that such was not the case. The corrugated and somewhat livid state of the pharynx rendered it impossible for me to detect any wound or cicatrix thereof in its surface; but looking at the extent to which the healing consolidating process had gone on at the exterior part of the wound, we might expect that the solution of continuity in the mucous membrane would be still more perfectly restored, and that submucous sanguineous effusion would afford the only indication that the membrane itself had been punctured. Moreover, the bleeding from the mouth having taken place, without either vomiting or coughing, was certainly more symptomatic of an injury of the pharynx, than of one either of the larynx or trachea.

7. The appearance of the awls to which I have alluded tended to confirm the suspicions entertained. As already mentioned, their shape near the point (of one more particularly) corresponded with the external puncture, and the curve of the blade was in accordance with the direction of the wound. The awls were tolerably clean and bright, and at first chemical and microscopic examination could detect nothing on them but a little rust and fatty matter. A more careful subsequent scrutiny, however, convinced me that some of the stains on one of the awls were peculiar, and repeated examination and comparison of the stains *in situ*, with the aid of the microscope, gave me a strong conviction that they could be occasioned by nothing but blood. A very minute portion of membranous substance was detached, and at first supposed to consist of fibrin, having a little of the colouring matter of the blood entangled in its tissue. A very eminent microscopist confirmed my opinion as to the presence of blood, but pronounced the fibres to be derived from linen or cotton, (the awls being wiped with a cloth or cotton handkerchief, such fibres would readily adhere, or they might even be derived from the pocket lining.) The presence of blood was a most important point of evidence, and it is well known that there is often much difficulty in detecting blood stains for medico-legal purposes. By chemical examination we may arrive at a strong presumption of the presence of blood, when it is in sufficient quantity to afford a solution of its colouring matter in distilled water. When, however, the stains are slight, and especially if they are on iron or steel, I believe chemical examination to be utterly unavailing for their detection, and that the microscope alone can aid us in our research. For this purpose it has hitherto been little employed, but numerous experiments which I made in consequence of the case under consideration, have convinced me that its agency is available with great advantage, and that much of the fallacy which is connected with the detection of blood-stains may now be readily obviated thereby. At a future period I shall enter into this subject in detail, but at present I shall only briefly state a few of the results of my inquiry. I have no doubt that by microscopic aid we can generally detect blood-stains, however minute or ancient, on metal, wood, linen, cotton, &c., when not recognizable by any other method; and we are, moreover, enabled to distinguish with certainty other similar stains which

are apt to be mistaken for those of blood. It is really surprising how minute or faint a stain of blood may be demonstrated by microscopic power, and how vividly the characteristic hue is made manifest. The colour of the blood is very peculiar: it is a strong, fixed red, differing distinctly from the two tints more especially assumed by rust, viz., a bright crimson and a yellowish red. The application of ammonia produces no effect on the colour of the blood, distinguishing it readily from any red vegetable pigment. When adherent to iron or steel the appearance of blood stains is very different from that of rust in another respect besides that already mentioned. It is more diffused and irregular, whereas, rust assumes a linear arrangement, or else presents the appearance of a fungus-like growth. Blood stains are more permanent than those of rust, that is, they are less liable to modification, not having the same augmenting corrosive tendency. The commixture of blood and rust, will certainly often produce very perplexing appearances, but patient and persevering observation will not fail to detect the combination; whereas, in such cases, the result of chemical examination is sure to prove most unsatisfactory. Much has been said by Orfila and others as to the resemblance of the stains produced by lemon juice on steel to those of blood, but with the aid of the microscope such a mistake is impossible, there is no analogy in colour whatever. On linen, wood, cotton, or paper, the slightest smear or trace of blood, though quite unrecognizable to the naked eye, can readily be made apparent with a moderate magnifying power, aided by a strong reflected light, though chemical examination will be utterly vain. I have been able again and again to recognize blood on a splinter of wood taken from a floor where it had probably existed for years, in spite of wear and washing. There is no stain more indelible than that of blood, and so subtle is it, that it seems to incorporate itself with equal facility with some of the hardest as well as the softest tissues. I acknowledge, however, that occasionally it may be somewhat difficult clearly to give our reason for pronouncing a stain to be that of blood, and that we must rely on evidence of a negative character, arriving at the conclusion that it is a blood stain, because it is like nothing else.

Ewell, Surrey, July 24, 1850.

CASE OF OBSTRUCTED VAGINA, WITH REMARKS.

BY W. NEWNHAM, ESQ., FARNHAM.

Read before the Annual Meeting of the South-Eastern Branch.

THE following case contains some hints of practical importance which may not be uninteresting. Its publication has been delayed till all the parties closely connected with it have passed away from the present life; and it is now presented with the hope of extracting

good to future sufferers under similar states of embarrassing difficulty.

In the year 1818, I was consulted by Mrs. S——, a highly amiable and intelligent lady, who gave me the following history of her case. About three years previously she was confined with her first child: it was a tedious and laborious case, and towards its close one of her medical attendants used some cutting instrument upon the soft parts, so as to facilitate the exit of the foetus. The exact nature of this surgical aid could not be ascertained, but immediately afterwards the child was born. In all probability this operation consisted in the application of the forceps, and the cutting instrument existed only in the imagination of the patient. After a short time her medical friends left her, the placenta not having been expelled or removed. *Next day*, from the considerable tumefaction of the abdomen, a doubt arose whether another foetus was not remaining in utero; but no attempt was made to remove the placenta. Symptomatic fever was now lighted up, and the patient being in extreme danger, it was proposed, *after a week*, to attempt the extraction of the placenta. The whole parts concerned were, however, so exquisitely sensitive, and the attempt gave such excruciating pain that it could not be persisted in, and was abandoned.

The case was now left to nature; and it is not exactly evident what became of the placenta. By the then unbroken excellence of an originally good constitution the patient struggled through, and, in about a month was able to walk across the room, and appeared convalescent. Soon afterwards, however, she was attacked with pain in the left iliac region, which was moderated by large doses of opium, and the constitution again suffered dreadfully. This state continued some months, and at length an abscess formed and burst externally. Here we evidently had pelvic abscess; and from the degree of lameness which remained, and which was not quite gone when she consulted me, it is clear that the motive powers of the thigh were materially implicated. At the end of twelve months from her confinement she was again convalescent, and finally regained her health perfectly.

When, however, she returned to her husband, she found an insuperable obstacle to coition. Before this marital congress, she had only noticed the paucity and apparent sluggishness of the menstrual fluid, which she attributed to her not being strong. Mrs. S. was then for a time under the care of a practitioner in the north of England, who, without examining the vagina, advised her to wear sponge pessaries; this she attempted to do, without success and without benefit, during a year and a half, after which she consulted me by letter. In this detail she gave me the above history, with many other circumstances not necessary to mention; and, moreover, stated that the pessaries she had used had been exceedingly small, and that she had never succeeded in introducing them further than half an inch, when the pessary or the finger pressed against a firm unyielding body. Having stated the impossibility of prescribing for her without seeing her, an interview was arranged.

On the 16th of January, 1818, I visited my patient, and on introducing the finger into the vagina I found its further progress arrested by a firm cicatrix, amounting almost to a total obliteration of the canal; but I remarked that the cicatrix might be carried upwards into the vagina, and, therefore, formed the hope that the passage was free above the strictured part. I detected a very small aperture in this cicatrized portion. I proposed to myself, if possible, to render this small aperture available for the purpose of dilatation, and if not, having ascertained that the canal above the cicatrix was free, to make such an incision through the contracted portion, as to admit first the finger, and then to allow the subsequent introduction of bougies. I explained to my patient the probability of success, and the possibility of failure. I found her quite willing and desirous of adopting any plans which I thought right for her.

Circumstances occurred which prevented my seeing her again till the 16th of February. My reasons for preferring dilatation to incision, if possible, were the uncertainty as to what parts might have been agglutinated together during the healing process, and the consequent dread of injuring the bladder or the rectum, and the apprehension of hæmorrhage in a part which was so little under ordinary control. I found, however, that though I could pass a fine probe through the cicatrix, I could not introduce even a small urethral bougie. Assured that dilatation would not answer my purpose, I resolved upon incision carefully. It was with great difficulty I succeeded in getting in a small director through the aperture in the cicatrix; and having ascertained that after about half an inch of contraction the point of the director became free, I passed a guarded bistoury through the contracted portion. I found great difficulty in accomplishing this, from the extreme contraction of the canal, but having succeeded, I enlarged the aperture by carefully cutting in the direction of both *sides* of the pelvis. The stricture extended, as nearly as I could ascertain, for rather more than half an inch, and, consequently, as only a few fibres could be divided at once, I was under the necessity of withdrawing and re-introducing the bistoury several times, in order that I might ascertain the progress I was making. At length I succeeded, in introducing a small finger through the cicatrix, and was rejoiced to find the parts *above* free from contraction, and apparently without disease. The cicatrix was uncommonly firm and unyielding, and, having now made a fair aperture, I thought it advisable to finish the cure by dilatation, accordingly I introduced a moderate-sized vaginal bougie, and directed my patient as to the mode of its daily employment. After waiting a considerable time, and the discharge of blood being only very moderate, I left her with directions.

On the 21st she informed me that the hæmorrhage had been very considerable for two days, so as to confine her to her bed and to occasion great faintness, but it had now subsided; that she had introduced the bougie, with great pain and difficulty; that she had a discharge of putrid coagula in very large quantities, apparently of

matters which had been detained above the cicatrix, and that on the whole she thought herself going on well. On the 22nd I found matters proceeding favourably, the finger could be introduced through the stricture with tolerable ease, and there was less soreness of the parts than I expected; the cicatrix appeared firm and unyielding.

On the 2nd and the 9th of March, the report is one of slow but constant progress; on the latter day, the present bougie being now readily introduced, was exchanged for a larger size.

Circumstances prevented my visiting my patient till the 27th. I had heard from her on the 22nd, that she had missed her usual catamenial period, though she was unable to account for this deviation from nature's accustomed regularity. She reports difficulty in using the larger bougie, but gradually increasing success. Her health to-day does not seem quite so good as before; since she has passed the regular period, she complains of pains in the back, and has some mucous vaginal discharge. Much progress has been made in dilating the stricture; the finger now passes readily and loosely through its parieties, which have greatly effused except in one direction, towards the left side of the pelvis.

On the 5th of April, my patient reported progress, and requests a still larger bougie, but complains of suffering pain, after passing the bougie through the stricture; probably from irritation of the os uteri.

On the 12th she reports progress, and that she was taken unwell that morning.

On the 20th, reports still improving, but that the menstrual discharge has been irregular and variable, and by no means as it used to be.

May 2nd.—A letter received to-day informs me that she *miscarried* on the 29th ultimo.

My case has terminated; it remains for me to add only a few practical inferences.

1. Never leave a patient till the placenta has been expelled or removed. This is a pretty well understood axiom; yet, occasionally, deviated from even by well-informed practitioners; the fatigue consequent upon a protracted case—the exhaustion of the patient—the fear of introducing the hand into the uterus, all plead for delay—for rest and refreshment—in the hope that nature will finish the work she has begun. These arguments should not be tolerated, for the fatigue of the medical attendant is not for one moment to be considered; the exhaustion of the patient will be increased by the local and constitutional irritation arising from retained placenta; the danger to the patient from manual interference arises not from the introduction of the hand into the uterus, but from the rude manner in which it is too often effected: while, on the other hand, delay is attended with the most frightful consequences to the patient. Even in the case of adhered placenta, the most fearful results have arisen from a small portion only being left to putrefy and come away with the discharges. Uterine phlebitis is a common consequence of this unhappy resolve, which, if properly managed, the cases in which the whole of an adherent placenta cannot be removed are inconceivably rare; a

fortiori, would I earnestly implore every midwifery practitioner never to leave a patient trusting to nature for the expulsion of the after-birth.

2. Never quit the bed-side of the patient without ascertaining that there is not another foetus in utero, and then there never will be the doubt which occurred in the present case. Nothing is more easy, and yet nothing is more frequently neglected, than after the removal of the placenta to introduce the finger into the vagina, for the purpose of ascertaining that there is not a second child, and that there is no *inversio uteri*. Without this easy and necessary precaution, the patient cannot be left in safety.

3. Supposing the placenta, or any portion of it, to have been left behind, and local and constitutional irritation to have followed thereupon, it is wrong to make a manual attempt, after some days have elapsed, to bring away the remaining portions, for now inflammatory action of the uterine tissues, and exalted sensibility of the uterine nerves, will render the attempt insupportable to the patient, as well as dangerous in the extreme; and, moreover, decomposition of the placental structures will have advanced so rapidly, that they will yield in every direction, and it will be impossible to remove them entire, and harm rather than good must result from the interference. In this case *tepid irrigation* will be found of the first importance.

4. Never venture upon prescribing for a patient under doubtful circumstances without all the aid that digital examination, as well as that afforded by the *judicious* use of the speculum, will give, the folly and inefficiency of doing so is strongly shown in the present case by the recommendation of sponge pessaries.

5. It is, perhaps, difficult to say what became of the placenta in this case, whether it came away in the discharges, or whether it was in any way connected with the pelvic abscess which followed. Doubtless this abscess originated in the circumstances of the labour and its after-treatment, and this alone would be a sufficient warning against *watching the natural history of retained placenta*.

6. Finally, in the treatment of every case, *respicere finem*; look to the ultimate consequences. The final history of my patient remains to be told. She never bore another living child; abortion after abortion succeeded each other, till a fine constitution gave way, and in a few years she fell a victim to phthisis. This is easy to be understood. During the changes which had occurred within the pelvis, the uterus probably had undergone some material change of structure, or had been bound down by newly contracted adhesions, so as to be incapable of its normal development; hence the continued abortion after weeks of gestation,—hence too, probably, the early death of the patient. *True*, that she had been enabled for some years to participate in the privileges of matrimony, but it becomes a question whether these did not in their result exhaust life; and as *life*, not *enjoyment*, is the first—the chief—the grand object of our care and attention, it becomes a question, whether even with the present well-intentioned judgment it would not have been wiser, and really better, to have

left things as they were, rather than to have attempted their improvement.

Happily, we are not answerable for results: it is ours to act up to the best judgment we can form; and, if we have duly weighed, and dispassionately considered, and finally acted upon judgment rather than prejudice, it is happy for us that we may safely say that we cannot "command success," but that "*we will do better and deserve it.*"

IMPERFORATE ANUS TREATED BY AMUSSAT'S OPERATION.

By THOMAS DORRINGTON, Esq.,

Surgeon to the Manchester and Salford Lying-in Hospital.

JOHN McMAN was born at 5 P.M. on July 21st, 1849. His anus was discovered to be imperforate on the evening of the 23rd, and the child was consequently brought to me. I tried to reach the rectum from the perineum, but without success, although I introduced a bistoury one inch and a half deep, and cut as far as I dare on every direction, subsequently exploring with a probe-director and female catheter.

The child began to vomit at 7 A.M. on the 24th, and the matter ejected was in smell exactly like the contents of the gut which escaped when at a subsequent period I opened the descending colon, and of a similar yellow colour, though hardly so dark. It also refused the breast. Considerable delay occurred before I could gain the consent of the parents to further proceedings, but this having been at last obtained, I performed Callisen's operation, as improved by Amussat, at 9 A.M. on the 25th. I ought to state that the vomiting of yellow fecal matter, and the refusal of the breast, continued up till the time of the operation. I commenced by an incision in the left loin, extending from a point about an inch distant from the spine, and just above the crest of the ilium, to nearly the superior spinous process of that bone. The gut was soon reached after dividing the intervening structures, consisting mainly of fasciæ, with a few muscular fibres; it was diagnosed by its blueish colour, and the quantity of veins ramifying on it. I passed an armed needle through its coats, to secure it more completely, and then plunged in the bistoury. There was a sudden escape of foetid gas, and then of feculent fluid, which escaped with such force, and in such quantity, that a jet of it flew across the room. The edges of the wound everted themselves immediately, and after the bowel was emptied I secured them by sutures to the edges of the external wound, above and below. I put on a sponge compress lightly, and secured it with a roller. *The child took the breast immediately after the operation was finished, and never vomited again.* He soon, however, began to moan, and exhibit signs of pain and uneasiness, and without any very peculiar symptoms sunk gradually, and died at half-past eight A.M., on the 27th, *forty-seven hours and a half* after the operation. The fæces escaped regularly from the wound till death.

Autopsy six hours and a half after death.—No inflammatory appearances in the peritoneum; the descending colon above the wound was enlarged, and contained a moderate amount of fecal matter: it was empty below the wound. The bowel was opened in a very satisfactory manner, outside the peritoneum, and just above the commencement of the sigmoid flexure of the colon. I passed a blunt probe downwards from the point, and found, by a careful examination of the original excision in the perineum, that the rectum, which was extremely short, terminated in a semi-cartilaginous cord, about half an inch higher up than the deepest part of the wound. Keeping the probe firmly pressed downwards, I then tried to cut down upon it by deepening the perineal incision, but did not succeed till after I had made several attempts; this satisfied me that I could hardly have relieved the bowel in this direction under any circumstances, and quite justified the course I adopted.

I think that, if the operation of Amussat had been performed on the 23rd, it would probably have succeeded in saving the child's life, judging from its *immediate* good effects in causing the vomiting to cease, and enabling the child to take the breast.

I believe that this is the first time that Amussat's operation has been performed, in this country at least, on so young a subject, and on the whole the result is not discouraging.

Provincial Medical & Surgical Journal.

WEDNESDAY, SEPTEMBER 4, 1850.

OUR readers are doubtless aware that a public meeting has been convened under the presidency of LORD ASHLEY, for the purpose of awarding a suitable testimonial to Dr. CONOLLY, in honourable acknowledgement of his unwearied and most beneficial services in the cause of the insane. Under any circumstances it would have been a source of gratification to us, that in the flattering recognition of the merits of Dr. CONOLLY, an additional instance is afforded of public appreciation—in general not too lavishly bestowed—of medical worth; but in the fact of his being a member of the Provincial Medical and Surgical Association, we see an additional reason for felicitation. To those who have the privilege of knowing Dr. CONOLLY, it would be a work of supererogation to expatiate upon the excellent qualities of his heart, or the powerful attributes of his mind; we will only here record our conviction, that public expression of admiration and esteem has never been accorded to a more deserving object.