had been adopted as the safer alternative.

As to the subsequent treatment of the case, I have really very little to say. I proceeded according to the ordinary principles of treatment in such circumstances, which are to effect delivery as soon as possible, and as safely as possible. In pursuance of these objects, I had to hurry the labour at first somewhat, and subsequently to finish it very quickly.

So long as I could wait without any risk to the life of the mother, I delayed, so as thereby to gain room, by the natural efforts of dilatation, for the transmission of the child. The bleeding never actually became urgent till the dilatation of the lower extremity of the uterine ovoid had so far advanced as to allow of the breech being at once drawn down and brought to engage in the cervix. I thereupon ruptured the membranes, and, finding a side presenting, hooked down a leg, turned easily, and pulled the breech down. The bleeding was then practically arrested till the first child was born by the mechanical pressure of the body of the child. Then, however, the hæmorrhage from the uterine surface, thus divested of this natural tampon, and laid bare by the prolapse of the first placenta, and partial separation of the second, became positively alarming, and would have undoubtedly proved suddenly fatal, had not the labour been at once terminated by the artificial delivery of the second child, the removal of the second placenta, and subsequent contraction of the uterus.

A case such as the present, had it simply been one of single presentation of the placenta, might have been expected to be a very easy one.

I. Because the presentation of the placenta was partial, and would not consequently necessitate a very large spontaneously-detaching area.

II. Because the children were both small and premature, and on that ground also might have been fairly expected to pass through a comparatively small aperture, which consequently would lead to a still smaller amount of spontaneously-detaching, *i.e.*, of bleeding area being needed.

Two peculiarities, however, removed the present case out of its normal category.

I. The fact that the presentations were transverse, preventing the descent of the head or breech, would have led to the continuation of dilatation and detachment long after the period when either head or breech would have passed the os uteri.

II. Though both placentæ reached the *edge* of the internal os *only*, yet the two masses occupied a much larger proportion of the spontaneously-detaching area than would have certainly been the case with a single placenta, with its apex at the inner os and its base on the sides of the body of the uterus; so that though the case, anatomically considered, is one of partial presentation of the placenta, yet practically it approximated in danger to the complete accident.

In speaking of the spontaneously-detaching area, as well as in discussing this subject in general, I cannot refrain from expressing my deep sense of the obligation which the obstetrical profession owes to the President of this Society for his recent contributions to our knowledge of the laws regulating this accident.

As, no doubt, most of the members of this Society are aware, in a paper read in October last year before the Obstetrical Society of London, and published in the last Heft of the *Archiv für Gynäkologie*, Dr Duncan has advanced an entirely new view regarding the natural mechanism of the separation of the placenta from the uterine surface in cases of placenta prævia. In that contribution Dr Duncan maintains, and supports by arguments, which to my mind at least seem incontestable, that the spontaneous detachment of the placenta when it presents is not due to contraction or diminution in all directions of the placental area, but to stretching of its uterine attachment through the opening of the lower segment of the uterine ovoid, to form a passage for the child.

In following out this view, Dr Duncan has defined, more rigidly than Dr Barnes and others who have travelled this ground before him, the exact limits of the spontaneous detaching area. According to him, the placenta, in an ordinary case of complete placenta prævia, with a full-grown child at full term, ceases to suffer any further separation when the dilatation of the lower segment of the uterus attains a diameter of 11 centimetres, *i.e.*, nearly 4½ inches. This amount of dilatation, as the result of careful measurements made by Dr Duncan, is shown to coincide with the circumference of a circle whose plane is at right angles to the longitudinal diameter of the uterus, and whose diameter measures 11 centimetres, whilst every part of its circumference, measured along a meridian line of the uterus, is 6 centimetres distant from the central point of the internal os uteri.

It follows, therefore, that in a case of placenta prævia such as I have been describing, if the head had presented, hæmorrhage, due to separation of the placenta, would certainly have ceased some time *before* a segment of the spheroid, formed by the lower extremity of

the uterus, with a chord of 11 centimetres, and an arc of 12 centimetres, had been obliterated by the natural dilatation.

I say *before* advisedly, because, even though the case had gone on to full time, such a segment would be the maximum amount of dilatation necessary; whereas in the labour which we have been considering, the smallness of the children rendered the amount of dilatation necessary considerably under a segment with a chord of 11 centimetres.

We are thus, through the labours of Dr Duncan, brought to such a degree of certainty, or at the very least of approximate certainty, in respect to the natural limit of bleeding in cases of placenta prævia, which is a very great advance upon the bewilderment induced by theoretical ideas regarding indefinite and indefinable zones.

But notwithstanding all that has been previously written upon this subject, and making full allowance for the above-mentioned and the three other able and more recent contributions of our President to the *Edinburgh Medical Journal* and the *British Medical Journal*, in regard to the origin and mode of arrestment of this kind of hæmorrhage, much has still to be done, as the whole subject is still enveloped in very much mist.

If I might venture a criticism upon Dr Duncan's theory of the origin and mode of arrestment of bleeding in such cases, which has been lately put forward and ably advocated by him, it is that I think he has paid too little attention to the curling arteries as a source of such bleeding, and too much to the venous sinuses in the placental area.

Dr T. Snow Beck, in the *Obstetrical Journal* for December 1873 and January 1874, has endeavoured to revive the views originally brought forward by Dr F. W. Mackenzie on this subject. He maintains, supporting his arguments especially by reference to Dr Mackenzie's experiments, that in bleeding from the uterine surface the bleeding is arterial and direct. On the other hand, Dr Duncan would have it an indirect oozing, the arteries inside of the uterine wall bleeding first into the sinuses, and then the sinuses discharging their blood into the cavity of the uterus through its free surface. Dr Duncan does not hold that the bleeding is regurgitant, in the sense that it is a reflux from the veins leading from the uterus towards the heart back again into the uterine sinuses.

Now, though I have not sufficiently studied this exceedingly intricate subject so as to be able to take up the strongly-affirmative position defended by Dr T. Snow Beck, I must confess that my leanings are towards his views; and I consider the facts, experiments, and arguments advanced by him, deserve a larger amount of attention from the thoughtful accoucheur than they have as yet received. It would be a great gain if the profession thereby were only led to abandon the exceedingly unphysiological and provedly hazardous treatment of post-partum hæmorrhage by means of injections of perchloride of iron, and to get free of belief in the dangerous nature of the exceedingly effective—and, as I am glad from extended experience for several years back to be able to testify, in

perfect accordance with the views of Dr T. Snow Beck—completely innocuous mode of arrestment of hæmorrhage post-partum by injection of cold water into the uterus.

But this is rather departing from my subject. In reference to the course of the bleeding, I would beg to state that the late Professor Goodsir—than whom, I am sure the members of this Society will allow, a more patient, more able, or more careful observer never lived—used in his lectures on the anatomy of the uterus to substantiate the views of Owen and of Simpson in regard to the platform arrangement of the uterine veins, and to describe the manner in which the successive platforms opened into each other obliquely, with the free edges projecting in such a manner as to act substantially as valves, and thus to prevent the escape of blood from a higher platform to a lower. I feel convinced that though it is difficult to understand how such small vessels, numerous though they be, as the uterine curling placental arteries, may give rise to such severe bleeding, both in connexion with placenta prævia and in hæmorrhage post-partum, yet the difficulties, on the other hand, to account for the arrest of hæmorrhage from the open uterine sinuses without the contraction of the organ, are still more difficult to meet.

In regard to this point, I do think that the existence of some such anatomical arrangement of the veins, such as Owen and Goodsir pointed out, gets us more rationally and easily over the difficulty than to trace it to the obstruction due to a change of shape in the uterine sinuses effected in the course of the dilatation of the uterine placental area, consisting in the elongation of them in the lateral direction, and the approximation of their walls in the longitudinal direction, as Dr Duncan urges. I do not think that Dr Duncan, even when backed up by the great and justly-respected name of Professor Tait, proves that point well. This part of Dr Duncan's paper is to me not at all convincing.

Before such change in the shape of the discharging orifice could be effective as a hæmostatic means, I feel certain that we would need a diminution of the discharging area in all directions; for I cannot imagine the mere friction of the opposed surfaces of openings, so large as those sinus openings are, to be able to resist the degree of tension, often great, to which the maternal blood in the uterine walls is subjected, both in placenta prævia and otherwise. In the case of the small curling arteries again, supposing them to be sources of hæmorrhage, we have the greater contractility of their walls, the fact, also, that the separation is by tear and not by cutting, and the exceedingly small diameter of the vessels, uniting, to afford conditions specially favourable for the process of nature's hæmostatics. Only in cases where the general blood-pressure was specially exalted, or in which the retentive power of the abdomen (so ably and convincingly urged by Dr Duncan as one of the means by which the bleeding is arrested from the free surface of the uterus, in cases of placenta prævia) was defective, would the natural hæmostatic means afforded by the torn surface of those arteries be expected to fail, pro-

vided we could account for occlusion of the sinuses by any certain means. That the veins inside the uterus have such an arrangement as could make their various platforms incapable of transmitting blood from a higher to a lower level is my contention, supported by the experiments of F. W. Mackenzie, and the dissections of Owen and Goodsir.

To make certain of the ground maintained by Mackenzie, T. Snow Beck and myself, however, we need extended observation and fresh dissections; and, while criticising this view of Dr Duncan, I feel constrained to assert, that it is much easier to find fault with his, than to suggest a better explanation for the arrest of the hæmorrhage. Will no one attempt to tell us why placenta prævia so very frequently gives rise to miscarriage?

Let me now draw the attention of the Society to another point of great practical value, involved in this patient's history, viz., that her previous pregnancy was interrupted by typhoid fever. The undoubted importance of this subject, as well as the great amount of uncertainty in the professional opinions regarding it, must form my apology to the Society for directing their attention to it for a few minutes.

As the patient was not then under my care, it would not be in good taste (and, besides, I have not sufficient knowledge of the course the disease ran) for me to go into the details of that occasion. Suffice it to say, that the patient aborted in the course of an attack of typhoid fever, and that, notwithstanding, she made an excellent recovery.

In so far as concerns the bearing of typhus and typhoid fevers upon the pregnant and puerperal female, I am surprised to find in the usual authoritative sources the most contradictory statements.

From what I have been able to gather, however, as well as from my own experience, I am led to affirm, in as emphatic a manner as possible, that while neither the pregnant nor the puerperal patient seem to be specially liable to these diseases, yet the researches of Hecker, Scanzoni, Wallichs, Kaminsky, and others prove that pregnancy forms no protection against imbibing the poisons of typhoid or of typhus fever. It must be allowed, however, that the divergence of opinion of professional authorities on this subject is very great, and really very unintelligible. It is curious, also, to notice the gradual advance of medical opinion on this matter.

Not many years ago, it was believed that pregnancy afforded almost absolute immunity against typhoid fever. Thus, Rokitansky[1] wrote, in the year 1842, "that pregnancy conditions an almost absolute immunity against the typhus (typhoid) process." In 1865, Niemeyer[2] says, in his article on typhoid fever, "Pregnant women and women giving suck are seldom seized; the puerperal state

[1] Path. Anatomie, III. Band, S. 258.

[2] Lehrbuch der Speciellen Pathologie und Therapie, 6 Aufl. 1865, Band ii. S. 578.

secures an almost complete immunity against abdominal typhus." In this opinion, it is right to state, however, that Niemeyer differs *in toto cœlo* from the great majority of his countrymen, as will afterwards be shown, by referring to Hecker's paper. Among British authors there seems, so far as I can find out, a very great paucity of remark on the subjects, both of abdominal and exanthematic typhus, in reference to pregnancy and childbed. It would appear that no doubt had ever existed in regard to the probability of such females taking the fever, but that little attention had been given to the subject, except in reference to the probability of abortion and the probable prognosis. Dr John Harley, in his article on Enteric Fever, in Reynolds's System of Medicine, vol. i. page 358, states that "abortion is almost certain to occur if a pregnant woman be attacked with enteric fever. The only two pregnant women who have come under my care aborted the one at the third month of gestation, the other at the fifth. Both recovered well. Phlegmasia dolens is apt to be a secondary complication in such cases." In his article upon Typhus Fever, *opere citato*, p. 440, Dr George Buchanan states, "that when pregnant women get typhus, if they are past the sixth month, they frequently miscarry; but this accident adds very little to the danger of the case, typhus fever therein differing much from typhoid or scarlatina." On the other hand, Scanzoni[1] mentions ten cases of typhoid fever, treated by Fingre Ritter and himself, six of whom aborted, and yet the abortion was not followed by any unfavourable turn in the course of the disease. Also, Wallichs[2] records two well-marked cases (the one an excessively severe and complicated one) of typhoid fever in women far advanced in pregnancy. Of these, the first carried her child to the full term, the latter aborted about the beginning of the eighth month. Both patients, however, did well.

Kaminsky of Moscow published in the Russian language a work, entitled, "Contributions to the Knowledge of the Influence of Typhus and Remittent Fevers upon the Course of Pregnancy," in which he asserts that he had seen 87 cases of pregnancy complicated with typhus or remittent fever, within a year. Of these, 55 were during the first half, and 32 during the second half of pregnancy. A reference is made to a German translation of this paper, but I have been unable to obtain the work referred to. From an abstract which Wallichs, *in loco citato*, gives of the paper in question, it is impossible to make out, however, what proportion of those cases were exanthematic typhus, abdominal typhus, or remittent fever.

It is evident, however, that in Russia, at least, it is no rare thing to find typhus or typhoid combined with pregnancy. In an exceedingly interesting paper, published in 1866, Hecker[3] maintains that abdominal typhus is very common in the Lying-in Insti-

[1] Lehrbuch der Geburtshülfe, IV. Auflage, II. Band, S. 13.
[2] Monatsschrift für Geburtskunde, XXX. Band, S. 253.
[3] Monatsschrift für Geburtskunde, XXV., II. Band, S. 423.

tution of Munich, and maintains that many of these cases of fever and diarrhœa during the childbed period, which, though they present no inflammatory lesion whatever, in either the uterus, its adnexa, or the abdomen, are usually regarded as mild puerperal fever, are nothing more nor less than abdominal typhus. This view he maintains with much ingenuity, and supports it further by reference to five post-mortem examinations of patients who had died of typhoid fever shortly after confinement. He maintains, in direct opposition to Niemeyer, that the lying-in period really predisposes to the reception of the poison of typhoid fever, though his *sectiones* do not help him much in the proof of that assertion, inasmuch as, according to the just criticism of Wallichs, every one of these five cases, on Hecker's own data, must have been confined whilst the fever-poison was in the system.

As corresponding with this view, but in regard to ordinary typhus, we may here quote the younger Ramsbotham,[1] who writes thus: "Another disease which has been confounded, in consequence of the name applied to it, with peritonitis, is a fever allied to typhus, of the most severe and aggravated form. To this the term puerperal fever would be peculiarly proper, and it might be retained, if it had not already been employed in so vague and undefined a sense. Typhus, indeed, is very rare in the puerperal state, the most uncommon, perhaps, of all the affections which have been described under this denomination. It commences at the time after delivery most usual for serious diseases to begin—about the second, third, or fourth day. It is, perhaps, ushered in by shivering, but this is by no means always the case. This is followed by pain in the head and along the spine, accompanied with great depression of spirits, mental agitation, and inquietude," etc., etc.

He afterwards relates the particulars of the outbreak of what he —without, it must be confessed, any very convincing evidence— regards as an outbreak of puerperal typhus.

In the summer of 1872, I wrote my friend Dr John A. M'Dougall of Galashiels to ascertain his views on the bearing of zymotic diseases upon pregnant and puerperal females; and, in a brief note in reply, he stated that he had had several cases of typhoid fever combined with pregnancy and the puerperal state, and, with one exception, they recovered. He had had two cases of typhus, and both had died. He was inclined to regard typhus as equally fatal with scarlet fever.

My own experience of either fever, in relation to pregnancy or the puerperal condition, has been small. Two and a half years ago I attended a lady who had a mild attack of typhoid fever during the fifth month of utero-gestation. Her pregnancy went on to full term, and she gave birth to a healthy boy.

In August last year, I was present in consultation at the birth of a child, at about the middle of the eighth month, which took place

[1] On Obstetric Medicine and Surgery, p. 622.

on the nineteenth or twentieth day of a well-marked case of typhoid fever. Both mother (who was a primipara) and her child did well.

Some years ago, I attended, in conjunction with one of my dispensary pupils, a patient in 18 Greenside Row, who, after an easy labour, fevered on the third or fourth day, and whose case turned out to be one of well-marked exanthematic typhus. She recovered well; and there were in her case none of those alarming symptoms referred to by Ramsbotham.

In January 1871, I attended a patient who was seized with a severe attack of typhus while nursing. She sank on the thirteenth day of the fever from exhaustion, but the course of this disease was that of an ordinarily severe attack. This case is mentioned to show that exanthematic typhus occurs in women who are giving suck.

I recollect, when I was a student, in the Edinburgh Infirmary there occurred a case of typhus in which the patient aborted and died immediately after.

Indeed, from all I am able to gather from these records, I am led to believe that neither pregnancy nor the puerperal condition give any immunity against the onset of typhus or typhoid fever. Pregnant women, women in the childbed week, or women giving suck, seem to be seized with these fevers in proportion to exposure to the virus just as readily as other people.

In case a pregnant woman is seized with either of these fevers, it is undoubtedly proved that she is exceedingly liable to abort. In case of her aborting, it would appear that there are no just grounds for the opinion, that typhoid is very dangerous, and typhus not, when we look at the statistics of Hecker, Scanzoni, and others.

In case of the coexistence of pregnancy with either typhoid or typhus fever, the patients are unquestionably in considerably greater danger than if pregnancy were absent; but typhus would appear to be just about the same in risk as typhoid, neither being necessarily fatal.

In connexion with the lying-in period, both these fevers appear to be much less fatal than measles and scarlet fever.

Article V.—*The Use of the Electro-Magnetic Current in Labour: Notes of Two Cases.* By A. E. M'Rae, C.M., M.D., Penicuik.

(*Read before the Obstetrical Society of Edinburgh, 11th March.*)

On a previous occasion,[1] I called your attention to the electro-magnetic current as an additional aid in the second stage of labour. I pointed out that by the use of the interrupted current applied to the abdomen and the perineum, uterine pains could be induced and intensified; that it was of use when the uterus was exhausted, or refused to act by other means of stimulation; that no danger was to

[1] See Journal for September 1873, page 243.