the presence of pneumonia. The correctness of this opinion was fully realised by the character of the physical signs, which, throughout the progress of the case, were exceedingly well marked. The dulness on percussion, to the extent of from two to three square inches, situated over the locality of the pain, at once indicated the existence of a circumscribed portion of consolidated lung; and that pneumonia was the disease upon which this sign depended, was further evidenced by the simultaneous occurrence of a fine crepitation around its edges. As the disease, instead of undergoing resolution, advanced to abscess, the physical phenomena became equally indicative of this more serious lesion. We now find, over the previously dull spot, an amphoric sound, elicited by percussion, pointing to the existence of a cavity communicating freely with a bronchial tube; while on auscultation a large gurgling rhonchus, corresponding to the spot over which the respiratory murmur was before inaudible, became at the same time painfully distinct. These circumstances, together with the history of the case, and the characteristic odour and appearance of the expectoration, rendered the nature of the disease unequivocally apparent.

The object of the early treatment, viz., the resolution of the consolidated portion of lung, was frustrated by the resistance offered by the system to the introduction of mercury, as well as the great difficulty which existed in producing counter-irritation over the affected part. It was omitted in the report, but which may now be stated, that a seton introduced in the mammary region, was equally unsuccessful in effecting a discharging surface.

This antagonism between internal disease, and the operation of remedies, is not uncommon, more especially when the former is confined to a small spot, and assumes from the first a sub-acute or chronic character. I have, however, in no other instance witnessed so persistent a determination (so to speak,) of the system to resist remedial measures as in the present case.

On the formation of abscess, the most marked benefit was derived from the inhalation of creosote. It removed the fœtor from the expectoration; it lessened the cough, and greatly diminished the amount of expectoration.

On the present condition of the case, it is necessary to make one or two observations.

The patient considers himself strong and well, and notwithstanding the most rigid injunctions on the part of his medical attendant, he unfortunately evinces extreme imprudence in reference to exposure to cold. The amphoric stroke-sound, together with the perfect pectoriloquy that now exists, indicate a small cavity in the original site of the abscess, lined probably by organized membrane. Its proximity to the heart causes, at each pulsation of that organ, a vibration of the air within the cavity, and thus gives rise to the double musical murmur so distinctly heard, yet so completely circumscribed in extent. This latter sound is unquestionably of rare occurrence, requiring for its production a cavity containing air only, of certain dimensions, communicating with the bronchi, and at the same time sufficiently near the heart, as to be influenced by its movements.

The pulsatory gurgling sound pointed out by Dr. Stokes, arising from the agitation of fluid in cavities,

excited by the action of the heart, will be met with more frequently than the phenomenon of the double murmur above mentioned. Of the condition of the apex of the right lung no doubt can longer exist in reference to the presence of tubercles. When the patient first came under observation, he had had no hæmoptysis; the result of percussion was a flat, rather than a dull sound; and the only indication derivable from auscultation, was a slight feebleness in the respiratory murmur. Under these circumstances, therefore, with a deranged condition of the circulation in the left lung, the physical signs alluded to might possibly have arisen from congestion, and a doubtful opinion upon this point was therefore expressed. Now, however, two years having elapsed, and the same phenomena obtaining, together with the fact, that a small quantity of blood has more than once been observed in the sputa, coupled with the recollection of a sister having died of phthisis, I have no hesitation in pronouncing, that latent tuberculization exists at the apex of that lung. The fact of the deposit, which is scattered, having neither increased nor excited surrounding inflammatory action, is unquestionably a favourable circumstance; still, however, in a case presenting similar conditions, even under the best directed auspices for the future, a rigidly guarded prognosis should be given.

(To be continued.)

ON PLACENTA PRÆVIA.

(A paper read before the Medical Society of University College, London, on Friday, January 30th, 1846.)

By JOHN ELLIOT WOOD.

The subject which I propose to advance for discussion this evening, is one which has lately engaged the attention of the profession generally, and has been made the basis of much controversy, which, I regret to say, has not in every instance partaken of that calm and philosophical character so indispensable for the elucidation of truth. And whether we regard the subject in a medical or moral point of view, it will be found well worthy of our serious consideration. If we are ultimately to arrive at truth—and that such will be the case, who can doubt?—our progress will be materially hastened by the multiplication of observers and thinkers; and, with this view, I would request your attention to the matter about to be brought forward.

There are few dangers and difficulties in the practice of midwifery, the advent of which is more to be dreaded than that kind of hæmorrhage which necessarily follows dilatation of the os uteri, when the placenta is wholly, or in part, attached over the orifice of the womb; the hæmorrhage arising from the cessation of that intimate connection which exists between the uterine and placental surfaces. Professor Nægelé has well observed, that “there is no error in nature to be compared with this, for the very action which she uses to bring the child into the world, is that by which she destroys both it and its mother.”

As may well be supposed, these cases of hæmorrhage have occupied the attention of writers on midwifery in all ages; some giving one explanation of their nature,

some another; but all agreeing in their great danger, and the importance of even the minutest variation in point of treatment as regards the established practice, which should even in the smallest degree diminish the risk to the mother and child.

In the very early ages of medicine, we find Hippocrates noticing this complication. In his book *De Morbis Mulierum*, he says, "that the after-burthen should come forth after the child, for if it come first, the child cannot live, because he takes his life from it, as a plant does from the earth." Dr. Rigby's Commentary on this statement is, that "Hippocrates *therefore* evidently supposed that this presentation of the placenta at the os uteri, was owing to its having been separated from its usual situation in the uterus, and fallen down to the lower part of it."* The fact is possibly correct, though I cannot conceive by what ingenuity it could be extracted from the above sentence of Hippocrates.

We pass over a very considerable time, during which medicine in general made but slight advances, until we come to the writings of Guillemeau in 1609. He says that, "when the placenta presents, the most certain and expedient method is to deliver the patient promptly, in order that she may not suffer from the hæmorrhage which issues from the uncovered mouths of the uterine veins, to which the placenta has been attached."

There is nothing in this observation that would lead us to believe that Guillemeau was not acquainted with the real nature of placental presentations, though Dr. Rigby's inference from it is that he had fallen into the error attributed by this writer to Hippocrates. Dr. Lee, however, with more correctness, states that "Guillemeau has made no observation from which it can be inferred that he believed the placenta to have been originally adherent to the upper part of the uterus, and to have descended from thence to the cervix."†

It is generally taken for granted by writers on the history of this subject, that from the time of Hippocrates down to that of Guillemeau in 1609, this erroneous view of the nature of placenta prævia was current; for no reason that I am aware of, except that this medical heresy was promulgated by later observers. Mauriceau, who followed the last writer, and whose treatise was published in 1668, certainly speaks of the placenta when at the os uteri, as being "entirely detached," and urges the necessity of immediate delivery in these cases; but nowhere does he express his belief in the erroneous opinion above alluded to. From one observation, indeed, at page 350 of his work, it is plain that he was aware, at least of the occasional attachment of the placenta to the cervix. Yet Dr. Rigby's commentary is as follows:—"These facts prove that Mauriceau considered presentations of the placenta to arise *solely* from its having been separated by some accident from the fundus, and fallen down to the os uteri."‡

We next come to Paul Portal, who practised midwifery extensively in Paris, and in 1685 published *La Pratique des Accouchemens, soutenu d'un grand nombre d'observations*, which was afterwards translated

into English. In his sixty-ninth observation he says, "I searched with one finger first, and found the after-burthen foremost, and closely joined round the inner surface of the womb. I again felt the after-burthen fastened to it; I peeled it off, &c." In the forty-third, fifty-first, and seventy-ninth observations, he again expressly points out the attachment of the placenta to the os uteri.

The next author who has alluded to the real nature of placenta prævia is Giffard, whose work was published in 1734. He speaks of it, however, only in a cursory manner, as if of a subject already understood, and concerning which he had nothing of novelty to offer. He makes one remark, which would lead us to infer, that many at that time doubted the attachment of the placenta to the cervix uteri. He says, "I cannot implicitly accede to the opinion of most writers in midwifery, which is, that the placenta always adheres to the fundus uteri." Again, in case 116, "Its adhering to the os uteri was, in my opinion, the cause of the flooding; for the os internum was gradually dilated, the placenta at the same time was separated, from whence proceeded the effusion of blood." And in case 224 he says, "The placenta adhered, and was fixed close and round about the cervix uteri, as I have found it in many other cases." Dr. Rigby, however, complains that these opinions of Giffard's, apparently so simply and clearly expressed, are given cursorily, and with a degree of hesitation.

Heister, in 1739, says that some of the moderns consider as a cause of hæmorrhage the adhesion of the placenta to the mouth of the womb, so that the more the os uteri is dilated, the greater is the separation of the placenta, and the more profuse the bleeding. We next come to Smellie, who, in his work published in 1752, relates nine cases to illustrate the adhesion of the placenta to the os uteri. "The edge or middle of the placenta sometimes adheres," he says, "over the middle of the inside of the os internum, which frequently begins to open several weeks before the full time; and if this be the case, a flooding begins at the same time, and seldom ceases entirely until the woman is delivered." Røderer, in 1759, gives a clear account of the symptoms and treatment of uterine hæmorrhage from the attachment of the placenta to the lower part of the uterus. The placenta can be felt, he states, firmly adhering to its inferior segment. He denies the possibility from any cause of the placenta being detached from the fundus uteri, and falling down to the cervix. Levret, in 1761, undertook the somewhat superfluous task of proving,—1st, that the placenta sometimes adheres to the circumference of the internal orifice of the uterus; 2nd, that when this occurs uterine hæmorrhage is inevitable in the latter months of pregnancy; and 3rd, that the only method of obviating this dangerous accident is to deliver immediately by turning the child. I may so far anticipate myself as to state, that Levret recommended forcing the hand through the placenta, instead of passing it between the uterus and its margin, as Mauriceau and Portal had always done.

Dr. F. H. Ramsbotham states that "the placenta presentation was noticed by Guillemeau, Mauriceau, and others, in France; Daventer, in Holland; Bracken and Pugh, who wrote respectively in 1751 and 1754, in this country, besides others; but they all held the

* *A System of Midwifery*. By Edw. Rigby, M.D., p. 349.

† *Ed. Med. and Surg. Journal*, vol. 57, p. 382.

‡ *Midwifery*, p. 250.

opinion that it was not originally apposed to this part of the uterus by nature, but that in consequence of some peculiar circumstances, it had become loosened from its attachment above, had fallen down by its own weight, and had thus accidentally placed itself over the uterine orifice." This observation, however, as regards at least the two former of these writers, we have seen to be incorrect.

To Dr. Rigby, in his "Essay on Uterine Hæmorrhage," published in 1775, we are much indebted for putting forth more prominently, perhaps, than had hitherto been done, the observations of the preceding writers, and the consequences which would naturally result from them. He insisted strongly on the distinction, so important in a practical point of view, between *accidental* and *unavoidable* hæmorrhage; the former being the consequence of numerous accidents which may occur during pregnancy and labour, the latter the result of the adhesion of the placenta over the os uteri. It is erroneous, however, to attribute to this distinguished writer the merit of having been the first to point out this distinction, as we find Giffard, in 1734, and Levret, in 1761, asserting that hæmorrhage, in placental presentation, was unavoidable. But if we are compelled, in justice, to deny to Dr. Rigby the claim of originality, or at least of priority of observation, we must admit that his work collected, and put in a clear point of view, truths which had hitherto been scattered and obscure, and that he was the chief means of spreading amongst British practitioners a correct knowledge of the subject.

From that time to the present, these views have been adopted by every author of note, and there is now no question either of the seat of the placenta in unavoidable hæmorrhage, or that this abnormal situation renders hæmorrhage inevitable.

With regard to the *causes* of placental presentations, they are as yet very obscure; and owing to the rare opportunities that we possess of investigation at a sufficiently early period, and the comparative scarcity of placenta prævia itself, it will be long before we can obtain anything like demonstrative evidence on this point. It is probable that the condition of the decidua shortly after the entrance of the ovum into the cavity of the uterus, will in some measure influence the situation of the placenta. In the ordinary state this effusion of plastic lymph has already obtained such a degree of cohesion and firmness as to prevent the ovum from passing beyond the extremity of the Fallopian tube down which it has travelled, but it can easily be supposed that if the decidua itself were wanting, or in a semi-fluid state, the ovum would escape into the cavity of the uterus, and, by its own weight, gravitate to the lower part of this organ, and becoming adherent there, would give rise to a case of entire or partial placental presentation.

Some interesting views on this subject have lately been brought before the profession by Dr. Doherty, in the *Dublin Journal of Medical Science*, July, 1845. "Such cases," he says, "may be divided into two classes,—those in which merely a lobule of the placenta overlaps the os uteri, and those in which the placental mass is wholly engrafted on the cervix. In their origin, I apprehend, they are quite distinct. The former arises from the placenta, when originally attached in a natural situation, as denoted by the insertion of the

cord, growing irregularly, and thus extending a portion of its margin into the cervical region of the uterus, in which case it gives rise to, for the most part, merely a partial placental presentation, and this is the more usual form of the anomaly. But the occurrence of full placenta prævia, where that substance springs from the whole disk of the mouth of the womb, is, I believe, referrible to a deficiency in the decidua, which should naturally extend across the orifice of the Fallopian tube, and the absence consequently of the support which ordinarily it is thus enabled to give to the ovum as it enters the womb. Thus, in a case, recorded by Dr. Lee, of a young woman who poisoned herself in the second month of pregnancy, wherein he found, as in his opinion is invariably the case, the Fallopian tube pervious, the ovum had taken up its position at the os, and was forming its placenta there; and Sir E. Home details the case of a female, pregnant eight days, in whom a small ovum was detected near the cervix uteri."

A question here naturally presents itself, whether a placenta, whose edges extend over the os uteri, can be said to be situated in a normal position, in what portion of the mass soever the cord may be implanted? Again, it is very unsafe to draw any inference, one way or another, from such a doubtful case as that recorded by Sir E. Home. As to the mode of growth of the placenta, however, Dr. Doherty is at variance with a great authority; for we find M. Gendrin, in his philosophical treatise on practical medicine,* expressing his opinion that this organ increases, not by extending its edges, but by increasing its centre, according as room is made for it by the enlargement of the womb; its margin never making any advance. But M. Gendrin has another theory concerning the source of the hæmorrhage in placenta prævia, for the support of which this view is very convenient, if not essential; and it would be well to know, before attaching too much weight to this speculation, whether or not it has the advantage of seniority over its fellow.

* Paris, 1839, vol. 2, p. 222.

(To be continued.)

SOME ACCOUNT OF THE EPIDEMIC FEVER WHICH PREVAILED IN LIVERPOOL, IN THE LATTER MONTHS OF THE YEAR 1844.

By GEORGE CHURCHILL WATSON, M.D., EDIN.,
Associate Member of the Surgical Society of
Ireland.

(Continued from page 50.)

Dr. Roche has furnished me with the details of two cases, both of which I saw along with him, and which I think worthy of particularising from the prominence of certain symptoms and complications.

Mrs. T. was seized with rigor on the 1st of November, after which the usual symptoms set in. Amongst these symptoms severe pain in the back part of the head was remarkable. In a few days epigastric tenderness was discovered, which was immediately met by leeches, and relieved. The symptoms assumed the remittent type early in the disorder, so marked, as to realise