11. A discharge from the nipple exists in all affections of the true breast-gland. In *cancer*, it is sanguineous and scanty; in *true cystic disease*, it is viscid and abundant, and can be readily increased by pressure on the cysts; in *inflammatory*, it is watery or purulent.

12. In cases of tumour of the breast, when the skin has ruptured or become ulcerated, the margin of the opening or sore reveals the character of the disease; in *simple* cases, presenting the appearance of being ruptured, cut out, or punched; whilst in *cancerous* diseases there is no such distinct margin, the edges of the wound appearing thickened, indurated, and everted.

OBSTETRIC MEMORANDA.

UNDER this head we shall from time to time, as materials come to hand from correspondents, publish records of cases remarkable in themselves, or illustrating points of interest in obstetric practice, therapeutic or manipulative. We shall probably in this way preserve from oblivion the notes of very many useful and instructive occurrences in private practice; for the great obstetric experience is that—for the most part hitherto unwritten—of the great body of general practitioners throughout Great Britain. We will only ask those who may forward cases for record to relate them with the utmost brevity, and equally to condense any appended remarks.

MATERNAL IMPRESSIONS.

By E. GARRAWAY, Esq., Faversham.

SCEPTICAL as many of us are in attributing the various deformities which we encounter in new-born children to the effect of maternal emotion, cases do every now and then occur which compel us to believe. A more marked instance I never met with than the following, which has just come under my notice.

A young woman, *not aware that she was pregnant*, though the sequel proved that she was six weeks advanced, saw a rat advancing towards her. She shuddered, clutched her left arm below the elbow with her right hand, and shut her eyes. The child, at the completion of gestation, was born with the left forearm undeveloped, as if it had been amputated below the elbow, and totally blind—amaurotic.

FIBROID TUMOUR OF THE UTERUS.

By ARTHUR W. EDIS, M.B.

ON June 26th, I was requested to see E. D., aged 50, a married woman, who was in great pain in the pelvic region, blanched, puffy under the eyes, with very feeble pulse and great exhaustion. For the last ten years she had been subject to a continuous sero-sanguineous discharge from the vagina, occasionally sustaining profuse losses of blood, especially after any unusual exertion. During the winter of 1866, she suffered from severe weight and bearing down. On examination *per vaginam*, a large fibroid tumour was detected, filling up the cavity of the pelvis, and interfering with the pelvic viscera. By means of firm and steady pressure on the tumour, it was passed up above the brim of the pelvis, affording great relief to the patient. A dose of opium was given; and the liquid extract of ergot (mxv), with bromide of potassium (gr. x), in decoction of cinchona, was prescribed. In a few days, the patient was enabled to leave her bed.

On July 26th, she reported herself as greatly relieved. The discharge was less, and more watery than sanguineous. She was more lithe and active; and, from having been indifferent as to whether she lived or died, now stated that she felt better than she had been for years, and took a fresh interest in life. She was ordered to continue the medicine.

On August 7th, she had rather a sharp attack of hæmorrhage, from over-exertion; but, with this exception, she had improved daily.

On September 4th, the discharge was very slight. Quinine and iron were ordered, and an occasional aperient.

On September 21st, she reported that the medicine suited her admirably, and that she felt much stronger.

On October 15th, she reported herself as quite a different being. Her health was very good; she had scarcely any discharge, and could move about without discomfort. She was, she said, better in every respect than she had been for ten years.

REPORT OF THREE CASES OF LITHOTOMY: WITH REMARKS ON THE OPERATION.

By WILLIAM PAUL SWAIN, F.R.C.S.,
Surgeon to the Royal Albert Hospital, Devonport.

THE following cases occurred in my practice at the hospital during October and November 1866.

CASE I. Henry Brown, aged 9, was admitted on October 16th. He had suffered for two years with symptoms of stone in the bladder. He was a delicate boy, and much emaciated with continuous suffering. There was prolapse of the rectum. He was cut for stone on October 20th. The lateral operation was performed, the groove in the staff being cut into immediately. The stone was caught on the first introduction of the forceps. Owing to its size, the external wound had to be enlarged to remove it. It was a large phosphatic calculus, perfectly round, measuring four inches in circumference, and weighing 3 vii gr. x. The urine ceased to run by the wound in a week, and the boy was discharged cured on November 18th. No incontinence remained.

CASE II. Albert Smart, aged 7, was admitted on October 18th. He was a well-nourished, but "rickety" boy. Owing to the length and tightness of the prepuce, circumcision was performed on admission. He was cut on October 27th, by the lateral method and direct puncture into the groove. The point of the forefinger of the left hand was passed into the bladder, and placed on the stone, before the staff was withdrawn. The forefinger of the right hand was then passed up the rectum, behind the stone, which was then pressed forwards through the internal wound, and removed, without the use of forceps. It was an oval phosphatic calculus, and weighed 3j gr. ix. Very little blood was lost. On November 14th, urine ceased to run from the wound. Between November 3rd and 14th, he had an attack of bronchitis, to which he had been previously subject. He was discharged cured on December 13th. No incontinence remained.

CASE III. William Mollen, aged 11, was admitted October 30th. He had suffered pain and sudden stoppage in micturition for more than twelve months. During the last fortnight, the pain had been more severe. He had never had incontinence, nor passed bloody urine. He was cut on November 3rd by the lateral method and direct puncture into the groove. The stone was removed, as in the former case, without the use of forceps, the forefingers only being used. It was a lithic acid calculus, and weighed fifty-six grains. There was slight hæmorrhage two hours after the operation, which was easily suppressed. The urine ceased to pass by the wound on November 12th. He was discharged cured on November 26th. No incontinence remained.

In the simple history of these three cases, there is nothing out of the common to arrest the attention of the profession. I wish, however, to refer to certain details in the operation of lithotomy which were followed in the above cases; viz., to the method of making the incision into the bladder, and to the manner in which the stone in two cases was removed therefrom.

First, then, with regard to the incision. In each of the above cases, I followed the practice of plunging the knife at once through the perineum down to the staff, placing the point in the groove, and then cutting outwards, and completing the external incision. In order to effect this, the following precautions must be taken; first, to place the patient perfectly "square" upon the table, having the buttocks brought exactly to the edge, and the thighs equally flexed and abducted; and, secondly, to have the staff held with the utmost precision, firmly in the median line, the handle being slightly bent backwards towards the abdomen, so as to make the curve prominent in the perineum. It must not be hooked up too close under the symphysis, and the finger must be placed in the rectum to regulate its distance from that intestine. It should then be distinctly felt through the perineum at a point about one inch in front of the anus; and at that point the knife (Liston's) should be entered with the blade lateralised, to correspond with the direction of the groove on the staff. The blade is then boldly thrust onwards until its point strikes the groove in the staff, along which it should be thrust until urine flows by the wound. Then leaving the groove, by a rapid sawing motion the blade is brought out, cutting the external wound in the integument to the required extent. As the blade recedes from the staff, the forefinger of the left hand is thrust along the roof of the incision until it reaches the staff, by which it is conducted through the opening in the urethra and neck of the bladder into the bladder, and in the majority of instances at once impinges on the stone. Should the incision in the neck of the bladder be insufficient to admit of this proceeding, it must be enlarged by replacing the point of the knife in the groove, and thrusting the blade a little further onward. The point of

the forefinger of the left hand, being now placed upon the stone,* must retain it *in situ* whilst the staff is being withdrawn. And this brings me to the second portion of the remarks I desire to make upon these operations; viz., the method by which, in two of them, the stones were removed from the bladder. It will be observed that, instead of removing the stones in the ordinary manner with the lithotomy-forceps, they were, in the last two cases, removed simply by the instrumentality of the left forefinger in the bladder resting on the stone, and the right forefinger in the rectum pressing the stone forwards from behind. As I have shewn, there is seldom any difficulty in placing the point of the left forefinger on the stone, and retaining it at the neck of the bladder close to the incision just made. Then, by passing the right forefinger up the rectum, taking care to keep well to the posterior surface of the intestine, (or we may, perhaps, push the stone away from under the left forefinger), we get its point behind the stone, which is thus placed between the two forefingers. With very little trouble, the stone is then pressed forwards through the incision, the right forefinger acting as the *vis a tergo*, whilst the left guides it forwards in the right direction, and prevents its slipping from side to side. The stone, once out of the bladder, is easily removed from the external wound by sliding the left forefinger over it, and hooking it forwards; and, even at this stage, the finger in the rectum may materially assist in the process. The rapidity with which a stone may thus be removed from the bladder, especially if the operator possess long and slender fingers, can only be conceived by those who have either practised or seen this manipulation.

Recurring again to the mode of incision, I would remark that, although I believe it is the practice of some lithotomists to follow the plan of immediate puncture into the groove, I am not aware that the proceeding has ever been formally brought under the notice of the profession. Mr. Allarton, in his pamphlet *Lithotomy Simplified*, in describing the median operation, writes (p. 25), "I carry the knife steadily and firmly on till it strikes the groove of the staff." This instruction, however, has never, so far as I can ascertain, been written with reference to the lateral operation. Mr. Coulson, in his book *Lithotrity and Lithotomy*, casually mentions the practice, but only to condemn it. He says, at p. 175: "Some operators, formerly rather than now, were in the habit of cutting on the groove of the staff with a single incision; but, unless the perinaeum be extremely shallow, I would not recommend such a proceeding." The nearest approach to it I have read is to be found in an excellent paper on Lithotomy by Mr. Teevan in the *Medico-Chirurgical Review*, January 1867. Writing on the distance from the anus at which the external incision should commence, he says (p. 209): "If the operation be commenced low down, a surgeon can thrust in his knife more boldly than when the high incision is resorted to, and hence will generally, at the very first incision, hit or get close to the staff"; and, again, a little further on, "I have observed that those surgeons who perform lithotomy with the low incision make only two cuts; that is to say, having made the external incision, they have not to introduce the knife again to divide any structures preparatory to entering the groove in the staff." But here, although Mr. Teevan evidently advocates hitting or getting as close as possible to the staff, he neither lays it down as a rule that the staff should be hit at the first puncture, nor does he seem to have ever seen any operator who systematically endeavoured to do so. Mr. Christopher Heath first pointed out to me the advantage of this mode of proceeding which he himself adopts; and I know that Mr. Fox, one of the surgeons to the South Devon Hospital at Plymouth, has for many years successfully followed this practice.

With regard to the extraction of the stone, as "there is nothing new under the sun", so this method of removing calculi from the bladder is as old, at any rate, as Celsus. Mr. Coulson, in his book before quoted, relates that Celsus used to introduce the index and middle fingers of the left hand into the rectum, and with the right hand press on the lower part of the abdomen, so as to bring the stone into the neck of the bladder. This done, an incision was made near the anus down to the neck of the bladder, and the stone thrust through the wound by the fingers in the rectum. (*Lithotrity and Lithotomy*. By William Coulson. P. 145.) The most difficult part of the operation, especially in children, is the introduction of the forceps and seizing the stone. Sir William Ferguson, who is as dexterous in the use of the forceps as any living lithotomist, in his *Practical Surgery*, writes: "Perhaps the most dexterous part of the operation pertains to the use of the forceps. With such a narrow wound, some care is required in slipping them towards the bladder; for, otherwise, their points might get between the prostate and rectum; indeed, even in using the forefinger, there is danger, particularly in

* "If possible, and it almost uniformly is so, the stone should be felt with the finger before any instrument is introduced, or any attempt made to seize it." (Liston, *Practical Surgery*, fourth edition, page 511.) "The left forefinger may be introduced... so as to feel the stone, maintaining it there *in situ*." (Holmes, *System of Surgery*, vol. iv, p. 457.)

children, of passing in this direction." Mr. Poland, in his article on Lithotomy in Holmes's *System of Surgery*, writes (p. 456): "The third step is the introduction of the forceps and the removal of the stone. Now, although the second proceeding is one of the utmost delicacy and moment, the present far excels it in difficulty of execution, and is often the cause of the delay and trouble witnessed in lithotomy." But, the forceps being successfully passed into the bladder, the dangers of their use are not at an end. A portion of the bladder may be pinched up with the stone; or the stone, being missed at the first plunge, is pushed back into the fundus, and much harmful contact with the inner surface of the bladder may take place in the repeated attempts to catch it; or, being fairly caught, it may fly into fragments; or, if the stone be not round, it may be caught in the long axis and so give great trouble in extraction. At any rate, supposing the stone to be fairly caught between the blades of the forceps, in the withdrawal the inner wound will have to be dilated to the size of the stone *plus* the thickness of the two blades of the forceps. All these dangers and delays are avoided by the use of a natural pair of forceps, such as the forefingers of the right and left hand provide. In operating on children, at any rate, I shall continue to pursue the plan of immediate puncture and of removing the stone without the use of the forceps, believing that the operation is thus performed more rapidly, and with less danger to the patient.

ON THE LOCALISATION OF THE FACULTY OF SPEECH.

By F. BATEMAN, M.D., M.R.C.P.Lond.,
Physician to the Norfolk and Norwich Hospital.

APHASIA, or loss of the faculty of articulate language, is a subject which has lately very much occupied the attention of the profession, both in our own country and on the continent. Two years ago, I published a short paper in one of our medical journals, in which, after mentioning at some length a most typical case which had occurred in my own practice, I briefly reviewed what was then known about the pathology of aphasia, beginning with Bouillaud's localisation of the faculty of speech in the anterior lobes, passing on to Dr. Dax, who placed it exclusively in the *left* hemisphere, basing his assertion on no less than a hundred and forty observations. I then concluded the bibliographical part of my paper by an allusion to the *ne plus ultra* of modern pathological topography, contained in the startling assertion of M. Broca, that the lesion in aphasia was in the posterior part of the third frontal convolution of the left hemisphere!

Since I first turned my attention to this matter, no subject has perhaps more engaged the attention of the profession than that now before us. Numerous cases and papers have been published in this country and on the continent, but these productions have, in my opinion, been marked by two faults or deficiencies. In the first place, their authors have in many instances written with preconceived notions, and their works bear the impress of a desire to prove one theory *against* another, rather than a desire simply to place on record cases which may speak, as it were, for themselves. Another great defect is, that among the numerous cases published, few, very few, have been supported by that inexorable scrutiniser of facts, necroscopic investigation. Now it so happens that it has been my singular good fortune to be present at three *post mortem* examinations of cases of aphasia, which have occurred in my own practice, or in that of other gentlemen, who have had the courtesy to give me the opportunity of carefully noticing the morbid appearances; and I propose to consider how far the experience gained by these cases tends to substantiate or invalidate M. Broca's somewhat startling theory as to the localisation of the faculty of speech.

Before speaking of the cases in question, I would just for one moment ask what speech is?

Articulate language depends on three conditions. 1. There must be integrity of thought, or at all events an idea must be conceived. 2. There must be a connection between the idea and the conventional signs which constitute the verbal forms of language, and this second condition is peculiar to man. 3. The idea being conceived, and the verbal form found, the muscles of phonation and of articulation must be able to obey the mandates of the will.

These being the conditions under which speech is possible, what are we to understand by the comparatively modern term, aphasia? Some authors have applied this word exclusively to cases where, the ideas being formed, the means of communication between the outer world do not

* Read before the Medical Section at the Annual Meeting of the British Medical Association in Dublin, August 1867.

exist, for want of the *memory of words*. I would prefer using the word *aphasia*, however, in its strictly etymological sense (*a-phasis*), and thus apply it to all cases where this faculty is abolished, from whatever cause. This definition necessitates a division into two classes.

1. Those cases where one or both of the first two conditions of speech are wanting, where either no idea is formed, or, if formed, the words to express that idea are not at hand, are not remembered. This I would call the *amnesic* form, and it is purely an intellectual defect. I had a capital illustration of this form in a man lately under my care in the Norfolk and Norwich Hospital. If I took up a poker and asked him what it was, he would say, "I know what it is, but I cannot say the word." And to prove that he really did know what it was, I asked him its use, and his answer was, "To make up the fire." I then asked him whether it was a walking-stick—no—was it a broom?—no—was it a poker? Yes, he would say instantly, adding that that was the word he wanted to find. This shows that the idea of a poker was conceived, was impressed as it were upon his brain, but the "memory of words" failed him, and I look upon his case as a typical illustration of the *amnesic* form of aphasia.

2. In a second class I would place all affections of speech dependent upon grave disorganisation of the central portions of the brain, such as effusion into the ventricles, clot in the corpus striatum or thalamus opticus, softening or abscess in the medullary substance not extending to the grey matter of the convolutions. Here the lesion may be purely a motor one; thought and language may both be present, and the patient may *will* to express his ideas in words; but, as the fibres are ruptured which act as the medium of communication between the surface grey matter and the coordinating centres of articulation, and volitional incitations are not conveyed, there will be no speech.

The first case to which I beg to call the attention of the meeting is that of a shoemaker, aged 60, who was admitted into the Norfolk and Norwich Hospital under my care at the end of last year, with the following antecedent history. Three months before admission, whilst at work, he suddenly lost the entire use of his right side and also of his speech, there being almost complete loss of articulate language for about a fortnight, at the end of which time he could just make himself understood. As I am necessarily limited to time, I will not dwell minutely on the clinical history of this (or indeed of any of the cases) further than to say that during his sojourn in the hospital he had a second attack, resulting in paralysis of the left side, and that he died three months after admission. A most careful *post mortem* examination was made, which I will not take up the time of the meeting by describing in detail; suffice it to say that the loss of motor power on the two sides was explained by a clot in the central part of each hemisphere; there was no obstruction of the middle cerebral arteries, and we clearly satisfied ourselves that the convolutions were in no wise affected.

The second case was that of a gentleman, aged 36, married, with no hereditary predisposition to disease, but who had been the subject of delusions for sixteen years, requiring the restraint of an asylum. About six months previously to his death, he began to hesitate in his speech and to give evidence of the loss of the memory of words. Some months later he was suddenly attacked with convulsions, followed by right hemiplegia, with total loss of speech, and died in a few days. I was invited by his medical attendant to be present at the autopsy, when there was found evidence of chronic thickening of the arachnoid, with congestion of the pia mater of the left side especially; there was no clot, no degeneration of cerebral matter; the anterior convolutions were especially examined, and found quite healthy. The most remarkable appearance that this examination disclosed was a deposit of rough bony matter, or exostosis, at the centre of the fossa, corresponding to the middle lobe of the brain on the left side, and to this rough surface the cerebral membranes were slightly adherent. In the absence of any more decided cause, I presume that this diseased condition of bone might account for the convulsions and subsequent paralysis; but in reference to the subject under consideration, I wish to call attention to the fact, that the disease was not in a part of the skull corresponding to the anterior lobes.

The third case was that of a gentleman, aged 51, whom I saw in the spring of the present year, and who had then been ailing for four or five months; his symptoms being those indicating want of brain-power, characterised more particularly by loss of the memory of words. He would call things by their wrong names; for instance, being in a room where the fire was burning particularly brightly, he said, "How bright the poker looks." The person to whom he was speaking said: "You mean the fire." "Yes," he said, "I mean the fire." He would be thus confused in the choice of words to express his thoughts; but he was quite conscious of this confusion, and would say he could not find words to describe his symptoms; the idea was conceived, but the means of communication with the external world did not exist. Without dwelling

on further minutiae, I may add there was loss of motor power on the right side, which suddenly culminated in an apoplectic seizure, with right hemiplegia, and total loss of speech, from which the patient sank in a few weeks. At the *post mortem* examination there was observed considerable congestion of the veins on the convex surface of the brain, there was no opacity of membranes or other morbid appearances either on the upper surface or at the base. The circle of Willis, and the arteries generally, were quite healthy, both middle cerebral arteries being especially examined and traced along the fissure of Sylvius. At the point of union of the middle third with the posterior third of the convex surface of the left hemisphere, was a dilatation or bulging out of the arachnoid, giving the appearance of a cyst. This contained at least two drachms of serum, the evacuation of which disclosed a tolerably well circumscribed portion of softened cerebral tissue, of about the size and shape of an apricot, with its upper segment depressed, so as to form a cup-shaped cavity. It was here the serum was lodged, and there was at this spot evidently an actual destruction of cerebral matter; the softened tissue was of yellowish grey colour, resembling a strong solution of gelatine in appearance. In the centre of this softened portion was a very small clot, or rather layer, of black blood, of about the size of an ordinary wafer. From the small size of the clot, and the great extent of the softening, it must be inferred that the softening preceded, and was the cause of the clot, and the recent date of the sanguineous effusion would also favour this view. The frontal convolutions were examined with great care, especially the third convolution, and the substance between it and the corpus striatum, but these structures were found quite healthy. The disease was, in fact, limited to the posterior third of the left hemisphere.

Now in all these three cases the frontal convolutions were examined with great care, and were found quite healthy; and in the third case, which was a marked instance of the *amnesic* form of aphasia, or that characterised by the loss of the memory of words, the lesion was not near the anterior lobe at all, being situated at the posterior part of the upper surface of the hemisphere. These three cases, then, are directly opposed to Broca's theory; and, although their number is too limited to upset that theory, I need not say that three negative cases, supported by *post mortem* verification, go far to outweigh three hundred cases where no autopsy has been made.

I wish to add that, in each of the above cases, there existed an abnormal condition of the left side of the base of the skull. In one case, as above stated (No. 2), there was actual disease of bone; in case No. 3, there was an unusual development of that part of the petrous portion of the left temporal which corresponds to the perpendicular semicircular canal; and in case No. 1, there was a remarkable bony prominence in the left middle fossa, not existing on the other side. It will doubtless be said, that unsymmetrical development of the two sides of the skull is not uncommon. I quite admit this, and desire simply to record what I have observed, without attempting to draw any inference from such observation. I cannot but think, however, that it would be extremely desirable, in future autopsies of persons who during life exhibited symptoms of lesion of speech, that the condition of the bones of the skull should be minutely examined, and any unusual appearances accurately recorded.

In reference to these researches, some may say, "*Cui bono?*" Why attempt to prove what seems an impossibility? that one side of a perfectly symmetrical organ can possess a property not appertaining to the other side. Now, although I am by no means prepared to say that it is so, the theory must not be dismissed on the ground of impossibility. It is not by mere accident that we are right-handed; for the recent investigations of a foreign pathologist have shewn that, in the development of the foetal brain, the convolutions of the left hemisphere are in advance of those of the right; hence it is that the child uses the right hand in preference to the left, and thus he becomes right-handed, or rather, as M. Broca suggests, we should say more properly *left-brained* (*gauche du cerveau*); and it has been suggested that not only we write, and do all kinds of work with the right hand, and consequently by the influence of the left side of the brain, but that we also speak from the left side of the brain, and that hence from early infancy the convolutions of the left side are considerably in advance of those of the right. Here then is a subject well worthy of the careful investigation of all labourers in the field of science. No question is perhaps engaging so much the attention of pathologists in all parts of the world as the localisation of this very faculty of speech, and who knows that these researches may not result in the localisation of other faculties? Some years since it was stated that paralysis of one of the upper extremities depended upon a lesion of parts posterior to the corpus striatum, and that paralysis of one of the lower extremities upon a lesion on a level with, or anterior to, the corpus striatum. These assertions have been shewn to be fallacious; and, at the present day, loss of motor power, limited to one limb,

cannot with any amount of precision be ascribed to lesion of any particular part of the brain.

This subject, though abstruse and but little understood, would seem to be particularly adapted for research and discussion, in such an assembly as I have now the honour of addressing, and although I frankly admit that the main object of our meetings is the discussion of subjects bearing more immediately upon practical medicine, still abstract questions of pathology should most certainly receive a share of our attention; and as one of the best essays on loss of speech in cerebral disease, has emanated from the pen of a distinguished professor of the Dublin school, it has seemed to me not an unsuitable occasion to introduce this subject to the notice of the British Medical Association, which I am sure contains amongst its members the elements necessary for contributing something towards the solution of this most interesting psychological problem—the localisation of the faculty of speech.

TOO LATE OPERATIONS FOR STRANGULATED HERNIA.

By GEORGE W. CALLENDER, ESQ.,

Assistant-Surgeon to and Lecturer on Anatomy at St. Bartholomew's Hospital.

THERE can be no question but that it is much better to avoid, if possible, opening the peritoneum in operating for the relief of a strangulated hernia. This was long ago understood and explained by Franco (*Traité des Hernies*, par Pierre Franco, ed. 1561) and Petit, and any such table as the accompanying shews at a glance the favourable results associated with so doing.

A.—Table shewing certain results of Operations performed at St. Bartholomew's Hospital for the relief of Strangulated Hernia during the year.

	First Series, 1852-3-5-6.		Second Series, 1864-5-6.		Totals.	
	Oper.	Dths.	Oper.	Dths.	Oper.	Dths.
Femoral Hernia { Sac opened . . .	21	14	31	11	52	25
{ Sac not opened . . .	24	6	9	1	33	7
Inguinal Hernia { Sac opened . . .	11	5	20	13	31	18
{ Sac not opened . . .	13	6	9	1	32	7
Umbilical Hernia { Sac opened . . .	3	2	3	3	6	5
{ Sac not opened . . .			1		1	

On the other hand, wounds of the peritoneum are not to be reckoned, in themselves, as a chief cause of death; for the cases in which the sac is opened include a large number of hopeless operations, cases far different, as a rule, from those tabulated under the heading of "sac not opened." Thus it is unfair to contrast the two classes, unless with some explanation of their surroundings, which will generally account for the fatal results, independently of this or that variety of operation.

I have elsewhere endeavoured to draw attention to this point (*St. Bartholomew's Reports*) whilst explaining that in one year, 1864, in eight fatal cases of inguinal rupture, in all of which the sac was opened, independent causes of death were apparent in six. Gangrene of the bowel with perforation, stricture (malignant) of a portion of intestine, rapid exhaustion, bronchitis, pleurisy, and carcinoma of the rectum, left only two deaths to unmixed peritonitis.

It may be laid down as a general rule, that time is the chief guide in determining whether or no the sac is to be opened, for when the history of strangulation extends over many days, the bowel must be seen, and its condition examined, lest, inadvertently, gangrenous intestine should be pushed back into the abdomen. Thus, also, the most unpromising cases are those in which strangulation has been of the longest duration; for instance, in one series of operations the strangulation had existed on an average for twenty-two hours in recoveries, but for seventy-two in the cases ending fatally; and, again, in the year 1864, in the inguinal ruptures, for which the sac was opened, the time of strangulation averaged fifty-five hours, in those for which the sac was not opened only twenty hours. As an instance of the need for opening the sac when strangulation is of long standing, such a case as the following may be instanced. In December 1851, a female, aged 75, had a femoral hernia strangulated for five days, at the end of which time it was returned by operation, without opening the sac. After her death, a portion of intestine was found in a state of gangrene, and feces had escaped into the peritoneal cavity.

The sac of this hernia, as is constantly observed in similar cases, had also sloughed; indeed, in dissecting a hernia it is easy to see that all the blood connections of even an old sac are still with the interior of the abdomen; so that, when compressed by the boundaries of the inguinal or femoral ring, it suffers from strangulation equally with the intestine or omentum. A case has been recently mentioned to me in which a surgeon had opened a supposed abscess in the groin, and let

out feces from a hernia, sac and intestine having sloughed. Yet the patient recovered, and the wound healed.

But it is not the protracted strangulation alone which may bring a case into a condition for the relief of which an operation comes too late. In the severity which from the first occasionally characterises the symptoms, and, in the rapidity with which they deepen in their urgency, it is not difficult to recognise the forecasts of a fatal result. There are no cases more remarkable in this way than those congenital ruptures which, having been thought to be cured for years past, suddenly burst out along the canal, and become strangulated, undergoing with rapidity such fatal changes as quite invalidate an operation.

I recollect very well seeing Mr. Stanley operate upon a congenital inguinal hernia, which consisted of full three feet of small intestine, which had been suddenly forced out of the abdomen, about twenty-four hours previously. The bowel was much distended and sodden, so that it could not be returned after division of the stricture, until several punctures had been made into its walls, letting out six or seven ounces of clear, limpid fluid. The patient died within twenty-four hours, from acute peritonitis. In April 1853, Mr. Stanley operated upon a congenital hernia which had been strangulated twenty-four hours. A quantity of serum escaped on opening the sac, and the intestine, several coils of which had passed out from the abdomen, was of a bright red colour, with lymph effused about its surface. This man sank continuously, but lingered four days, and then died from a peritonitis which involved every part of the serous membrane.

In some of these cases, fatal changes are established even more rapidly. In August 1864, I was sent for to the hospital to operate on a man whose congenital hernia had been strangulated less than twelve hours, yet the eighteen inches of bowel, which had been forced out from the abdomen, were so sodden and dark coloured, and there was such a large escape of turbid serum from the peritoneum, that it seemed to me quite impossible for him to recover, and he too died the day after the operation.

Although I have always regarded these suddenly reproduced congenital ruptures as anxious cases, yet I had no idea that they were so unfavourable in their results, until, on looking through my notes, I found that of nine consecutive operations, only two were recovered from.

Whilst fatal disorder, such as no operation can interpose to save from, is rapidly engendered in some cases, (cases in which the sac has almost invariably to be opened) we now and then come across a patient who seems to light up a fatal peritonitis on the smallest possible provocation. Some years ago, February 1851, a man died rapidly in one of the medical wards, with symptoms of acute peritonitis, which set in whilst he was recovering from acute rheumatism, and, on examining the body after death, the fore-part of a knuckle of intestine was found lying in the femoral ring, constricted, and evidently forming the centre whence the peritoneum had inflamed. There was no obstruction to the passage of feces through the canal.

In the great mass of cases, the worst symptoms are, however, less quickly developed, and are better tolerated in their accession. Hence, especially amongst the poor, the rupture is left unnoticed, sometimes for many days, and these cases furnish the bulk of the operations to which I wish more particularly to refer. I take the fatal cases of femoral hernia in which the sac was opened, and what can be hoped for when an operation is performed under such circumstances as the following histories disclose.

E. M., aged 55, was admitted into Lucas Ward, under the care of Mr. Wormald, February 1st, 1853. A knuckle of congested intestine was returned by operation, after four and a half days' strangulation. The patient died twenty-five hours later. The intestinal contents had escaped into the peritoneal cavity. Along the line of constriction, and there only, an ash grey slough of intestine showed that the bowel had lost its vitality. On the inner side of this was a round aperture, and through this the feces had escaped.

S. S., aged 52, was admitted into Lucas Ward, under the care of Mr. Lloyd, with two days' constant and stercoraceous vomiting. The sac contained a large quantity of omentum, which completely covered a small knuckle of intestine, of a perfectly black colour, all its coats having given way on the inner side, just under the edge of Gimbernat's ligament. The opening into the intestine was enlarged, and the parts protruded were secured in the wound. She lingered for fifty-six hours, and then died.

M. R., aged 60, after seven days' strangulation of a rupture, was admitted under Mr. Stanley. After the bowel had been returned, she sank rapidly from general peritonitis, symptoms of which existed at the time of the operation.

A. T., aged 75, came under Mr. Lloyd, with hernia, strangulated four days. This patient died soon after the operation, and a portion of

intestine was found gangrenous, and at one part perforated by as many as five distinct openings.

A. P., aged 44, had a strangulated hernia for six days, and was then admitted under Mr. Stanley, March 1855. A knuckle of black intestine was laid open, and left in the wound, the patient sinking on the third day.

S. S., aged 75, came to St. Bartholomew's with a hernia, which had been strangulated four days. Mr. Stanley opened the sac. Simultaneously with the division of Gimbernat's ligament, faeces began to flow from the outer and lower side of the intestine. Gradually sinking, she died eighty hours after the operation.

M. H., aged 48, had a femoral hernia strangulated four days. After opening the sac, the intestine, of a very dark colour and its surface dull and granular, was returned into the abdomen by Mr. Lawrence. On the third day after the operation, faeces and foetid gas escaped from the wound in the groin, and she slowly sank and died, but not until the fifteenth day after the operation.

The seven preceding histories are taken in order from the fatal operations of the first series in Table A. It would be easy to multiply this array of hopeless cases, but it is sufficiently evident from those referred to, that they are fatal to life, from no influence exercised by the operation of opening the sac. Yet it is from an association with such cases that this operation has been brought into discredit, and most unfairly so. It should be judged of as a necessity, not as a choice, and with our present knowledge of the subject, should cease to be placed in comparison with the operation performed for a less severe class of cases. Take away the hopeless strangulations, and deal only with those ruptures of a less severe nature, in which the sac has to be opened because it is the seat of stricture, and the results are as favourable as when the peritoneum is not incised.

But to strengthen the proposition that the mortality after opening the sac is so high, simply because the operation is so often not performed until it is too late, we may refer to the causes of death in those cases in which the sac is not opened, an operation, which, if carefully selected, is, I believe, well nigh invariably successful. Still keeping to the femoral ruptures, Table A shows that of thirty-three of these operations, only seven died, and in Table B I have arranged these fatal cases in order, adding the cause of death in each.

B.—Table shewing Cause of Death in Seven Cases of Femoral Hernia in which the Sac was not Opened.

1 ..	aged 51 ..	Strangulation 42 hours ..	Death from acute peritonitis.
2 ..	aged 70 ..	Strangulation 12 hours ..	Death from acute peritonitis.
3 ..	aged 75 ..	" "	Death from diffuse cellulitis on the sixth day.
4 ..	aged 58 ..	Strangulation 21 hours ..	Death from diffuse cellulitis and bronchitis.
5 ..	aged 48 ..	Strangulation 48 hours ..	Death from gangrene of bowel. The sac ought to have been opened.
6 ..	aged 75 ..	Strangulation 4 days ..	Death from gangrene of bowel. The sac ought to have been opened.
7 ..	aged 54 ..	Strangulation 3 days ..	Death from internal strangulation of bowel by the mesentery.

Surely if only two of these seven deaths can by any possibility be made dependent upon changes within the abdomen, consequent upon this operation, we must look for something beyond the mere wound of the peritoneum to explain the difference between results so favourable on the one hand, and conclusions so disastrous on the other.

The disparity, however, I repeat, disappears if we separate from the operations in which the sac is opened the hopeless cases which we have been considering, and thus if any useful comparison is to be made between the two methods, the cases must be rearranged in the tables. First, by grouping together complicated cases, that is, all those in which death is due to general, that is, diffused, peritonitis, or to gangrene, established prior to the operation, and secondly, by subdividing uncomplicated into those in which the sac is opened, and those in which it is not. This has been done in Table C with the cases already given in Table A, and I hope that for the future this arrangement will be adopted. If it is, we may hope to arrive at a comparatively fair estimate of the results of the two operations.

C.—Table A. re-arranged.

		Operations.	Deaths.
Femoral Hernia	Complicated	Sac opened	31
		Sac not opened	3
	Uncomplicated	Sac opened	21
		Sac not opened	30
Inguinal Hernia	Complicated	Sac opened	24
		Sac not opened	6
	Uncomplicated	Sac opened	7
		Sac not opened	26

After all, each case must be judged of by its own surroundings; for we all know how often age, habits, and if I may so term them, the small accidents of a case, determine the issue of life or death. This

fact, however, is quite clear, that the great mortality after opening the hernial sac is occasioned by the fatal changes which are established in the peritoneum, or in the bowel, before the operation is commenced.

LOCAL ANÆSTHESIA IN THE REDUCTION OF HERNIA.

By W. ST. JOHN COLEMAN, L.R.C.P., L.R.C.S.I.,
Wavelands, Miltown Malbay.

I AM not aware of Dr. Richardson's spray producer having been laid under contribution in the reduction of ruptures, inguinal or otherwise, and therefore place upon record the particulars of a case in which I successfully employed it. If, however, my communication be not original, it will serve an equally useful purpose in sustaining the experience of others, who may have preceded me in this particular use.

Master H. G. M., whilst playing with his brother fell heavily under him, and though he complained of sickness of stomach throughout the day, the injury which he sustained was not suspected. I saw him next day, and found him the subject of a right inguinal rupture—hard, and of about the size of a small pear. He had had sickness of stomach throughout the preceding day and night, and up to the hour I saw him. His bowels had been scantily moved shortly after the accident, but not at all on the day of my visit. The abdomen was tense, but neither it nor the rupture were painful under pressure. The pulse was full and frequent.

I endeavoured without avail to reduce by the taxis, and having tried it, for five or seven minutes, desisted until a bath was procured. I was alike unsuccessful in the bath, or when I had placed him on a table with the right flexed, afterwards. I now tied the left foot to the end of the table, and, holding the right knee flexed over the abdomen, upset the table so that the contents of the abdomen might draw upon the strangulated gut; this, too, was unproductive of any good. The application of cold was lively enough in my recollection, but I could not obtain ice, or the nitre and ammonia evaporation conveniently. I then bethought me that Sir A. Cooper had recommended under like circumstances the exhibition of ether, and I determined to give the spray producer a trial, on proceeding to operate. I did so. The child complained much of the cold; but the tense skin grew looser, and shortly corrugated. I now again tried the taxis, and was rewarded in a minute or two by the welcome "gurgle" of the returning intestine. His bowels were opened the same evening, and we kept them loose for a few days. He fed almost on "slops," and recovered without a bad symptom.

However simple this mode of procedure, the success which attended its exhibition is, I am of opinion, sufficient to warrant me in making it public to the profession.

ON A MOVEABLE STAND CARRYING BOTH MICROSCOPE AND LAMP.

By LEONARD W. SEDGWICK, M.D.

FOR two years back, I have used a moveable stand, which carries both microscope and lamp, and find it very convenient and useful. I will not tell the thrice-told tale of how the thing has been done, or not been done, before; but will simply describe my device, which has been executed for me with his usual success by Mr. Collins, of Great Titchfield Street. I believe it is novel; but, if it is not, it deserves, I think, to be revived.

I have a mahogany stand, ten inches and a half by ten, and thick enough to bear some weight without warping, on which is fixed the microscope; on the left side of the stand, an inch from the edge, is a slit half an inch wide, reaching nearly to both front and back; in this slides a brass rod, fixable at any point by a clamping screw, and carrying the lamp and bull's-eye condenser, each capable of any adjustment as to height and position. The stand has a small metal roller at each of the corners nearest the observer, and one in the middle of the front edge; these rollers move sideways. On them the whole rests firmly; and, being three, they accommodate themselves to any inequalities of the table. Here, then, is a simple means of moving the microscope sideways without disturbing the position of the light. If the observer have a friend sitting by him, there is no need of any changing of chairs or of re-adjustment of mirrors, and, if he is mounting or dissecting, there is no need of changing his seat; his work is always before him, for he can push on one side what he is not using. The plan is simple, easily constructed, and adaptable to any microscope, any lamp, and any table; the comfort of the arrangement can only be fully appreciated by those who have experienced it. I cannot say more of it, and can only offer it for inspection and criticism.