phthisis. Her complexion was pale and waxy, the lips livid, the respiration laborious, and the cough considerable. There was no expectoration; the pulse was rapid and feeble. She was extremely weak; her voice was low—a faint whisper; and the chief complaint was pain under the left mamma. Motion was too painful to allow of a full examination of the chest, and it was only ascertained that the left side was dull on percussion, and that there was no respiratory murmur there. The cough left her soon after admission, but the other symptoms remained unchanged. The pain in the side was incessant, and was aggravated by her constantly lying upon it. She was unable to lie either on the right side or on the back. Nothing gave any relief to the wearing pain. At last, an œdematous ridge was perceptible from the left armpit to the abdomen; the breathing became more difficult, the lips more livid, and the circulation more languid. She slowly but steadily sank, and died about six weeks after her admission.

POST MORTEM EXAMINATION, twenty-four hours after death, July 24th. Decomposition had set in to such an extent that a very strict examination was all but impossible. It was ascertained, however, that the left pleural cavity was full of blood, and that it was making its way to the surface. The cartilages of the third, fourth, and fifth ribs, and a small portion of the bones, were partially absorbed. The intercostal muscles had disappeared at the part affected. The skin was œdematous to a corresponding extent; and at the spot so often complained of, below and behind the left mamma, the effusion was only separated from the air by the thickness of the skin. The blood was separated into bloody fibrine, which adhered everywhere to the pleura; and into sanguineous serum: the whole amount completely filled the cavity of the pleura, and the lung was completely carnified. A small white spot was perceptible at the apex, from which it was conjectured the blood had flowed. A few tubercles existed in the right lung; but the stench was so sickening that we could not get further particulars.

In consequence of her great feebleness during life, the only history we got was, that she had been ill a long time with cough and weakness, and that speaking was a painful effort. The time of the effusion could not be ascertained. It would seem, from the *post mortem*, that she had had phthisis originally; that a small vomica had burst into the pleura; that this had been attended with, or followed by, profuse hæmorrhage. Of course this is only a conjecture, in the absence of any history of a blow or wound. But it is of course possible that in her unhappy profession she may, while drunk, have been the subject of violence, which had produced rupture of an intercostal artery.

The case, as it stands, is interesting from the insight it gives us of the conduct followed by blood when effused into closed cavities. It is rarely, if ever, absorbed, but remains as bloody fibrine, attached to the walls, and bloody serum free. I have known an hæmatocele remain unchanged for two years; the blood was never absorbed; never apparently decomposing, becoming putrescent or organising. We find the same in the uterus when the catamenia are retained; the bloody secretion remains for years almost unchanged, the only sensible difference being that the globules shrink, and the serum is absorbed. In the case above, the blood acted as the matter of an abscess is currently reported to do—as if making its way to the surface of the body.

It is not right to draw deductions from one single case; yet I cannot abstain from asking—What is the bearing of this one upon the generally received opinion that the blood is the chiefest of all the organs of the body? If the blood is the life, surely when so large a quantity is effused, and in contact with the living body, there ought to be some attempt at organisation. The absence of this, in almost every instance where blood is effused, should teach us that the organic nutritive power resides not in the blood, but in the tissues; and, consequently, that any doctrine, built chiefly or exclusively upon presumed changes in the blood, must fail of explaining satisfactorily the phenomena even of so-called blood diseases.

Original Communications.

LABOUR, WITH PERFECT OBLITERATION OF THE OS UTERI.

By R. G. MAYNE, M.D., Surgeon, Leeds.

I HAD drawn out the following statement many weeks ago, but was prevented from forwarding it by much occupation of

another kind. Having read, however, the "Case of Labour attended with complete Occlusion of the Os Uteri," by John Hatton, Esq., of Manchester, in the JOURNAL of August 15th, I felt stimulated to dispose of my case as being analogous to his in its most prominent feature, though happily different in its results, and in some other points.

I record the following case, not from the certainty of its being positively singular, but because I have met with nothing like it in my own practice, neither have heard nor read of an instance quite the same in that of others.

I had been engaged to attend, in her next confinement, Mrs. W., a respectable married woman, the mother of several children, and was accordingly summoned to her about 10 A.M. on 5th January last. I found her walking about her bedroom, with regularly recurring pains of considerable power. Having remained about half an hour, she suggested that I might safely see my other patients in the neighbourhood, as she felt she should be some time in requiring my assistance. I willingly embraced her considerate proposal, and was absent about three quarters of an hour.

On my return, she was still pacing the apartment, but evinced stronger indications of advancing labour; and I took the first opportunity of making an examination. My finger, introduced to the extent of two inches, encountered a large globular resistant mass, at once understood to be the head of the child. It was not the soft scalp, however, partially mobile on the yielding cranial bones and more or less supplied with hair, with which my finger came into immediate contact, but a tense, smooth, and what, though invisible, I felt to be a glistening surface. Surprised, I searched for the rim or border of the expanded os, supposing at first that my finger glided over the unruptured membrane; but the familiar boundary sought for was not to be found. Upon renewal of the expulsive efforts, I pursued my search, now seeking for the os itself, being satisfied that it had not yet expanded, though the pains were powerful, and the birth advanced to such a point as rendered that condition of it scarcely credible. Again and again I returned to the task, as opportunity was allowed, till labour had proceeded so far—further excited, no doubt, by the activity of my digitations—that the index finger could easily traverse the entire highly convex surface of the protruded womb, and into the surrounding angle of its junction with the vaginal parietes. There was no opening whatever. I do not state this under any qualification, but mean distinctly to imply that there was an obliteration of the os uteri so complete that, but for circumstances to be afterwards mentioned, and had it been a primiparous case occurring very early in practice, I might possibly have misjudged it to be an instance of total absence of the os, and come out strong in those days of lofty aim in support of the fanciful doctrine that impregnation must have taken place through convenient vessels in the walls of the vagina, or of an imperforate uterus, or in some other mysterious and very unintelligible manner.

With the advantages of many years experience, I confess to having felt for the time not only at a loss, but not a little anxious, the patient's straining having become violent, and her complaining loud. I of course knew where the os ought to be, and endeavoured most assiduously, from time to time, to discover, or induce by pressure with the tip of my forefinger, any lineament or trace of its former existence, but in vain. At length I did perceive a single minute elevation of membrane, such only as might have been scratched up by a small pin's point, in what I believed to be the true location. Strange to say, this gave me confidence, although I then suspected, and am since satisfied, that it was produced by the sharp pointed nail of the finger energetically employed to explore. Like others, I had before met with extremely contracted and rigid *ora*, especially in *primipara*; but this was an altogether different case; and while, in the most obstinate of the former, steady patient exercise of the ordinary simple expedients had sufficed, here there was literally no loophole, however small, to gain an entrance by: there was, at the same time, increasing risk of rupture of the anterior-inferior wall of the uterus; and delivery, I therefore decided, could only be effected by cutting through the abnormal substance which occluded, or rather, as already said, perfectly obliterated its orifice.

The circumstances were to me entirely novel; and, with some fears for the effect of such a proposition on the mind of my patient, though I knew her to be endowed with much good sense, I began by casually asking whether she had experienced any pain or uneasiness in the region of the womb after she became aware of her pregnancy, to which she readily replied, "Oh yes, a deal of pain"; and, to further questioning, stated

that this had existed for some time, about four or five months after that event, but then gradually got better; that, unlike her other occasions, she had no "show", nor any vestige of discharge whatever. I informed her of what was necessary to be done, and she at once assented.

I had no instrument with me but a lancet, and time did not allow of going or sending home for anything better, as the vertex, pressing powerfully on the highly stretched wall of the uterus, now reached the *labia pudendi* at every pain. I wound a narrow strip of rag round the handle and joint of the lancet, to render it fixed; and, taking into my service the nurse, who, fortunately, was a young intelligent married woman, I instructed her (placed at the patient's back) to insert the fore and second fingers of her right hand behind the *fourchette* or posterior angle of the vulva, and keep back the parts during next pain, so that I might see the actual state of the os, or of the adventitious substance by which it was hidden. This done, there was presented to view a quite smooth and continuous surface, the only distinctive indication discoverable being a slightly more livid hue of the new vascular formation which occupied the space of the natural aperture. I hastily pointed out this to the nurse, who, as well as she was able, marked the distinction. On recurrence of a pain, I made three or four scarifications with the lancet, all in the same line from right to left of the patient, producing an elliptical gap or separation in the new substance. I thus obtained partial admission of the point of my finger, with which I by degrees widened the opening to the extent of a shilling piece, distinctly perceiving that, in so doing, I had to *break down* what was more substantial than membrane—in fact, to the best of my judgment, was homogeneous with the *parenchyma* of the organ itself. I then ruptured the proper membranes: there was a considerable discharge of the waters, and everything went regularly on as in an ordinary case, but so rapidly that, within ten minutes from my using the lancet, a full-sized healthy female child was born; and no unfavourable circumstance has happened to mother or child to this time.

Such a case occurring to the young student, as yet innocent of the peculiarities occasionally to be met with in this branch of his profession, might prove (judging from my own recollections) bewildering in the extreme. But, in the hands of an ignorant midwife, or of one of those infamous impostors whose malpractice and its frequently horrible results our legal functionaries delight to shield from punishment, the reports of torture and mutilation, and of life sacrificed to gross presumption or bold reckless ignorance, indicate as too probable a more lamentable issue. In the outset, I have alluded to its singularity in my own experience, and have qualified my admission of anything similar in that of others. With your permission, I will, on a future occasion, state the grounds of that qualification.

SOME PRACTICAL OBSERVATIONS ON THE BEST METHODS OF INTRODUCING THE CATHETER IN CASES OF EXTREMELY NARROW AND IRRITABLE STRICTURE.

By HENRY THOMPSON, Esq., F.R.C.S., M.B.Lond., Assistant Surgeon to University College Hospital; Consulting Surgeon to the St. Marylebone Infirmary.

[Read before the British Medical Association, July 30th, 1857.]

THE object of the paper which I have the honour of presenting to the Association, is to offer as briefly as possible a few practical remarks on the best methods of passing a catheter through narrow and obstinate, or irritable organic stricture of the urethra, and of introducing it into the bladder, whether in the crisis of retention or otherwise.

I venture to recommend to the consideration of my brethren a course of practice which I have myself found successful in several cases of considerable difficulty, with the assurance that in the very large majority of cases it will enable us fairly and completely to afford relief by the catheter alone, without having recourse to the knife, and especially to that uncertain and sometimes hazardous method of dissecting through the deep tissues of the perineum, from the point of a catheter introduced as far as to the face of a stricture, but not through it—in other words, the operation long known as that of "the perineal section". And I conceive that, without entering upon any discussion of the relative merits of the knife or of the trochar, there can be but one opinion, *that relief by the catheter, where it can be afforded without force or damage, is*

the most desirable result for the patient. At the same time, it may be stated, once for all, that the faintest approach to anything like forcible catheterism in stricture of the urethra is deprecated here, not only as a dangerous, but also as generally an unsuccessful expedient.

It is now many years since Mr. Syme, of Edinburgh, expressed in forcible terms, his belief that in every case of stricture, if the urine passed outwardly, a catheter could with care and perseverance be passed through it into the bladder. Five years ago, I stated in my work on stricture, presented to the Royal College of Surgeons for the Jacksonian Prize, that my experience at that time compelled adhesion to the opinion that cases of failure with the catheter ought to be exceedingly rare. And I may say now, that a much larger experience has but confirmed that belief. During the period named, I have met with but one among the numerous and obstinate cases which I have been called upon to treat in private and in public, in which I have not, sooner or later, succeeded by the gentle use of the catheter in reaching the bladder. The case referred to was sent me by a gentleman in the Isle of Wight, and was an example of the disease in its most advanced stage and aggravated form, occurring in a thoroughly broken constitution. I ultimately relieved him by puncture of the bladder; and at death, about two months subsequently, with disorganisation of the kidneys, no catheter, however small, could be carried through a long and indurated stricture, although the urethra had been laid open as far as to the point of obstruction; nevertheless, the canal was not absolutely obliterated, for a drop or two of urine would occasionally pass through.

I beg leave, with the utmost deference, in the presence of so many of my distinguished *confrères*, to advert very briefly to the two or three simple means which have appeared to me greatly to facilitate the introduction of the catheter, in those cases in which the existence of more than usual difficulty has been verified. I would, however, first advert, and but for a moment, to the use of those constitutional means which, by the consent of all surgeons, are to be regarded as of great value in the circumstances described. And in order to do so in as few words as possible, we will suppose the case of a patient, with a twenty years' history of stricture, with fistulæ in the perineum, through which a large portion of the urine passes, with micturition painful and exceedingly frequent both by night and day, little urine passing by the external meatus, and that either in a slender stream or by drops. Further, that ordinary means, while visiting his surgeon on several occasions, have not enabled the latter to pass any instrument through the stricture, and that it has therefore become obvious that some other measures must be adopted in order to afford relief. There can be no doubt that in such circumstances much local irritation exists about the seat of stricture, and that there is much congestion of the already narrowed portion of the canal. The obstruction thus produced is often very materially diminished by a few days of rest in bed, by laxative medicines, a free supply of mild drinks to dilute the urine and destroy its acidity, hot hip-baths, a warm temperature to promote the action of the skin, and opiates, especially *per rectum*, if necessary.

After noting the improvement which usually follows such treatment in the case of a patient whose ordinary habits of life aggravate his complaint; after a week's lapse of rest to the urethra since any preceding attempt; the mode of conducting the first trial of the catheter constitutes one of the points which to me appears worthy of attention. In such circumstances, I prefer of all seasons to visit the patient early in the morning while he is in bed, when the skin is warm and moist after sleep under ample coverings.

When introducing an instrument, it is necessary that there should be no chill: all unnecessary exposure should be therefore carefully guarded against. Premising that the precise situation of the stricture has been carefully verified beforehand by passing a full-sized graduated instrument to the obstruction, and noting its distance in inches from the external meatus (or if not that, it is done at once), a small silver catheter may be selected. The size of this is, I think, to be determined by the known size of the stream of urine habitually passed; and this is best learned by ocular observation on one or two occasions, as patients often fail to represent it very accurately by words. As a rule, without entering upon the grounds on which this conclusion is based, the magnitude of the catheter selected should be a little less than that of the stream of urine. In the next place, it will be obvious in oiling the instrument, that the smaller it is, the less will be the quantity of oil which can be made to adhere to it; while it is