

experiment—that is, allowing for some subsidence, the injection had led to a rise of pressure of more than 80 mm. of mercury. An hour after the injection more than 30 per cent. of this rise was still in evidence, and excitation of the sciatic nerve again produced the normal increase in blood pressure as before induction of shock, thereby indicating that some recovery of the vasomotor centres had taken place.

In many cases the first pituitary injection is followed by temporary depression in amplitude of respiration. This is usually slight, and rarely amounts to complete arrest. In all the cases in which we met it recovery was apparently complete. We have never known it follow a second or subsequent injection. It is more pronounced in the dog than in the cat.

In short, the increase in blood pressure produced by this body lasts thirty to sixty times as long as that (increase) of the same amplitude from adrenin. Simple repetition of dose is sometimes useless for raising blood pressure, but the first dose confers no "immunity" against adrenin. Given after the pituitary effect has almost, or quite, passed off, adrenin increases arterial pressure certainly to no less an extent, and seemingly for a longer time, than it does in the absence of a previous pituitary injection. It appears, then, that extract of the posterior lobe of the pituitary body promises to be of some value in the treatment of shock when severe enough to call for intravenous injection. A single injection of it suffices to permit of much recovery, and does not interfere with the subsequent use of adrenin if employment of that drug is indicated.

An additional point in its favour is the above-quoted observation of Schäfer and Herring, that it does not diminish, but increases the activity of the kidney.

Further study; however, of its dosage and after-effects is desirable before it can be advocated for general administration to the human subject.

#### ACTION OF ERGOT.

The sustained arterial tonus evoked in pithed cats without unduly high arterial pressure by intravenous injection of some samples of ergot preparations, led us to hope that it would be reproducible by them in less shocked animals. But in no case have we succeeded in reproducing the desired result. The normal effect of our samples of ergot preparations in surgical shock was zero or further fall of blood pressure. We cannot, therefore, point to any preparation of this drug as of value in the treatment of shock.

#### SUMMARY.

In fully anaesthetized animals non-destructive manipulations of abdominal viscera are more productive of shock than are gross injuries.

Such manipulations produce shock most rapidly when they implicate the parietal peritoneum, the peritoneal ligaments, or the mesenteries. Shock is more readily produced under chloroform than under ether.

In severe shock hypodermic injections are but slowly, if at all, absorbed into the blood stream.

In severe shock intravenous injections of adrenin, and of extract of the posterior lobe of the pituitary body, raise arterial pressure to a greater extent than in the normal state.

A single injection of the latter influences arterial tone for upwards of an hour without producing abnormally high arterial pressure.

This influence is sufficient to enable considerable recovery from shock, and does not interfere with subsequent injection of adrenin, if that be desirable.

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- <sup>2</sup> *Blood Pressure in Surgery*, Philadelphia, 1903.
- <sup>3</sup> *Journ. of Physiol.*, vol. xxxvi: Proc. Physiol. Soc., p. xv.
- <sup>4</sup> *Journ. of Physiol.*, vol. xxxvi, p. 209.
- <sup>5</sup> *Ibid.*, vol. xiv, p. 52.
- <sup>6</sup> *Ibid.*, vols. xvi et seq.
- <sup>7</sup> *Arch. de Physiol. Norm. et Path.*, 1898.
- <sup>8</sup> *Journ. of Physiol.*, vol. xxxii, p. 401.
- <sup>9</sup> *Ibid.*, vol. xxix, p. 286.
- <sup>10</sup> *Ibid.*, vol. xxxi, p. 81.
- <sup>11</sup> *Ibid.*, vol. xviii.
- <sup>12</sup> *Journ. of Exper. Med.*, vol. iii.
- <sup>13</sup> *Journ. of Physiol.*, vol. xxv.
- <sup>14</sup> *Phil. Trans.*, B., vol. 199, 1906.
- <sup>15</sup> *Journ. of Physiol.*, vol. xxxvii: Proc. Physiol. Soc., p. lvi.

AT a meeting of the Medical Society of Italian Switzerland, held on August 15th, it was unanimously decided that all the Poor-law medical officers belonging to the society should resign. This decision was come to in consequence of the failure of the society to obtain for them from the Government and the Grand Council any redress of their grievances.

## SEVENTY-SIXTH ANNUAL MEETING

OF THE

# British Medical Association.

Held at Sheffield on July 24th, 25th, 27th, 28th, 29th, 30th, and 31st.

### PROCEEDINGS OF SECTIONS.

## SECTION OF OBSTETRICS AND GYNAECOLOGY.

RICHARD FAVELL, M.R.C.S., President.

By way of opening the work of this Section the PRESIDENT, Mr. Richard Favell, contented himself by bidding a few words of welcome to its many foreign guests, and then called on Dr. Herman to open the first discussion.

### DISCUSSION ON THE TREATMENT OF UTERINE DISPLACEMENTS.

#### OPENING PAPER.

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#### THE TREATMENT OF UTERINE DISPLACEMENTS.

I HAVE been asked to open a discussion on "The Treatment of Uterine Displacements." The question which first arises is—What are uterine displacements? I would divide them into two classes: the common and the rare. The rare displacements are inversion, and the cases in which the uterus has been found dragged into hernial sacs. I shall not discuss these, because as to the principles on which they should be treated there is no difference of opinion; and as to details, no individual has treated so many of these rare cases as to enable him to lay down the law about the comparative merits of different methods.

The common displacements, those which every man in family practice must treat, are those which I think it is intended we should discuss to-day; and because they are so common I hope the discussion may be interesting and instructive to us all.

The conditions meant by the words "displacements of the uterus" are nothing but descent of the uterus and its effects. The Obstetrical and Gynaecological Section of the Royal Society of Medicine has recently been discussing the means by which the uterus is supported. One writer says that the uterus is held up by the fibrous tissue surrounding the vessels—in other words, by the pelvic fascia; another says that the main support of the uterus is the levator ani. I think that these authors are both right. The uterus is supported by the pelvic fascia, the connective tissue in which the vessels run, and the levator ani muscle.

In most of the common cases of displacement there is nothing the matter with the uterus; the disease is that the pelvic floor is weakened or injured, and the change in the place of the uterus is only the palpable sign of defect in the pelvic floor. Thirty years ago books were full of descriptions of various versions, flexions, flexures, torsions, etc., of the uterus; but it is now indisputable that a curve in the canal of the healthy uterus is not disease. Change in its position may disturb the circulation through the uterus. When the uterus sinks, it generally leans back. In about one case in ten the folds which bound the pouch of Douglas on each side are unusually firm, and are made more tense when the cervix sinks or is moved forwards by the falling back of the uterine body. If this is the condition of the utero-sacral ligaments, when the body of the uterus sinks into Douglas's pouch the veins which return the blood from the uterus are compressed against these folds, and the body of the uterus in consequence becomes congested, swollen, and tender.

The late Dr. H. G. Sutton defined "disease" as the "condition of being without ease." Uterine displacement produces pain, which is not severe, but it makes the patient without ease—that is diseased. To understand a patient who is without ease because the supports of her uterus are not efficient, we must look at the case from two points of view—the subjective and the objective. The doctor may judge that the womb is displaced, or he may think that a pessary which a patient is wearing is a badly constructed and inefficient thing. But if the patient is at ease she cares not what may be the shape or the position of the uterus or what may be the defects of the support which has given her ease. Conversely, if the patient cannot stand or walk without pain, she will not be contented with being told that there is nothing the matter.

A slight descent of the uterus occurs in all women during exertion. There are virgins, nulliparous, and parous women in whom this slight descent causes discomfort whenever they stand or walk. When such women are examined in the horizontal position no morbid change can be perceived. The descent when the patient is upright takes place by a slight inversion of the upper part of the vagina. When a frame is put into the vagina which keeps it extended, inversion of the upper part of the vagina cannot take place; the descent of the uterus is hindered, and the patient is relieved of the pain which prevented her from standing or walking with comfort.

Liability to pain, and the power of local pain to affect the patient's general well-being, do not depend only on the local conditions. Women in whom trivial local changes take away ease from their lives, and whose ease is restored by simple local treatment, are almost invariably neurasthenic. Neurasthenia is not the subject of our discussion. But it is not possible for one who is not aware of the frequency and importance of neurasthenia to judge rightly as to the effects of treatment in the minor diseases of women. In cases of slight descent of the uterus, if a poor woman is taken into a hospital, kept in bed, and given plenty of food and sound sleep, she will soon lose her pain without anything being done to support the uterus. If a richer patient goes to her family doctor, and he is acute enough to discover what were the influences which have pulled down her nervous strength, and to remove them from her or her from them, she will get well without mechanical treatment, slight descent of the uterus notwithstanding. And, on the other hand, if the patient has been so unfortunate as to inherit nervous weakness, and to have been from childhood overpressed and worried by unfavourable surroundings and unwise though well-meaning parents and guardians, it is probable that her symptoms will not be removed, however thoroughly the uterus and its supporting structures have been mended by surgical skill or assisted by mechanical appliances.

For the credit of the profession it cannot be too clearly recognized that there are cases in which the symptoms of prolapse are due to neurasthenia; that if the weakness of the nervous system is recent and slight, restoration of nervous tone will remove the symptoms without local treatment; and that if the neurasthenia is of long standing and of grave degree, local treatment, however successful in amending the position of the uterus, will not cure the patient. But one who has this constantly before his mind will meet with patients who are not neurasthenic or hysterical, who in every way except the local trouble are in the best of health, and yet who feel the continual dragging, aching pain, and the irritation of the bladder which descent of the pelvic floor produces, as an intolerable nuisance, to get rid of which they are willing to endure a good deal.

The beginning of descent is accompanied and permitted by inversion of the upper part of the vagina. The simplest and safest treatment is the insertion of a pessary to keep the vagina extended. If such an instrument could in every case be retained with comfort, we should have nothing to discuss to-day, for no sensible person would advise or permit any other treatment.

Unfortunately, in many cases the vaginal orifice has been so enlarged by tearing during childbirth, that any pessary which is put in the vagina comes out when the patient exerts herself. In a few virgins and nulliparæ the fourchette is so far back that a pessary cannot be

retained. In these cases the first thing to be thought of is a pessary retained by straps attached to a waistbelt. Either a cup on the top of a stem, or a Hughes-Davies's pessary, which is a Hodge pessary hinged to the top of a stem, the stem being in each case held up by four straps tied to a waistbelt. Or a Cutter's pessary, which is a loop or bar supported by a stem curved back over the perineum, and there held up by a strap attached to a waistbelt.

These appliances are harmless. But some patients like them not. In some the vaginal orifice is so large that even though the top of the vagina is kept at its proper distance from the vaginal orifice, yet the lower part of this mucous canal bulges out by the side of the pessary.

And a vaginal pessary, if retained, does not always give comfort if the body of the uterus is tender. The pessary will not benefit the patient unless the uterus so rises that it comes to lie with its long axis in the axis of the pelvic brim. In some cases it is difficult to find a pessary which will keep the uterus in this position.

I think the following broad statement is in harmony with experience: That keeping up the uterus by a mechanical support in the vagina is harmless and generally effective; but there are cases in which it fails, and if the patient is to be cured she must be treated in some other way.

Here I wish to invite opinions upon a matter as to which I have no materials from which to judge. Operations have been devised, by Hey Groves in this country, and by others in America and on the continent of Europe, to prevent descent of uterus and bladder by sewing together bundles of the muscles and fascia which form the pelvic diaphragm. I think these operations sound in principle, for their aim is to restore to the uterus its natural supports. I have never done one of these operations, nor have I seen one done, or read an account of one which influenced my judgement. A mere record that a surgeon performed a certain dissection and stitching, and the patient left the hospital saying she was well, is not enough. We want to know what was that patient's condition five years afterwards. We want to know whether it is in any way possible to make the pelvic floor as capable of supporting the intra-abdominal pressure as it was in childhood. When this has been shown to be possible, then it will be time to discuss details of technique.

In the foregoing remarks I have assumed that the pelvic floor has been injured. This I think a reasonable inference, but only an inference. I know of no one who has shown by dissection of women who have died soon after childbirth what the condition of the pelvic fascia and levator ani muscle then actually is.

I pass from operations theoretically excellent to those of which we have some experience. There are two operations to correct the position of the uterus that may be done by the vagina. The fundus uteri may be stitched to the vagina or to the cellular tissue between the uterus, vagina, and bladder, or the round ligaments may be shortened by a vaginal operation. Either of these operations is effective in the cases in which the sole trouble is that the uterus is retroflexed, painful, and tender. By shortening the round ligaments the fundus uteri is pulled forwards, the veins in the broad ligaments are no longer pressed upon, and the patient is cured. But when, as in most cases, descent is the chief trouble, the only effect of these operations is that the uterus comes down leaning forwards instead of leaning back—a benefit inappreciable by the patient. These operations are not difficult or dangerous. But mishaps nevertheless sometimes occur when cutting and stitching is being done. One operator, whose confidence in his own surgical skill is such that he has given up the use of pessaries, and instead performs vaginal fixation after a method of his own, published last year an account of 100 such operations. In two of them he wounded the bladder, once he wounded the rectum, and in two cases there was great hæmorrhage from a wound of the corpus cavernosum of the clitoris. All the patients recovered. But his candid relation of the accidents I have mentioned should, I think, make the average man pause before he urges operation on a reluctant patient as a better thing than a pessary.

The effect of these vaginal operations is only to change the position of the uterus in relation to the pelvic axis; they do not prevent descent. If the pelvic floor is weak the uterus comes down, leaning forwards instead of leaning



back. Some advocates of these operations urge that extensive colporrhaphy should be done at the same time, so as to contract the lower part of the vagina. The chief benefit from this is that it enables the vagina to retain a pessary. We know that peritoneal adhesions are often absorbed. The vesico-uterine cellular tissue is loose. I am not acquainted with the work of any operator who has changed the position of the uterus by a vaginal operation and has followed up his cases, and found that the change in position lasted so long that it may reasonably be considered as permanent.

Methods of treatment have been proposed which do not seem to me sound in principle. They consist in attempting to stiffen the pelvic floor by injecting paraffin or an irritant which will produce inflammation. It is hoped that cellulitis thus produced will lead to the organization of so much fibrous tissue that the pelvic floor will be held in its place. There is no way of predicting the exact effect of an irritant injected into the cellular tissue or of limiting the inflammation thus excited. Cellulitis following delivery often ends either in suppuration or in complete absorption of the inflammatory exudation. I have been told of a case in which the injection of paraffin led to suppuration, which was very protracted, owing to the difficulty of getting out the paraffin.

In most cases of uterine displacement the best treatment still is mechanical support. This is safe and certain. It may from a practical point of view be described as permanent, because a patient who finds comfort from a pessary can wear it, with proper care for cleanliness, as long as she pleases. The chief advance in the last thirty years has been in the dissipation of the false pathology which attributed the symptoms produced by anaemia, neurasthenia and hysteria to trifling alterations in the shape of the uterus. In nine cases out of ten the shape of the uterus is unimportant. The thing required is to support the uterus in its function as the central point of the pelvic floor.

But there are patients who like not pessaries, and carry their dislike to the point of being willing to undergo an operation rather than wear one. There are others in whom every pessary fails. When the vaginal orifice has been greatly enlarged and prolapse has lasted long there is descent, not of the uterus only, but of the vagina and the bladder. In such cases, if the uterus is fixed to the anterior abdominal wall, the vagina and bladder still come down. If the uterus is pushed up by a pessary the vagina protrudes around the pessary. No doubt a mechanical contrivance could be made to press up the whole mass, but the larger the pessary the more painful is its introduction and removal, the more irksome its pressure, and the greater is the possibility of injury from continuous pressure. In such cases greater relief follows a successful operation than can be given by any pessary. An operation to permanently contract the lower part of the vagina, should be postponed until it has become certain that the patient will not become pregnant, for in proportion to the success of an operation in preventing prolapse it will obstruct delivery. The operation should consist in dissecting off a large piece of mucous membrane. Its success, in contracting the vaginal orifice will depend upon the size of the piece of mucous membrane removed. The comfort and rapidity with which healing takes place depends upon the nicety with which the raw surfaces are stitched together.

But colporrhaphy alone will not permanently cure prolapse; because after the most perfect colporrhaphy the uterus and upper part of the vagina are no better supported than they were before the operation. When the patient again stands and walks inversion of the upper part of the vagina again takes place, the uterus descends, and acting as a blunt wedge gradually dilates the lower part of the vagina, so that after a period of time, varying with the degree of contraction brought about by the operation, there is again a protrusion. To prevent this, the uterus must be kept from descending. This can be done by inserting a vaginal pessary. The removal and reinsertion of a vaginal pessary will be disagreeable to the patient in proportion to the narrowing of the vagina produced by operation. When the patient comes for advice in the first instance, her medical adviser has to compare for her the advantages and disadvantages of operative and mechanical treatment; and the safety and ease of the latter may properly incline him

to recommend, and the patient to prefer, mechanical treatment to operation. But the case is otherwise when an operation, with an anaesthetic and many days in bed, have in any case to be submitted to. She may then reasonably ask that the operation should be as complete as possible, so that she may be free from the necessity for subsequent visits to a doctor. Permanent relief can be given by combining ventral fixation with colporrhaphy. The uterus being thus held up to the anterior abdominal wall there is nothing to again bulge open the vaginal orifice after it has been contracted by operation.

The essential points in this operation are (1) that the peritoneal covering of the uterus should be stitched to the muscle of the abdominal wall, not to the parietal perineum. When peritoneum is united to peritoneum the adhesion is in time absorbed, but it first becomes thinned and stretched into a cord. Some people appear to think this desirable, and call the operation so performed "ventral suspension." I think this effect an operative failure. The cord does not keep the uterus up, and it puts the patient in peril of intestinal obstruction. If the uterus is sewn to muscle the adhesion is not absorbed nor does it elongate. (2) The surface brought into contact should not be a large one. There are those who think that if pregnancy follows ventral fixation there is likely to be difficulty in delivery. But in 95 per cent. of the published cases there was no such difficulty. In some in which labour was complicated it is not certain that the difficulty was due to the attachment of the uterus to the abdominal wall, for the complications were such as occur as often in uteri which are free. In one published case the difficulty only existed in the imagination of the doctor, for while he was preparing to perform Caesarean section the patient was naturally delivered. But if we admit that fixation of the uterus may cause dystocia, it will be obvious that dystocia is more probable if the uterus is fixed over a very large area than if fixed in an abnormal position. I think that to attach the uterus by a surface of an inch transversely and half an inch from before backwards is enough; and that the best place to sew it to is the place opposite which it comes if pulled up in the axis of the pelvic inlet. To pull it up to the umbilicus is to put additional strain on the stitches, and to stitch the uterus close on the top of the symphysis I can quite believe would interfere with the bladder and with delivery in case of labour.

Although I think that the risk of difficulty in delivery from ventral fixation of the uterus has been exaggerated, yet I agree with those who think that the operation should not often be advised for women young enough to make subsequent pregnancy probable. Ventral fixation will not cure the patient unless extensive colporrhaphy is done also. If the body of the uterus is attached to the abdominal wall, of course it cannot come down, but the lower part of the vagina will become inverted; there will still be a mass protruding from the vulva. From the patient's point of view it matters not of what structures an uncomfortable protrusion is composed. Great contraction of the lower part of the vagina means great obstruction to delivery. If it is certain that there will be no further pregnancy, the combination of ventral fixation with extensive colporrhaphy is the best way of permanently curing prolapse.

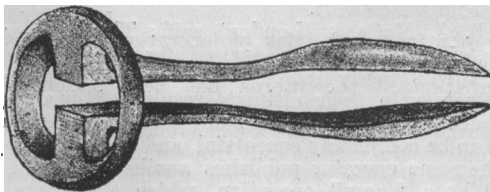
In a patient who is so young that pregnancy is probable, who suffers from prolapse, does not like a support with straps outside, and yet cannot keep in a vaginal pessary, an extension of the perineum forwards, by raising flaps of skin and mucous membrane, but not cutting away anything, will enable the patient to retain a vaginal pessary, and yet will not, in the event of pregnancy occurring, expose her to greater risk of laceration than occurs in most primiparae.

Extirpation of the uterus has been performed and recommended as a radical treatment. If the uterus is taken out, of course it cannot come down. But the vagina comes down; the patient still has a protrusion; the only difference is that at the bottom of the inverted vagina there is a scar instead of the uterus. The only treatment of this kind that is effective is removal of the uterus and vagina. This is a simple operation, and its immediate result is entirely satisfactory. But its applicability is limited to elderly widows. The only case which I have treated in this way I have not been able to watch long

enough to be sure of its permanence; but so far as I have watched it the result was satisfactory.

#### DISCUSSION.

Professor Bossi (Genoa) said: I have had an extensive clinical experience of the treatment of displacements of the uterus, from which I can formulate the following conclusions: (1) Plastic operations, however extensive, are in many cases followed by recurrence. (2) Median colporrhaphy is the only operation that gives really satisfactory results in cases of prolapse, but it should not be performed on a woman until after the menopause, lest the marital functions be interfered with during active sexual life. (3) Ventral fixation is unsafe, especially if there is a possibility of conception. (4) The Alexander operation, though the most logical, is sometimes followed by recurrence. In addition to these conclusions, my colleagues may be interested in what after many years I have called the functional treatment of displacement of the womb, especially of retroflexion and retroversion. I have clearly shown in many of my former publications (a) that displacements of the womb during pregnancy are much more common than is generally reported or believed, (b) that many such cases are not reckoned in the statistics because they cause abortion in the early months of pregnancy, (c) that when a displacement of the womb during pregnancy is spontaneously reduced it recurs in the puerperium. In this way displacements occurring during the early months of pregnancy come to be attributed to the pregnancy itself. A large clinical experience justifies me in stating: (1) That the use of a pessary during the early period of pregnancy will nearly always prevent an abortion; (2) that even the most obstinate cases of retrodeviation may be corrected by the application of a pessary some days (eight or ten) after delivery and leaving it *in situ* for sixty days afterwards, and if this be not effected after one pregnancy, it may be so after a second. As for what I have called functional treatment, the object of which is: (1) To prevent abortions (prophylaxis of abortion), (2) to avoid surgical operations, which do not always do any good and are sometimes dangerous, I may say that for fifteen years I have urged that as a point in gynaecological prophylaxis students should be taught never to lose sight of a patient after delivery without ascertaining, by aseptic internal examination fifteen to twenty days after the birth of the child, whether there is a displacement of the womb, most probably backwards. I need hardly explain that the posterior displacements of the womb which occur during the puerperium are not recognized because they cannot be detected by external examination only, but if one takes advantage of the physiological involution of the tissues which occurs normally in the puerperium one may obtain marvellous results by orthopaedic treatment. Although the term "displacement" may indicate only an alteration in the position of the womb by version and descent, I take this opportunity to show a new intrauterine pessary, the chief object of which is to remedy stenosis of the womb caused by flexion. I recently published a paper under the title *Der schneckenförmige Uterus*<sup>1</sup>—the snailshaped uterus—in which I pointed out the enormous number of young girls or married women in whom one finds the uterus folded on itself, generally in anteflexion, so that the cervical canal is completely obstructed. This condition leads to dysmenorrhoea, most various nervous and physical affections, derangement of metabolism and sterility. I have found from long experience that these cases can be very successfully dealt with by internal multiple incisions corresponding to the flexion of the cervix followed by the introduction



of the stem pessary here shown. I leave it in place for from twelve to twenty days after the slight operation, which is generally supplemented by curettage, as these flexions are associated with endometritis. It causes no discomfort.

<sup>1</sup> *Zentralbl. für Gynäk.*, 1903, No. 20.

and the patient can leave her bed at the end of a week and walk about a little without any pain. I have never had any troublesome reaction, and have frequently found in cases of hydrosalpinx and pyosalpinx, the tubes empty themselves in a way that induces me to retain this pessary as a useful means of draining the uterus. It has important advantages over others as affording free draining of the uterus.

Dr. J. RIDDLE GOFFE (New York) said: We must all accept the pathology of displacement of the uterus as given by the reader of the paper. There is no question, it seems to me, that displacement of the uterus is a pathological condition that demands treatment and for which the patient requires relief; even in cases of neurasthenia, in which the nervous element is the most prominent factor, the cure is hastened by relief of the displacement. With all due respect to the reader of the paper, I must affirm that in my opinion the normal supports of the uterus reside in the uterine ligaments and not in the floor of the pelvis, although we must admit that the floor of the pelvis in a very indirect way lends an adventitious support. This conclusion is reached in two ways—first in reasoning by analogy. For instance, Nature's plan of retaining organs in place is by suspension from the bony framework of the body. The application of this principle is readily recognized in the lungs, the heart, the liver, the intestines, etc. It would be logical to infer that the same method of support would be found in the uterus, and yet in a most irrational way an exception has always been made of the uterus. I say illogical, for, weight for weight, there is no other organ in the body that has so many ligaments. What are they for if not for support? Moreover, in the study of cases of complete laceration of the perineum, that is, when the tissues are torn clean through into the rectum, the experience of all observers is that the uterus, as a rule, remains in normal position, the exceptions being those in which there is a complicating inflammation of the appendages causing the prolapsus. Now, if the entire support below has been removed and the uterus remains in place, the only logical conclusion is that the support comes from the ligaments. If they can hold the uterus under these circumstances they certainly must be sufficient under normal conditions. Accepting, then, this conclusion, it naturally follows that the efficient tissue to use for the restoration of the uterine supports is to be found in the uterine ligaments. The necessity of repairing the perineum, or floor of the pelvis, resides in the fact that when the perineum is torn down to the rectal wall but not through it, the rectocele pulls upon the neck of the uterus; in other words, it constitutes a new force that does not exist under normal conditions, and must be overcome by a perineorrhaphy. Regarding the use of pessaries, Dr. Pozzi has spoken of the almost universal reaction against their use which has occurred in France. I may say that a similar reaction has occurred in America.

Dr. ARNOLD W. W. LEA (Manchester) stated that in many cases of descent of the uterus the symptoms were largely due to laxity or feeble vascular development of the abdominal walls with some degree of ptosis of the viscera. This was often combined with neurasthenia or malnutrition. In these cases it was essential to treat the general health, and suitable gymnastic exercises to strengthen the abdominal and pelvic floor muscles were of great value. These should be continued for some months, and might be supplemented by massage and the use of an abdominal belt. All operations for fixation of the uterus which were followed by the formation of a band of adhesions were attended by some danger of intestinal obstruction. If ventrifixation was carried out it was essential to close the utero-vesical pouch of peritoneum in order to avoid the possibility of a loop of bowel slipping between the uterus and bladder, as was first advocated by Sir W. Sinclair. The risk in subsequent pregnancies was slight if care were taken to pass the sutures through the lower part of the anterior wall of the uterus, thus leaving the fundus free to expand during gestation. He regarded intraperitoneal shortening of the round ligament as a safe and effectual operation for retroflexion, the ligaments being folded and sutured into the anterior wall of the uterus. This procedure was not sufficient if the uterus was considerably enlarged or prolapsed. Very frequently curettage and repair of the pelvic



floor were required, and in his opinion these operations should usually be performed at the same time as the fixation of the uterus.

Dr. MAY THORNE (London) said: I desire to record entire agreement with Dr. Herman that the cure of symptoms in many patients is largely dependent on attention to their general health. In dealing with all cases of displacement of the uterus it is well to bear in mind that in numerous patients a misplacement is present which does not give rise to any symptoms. Apart from displacements occurring after obstetric accidents, there are a certain number of cases, notably in young girls, where after some sudden and severe exertion during the menstrual period an acute dislocation of the uterus occurs giving rise to dysmenorrhoea and other acute pelvic trouble. This condition is usually completely rectified by replacement under an anaesthetic with the wearing of a pessary till the normal position of the uterus has been maintained for some months.

Dr. HELLIER (Leeds) said he agreed with Dr. Herman as to the therapeutic value of rest and general treatment in producing marked amelioration of uterine displacement, the diminution in the size of the uterus being demonstrable with the sound. The stem with external supports was quite the most effective pessary, and it had the advantage that if it broke it would not stay in, as contrasted with the Zwancke pessary, which when it got out of order would not come out and caused tragic consequences. What was the best treatment for the worst cases of procidentia, which pessaries would not support, and when the parts were becoming ulcerated? He performed anterior and posterior colporrhaphy, doing a very extensive operation, amputating the cervix if much hypertrophied; with this some fixation of the fundus must be combined. He had done ventrifixation, but lately he had been employing vaginal fixation after Wertheim's plan. This seemed to assist to cure the cystocele, and also the uterus in this position shows less tendency to descend than in a retroflexed position. He kept this operation as far as possible for women who had finished child-bearing.

Dr. J. INGLIS PARSONS (London) was glad opinions were changing as to how the uterus was held up, and that some importance was given to the connective-tissue ligaments of the uterus. In his first paper, published ten years ago, he pointed this out. His observations were based on Dr. Savage's book on pelvic anatomy. In order to produce repair in this connective tissue, he invented the operation of injecting the utero-pelvic band within the broad ligament with quinine. His object was to produce repair by stimulation, similar to that which occurred after injury to the ligaments of an ordinary joint. There was, as a rule, no temperature after the injection of quinine, showing that it was not ordinary inflammation which occurred. He had now done 150 cases, and claimed a success of 90 per cent. He now found it possible to inject a larger quantity of quinine than he did at first, with better results. He had found that after the uterus was held up by injection of quinine, it made the patient more comfortable to repair the perineum if it was ruptured. His operation only took five minutes to do, and it did not interfere with pregnancy in any way, nor did there appear to be any danger from it. In addition to his own cases the evidence of other men was impartial. Between them they had done seventy cases, and their percentage of successes was greater than his own. Dr. Rice, of Derby, had done thirty-one cases. All but one of these were successful. Three of the patients had had children since the operation with no return of the prolapse. One patient was aged 72. Dr. Eugene Carlier, of Brussels, had done fifteen cases, and had written to say that he was very satisfied with the results. Dr. Sydney Kent, of Bexhill, had operated on nine patients, all successful. Two of them had had children since the operation without any recurrence of the procidentia. Miss Lea Wilson, medical officer to the Dhankorbai Hospital, Nasik, India, had operated on eight patients. All of them were immediate successes, but she had not been able to follow them up to know the remote results, except in one case, when she heard that the patient was quite well after a year, and was seven months pregnant. Dr. John Aikman, of Guernsey, had operated on seven patients. In every case the uterus was held up, and had remained so in some cases for many years, though the uterus had been procident. In consequence of the success of this opera-

tion for prolapse he had entirely discarded ventrifixation for retroflexion. A serious objection to fixation was the difficulty that so often occurred during labour. This did not apply to ventro-suspension for retroflexion, as only two silk sutures were used, after Kelly's method. The two adhesions which formed were capable of stretching and allowing the expansion of the uterus. Dr. Beverley McMonagle<sup>1</sup> had published 250 cases and had not known a single case of difficulty in labour, although one woman had triplets. Dr. G. R. Holden had followed up 214 cases. Of these 83 had children, 5 cases required forceps, and 5 cases had some abdominal pain, no other abnormality occurred. His own experience of this operation was very similar. Several of his patients had children without difficulty, and he had never heard of any cases of intestinal obstruction nor of recurrence of the retroversion.

Dr. LLOYD ROBERTS (Manchester) was in complete accordance with Dr. Herman with regard to neurasthenia in minor displacements, and thought many women would know nothing of their "insides" if they were not told. Therefore they should in many cases treat the neurasthenia symptoms, and not the uterus at all. There were, of course, cases that needed treatment, and in which the displacement was evident enough. Such might need operation, but each case should be judged on its own merits. Pessaries would still hold a place in the treatment of prolapse, and, these failing, he would advise extensive repair of the perineum combined with colporrhaphy. A large ulcerated oedematous cervix should be removed or the uterus curetted. He did not think that any injections such as quinine and paraffin were ever justifiable. Some cases of prolapse might need abdominal suspension or fixation, but these were in the minority. Ventral fixation was justifiable in elderly women, and effectually prevented the womb coming down. Ventral suspension of the uterus in properly selected cases was usefully performed after Gilliam's method. The speaker also referred to a case in an elderly nullipara where cancer developed in the body of the uterus which had been fixed up to the abdominal wall.

Dr. DONALD (Manchester) wished to speak merely on one portion of the subject—mobile retroversion. It was true, as many speakers had said, that retroversion in some cases gave rise to no symptoms, while in others there was much discomfort. The speaker believed that the explanation of this was not the neurasthenia—on which Dr. Herman seemed to rely—but that in the one class of case the uterus was healthy and normal in size, and that in the other class the uterus was enlarged and diseased. He believed that the view that a retroversion caused congestion and enlargement of the uterus was responsible for the usual methods of treatment by pessaries and ventrifixation. In the speaker's opinion, this kind of treatment was illogical and unsatisfactory. The object of treatment should be to obtain a healthy uterus. In most of the cases of backward displacement of a mobile uterus which give rise to symptoms we find enlargement of the uterus due to a chronic metritis. The logical treatment was to curette thoroughly, in order to induce further involution of the uterus. This diminution in size could be proved by actual measurement to occur in almost every case. If there was a tendency to prolapse with the retroversion, a plastic operation should be performed on the posterior wall after curetting. In the great majority of cases this was all that was necessary. Pessaries for retroversion were only a faith cure. Ventrifixation was far too frequently performed in these cases. It had all the glamour of a major operation for the young surgeon. The speaker thought that more attention should be paid to the condition of the uterus and less to its position in cases of backward displacement.

Dr. F. J. McCANN (London) said: The number and variety of the operations devised for the treatment of displacements really indicate that many of them are unsatisfactory in their ultimate results. It is a common experience to read long lists of cases operated on and alleged to be completely successful, and two or three years later the same operator publishes another series of cases treated by a "new" operation, which also gives completely successful results. Fixation operations, especially by the abdominal route, are extremely easy to perform. They have the glamour of a major operation, and not infrequently the emolument attaching thereto, and hence

<sup>1</sup> Noble's *Gynaecology*, page 561.

there is a tendency to resort to operation when the justifiability of so doing may be questioned. The cases suitable for a particular operation must therefore be carefully selected. In many cases, however, but especially the thin neurasthenic patients, general treatment is indicated. There is a tendency to take a somewhat narrow view of the pathology of pelvic displacements, forgetting that in many instances the pelvic displacement is only a part of a general descent of the abdominal contents which no pelvic operation *per se* will cure. I am, therefore, fully in accord with Dr. Arnold Lea when he advises exercises to strengthen the abdominal muscles. This method of treatment I have advised for some years with the addition of abdominal massage, and it should be used either instead of operative treatment or in addition to such treatment. It is the descent of the organs which gives rise to the symptoms. For cases of marked prolapse, with enlarged uterus and thickened cervix, it is now the fashion to advise and practise excision of the uterus and vagina. I have never seen a case in which I considered this operation was indicated. I have, however, operated on a large number of patients by a method to which I will shortly refer. It seemed to me that what was necessary was to tighten the vaginal vault and so imitate the conditions which obtain after the menopause. For this purpose I perform the following operation: The cervix is removed by a supravaginal amputation and the bases of both broad ligaments ligatured. The whole vaginal vault is excised, the bladder being completely separated from the uterus and the remains of the vaginal walls. One or, if necessary, two purse-string catgut sutures are inserted in the bladder wall, and when tied destroy the cystocele. The bladder is pushed upwards and fixed by two sutures, passed through the vaginal walls and the uterus close to the peritoneal reflection. The peritoneum of the pouch of Douglas is treated in a manner similar to what has been done to the bladder. The vaginal walls are united to each other and to the cervical stump. A large piece is now excised from the posterior vaginal wall, and a longitudinal reef suture or sutures introduced to destroy the rectal pouch. The operation is completed, and the perineum extended forwards. I have not yet published my results, because the best test—the time test—is not yet applicable. I have not been able to follow up all the cases for five years. So far the results have been admirable, and even if the operation were a failure, a small pessary could be worn. Moreover, the patient retains her uterus and vagina. The excision of pieces of vaginal mucous membrane is unsatisfactory, what is required being an excision of the whole thickness of the vaginal wall down to the bladder in front or to the rectum behind, and subsequent stretching is not likely to occur.

Dr. CUTBERT LOCKYER (London) said: In the majority of cases of backward displacement in which the uterus is mobile, no operation is necessary beyond curettage for the accompanying endometritis and subinvolution. After digital reposition, if a Hodge's pessary is worn for a few months the uterus will stay in its correct position of its own accord. The condition may be congenital and give rise to no symptoms whatever, or if there are symptoms they may arise from neurasthenia which no operation can cure. In a few of these cases the uterus falls back when the pessary is removed even after prolonged use of the support, and the patient tires of repeated examinations and will ask if there is no alternative; it is then that we are justified in offering the chance of a radical cure. In backward displacement, when the uterus is fixed by adhesions or anchored by bands in a state of partial fixation, great tenderness and pain on examination is noticeable; the use of a pessary gives no relief and the treatment should be ventral suspension. In a certain percentage of cases of backward displacement with diseased appendages bilateral removal of the tubes with one or other ovary is necessary, the uterus will then be devoid of its lateral supports, and should be kept in position by ventro-suspension. A retroflexed gravid uterus may become fixed in the pelvis by impaction and cause grave symptoms. Digital reposition in the knee-face position is possible in many cases, after which a pessary should be worn until the sixteenth week of gestation. If manual reposition fails, a watchspring pessary as used by Sir W. Japp Sinclair will generally restore the uterus to its normal attitude in the pelvis; the speaker had never known this

simple plan to fail. Descent of the uterus is commonly found in connexion with backward displacement, and the resultant symptoms are more referable to the descent than to the secondary retrodeviation. The treatment should at first be prophylactic, by exercising due care in the puerperium and on first getting up, followed by two or three examinations during the next few months if symptoms suggest displacement. If found, it should be corrected and a pessary used. In neglected cases, where not only the pelvic floor has fallen but the bladder and rectum have done likewise, where, in fact, there is supravaginal elongation of the cervix and the os uteri and vaginal walls are outside the vulva, the speaker believed a plastic vaginal operation with removal of the cervix to be far better treatment than the use of the cup and stem or Zwancke's pessaries. In such cases any form of suspension would be unavailing, for it is too much to expect to keep up the prolapsed pelvic floor by fixing or anchoring the uterus to the anterior abdominal wall. The union may hold, but a pedicle will form and allow the uterus to descend to its old position on the perineum or even outside the vulva. When a patient's distress is sufficient to indicate an operation for displacement of the uterus, due regard must be taken as to the amount of prolapse present. In a case of backward displacement with only slight or no descent, some form of suspension might be employed. During the past two years I have abandoned fixation of the body of the uterus, and have followed the method described by Tod Gilliam at the meeting of this Association in Toronto, 1906. He sews the cornual portions of the round ligaments to the anterior abdominal wall, leaving the body of the uterus quite free. None of the patients for whom this had been done had become pregnant, but Gilliam asserted that no bad effects might be anticipated in fertile women on whom this procedure had been carried out. It was very noticeable at the Toronto meeting that the older operation of ventrifixation was strongly condemned by both American and Canadian gynaecologists, whereas Gilliam's operation was almost universally approved. For advanced degrees of prolapse, where the body of the uterus alone remains in the pelvis, whilst the cervix with cystocele and rectocele lie outside the pudenda, I follow a plan of treatment of my own; this consists in (1) freeing the cystocele thoroughly from its stretched vaginal covering and also from the cervix; (2) making bare the rectocele in the same way; (3) amputating the cervix at the isthmus; (4) sewing the most dependent part of the cystocele to the uterine stump by Macewan's looped stitches; (5) reducing the size of the rectocele by circumferential purse-string sutures; (6) cutting away the redundant vaginal skin flaps and uniting their edges to the edges of the uterine canal, and so covering the raw cervical stump; (7) finally performing perineorrhaphy to narrow the introitus vaginae. The extent to which the vulval orifice is closed is a purely marital question. This is a long and tedious operation, but in the six cases in which it has been employed the results have been uniformly good.

Dr. T. ARTHUR HELME (Manchester) welcomed Dr. Herman's timely protest against a too-pronounced tendency towards operative interference. All must agree that general treatment by rest—or the opposite, that is, exercise and gymnastics—was of the highest importance. Dr. May Thorne had already alluded to the possibility of acute displacement in schoolgirls. In his own experience he had been struck by the frequency of the occurrence of definite local pain and discomfort in young women and girls, in whom on examination a retroversion or flexion was discovered; and he had also been struck by the frequent association of tenderness and shortening of the left utero-sacral ligament especially. He had quite satisfied himself of the occurrence of acute displacements in virgins, the probable cause being excessive violence in games or excessive strain in work; and he was also satisfied that constipation and want of regular attention to the state of the bowels in schoolgirls played an important part in the causation of these two conditions. Local treatment, in the present state of anatomical knowledge, must be largely empirical; the choice of operation, when indicated, depended largely on the individual preference and style of the surgeon. In mobile retroversion or flexion he had found Gilliam's operation—that is, shortening of the round ligaments—to be eminently useful and satisfactory; it



had none of the associated risks of ventrifixation, and approached most closely to the ideal operation; it was a simple operation, took only a few minutes, and left the uterine body perfectly free. In procliptia he had come to select the operation according to the size of the uterus, the degree of hypertrophy of body and cervix. When the uterus was hugely enlarged—for example,  $4\frac{1}{2}$  in. to 6 in. long—he preferred to extirpate the uterus; the results of the operation depended on the method. He had seen cases where the extirpation had done no good, the only result being that at the apex of the protruded mass one saw a scar instead of the uterus. In fact, the extirpation of the uterus was merely an incident in the operation, not the object. The chief object was to bring together the divided ligaments so as to form a firmer cicatrix, and if this were done the operation was a useful one. When the uterus was moderately enlarged he selected the high amputation of the cervix, removing a wide area of vaginal skin from the upper end of the canal, together with colporrhaphy and repair of the outlet. When the uterus was small, it was a shorter and simpler proceeding to perform abdominal fixation, together with repair of the outlet. These were merely the expressions of his own experience, and the two points he wished to emphasize were the value of the Gilliam operation, and the importance of sudden strain and want of attention to the bowels as factors in the causation of pelvic trouble in young girls.

Dr. FOTHERGILL (Manchester) thought the most important displacement was the classical "prolapsus uteri," or sacro-pubic hernia, as some preferred to call it. He would confine his remarks to this subject. Its one constant and essential cause was relaxation of the tissue connecting the uterus, vagina, and bladder with the walls of the pelvis and the upper surface of the pelvic diaphragm. The view that deficiency of the pelvic diaphragm and perineum was an essential factor was held in the face of two well-known facts; first, there were numerous women who had no prolapse whatever, although the perineum and pelvic diaphragm had been torn right into the rectum for years; secondly, prolapse was commonly observed in virgins in whom the pelvic diaphragm and perineum were absolutely perfect to begin with, and only became relaxed as a result of the prolapse. To state accurately the mode of attachment, it was necessary in the first place to cease to believe in special creations such as the so-called "transverse ligaments," "ligamenta cardinalia," and the like. These were the artifacts of anatomists, and had no existence in nature or in gynaecology. Embryology showed that the pelvic connective tissue was simply the remainder of the primitive mesoblastic matrix within which the pelvic organs were developed. Part of this was dense and part was loose. The dense part formed the coverings of the muscles, the viscera, and the vessels and nerves. Certain muscles, with their connective-tissue coverings, formed the pelvic diaphragm. Certain viscera with connective-tissue coverings lay within the pelvic diaphragm. The viscera were attached to one another and to the inner surface of the pelvic cavity by connective tissue, partly loose and partly dense. The dense part of this connective tissue was that surrounding the blood vessels, the lymphatics, and the nerves. Thus the viscera were really held in position by the perivascular sheaths. It followed that relaxation of the connective tissue at the sides of the uterus, bladder, and vagina caused these organs to be loose. If they were loose they would descend in prolapse. Given a perfect pelvic diaphragm and loose viscera, the viscera would descend and would gradually dilate the pelvic outlet and stretch the perineum. Given a torn or relaxed pelvic diaphragm, together with loose viscera, the viscera would descend more quickly than with a perfect pelvic outlet. Given a torn pelvic diaphragm and perineum without loose viscera, nothing would happen. The moral was that operations for the cure of prolapse should be designed to bring together in the middle line in front of the cervix tissues which were before widely separated, and lay at the sides of the cervix and vagina. Secondly, that inasmuch as injury or relaxation of the pelvic outlet was an accident favourable to prolapse, operations designed as above indicated should be supplemented as a rule by narrowing the outlet of the pelvic diaphragm and repairing the perineum.

Mr. R. J. JOHNSTONE (Belfast) said: Like Dr. McCann,

I think that it is the upper and not the lower end of the vagina which requires to be narrowed. I have adopted for this purpose a method of colporrhaphy which was first introduced by Noble. Two wide strips of the vaginal wall are dissected off on either side, leaving a narrow band of the wall anteriorly and posteriorly. In the after-treatment the wounds are not sutured, but the vagina is packed with gauze and allowed to granulate, so that a massive attachment by means of scar tissue is formed between the lateral aspect of the vagina and the lateral pelvic tissues. The wounds are soundly healed in from two to three weeks, and I have seen several patients lately who have had no return of the prolapse two and three years after operation. High amputation of the cervix is also done if that structure is enlarged.

Dr. BLAIR BELL (Liverpool) pointed out that the treatment of retrodeviation could only be efficient if the etiology was thoroughly appreciated. He would take one class of case to illustrate this, and a class which, although the largest, had not been mentioned by any of the speakers who had preceded him. He referred, of course, to puerperal retrodeviations. In regard to these he pointed out that a uterus which did not become retrodeviated after the first confinement never did so subsequently in this class of case. He thought, therefore, that all patients should be examined six weeks after parturition, when, if a backward displacement was found, it could be cured if, after replacement, a pessary was inserted for a few months. If the condition was not discovered until too late for this, it could be cured in a similar way after the next confinement, when pregnancy had once more softened the uterus. Short of this there was no curative treatment except operation, and he was strongly opposed to pessary treatment in chronic cases. If an operation had to be performed he always, after doing any necessary vaginal repairs or uterine curettage, performed Gilliam's operation or his own "sling" operation, both of which he described in the *Practitioner* recently (June, 1908). One or other of these operations was applicable also to all those other classes of retrodeviation which called for operative interference. As for cases of prolapse, he had listened with much interest to the academic contribution of several of the speakers, but he thought they were endeavouring in this discussion to find out which method or methods gave the best results. He was surprised at the number of certain operations, all free from danger, for this condition. It was a revelation to him. However, the repair of attenuated perivascular tissues or the reapproximation of debilitated and lacerated levatores ani and fasciae were, in his opinion, at once the day-dream of the optimistic gynaecologist and the nightmare of the relapsing and pessimistic patient. Whatever might be held to obtain normally, failed to be reproduced in its effect by colpo-perineal operations. The uterus had to be supported from above—such were the reparative limitations of man. Personally, he always repaired the vaginal walls and outlet—not to keep the uterus up, but to cure the rectocele and cystocele—curetting to remedy the oedematous condition of the endometrium, and then suspended the uterus by means of Gilliam's operation, which was absolutely free from all those dangers alluded to by other speakers which follow ventrifixation.

Dr. BUIST (Dundee) said that the problem of uterine displacements was one of the equilibrium of the uterus under several factors, and to select any one factor as the cause of uterine displacement—for example, disturbance of the perivascular tissue—was as erroneous as the selection of another, impairment of the perineum. To describe the uterus as suspended by its ligaments would lead to intellectual difficulties in understanding the flotation of a balloon, and a balloon when tethered could, according to this theory, be suspended by its cable. He would emphasize the relations of the rectum to the uterus in the production of uterine displacement and of its symptoms. In the population of working women who formed the hospital clientèle in Dundee he would fail in any treatment, whether by pessary or by operation, if he did not treat this factor in the first place.

#### REPLY.

Dr. HERMAN said that the practitioner should always remember, as Dr. Pozzi had told them, that changes of other kinds—dysmenorrhoea, salpingitis, etc., existing along with displacements—needed their own appropriate

treatment, and were not necessarily either causes or effects of uterine displacements. He had not referred to them, because the subject of discussion was displacements of the uterus. He did not regard the so-called ligaments as in any way supports of the uterus, for they were never tense. He had seen cases of sudden displacement resulting from over-exertion in young women. If such displacements were promptly treated by rest and laxatives, a very few days in bed removed all symptoms. If they were not properly treated, there was risk of long-standing invalidism resulting. But he thought it would be a great injury to the education of the young if the opinion got abroad that the risk of a few girls now and then needing a few days in bed should lead to the restriction of active open-air exercise. He agreed as to the advantages of posterior colporrhaphy for rectocele. It was in such cases a complete and satisfactory cure. He did not think that endometritis was the cause of retroversion. He believed that endometritis in virgins was extremely rare. It had been suggested that the treatment of prolapse should begin by curetting. But why? He could see no reason for curetting just because there was prolapse.

### THE INFLUENCE OF THE SUPRARENAL GLANDS ON THE BONY SKELETON IN RELATION TO OSTEOMALACIA AND RICKETS.

By Professor L. M. BOSSI,

Director of the Royal Clinic of Obstetrics and Gynaecology at Gen. ca.  
A PROPOS to the discussion upon the Caesarean section and other surgical methods of enlarging the pelvis, I have the pleasure of laying before my English colleagues a method of treatment which may render Caesarean section unnecessary in cases of osteomalacia in pregnant women. Though many methods of drug treatment have been proposed for osteomalacia, not one up to the present has proved satisfactory. Yet if my successful results continue, I believe I have discovered a method which, so to say, will completely abolish the Caesarean operation for osteomalacia and which reveals an important physio-pathological truth—that the suprarenal glands exercise an important influence upon the ossification of the skeleton. It is, of course, possible that you may have read the articles I have published on this subject in the *Archiv für Gynäkologie* and the *Zentralblatt für Gynäkologie* during the past two years. It is, however, a pleasure to place before you the brilliant results I obtained with extract of the suprarenal glands, more especially with the adrenalin prepared by Messrs. Parke, Davis and Company, in the first patient upon whom I tried it.

A woman in the seventh month of her seventh pregnancy, in an extreme degree of osteomalacia, was admitted to my clinic in November, 1906. The disease had commenced in the early months of her fifth pregnancy with the usual symptoms—pains, difficulty in walking, anaemia, osteomalacic cachexia, and insomnia. In the month of August she had become quite unable to get about at all, became completely bedridden, and could not even move on account of the pains and her extreme debility. When admitted to the clinic in November her condition was most deplorable, the symptoms became worse and worse, her abdomen became more and more distended as the pelvis continued to contract, and day and night she shrieked aloud under the torture of her horrible pains. I had almost decided to perform Caesarean section before the term of her pregnancy on account of her pitiable condition, but on December 16th, 1906, after experiments which demonstrated relations between the suprarenal glands and the ovaries, and after considering the ischaemic effect of adrenalin (Takamine), I commenced treating her with doses of 0.5 c.cm. of the 1 in 1,000 solution prepared by Parke, Davis and Company. Immediately after the first injections the pains began to disappear and the insomnia was relieved, and after the injections had been continued for a few days the patient's condition was marvellously changed. She was able not only to get out of bed and walk with hardly any pain, but her abdomen, instead of growing larger, became less distended because the pelvis dilated, and at the term of her pregnancy she was spontaneously delivered. She again became pregnant without

any symptoms of osteomalacia, and ten days ago (July 19th, 1908) had another spontaneous accouchement.

Apart from this case, I myself and other physicians have had others most happily treated by the same method. There have also been failures, but it should be known that I have always stated that for success by this method one cannot count on any cases except those of acute classical osteomalacia, or such as have become acute owing to the pregnancy.

In regard to the dosage, I can affirm that women with acute osteomalacia can, without any danger, support two injections a day of 0.5 c.cm., or even of 2 c.cm., of Parke, Davis and Company's solution of adrenalin. After some days, however, there are symptoms of intolerance, but after an intermission of four or five days the treatment may be resumed with the same doses.

I think we have not only discovered a new method of treating osteomalacia, but also evidence that (1) insufficiency in the function of the suprarenal glands must be the cause, or a factor in the causation, of osteomalacia; (2) the suprarenal glands exercise an important influence upon the ossification of the skeleton—a fact previously unknown, but which I consider I have proved by radiographs on human patients and on sheep. These radiographs I have pleasure in exhibiting to the Section. In sheep from which I removed one suprarenal gland I induced, as the radiographs show, an experimental osteoporosis.

I need hardly point out that if these results are confirmed the method will find useful application in the cure of rickets in children, as one will be able to accelerate the ossification of the skeleton in rickety subjects by the administration of adrenalin.

Dr. BUIST ascertained from Professor Bossi that the dosage of adrenalin in cases of osteomalacia was 1 c.cm. of the commercial solution morning and evening, and referred to the investigation on the production of rickets which was being carried out under a grant from the British Medical Association.

### DISPLACEMENT OF THE URETER IN CERTAIN CASES OF PELVIC TUMOURS.

By CUTHBERT LOCKYER, M.D., F.R.C.S.,

Consulting Gynaecologist to St. Mary's Hospital for Women and Children, E.; Senior Physician to Out-patients, Samaritan Hospital for Women; Obstetric Physician in charge of Out-patients, Great Northern Central Hospital, London.

PROBABLY every one who has had considerable experience in the surgery of the female pelvis can recall some accidents with the ureters. In a series of 264 laparotomies I have had the misfortune to divide the ureter upon four occasions. The first time this happened I was dealing with a retroperitoneal fibroid. In performing Wertheim's operation for advanced cervix carcinoma I have twice cut the ureter, and the fourth accident occurred six weeks ago in a case of pyosalpinx and dense adhesions. It is my intention to remark on each of these 4 cases later on.

The kind of growth most likely to displace the ureter in the pelvis is a solid retroperitoneal tumour, such as a burrowing fibroid of the uterus, especially if it starts low down—for example, in the cervix. Parametric cysts which burrow downwards and forwards, raising the bladder, will do the same thing. Carcinoma spreading from the uterus into the broad ligaments does not displace the ureters, but it surrounds these ducts. Intracervical carcinoma of the type which enlarges the cervix to twice or three times its usual size, but which has not yet spread into the adjacent connective tissues as a solid growth, will displace the ureter only in so far as the cervix has expanded. The ureter will be found adherent to the side of the expanded cervix around which it has to take a wider and more lateral course to reach the bladder. In the strictest sense of the term, the ureters are displaced from their straight or converging course towards the bladder by an enlarged cancerous cervix, they are bowed outwards by it, but they never part company with the neck of the womb, even when the parametrium is infiltrated with solid cancerous growth. I have found the ureter taking its normal course with the pelvis blocked by carcinoma spreading from the



cervix. In one particular case, where I explored with the object of performing Wertheim's operation, the right ureter above its point of entry into the ureteric canal was the size of an umbilical cord; from that point onwards to the bladder it was lost in a carcinomatous mass, which surrounded it and compressed it against the side of the cervix. In another case of exploration for advanced sarcoma of the uterus the ureters were not apparently dilated, although the uterus was quite fixed by growth; this might be explained by regarding the growth of a fibro-sarcoma as growth *en masse*, such as occurs in fibroids of a benign character, and not as an extension by individual columns of cells such as we find in the parametric lymphatics in cases of spreading cancer. My experience with advanced cases of sarcoma is limited to this one case, so that I will not attempt to deduce from it any arguments as to the behaviour of sarcoma on the displacement of the ureter.

Burrowing cysts influence the position of that segment of the ureter which lies between the cervix and the bladder. This they may elevate, probably quite secondarily to the raising up the bladder which they bring about; or, again, they may depress and stretch this segment. Between the cervix and brim of the pelvis broad ligament cysts depress the ureter. I have found the duct *post mortem* embedded in the lowest extremity of a cyst wall. Whilst operating, also, I have dissected off the ureter from the bottom of a retroperitoneal cyst, which, in being dragged up and dissected out of the pelvis and from the expanded meso-sigmoid above the pelvis, had carried the ureter with it. There was no notable displacement of the bladder in this case, so the ureter lay beneath the cyst throughout its whole course to the bladder. After its separation from the cyst it lay like a flaccid rope across the pelvic cavity; the ureter in this case was the size of a man's index finger, and the corresponding kidney was found at the autopsy to be a mere shell. These burrowing cysts, therefore, can compress as well as displace the ureter. In order to do this, however, they must be of very large size and long-existing; then compression may occur at the brim of the pelvis.

A burrowing fibroid may displace the whole pelvic segment of the ureter, pushing it up entirely out of the pelvic basin, so that it may be said to have only an intra-abdominal course.

In a case of this kind I found the ureter ascending transversely from without in, between two lobes of a fibroid well above the level of the pelvic brim. The displacement was so gross that I deem the case worthy of a full record.

F. S., aged 39, single, was admitted into St. Mary's Hospital for Women and Children on August 9th, 1904. The patient was emaciated and anaemic on admission. She had been ill for four years; during this time she had become an inmate of the Soho Hospital for Women, where she had refused operation. After leaving Soho she went to the Croydon General Hospital, but no active treatment was carried out. When I saw her she had known of the presence of an abdominal tumour for four years. This had latterly grown so rapidly and caused so much distress that she had been unable to follow her work as a cook for the past year, and she was losing flesh. She complained further of periodical floodings of seven days' duration. The tumour reached to the umbilicus, and the abdominal measurements were as follows: Girth 39 in., xiphi-sternum to umbilicus 9½ in., umbilicus to symphysis 10 in., umbilicus to right superior spine 10 in., and to left superior spine 10 in. Urine: Specific gravity 1012, acid, no albumen, no sugar. Cardiac and pulmonary systems normal. Operation Thursday, August 11th, 1904. Incision from above umbilicus to within an inch of symphysis pubis. Parietes were very thin, divided by a single incision. The tumour was enormous in size compared with the abdominal cavity and pelvis (the woman was very slight and fragile). Adherent bowel was separated from the growth and the mesosalpinx on each side tied off. The bladder lay over the entire lower half of the abdominal part of the tumour; my house-surgeon assisted in stripping it off, and accidentally tore through the thin wall. This was at once sewn up in three layers. The growth now presented two lobes: an upper abdominal and a lower pelvic lobe, separated from each other by a deep sulcus. The pelvic portion had

burrowed into both broad ligaments and had to be shelled out. On the right side I was struck by a very thick vessel which ran upwards and inwards from the pelvic brim and entered the sulcus between the two lobes of the tumour. I cut this between two ligatures, after remarking on its peculiarity. The tumour was removed by dividing the vagina, which was its final attachment. When proceeding to close the enormous chasm caused by laying open both broad ligaments I noticed the stump of the cut right ureter lying on the external iliac artery at the pelvic brim, and at once removed the corresponding kidney, drained the pelvis with gauze, closed the abdominal wound, and used a collodion scab for all but the lower angle, which was occupied by the gauze. No urine was passed during the night, therefore at 11.30 of the next day (August 12th) I reopened the abdomen and found the left, and only, ureter cut close to the base of the bladder. A loop of silk was passed through the urethra into the bladder, the bladder wound was reopened, the loop of silk drawn through it into the pelvis, the loop divided, and each end of silk sewn to the cut end of the ureter, and by traction on the two silk threads the ureter was drawn into the bladder, and whilst it was kept taut the bladder walls and paravesical tissues were sewn to and around it. The silks were subsequently fixed with a certain amount of play to the skin of the thigh by adhesive plaster. Next the distal cut end of the ureter was tied and invaginated in the bladder wall. The pelvis was packed with a Mikulicz's drain, and the abdomen closed. A urinary sinus formed, and convalescence was protracted, but was finally complete. I have seen the patient off and on until 1907; she has increased 2 st. in weight, has resumed her work as cook, and is quite free from urinary or other symptoms.

In this case I do not suggest that the left ureter was displaced; no doubt I cut it through in dividing the vagina, not having dissected it off that structure previously, as I should have done. On the right side, however, the division of the displaced duct was deliberate, the upward and inward course and the losing of itself in the sulcus between the two lobes of the loaf-like growth led me entirely astray.

The second occasion on which I divided the ureter happened on November 4th, 1905, when dealing with a very advanced case of cancer of the cervix by Wertheim's method. After tracing the left ureter from the pelvic brim to the cervix, it was accidentally divided 1½ in. from the base of the bladder. The patient's condition at the end of the panhysterectomy was so bad that Mr. Targett, who kindly helped me with the operation, advised my leaving the ureter to take its chance; so after cutting off the distal end of the ureter flush with the bladder and oversewing its entrance into the bladder wall with fine silk, the operation was concluded, and the patient put back to bed, with the patent cut end of the left ureter lying in the paravaginal or parametric cellular tissues beneath the peritoneum. Whilst the urine from the bladder kept quite sweet, that passed through the ureteral fistula at the top of the vagina soon became foul and purulent; the lower part of the abdominal wound supplicated, but healed well later. This patient died of sudden secondary haemorrhage per vaginam on November 27th, 1905, twenty-three days after the operation.

Before death I cut down to try and find the bleeding vessel, without success; the haemorrhage had ceased, pulsation was gone, and scar tissue had formed.

*Post mortem* the peritoneum was healthy; internal union was perfect. In the pelvic cellular space below the peritoneum a recent blood clot was found in the left half of the pelvis, otherwise the cavity was clean. No phosphates were found, whilst into the space opened the free end of the left ureter. There was a tiny vesical fistula corresponding to the mouth of the left ureter, which I had oversewn with silk Lembert fashion at the operation. The left kidney was enlarged and the left ureter much dilated.

The conclusions are obvious: It is no use to leave a cut ureter to take its chance, and I should not again cut off the distal end flush with the bladder, but invaginate it after ligaturing the duct. The sepsis which led to secondary haemorrhage from a vessel in the left half of the pelvis was no doubt due to the decomposition in the cellular tissues of the urine escaping from the cut left ureter.

My third accident with the ureter, which also happened during the performance of Wertheim's operation, occurred on March 15th, 1906. The left ureter was first found where it crossed the iliac vessels, and from thence it was traced forwards to the uterus and seen to enter the ureteric canal at the side of the cervix. The bladder was then stripped off the front of the uterus and the anterior portion of the ureter brought into view. I now passed my finger into the ureteric canal from behind forwards, and whilst the uterine vessels were hooked upon the finger I passed a ligature underneath them, which my friend Mr. Targett tied far out—quite 1 in. away from the cervix. Then at my request Mr. Targett cut the structures lying upon my finger just internal to the ligature. They proved to be the uterine artery with its accompanying veins, and also the left ureter.

I thought at the time and wrote in my notes that the ureter must have taken an abnormal course and not stuck to the cervix as it is wont to do, but subsequent experience has taught me that it is by no means unusual, especially in cases of old pelvic inflammation, for the ureter to adhere intimately with the uterine vessels, and for the finger, whilst dilating the ureteric canal, to pass beneath the ureter instead of above or to the side of it, thus the structures in the roof of the canal lying on the finger will include the ureter, and so the above accident may happen even when division is made far out against the pelvic wall. Before ligation and section of the transversely-running uterine vessels the ureter should be seen lying in the floor of the canal at a good distance from the overspanning vascular arch, then accidents cannot happen. In this case I adopted the plan described in Case 1. There was no fistula or leak of any kind, but after the patient left hospital she came back complaining of frequent micturition and foul urine. I gave her urotropin and told her to see me again; she came, bringing an end of silk incrustated by phosphates, after the passage of which she has been quite well.

My fourth, and I trust in every sense my final case of section of the ureter in the living subject, occurred two months ago (May, 1908). The case was one of pyosalpinx with adhesions of bowel and omentum. I had drawn up the right appendages with considerable trouble as the ovary and end of the tube were matted to the floor of the pouch of Douglas. I then tied off the infundibulo-pelvic fold, which was short and thickened, and having cut this fold I found my incision had included the ureter, which was therefore cut close to the right pelvic brim. The displacement here must have been of my own making, to a very great extent. The utero-sacral, infundibulo-pelvic, and broad ligament folds were all thickened and contracted, and in the forcible elevation prior to tying off and section I must have dragged up the adherent ureter. After consultation with my colleague, Mr. Butler Smythe, it was decided that the lower end of the ureter was not suited for a ureteroplasty, and it was decided to remove the right kidney. This was done. No drainage in the loin was used, as Case 1 had done well without it; but in this case the kidney was very adherent, and no doubt some suprarenal vessels bled, for three weeks after the nephrectomy the patient developed pyrexia, with pain and tenderness in the right loin. In consultation with my colleague, Mr. Keep, we decided to explore, and a collection of *coli* pus was evacuated through an incision in the loin. The patient has now quite recovered.

This displacement of a ureter by dragging is not an unknown accident. A well-known and distinguished surgeon sent me a solid retroperitoneal growth to examine some years ago, and in its adventitious capsule lay a segment of the ureter 10 in. in length. The surgeon explained to me that although this heavy growth was dragged up without much force the ureter tore off with it at its upper end.

There is no doubt that injury to the ureter is a serious surgical accident. One of my 4 cases died as the indirect result therefrom and each of two others lost one kidney. In the fatal case I could have done vesical implantation, but the operation for cancer was so severe and took so long that the patient could not have held out long enough for the necessary suturing.

In departing from a not uncommon mode of recording "surgical successes" by thus briefly relating my mishaps with the ureters I trust I have established a few danger

signals which will prove helpful to others engaged in similar work to myself.

## DISCUSSION ON CAESAREAN SECTION *VERSUS* OTHER METHODS OF DELIVERY IN CONTRACTED PELVIS.

### OPENING PAPERS.

I.—ROBERT JARDINE, M.D. Edin.,

Professor of Midwifery, St. Mungo's College, Glasgow; Physician,  
Glasgow Maternity and Women's Hospital.

DURING the last twelve years it has been my good fortune to be one of the physicians in charge of the Glasgow Maternity Hospital, where these operations are so common that some of them are performed each week. I once had to perform Caesarean section three times inside of fourteen hours. I have thus had a very large experience in dealing with cases of obstructed labour, and the remarks which I shall make are based upon that experience. I trust that those who take part in the discussion will do the same. Let each man speak of what he knows from actual experience, and not from what he has learned from reading about what other men have done, and the discussion will be a practical one.

In deciding as to which operation one should do the personal element must always be taken into account. An expert in midwifery should, of course, be able to do all these operations with equal facility, but the major part of midwifery is in the hands of general practitioners who cannot be experts in the more difficult operations, and unless they can call in the assistance of an expert they must face the situation alone and do the best they can. In midwifery work we generally have two lives to consider. Of course, the claims of the mother are paramount, but we must not forget that the unborn babe, too, has claims which should not be disregarded. "Save the mother and never mind the child" is what we are usually told by the anxious relatives, but it is our duty to save both if we possibly can. By the performance of a Caesarean section in a case of obstructed labour this may be possible in the hands of one who is accustomed to do abdominal surgery; but if the services of such a man are not available, and the practitioner in charge of the case does not feel that he is justified in attempting such an operation, he must decide upon the operation which he knows he can do with the greatest safety to the mother, even if it should mean that the child has to be sacrificed.

The environment of the patient must also be taken into account. If the patient is in a modern hospital any operation may be performed, but if she is in a one or two roomed house without any provision for proper nursing, then a Caesarean section or even a pubiotomy or a symphysiotomy may be prohibited, whereas an induction of labour or a craniotomy could be done without much risk. In my remarks I shall attempt to keep these considerations in mind.

### *Caesarean Section.*

When I was a student of medicine this operation was not spoken of with favour, as up to that time in Scotland the results from it had been fatal so far as the mothers were concerned. This disastrous record was broken by Berry Hart in March, 1888, when he performed Porro's operation and saved the mother and child. A few weeks later Murdoch Cameron successfully operated in the Glasgow Maternity Hospital, doing the ordinary operation without removing the uterus, and in a comparatively short time he had opportunities of repeating the operation many times. The operation is now being performed everywhere with marked success. It is a very easy operation to perform, and if the surroundings of the patient are good, and strict aseptic precautions are adopted, the risks to the mother are not much greater than the risks of a difficult forceps delivery, while the risks to the child are less; in fact, there should not be any risk to the child. I am referring to cases which are operated upon before labour has actually commenced or very early in the first stage. If, however, the patient has been a long time in labour, and more



especially if she has been carelessly examined, as is too often the case with patients sent into hospital, then there is considerable risk. If attempts have been made to deliver the woman the risk is so great that I would decline to do a section unless it were impossible to deliver through the pelvis. If one is forced to operate on such a case the safest plan is to remove the uterus.

When the true conjugate is  $2\frac{1}{2}$  in. or under, Caesarean section is the operation which should be performed, even if the child is dead. With a true conjugate of  $2\frac{1}{2}$  in. or less, delivery through the natural passages by craniotomy of an ordinary-sized child should not be attempted, as there is not sufficient room to use the instruments in. In such cases a pubiotomy or symphysiotomy is out of the question; and induction of premature labour is also contra-indicated, as a viable child cannot be born alive through so small a pelvis. Such cases give the absolute indications for Caesarean section, and one's position is quite clear. When we are dealing with pelves of from  $2\frac{3}{4}$  in. to  $3\frac{1}{2}$  in. true conjugates, we have a much more difficult problem to solve, because it is possible for live children to be delivered at full time through the natural passages even without assistance. Some may be disposed to question the statement that a live child can be born at full time through a  $2\frac{3}{4}$  in. conjugate without assistance or even with the assistance of forceps, but I have had that experience more than once.

In dealing with contracted pelves the size and shape of the child's head, and also the way in which it presents, must be considered. The consistence of the head must also be borne in mind. Some heads mould to a great degree, and will pass where a smaller but harder one would become impacted. In a flattened pelvis, which is the variety of contraction which we most frequently meet with, if the presentation is an anterior parietal one, and the head is long and narrow, with a bitemporal diameter of not more than  $2\frac{1}{2}$  in., the labour may not be a very difficult one.

On the other hand, if the presentation is a posterior parietal one, the delivery will be a difficult one, and if the child is to be saved, a cutting operation will be necessary. A head of the ordinary size and shape will not allow of the delivery of a live child through a  $2\frac{3}{4}$  in. brim without an enlargement of the pelvis, and even then the risk to the child will be great. In such cases a Caesarean section should be done. The size of the head relative to the brim should always be considered. The following case will illustrate the importance of this:

A primipara with a rickety flat pelvis, with a true conjugate of  $2\frac{3}{4}$  in., was placed under my care at the hospital when she was about seven months pregnant. I found that the child's head could be pushed into the pelvis with great ease. The patient reported herself regularly, and as she neared full time I found the head could still be pushed into the brim, and when labour commenced I was quite convinced that she would deliver herself, and she did so of a live child with a very long, narrow head. The bitemporal diameter was under  $2\frac{1}{2}$  in., which allowed the head to pass in the transverse of the pelvis without difficulty. The size of the pelvis indicated a Caesarean section at full time, and if I had not taken into consideration the size of the head and the way it presented, I could easily have justified myself in doing a section.

Quite recently I have had several similar cases within a few days of each other. One of them was an extremely interesting case.

Two years previously I had delivered the woman of her first child by doing symphysiotomy after an attempt at delivery by her medical attendant. The true conjugate was slightly over 3 in. The child was alive and weighed  $8\frac{1}{2}$  lb. It is still alive. She was admitted again in May of this year. The child's head did not overlap so much as in the previous labour, and there seemed a possibility that it would come through as it was an anterior parietal presentation. After the patient had been in the second stage of labour for several hours my resident reported that very little, if any, advance was being made. I went to the hospital, intending to do a symphysiotomy if it should be required. The patient requested to be allowed to sit up in bed for a few minutes before getting the anaesthetic, and I allowed her to do so. When she was anaesthetized we found that the head had passed the brim, and was at the outlet, so the operation was not necessary. The child was alive and weighed  $7\frac{1}{2}$  lb. Its head was long and narrow in shape. An ordinary shaped head would not have passed. The exertion the patient underwent on raising herself in bed probably assisted the descent of the head.

If, in addition to the flattening of the pelvis, there is a general contraction, Caesarean section will be necessary

much more frequently than in the ordinary flattened pelvis.

I am a strong advocate for Caesarean section, but I do not for a moment urge that it should always be done in preference to any of the other operations. There is a legitimate field for them, and I shall try to indicate the class of cases in which they are to be preferred.

I shall not discuss the method of performing Caesarean section, but there is one point I should like to hear an expression of opinion on, and that is as to whether or not the woman should be sterilized to prevent future pregnancies. If the woman is suffering from osteomalacia, this should certainly be done by the removal of the uterus and ovaries. In a case where the uterus is to be left, sterilization can be effected by ligaturing the Fallopian tubes in two places and cutting out the intervening portion. I have never known this method to fail.

If the woman knows this can be done she is almost sure to demand it, and if her husband also wishes it, I always give effect to their wishes. In a number of cases where I have done the operation for the second time I have always sterilized the patient at the second operation. Unmarried women I do not sterilize unless there is a special reason.

#### *Symphysiotomy and Pubiotomy.*

I shall consider these two operations together. There is very little to choose between them, but, on the whole, pubiotomy is the more satisfactory of the two. There is less risk of injuring the bladder. Symphysiotomy is an old operation which has been revived within recent years, while pubiotomy or hebotomy is a comparatively new operation. It is usually stated that this operation was first suggested by Gigli, an Italian, in 1894, but Sir William MacEwen, of Glasgow, had suggested it years before, and had offered to do it in the Glasgow Maternity Hospital, but an opportunity of performing the operation had not been afforded him. If he had been an obstetrician undoubtedly he would have done it years before it was done by Bonardi, of Lugano, in 1897.

I cannot say that I am very much in favour of these operations, but still there are cases in which they are to be preferred to a Caesarean section in the interest of the mother. The risk to the child is always very much greater than when a section is done.

If there is much disproportion between the size of the head and the brim of the pelvis, either of these operations is contra-indicated. If the true conjugate is below 3 in., they should not be done unless the child's head is very small. A conjugate of about  $3\frac{1}{2}$  in., with an ordinary-sized head, will give a suitable case in a flat pelvis; but if there is much general contraction, then I think a Caesarean section should be done in preference to either of these operations.

A Caesarean section is a much more satisfactory operation than either, provided the case has not been handled, and the labour has either not commenced, or at all events has not been prolonged. On the other hand, if the labour has been prolonged, and especially if attempts have been made to deliver, then I think there is less risk in doing one of these operations than in doing a Caesarean section, provided, of course, that the disproportion between the head and the brim is not too great. The risk to the mother will be less, but to the child it will be greater. In primiparae the risk of tearing the vagina and opening into the joint or incision in the bone is much greater than in multiparae, and this must be borne in mind. It is impossible to guard against tearing, and this is one of the great drawbacks to these operations. If the case has been conducted on aseptic principles from the beginning, and the vagina is aseptic and can be kept so during the puerperium, no harm may result; but if the vagina is, or becomes, septic, you have to deal with what to all intents and purposes is a septic compound fracture of the pelvis, and the result may be far from satisfactory.

Either of these operations may be performed by a general practitioner, especially symphysiotomy, as no special instruments are required, so that it would be better for him to perform one of them, if the disproportion between the head and brim were not too great, rather than attempt a Caesarean section, if he has not done any abdominal surgery. The nursing of the patients is somewhat difficult, and if the bladder is injured the difficulties are markedly increased. The patient must be kept in bed

for at least five weeks before she is allowed up. This length of time is a minor consideration, but it must be taken into account in dealing with hospital patients from the point of view of cost, and also from the point of view of the patient, who may have to work for her living, and who can ill afford to be laid aside for a long time. The size of the pelvis is usually slightly increased, and subsequent deliveries may be possible without a cutting operation. If it is necessary, the operation may be repeated. I have done symphysiotomy three times on one patient, and the second and third time it was performed with great ease with practically no bleeding.

#### *Craniotomy.*

When the child is dead and the mother's pelvis is large enough to allow of the use of instruments, craniotomy undoubtedly is the operation to perform. If the child is alive, are we ever justified in destroying it? Certain theologians say no; but I am an obstetrician and not a theologian, and I have no hesitation in saying that under certain conditions we are justified in doing craniotomy on a live child. If the conditions present are such that an operation to save the child can only be done at an enormous risk to the mother, then, I say, the child should be sacrificed.

In hospital work we see a good many such cases each year. The patients are sent in in a very dirty condition after repeated attempts at delivery. The child is still living, but it has been subjected to a great deal of pressure and its vitality is low, so that, even if it were delivered by section, its chances of surviving would not be great. If a section will give the best chance to the mother then a section should be done, but in the vast majority of these cases a craniotomy will give the best chance, and it is the operation which should be performed. That has been my experience, and in such cases I now decline to do a section unless a craniotomy is contraindicated.

#### *Induction of Labour.*

This is an operation of which I am not very fond, as the results in regard to the child are very uncertain. The risks to the mother are very slight provided proper precautions are adopted.

This operation may be used in the same class of cases as those in which a pubiotomy or symphysiotomy might be done at full time. The true conjugate should be at least 3 in. in length. There is a saying that a seventh-month child has a better chance of surviving than an eighth-month one. It is difficult to understand how this belief has arisen, but it is a belief which is pretty universal. Unlike many wise saws, it is not a true one. The nearer full time the better the prospect of the child surviving, provided there is sufficient room for it to be delivered without much difficulty. I do not think there is much use inducing labour before the thirty-second week of intrauterine life. Each case must be carefully studied, and the time selected for the induction when the head can only be pushed into the brim with difficulty, provided the thirty-second week of intrauterine life has been reached. The patient should be seen once a week as she nears this time, and a careful examination should be made each time to estimate the size of the head relative to the size of the brim. The head is the best pelvimeter we have. As soon as the head is too large to be pushed into the brim with ease, it is time to bring on the labour. It is quite absurd to lay down a rule that labour should be induced at a certain week with a certain size of pelvis—say, at the thirty-second week, with a 3 in. conjugate. The size of the head relative to the pelvic brim is one of the most important factors in the case—in fact, it is the most important factor, and unfortunately it is just the one which is most frequently disregarded.

Induction of labour is an operation which a general practitioner can perform with ease and safety, and it is the one he would probably be wise in selecting, provided the pelvis was not much contracted. This operation is very useful in cases where the pelvis is an ordinary size, but at full time the child's head is too large to pass with safety. If a child has been delivered dead after considerable difficulty, in a subsequent pregnancy it would be well to examine the patient carefully a few weeks before full time and to induce labour if the head were again large.

A symphysiotomy or pubiotomy would be very suitable at full time in such a case.

Induction of labour is, of course, frequently done in the interest of the mother, as in cases of pernicious vomiting, but with this class of cases we are not now dealing.

The drawback to this operation is the uncertainty regarding the child. The dates given by the woman from which you calculate the time of pregnancy are very often wrong, but if you judge your time for induction by the size of the head relative to the size of the brim, this is not of much consequence. Some premature children have so little vitality that they perish during an easy delivery. I have seen quite a number of these, and when the women have returned, as they usually do within a year, to have the operation repeated, I have advised them to wait until full time for Caesarean section.

While I am a strong advocate of Caesarean section, when it can be performed under favourable conditions, I quite recognize that there is a legitimate field for these other operations, and, in fact, I perform many of them every year. I would like to impress upon every practitioner who is doing midwifery work the importance of carefully examining, before full time, every case in which he has the least suspicion of a contracted pelvis, and he will have time to decide what method of delivery should be adopted. If the patient should be in labour when first seen, make a careful examination, with every aseptic precaution, and, if the head is above the brim and overlapping much, the case is one in which there will be great difficulty in delivering, and if you will take my advice, do not tackle it lightly. Do not apply forceps unless you are quite sure you can deliver. If you fail with forceps, do not under any consideration attempt to do version. The majority of cases of rupture of the uterus which have come under my care have been caused in that way. If you are not prepared to do any of the bigger operations, send for somebody who can do them, or, if the patient is poor, have her transferred to hospital at once. Do not try to deliver unless you are quite sure you can do so. It is neither fair to the patient nor to the man who will ultimately have to complete the work.

#### II.—Professor ZWEIFEL, Leipzig.

#### SUBCUTANEOUS SYMPHYSIOTOMY AND EXTRAPERITONEAL CAESAREAN SECTION.

I HAD fixed upon my subject before being informed of the proposed theme of this discussion because I wished to impart my recent experience of two operations relating to childbirth in contracted pelvis, namely, subcutaneous symphysiotomy and the extraperitoneal abdominal hysterotomy. I need not repeat here the technical details of the former operation, as they may be seen in the drawings presented (See Figs. 3, 4, 5, page 802). The technique presents no difficulty, and may be completed in about ten minutes, while a Caesarean section is sure to require much more time.

The main feature of extraperitoneal abdominal hysterotomy consists in detaching the peritoneum from the bladder and the uterus as far up as the peritoneum is lightly adherent.

The technique of this is very difficult under the condition, proposed by Sellheim, that the peritoneum must be pulled off without any lesion; it is very easy and very advisable when there is no fear of small apertures. After several personal experiences I now intentionally make a small transverse incision through the peritoneum above the vertex of the bladder to enable me to detach the peritoneum under supervision of the eyes, and I then close the incision with sutures before opening the womb. This proceeding, introduced by Frank of Cologne with a transversal incision of the uterus, was merely a modification of Kehrer's method of performing Caesarean section in the year 1882 (see Figs. 6 and 7, page 803).

My own experience is founded upon 16 subcutaneous symphysiotomies and 4 extraperitoneal abdominal hysterotomies, and as material available for comparing with these operations I have performed 52 open symphysiotomies and 130 ordinary Caesarean sections. Among the 16 subcutaneous symphysiotomies not one patient died in consequence of the operation; among the open symphysiotomies



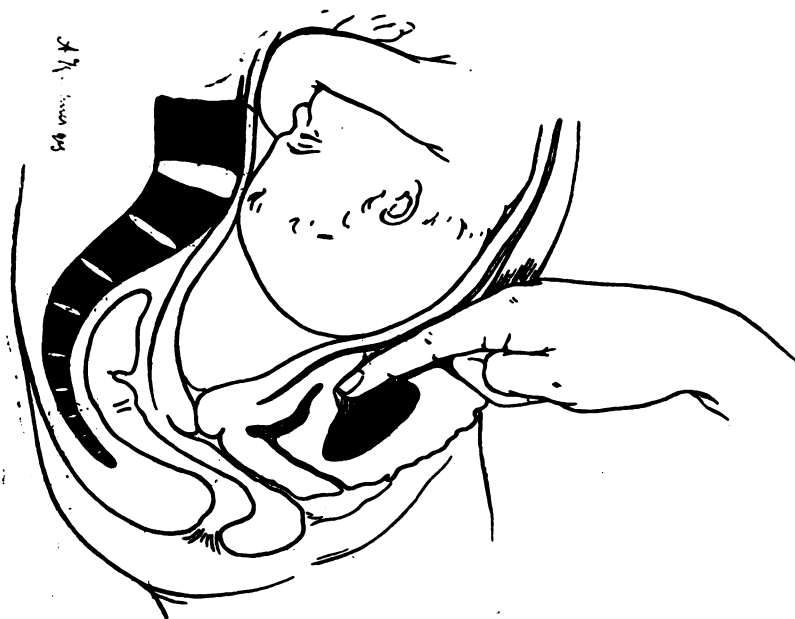


Fig. 1.—Introduction of one finger.

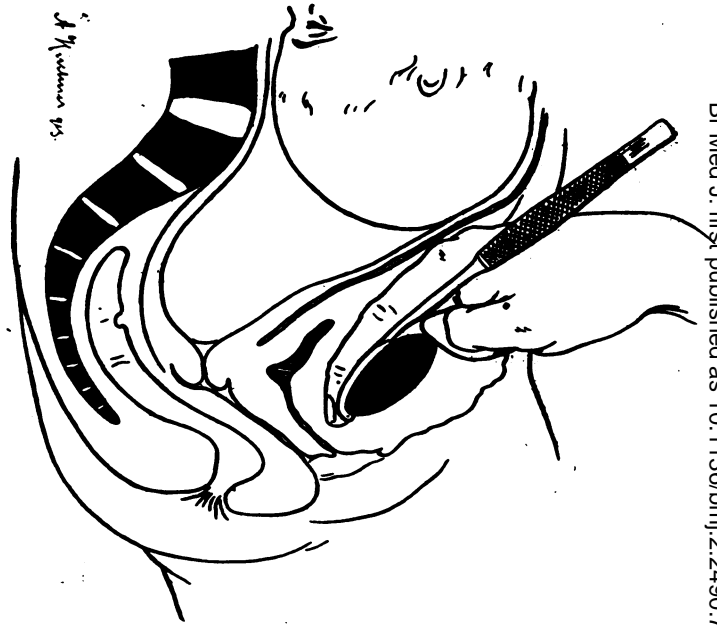


Fig. 2.—The Pott herniotome used for incising the cartilage of the symphysis.

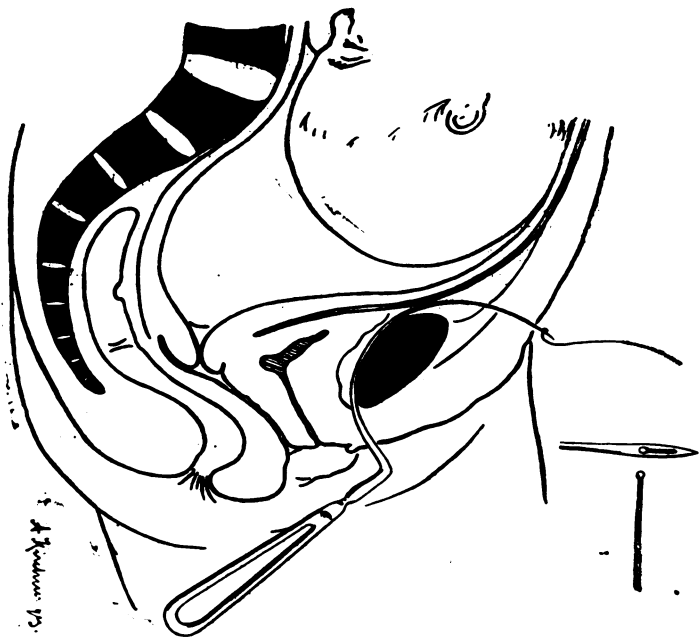


Fig. 3.—The Bümm needle armed with the wire saw.

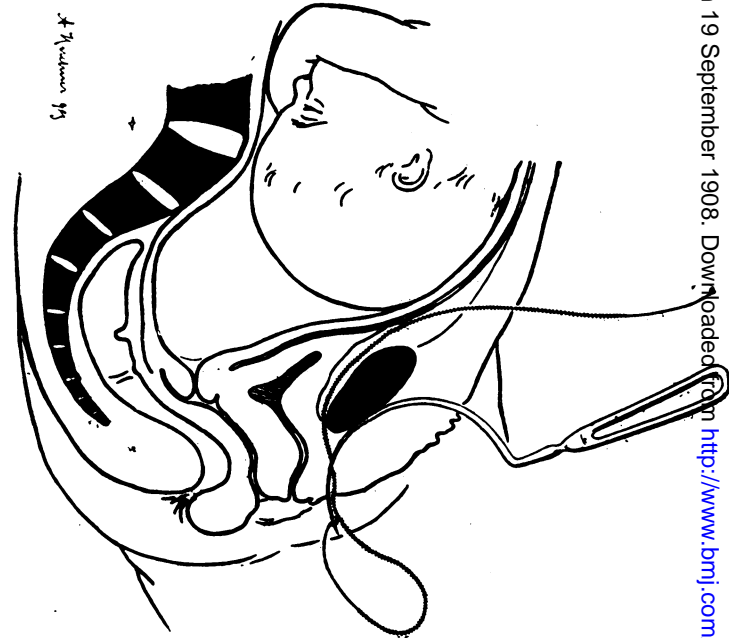


Fig. 4.—Introduction of the needle on the anterior side of the symphysis.

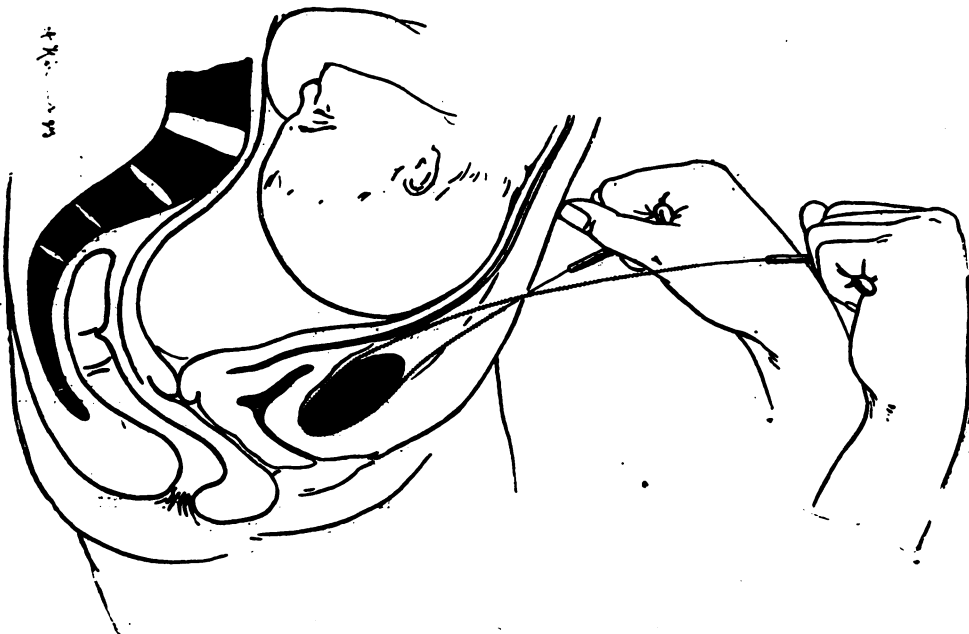


Fig. 5. Sawing through the cartilage.

I lost 3 women. In the first 100 of my Caesarean sections I lost 2 patients, but in the following 30 there were 5 deaths, so that the total mortality amounts to 5.3 per cent.

The 4 patients of extraperitoneal hysterotomies all

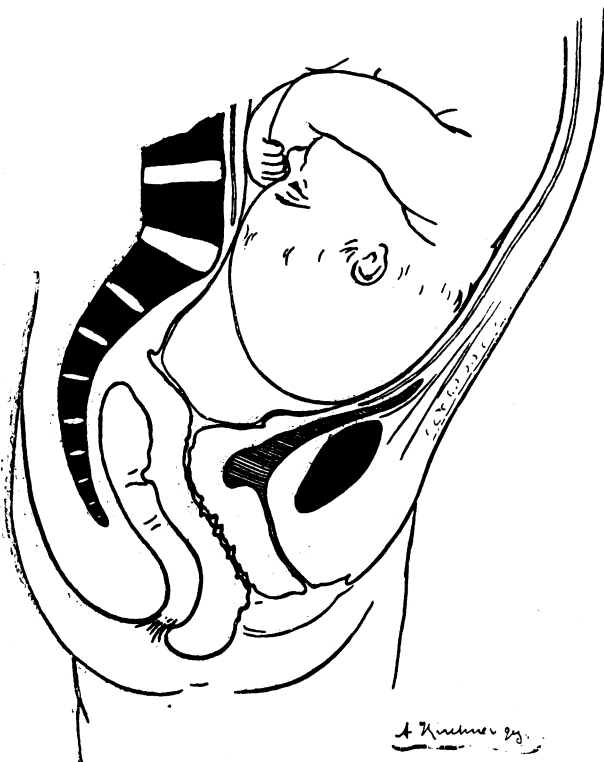


Fig. 6.—The normal position of the peritoneum.

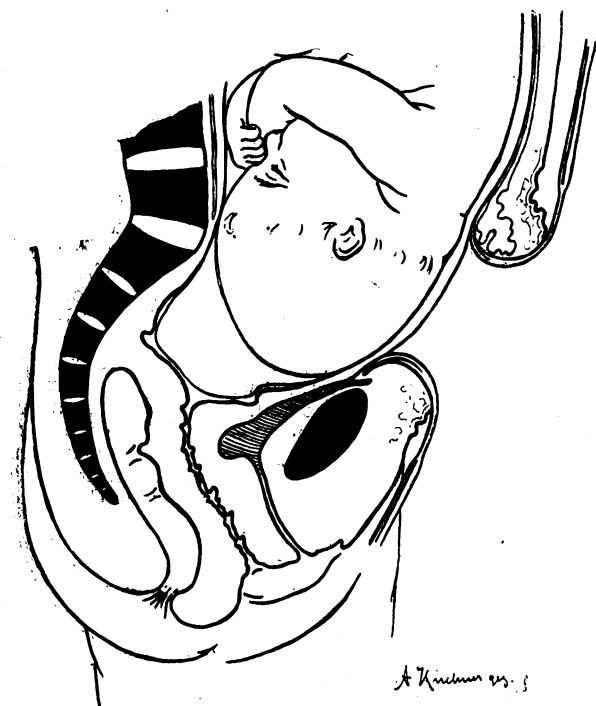


Fig. 7.—The peritoneum pulled off and retracted; the uterine wall prepared for section.

recovered, 3 without any complications; the fourth had fever both before and after the operation, but recovered ultimately after the bursting of an abscess.

My experience in this latter case, operated upon in a feverish condition, corroborates my previous opinion that in operating upon feverish patients *intra partum* a great risk is taken, and that it is better under such circum-

stances not to perform any operations which are not absolutely required for the recovery of the mother.

Of the children born after symphysiotomy, open and subcutaneous, I lost 5; of those extracted by Caesarean section I lost 2. As the death-rate is not a sufficient criterion of the value of an operation, it is necessary to consider the complications also. As regards these, I must repeat that it is unscientific to compare the figures of the modern pubiotomy or hebostotomy with those of Sigault's operation, as the latter had many defects which were in the course of time eliminated. Then hebostotomy was taken up, and profited by these improvements. Comparison is only admissible between the subcutaneous methods, and these alone may be compared with Caesarean sections.

The course of my own symphysiotomies, in which spontaneous birth was waited for in bed, has always been perfectly even, generally without fever, or with at most only one or two slight elevations of temperature. The patients were able to get up in the third week after parturition, generally on the seventeenth day; but when forceps operations or Kristeller's expressions were performed we often had serious damages. In regard to the prognosis of future confinements, I most distinctly adhere to my statement that a woman cured after a symphysiotomy will probably possess a chance of enlargement of her pelvis in a subsequent confinement without the least loss of the use of her legs.

The widespread prejudice against symphysiotomy is, I am well aware, continually being fed by reiteration of the imperfections of the old method. It would be only common justice to give a trial to the modern improved methods before condemning symphysiotomy generally.

In the 4 cases of the extraperitoneal method of Caesarean section which I performed myself the extraction of the child was not possible by hand, and was only effected by forceps. This is an insignificant complication, and may arise from the Trendelenburg position. As more important must be considered that in the first case complicated with fever the womb remained in the first month after operation elevated and slightly fixed with the corpus uteri in a retroflected position. The most important question is whether in subsequent pregnancies a rupture of the uterus would not be likely to happen. To avoid such a rupture we consider it most important to maintain the original mobility of the womb.

*The peritoneum investing the anterior wall of the uterus and the posterior surface of the bladder is originally so loose that both these organs may be moved one past the other.* In consequence of this pliable disposition, the bladder can rise while urine is accumulating, and, on the same principle, the uterus can rise during parturition and return to its former position after delivery.

What may we consider as the consequence of this anatomical fact? *That the incision of the peritoneum must be transverse, and that its edges must be united in such a manner as to replace all the organs absolutely in their previous position.* Should this be effected the danger of rupture in future confinements is not greater than after ordinary Caesarean section.

To prevent peritonitis from an infected womb Sellheim has proposed to make a *fistula from the uterus through the abdominal wall, with sagittal cuts and sagittal sewing of the peritoneum, and to extract the child through this large aperture.* I cannot agree to this proposal, because the womb loses its adaptability in case of suture of the peritoneum uteri with the peritoneum parietale in a sagittal direction.

At any rate, these new methods leave full scope for new ideas and for further improvements in future years.

#### DISCUSSION.

Professor VON KRÖNIG (Freiburg) said: Allow me to add a few words to the addresses of Dr. Jardine and my former chief, Zweifel. As his assistant it was my privilege to observe a large number of his operations, and later to put into practice on my own account the experience thus gained. Corresponding to the various proposals which have been made for further improvements in operations on the pelvis, I have myself carefully tested the different methods. As the material which I had in my earlier work in Jena is no longer at my disposal, I will simply confine



my remarks to my experience during the last three years in Freiburg. During this period I have performed operations by all the four methods—the so-called open symphysiotomy, open hebosteotomy, the subcutaneous symphysiotomy, and the subcutaneous hebosteotomy. I can here, of course, only give my judgement very briefly, when I say that in my opinion no fundamental difference exists in the results of symphysiotomy and hebosteotomy (pubiotomy). In both cases serious ruptures, lacerations of the yielding parts of the mothers, may occur, unless, as Zweifel and Morisani demand, we await spontaneous expulsion after the operation. Should the head of the child already be in the exit of the pelvis, dangerous ruptures of the mother's yielding parts are no more to be feared; it is then only a question of a rupture of the perineum, which is easily repaired. The question whether we should perform the section of the pelvis openly or subcutaneously is already answered by Zweifel in favour of the latter method. The formation of haematomata must be regarded as a disadvantage of the subcutaneous method, for, as is shown by examples in literature, they fester, and may thereby lead to serious complications. Still I, also, believe that the subcutaneous method is superior, although in my opinion its advantages cannot be considered fundamental. I have had equally good results with both methods. All my 21 patients operated on by these methods have been dismissed without their walking powers being affected. In 8 cases, it is true, the convalescence lasted as long as from four to nine weeks. On the other hand, the results have not been so favourable as far as the child is concerned. In 21 sections of the pelvis I have lost 4 children during or soon after birth. Considering that we perform the operation merely with a view to save the life of the child, we must admit that this is not exactly an encouraging result. Sections of the pelvis will, in my opinion, only be attended with good results for mother and child when we cease to perform them under conditions in which the mother's pelvis and the child's head are in a high degree disproportionate to one another. I am convinced that the operation should not be performed as a general rule under 7 cm. conjugata obstetrica; and even when over this limit it should only be performed when not too small a segment of the child's head has entered the pelvis. A powerful rival to the section of the pelvis has certainly arisen in the cervical Caesarean section proposed by Frank and modified by Sellheim. Cervical Caesarean section has these advantages over the classical Caesarean section—that it is technically easy to carry out, that the loss of blood in the operation is minimal, and that, owing to the circumstance that the wound is quite extraperitoneal, the danger of peritonitis is exceedingly small. I have lately performed five cervical Caesarean sections in non-infectious cases; the patients all got up on the first day after the operation. None of them passed a convalescence in the clinic of more than twenty days. Mother and child are thus saved, whereas there is, as we have seen, no certain guarantee for the child in the section of the pelvis. The hope expressed by Frank and Sellheim in their first publications with regard to the cervical Caesarean section, namely, that, owing to the extraperitoneal position of the wound we should be able to perform it even in infected cases, has, unfortunately, not been realized. It has happened to Sellheim as well as to ourselves, that upon our attempt to perform the cervical operation upon women in labour with infectious fever, an abscess has formed between the bladder and the cervix, which in both cases perforated into the cavity of the abdomen, and was followed by fatal suppurating peritonitis. I will therefore sum up by saying that, at all events in cases of somewhat great disproportion between the child's head and the pelvis, cervical Caesarean section should replace the operation of the section of the pelvis. In any case, I should not proceed to a section of the pelvis unless the conjugata obstetrica measured at least 7 cm.

Dr. C. HUBERT ROBERTS (London) related two cases of repeated Caesarean section. In the first patient there had been three previous labours: (1) Full time; instrumental; child dead (1899). (2) Full time; prolapse of the cord; craniotomy (Queen Charlotte's Hospital, 1901). (3) Caesarean section (Dr. Griffith, 1904). Mother and child did well. Becoming pregnant a fourth time, she was examined on the probable date of confinement—September 19th, 1905. The fetus was lying with the

head above the brim, and with the patient sitting up in the Walcher position the head could not be got to engage at all. The measurements of the pelvis were: (1) interspinous, 10½ in.; (2) intercrystal, 10½ in.; (3) external conjugate, 6½ in.; (4) diagonal conjugate, 3½ in.; (5) estimated conjugata vera, 3½ in. The patient elected to undergo Caesarean section for the second time, and on September 12th she was admitted with fairly frequent pains and in the first stage of labour. The same evening, some fourteen hours after pain commenced, the speaker performed Caesarean section. The parietes were very thin, and the incision was made through the old scar of the previous Caesarean section. On exposing the uterus the old line of incision was plainly visible, and two silver sutures and some others (of silk?) were seen covered by transparent peritoneum. There were no adhesions of the uterus to the anterior abdominal wall, or to bowel or omentum. The uterus was incised somewhat to the left side of the original scar and at rather a higher level. After the child, weighing 6 lb. 11½ oz., with a head circumference of 13½ in., and the membranes, had been removed, the uterine incision was closed by deep and superficial silkworm-gut sutures. The patient was also, according to her desire, sterilized, by ligature and excision of a portion of both Fallopian tubes. The abdomen was closed in the usual way after cutting out the old scar tissue in the abdominal parietes. The patient stood the operation well; her recovery was free from incident. The child likewise did excellently and was nursed by the mother throughout, a point considered by the speaker as important. The second patient, aged 36, was admitted to Queen Charlotte's Hospital, September 23rd, 1907. At her first and only previous confinement she had been delivered at the same institution by Caesarean section, performed by Dr. Griffith. Her pelvic measurements were: Interspinous, 11 in.; intercrystal, 12 in.; external conjugate, 7½ in.; diagonal conjugate, 3½ in.; conjugata vera estimated 2¾ in. to 3 in. Labour pains commenced three days after her second admission, and Caesarean section was again performed, this time by the speaker. The incision was made to the right of the old wound, and on opening the peritoneal cavity some adhesions were found to the scar in the uterus, which was plainly visible. These were separated and a 6-in. incision made to the right of the old wound. The child, weighing 7 lb. 2 oz., with a head circumference of 14 in., was removed, as also the placenta and membranes. The incision in the uterus was closed with deep silkworm-gut sutures, and a superficial set of Lambert sutures of the same material. Both tubes were now resected, according to the patient's previous request. The old scar tissue was cut out and the abdominal wound closed in the usual way with interrupted silkworm-gut sutures. The patient made a good recovery, and the child likewise did well, being partly breast and partly bottle fed. These two cases are of some interest in the history of conservative Caesarean section, which nowadays is so frequently performed, and with such excellent results. The speaker observed that there were many such cases on record, this operation having been performed with success as many as five times upon the same patient. As these patients requested him to do so, he endeavoured to sterilize them, but did not think it was often justifiable to do so in such cases, since the risks of repeated section, even if it proved necessary, were so small. Pregnancy following Caesarean section usually ran a normal course, and examples of spontaneous rupture of the uterus before or during labour were very rare. He preferred Caesarean section to either symphysiotomy or pubiotomy, and, with the present low mortality of the operation, Caesarean section should render the performance of craniotomy on a living child almost obsolete. He had only performed symphysiotomy once, but with good results.

Dr. MAY THORNE (London) said that in many cases of seriously deformed pelvis met with in India there was no question of induction of labour, as the patients were first seen in labour. As neither symphysiotomy or pubiotomy would give sufficient space to deliver a living child, Caesarean section had to be performed.

Dr. R. J. JOHNSTONE (Belfast) said: I agree completely with Dr. Jardine that Caesarean section should be performed as the operation of election. My principal object, in speaking this morning is to introduce to the members of:

the Section a method of performing pubiotomy which I have adopted with success in two cases. I use an aneurysm needle made about  $1\frac{1}{2}$  in. longer than the ordinary size, and with a proportionately larger hook. This needle is passed from below. The instrument may be used for vaginal hysterectomy or as an ordinary aneurysm needle as well.

Dr. A. DEMPSEY (Belfast) said: I would recommend the early application of the forceps in cases of contracted pelvis when the contraction gives hope of delivery in this way. More serious operations may not be found necessary if the forceps are applied before tumefaction or drying of the passages takes place. I strongly object to the performance of craniotomy on a living child; Caesarean section should be at once done if the conjugate is  $2\frac{1}{2}$  in. If one has had an opportunity of examining the patient during pregnancy and satisfying oneself as to the amount of contraction, a time should be fixed for the performance of the operation before labour begins. This permits of more perfect antiseptic precautions, and gives better prospects for both mother and child. For my own information I would like Professor Krönig to say why he gets patients up after Caesarean section at the end of twenty-four hours. Is it to the advantage of the mother to get up so early? We are in the habit of keeping patients in bed after natural labour for at least a week. One would think, after a serious operation like Caesarean section, a longer period of rest in bed would be desirable.

Dr. ARNOLD W. W. LEA (Manchester) referred to the comparative frequency with which contraction of the pelvic outlet was a cause of obstruction to delivery. He mentioned 3 cases in which Caesarean section had been required for this reason. Narrowing of the outlet was met with in the most extreme form in kyphosis, but he had also observed it in oblique pelvis associated with sacroiliac disease and in the funnel-shaped pelvis. If the diameter between the tuberosities of the ischium was less than 3 in., operative delivery was usually required, and in many cases Caesarean section was the most suitable procedure. He wished to know the experience of those who had performed symphysiotomy or pubiotomy for this condition.

Dr. C. E. PURSLOW (Birmingham) said that among his cases of Caesarean section there had been one in which the patient had been in labour for many hours, and attempts at forceps delivery and version had been made by two medical men. She was brought to the hospital, and on examination there appeared to be imminent risk of rupture of the uterus, as the organ was in a state of tetanic contraction, and there was a perceptible retraction ring. The fetal head had not engaged in the brim. Caesarean section was at once performed, followed by hysterectomy, as there was a commencing tear in the lower part of the uterus, indicated by extensive haemorrhage into the broad ligament. The patient made a good recovery. He had known a case in which exsection of a portion of each tube had failed to prevent a subsequent pregnancy.

Dr. J. WISE MARTIN (Sheffield) thought that no woman should be subjected to the danger of Caesarean section more than once, and therefore advocated sterilization.

Dr. R. C. BUIST (Dundee) said it was now recognized as a cardinal principle that if the operator had a choice he should perform Caesarean section before the labour had actually commenced. As regards symphysiotomy, it was not easy to understand those operators who describe it as difficult. He referred to the subcutaneous methods of Ayres and Herman. For general practice it could be performed in circumstances where Caesarean section was excluded by external conditions. It could be done in the dark. He had seen nothing in the published descriptions of hebostotomy to attract him from symphysiotomy. In patients who seemed in successive pregnancies to have a tendency to slight relative disproportion of child to pelvis he thought Prochownik's method of restricted diet should be remembered. He had had several very good cases.

Dr. C. F. SMITH (Toledo, Ohio) said: In all obstetrical procedures the interests of the child must be considered, even more than this discussion leads me to believe is here the custom. Certain religious tenets require that the child be given an equal chance with its mother. Society as well as Church demands the same. No other obstetrical operation in difficult labour offers to the child the chance for life that Caesarean section does. Performed

in from fifty to one hundred and twenty seconds, so far as the delivery of the child is concerned, there can be no infant mortality inherent to the operation itself. In the various vaginal operations, in symphysiotomy, in hebotomy, in the subperitoneal cervical section, a longer time is required in the performance of the preliminary or preparatory operation than is required for the delivery of the child in the classical Caesarean section, and, further, after these preparatory operations have been made, forceps must still be applied for the delivery of the child. Practically all cases of rupture of the uterus in labour occur in the lower zone of the uterus. If incision is made in the cervix and lower structures the danger of rupture in a subsequent labour is certainly great. In my own practice the uterine incision in Caesarean section is now always made at the fundus, where the uterine wall is the thickest, and where the least strain is thrown upon the structures during subsequent labour.

#### REPLY.

Dr. JARDINE, in reply, thanked the members for the kind way in which they had received his remarks. He said that Dr. Lloyd Roberts had misconstrued his remarks in reference to the time for operating. When possible he always selected a time immediately before the commencement of labour, and if labour had commenced he operated as soon as possible after the commencement of the pains. In reference to the size of the pelvis, it was a question of the relative size of the head to the pelvis. He would do a section with a pelvis of a 5 in. true conjugate if the child were normal and its head too large to pass. Dr. Dempsey had advised early application of the forceps. Dr. Jardine objected to this, and said that the head should always be allowed to mould. Much mischief was done by too early application of the forceps. In regard to sterilizing the patient he would not go quite so far as Dr. Martin, but, if the patient desired it, he thought that one ought to give effect to the desire. Tying and cutting the tubes had always been effectual in his hands. The uterine incision he made was a high one and a small one. He thought that the weak point in the extraperitoneal incision through the lower uterine section would be the risk of rupture in subsequent pregnancies.

### SCOPOLAMINE-MORPHINE NARCOSIS IN LABOUR.

By Professor KRÖNIG,  
Freiburg.

It is with diffidence that I venture to lecture before so enlightened an assembly upon a method of anaesthetizing during childbirth, more especially as we in Germany well know what a high standard anaesthetics have attained in Great Britain, ever since its earliest development in this country. Only the friendly invitation of your President and Vice-President have encouraged me to deliver this lecture.

The endeavour to diminish the pain of childbirth is not a new one, and probably none of the usual narcotics have remained untried. But the methods hitherto in use have failed because we did not succeed, without endangering mother and child, in making the narcotic effective for a sufficient time during labour. On the other hand, the demand for the diminution of pain during labour has been pressed all the more earnestly upon accoucheurs, since we observe that, owing to their increased mental occupations, modern women suffer far more intensely from nervous exhaustion, manifested in their diminished power of resistance against the pains of childbirth. We may regard the generally recorded increase in operative confinements, and more especially in the application of the forceps, as a consequence of this increasing incapacity on the part of the mother to bear the labour pains up to the delivery of the child. It is true that the frequency of operative confinements in maternity hospitals has not increased; according to statistics this does not apply to private practice. In hospitals recourse to the application of the forceps is taken only in cases where symptoms prove that the life of the mother or child is in evident danger.

In private practice, on the other hand, it is only in the vast minority of cases that this so-called classical indication calls for the application of the forceps; by far the



largest number of operative confinements is necessitated by nervous exhaustion on the part of the woman and by the want of will power to bear the pain to the end. The classical indication for the use of the forceps is eclipsed by the so-called deliverance forceps, the application of which has increased in an alarming manner, especially among women of the better classes. In taking the history of women of the better classes we frequently are told that in their several confinements recourse had been taken to operative measures, from which we might be inclined to infer that in such women spontaneous child-bearing is almost an impossibility; but on examination of these women no cause is apparent for this impossibility, either in the soft or in the bony passages. It would be well especially to accentuate the fact that, as regards the frequency of operations, figures taken from private practice, and not those from the maternity hospitals, can alone be looked upon as conclusive. In Germany the frequency of confinements with the forceps in hospitals is also as small as that in the English maternity establishments, but the proportions undergo a considerable change if, for purpose of comparison, we take the conditions of private practice—at least, as far as reports are at our disposal on this point. On the occasion of a discussion of the Obstetrical Society in Berlin it was shown that amongst accoucheurs who practised in the better situated classes in that city the births in nearly 40 per cent. of the cases ended with the application of the forceps. I do not think that in this instance it is a case of "furor operativus" on the part of the doctor; but in private practice, as in contrast with the hospitals, we often have to deal with persons of nervous disposition, who are overcome by such a state of nervous prostration that every moral effort on their part to endure the labour pains to the end is paralysed.

No doctor who avails himself of the opportunity of observing the labour of these women from beginning to end can accept the statement that the pains of childbirth are physiological pains, and should not be allayed. Labour pains which are felt so intensely cannot be described as physiological and advantageous. We have all known but too many cases where deliverance has been immediately followed by severe continued states of nervous exhaustion. We can really not be surprised at this if we realize only for a moment what the moral impressions of a woman must be during childbirth.

I have mentioned all this only in order to show that the demand for the diminution of pain during childbirth is in part thoroughly justified. By this diminution of pain we not only confer a great benefit upon women, but we also remove, especially in the case of nervously disposed patients, many a factor which might act injuriously on the nervous system.

Having recognized the necessity of diminishing labour pains, especially in the case of sensitive and nervously disposed women, we cannot possibly content ourselves with the means hitherto at our disposal. None of the usual forms of inhalation narcosis, ethyl chloride, laughing-gas, chloroform-ether, which have all been tried, can serve our purpose; with them one can indeed produce a short passing state of insensibility, but never sufficiently effective and long to cover the whole painful course of labour; the reason is that these narcotics would, in cases of long-continued labours, produce disorders of too severe a nature to vital organs. We can, therefore, only make use of narcotics which have the advantage over those previously mentioned that they produce a more extended period of relief from pain or of unconsciousness. From this point of view such narcotics as hydrate of chloral, morphine, aspirin, and veronal have been administered orally, hypodermically, and per rectum; but, unfortunately, it has been found that, when given in non-poisonous doses, their action has been too inadequate to produce any appreciable relief in labour pains.

Although a strong advocate of spinal anaesthesia I cannot recommend it for confinements, for it paralyses the muscular action of the abdominal walls—a factor of paramount importance in normal labour.

I believe that, though it has met with so much opposition, that wonderful narcotic scopolamine, given in conjunction with morphine, surpasses every other hitherto applied. Introduced into surgery by Schneiderlin and Korff, and by Steinbüchel into obstetrics, scopolamine adapts itself,

according to my conviction, because, given in combination with morphine in small doses which are innocuous to mother and child, it possesses, besides its analgesic action, the beneficial quality of producing prolonged interruptions in our mental associations. Hence the result of these interruptions is that impressions fade from the memory of individuals after the administration of scopolamine. This peculiar mental condition produced in women by small doses of this narcotic, not injurious to mother and child, we have named "Dämmerschlaf," a term which signifies that the patient is lulled into a sleep verging upon consciousness.

We by no means wish to create the impression that our method is a simple one. On the contrary, I believe that no doctor who has only made a few trials of this method can attain the same results as we have done in the long run of our wide experience.

Allow me to explain the mode of procedure in brief. We make use of two separate solutions—one a 300th per cent. solution of the scopolamine hydrobromicum manufactured by the firm of Merck, and a 1 per cent. solution of morphine. Both solutions are kept in transparent vessels protected from light and heat. Should the scopolamine solution turn cloudy it must be destroyed. We usually make the first injection when the patient has pains lasting at least thirty seconds and which recur in regular intervals of four to five minutes. We then inject a first dose of  $4\frac{1}{2}$  decimilligrams (0.00045 gram) of scopolamine and 1 centigram (0.01 gram) of morphine. The first effects are generally apparent about half to three-quarters of an hour later; the patients become sleepy, slumber during the intervals of labour, awake when the pains return, and manifest their suffering. But as their consciousness is as yet not quite lost, a second injection is made about an hour after the first. This injection consists of scopolamine without morphine, in a quantity of  $1\frac{1}{2}$  to  $3$  decimilligrams, according to the condition of the patient's strength and the state of her unconsciousness. Half an hour after the second injection we begin to test the patient's perceptive capacity. This can be achieved in various ways: For instance, some object is shown to the patient; about half an hour later the same object will be shown to her again and the question put to her whether she has seen it before. If she recognizes it we know that the desired stage of unconsciousness is not yet attained; and it is also an indication for another injection of scopolamine equal in strength to our second one. For testing the perceptive power we show such objects, or put such questions, as bear a certain relation to the confinement, and are therefore sufficiently familiar to the patient. For instance, half an hour after the second injection she may be asked how many injections have been administered to her. If she cannot remember the injection she received half an hour previously, the proof of amnesia exists and the injection need not then be repeated. Let us suppose that we now have to make an internal examination; this examination can again, after half an hour, be made use of as a test of the patient's mental condition.

It certainly requires practice, experience, and tact to select tests which harmonize with the standard of the patient's intelligence. For the sake of brevity I confine myself to these examples.

Anyhow a fresh injection is only given when the test proves that the object shown thirty minutes before has been retained in the patient's memory. As a rule, all injections following the first contain scopolamine only; in exceptional cases morphine is added, if a slight state of excitement prevails. We have sometimes even kept patients semi-unconscious by means of scopolamine for twenty-four hours. We accoucheurs place great stress upon this particular condition of semi-consciousness because the labour pains, although apparently perceived, are nevertheless immediately forgotten. From this point of view we understand Kraepelin's explanation of dawning-sleep. He says the patient perceives but does not appreciate. The women, anyhow, on awaking have no recollection of anything that has occurred.

Now the question has often been raised whether in these cases one is justified in speaking of a complete absence of pain. As a matter of fact there is a certain dissimilarity from complete narcosis. In "dawning-sleep" women awake for a short time during the pains; they manifest

their suffering, but fall asleep again in the intervals between the pains. The pain is, therefore, in truth momentarily perceived, so that this condition cannot theoretically be placed on the same plane as the complete suspension of feeling which exists in the case of inhalation-narcosis. But in practice the fact remains that after a successful "dawning-sleep" women awake *post-partum* perfectly happy, and declare that they have felt nothing; in fact, it frequently happens that they will not believe that they have been delivered and that they have a child which they may call their own. Of their own accord they declare their confinement to have been painless.

Practical experience teaches us furthermore that the relief which is achieved by narcosis in general surgical operations is also attained in labour by means of "dawning-sleep."

Women who have gone through several previous confinements without scopolamine, and this time with it, invariably admit that this time their general state of health is very much better, and that they have no feeling of exhaustion whatever.

Steinbüchel recommended scopolamine-morphine as a relief for labour pains before we did. But I believe that, through better dosing, we have so far improved the method that in 80 per cent. of our cases we have obtained a state of complete amnesia covering the duration of labour; at the same time, I hope to prove by statistics that no ill-effects either for mother or child ensued. At this point I must mention the painstaking observations and researches of my assistant, Dr. Gauss, carried out in my clinic at Freiburg.

In our method everything depends upon the correct dosing of scopolamine. Just as in the case of complete narcosis we must neither overdose nor underdose. In underdosing we obtain an insufficient effect, whereas in overdosing disturbances occur in the expulsive power of the uterus. The only standard we recognize as available for the correctness of the dose is the test of the patient's consciousness. We must continually ascertain during the course of labour whether the interruption of her mental association is so complete that sensation disappears with sufficient rapidity from the memory. Experience has shown us that in general the knowledge of what has happened during labour is probably obliterated *post partum*, provided that objects distinctly perceived by the patient are totally forgotten after the space of about half an hour. Hence our whole aim in this method is to test the absence of mental association. From this fact alone you will gather that an uninterrupted observation of the patient by trained attendants is imperatively necessary.

In the last few months we have got still better results of complete amnesia by causing the patients to inhale 5 c.cm. of "ethyl chloride," given with the Herrenknecht inhaler, at the moment when the child's head passes the vulva. Our reason for so doing is the following: We have ascertained that in several cases the amnesia was not complete because of the last acute attack of pain remaining fixed in the woman's memory. Notwithstanding the prolonged interruption of association under the influence of scopolamine, the labour was sometimes declared by women to have been painful; this arose from the fact that in their partial unconsciousness they regarded the last felt pain not as a momentary one, but as one which had existed for hours. [We have latterly also combined the injection of scopolamine and morphine with a small dose of veronal, in order to quiet the nervous system at the opening stage of labour, when the pains have not yet set in at regular intervals of five minutes. If pains are felt at all when the patient is admitted to the labour ward we at once administer 2 decigrams of veronal. We could not use larger doses; for on administering from  $\frac{1}{2}$  to 1 gram of veronal a distinctly adverse influence was noticeable in the labour pains.]

I must not omit to draw special attention to one point in the application of our method. It has been ascertained that the external surroundings of the patient, and especially the influence of sensory impressions such as loud noises, strong light, etc., are a considerable drawback in achieving good results. If six or seven women undergoing labour happen to be next one another in the same room, as I know is the case in some maternity hospitals in large cities, it must be almost impossible to obtain a moderately successful "dawning-sleep." In the first place it has been proved that under disquieting conditions scopolamine

induces excitement in the patient. I mention this fact intentionally, because I believe that failures which have attended the application of our method in very large hospitals are partly attributable to it. In the hospitals under Leopold in Dresden, and under Bumm in Berlin especially, the method has not met with the same success as in hospitals with smaller material. But also under the conditions prevailing at the Freiburg hospital the importance of avoiding strong visual and acoustic sensations has been clearly proved. In the case of confinements taking place in the general room, with two or three women disturbing one another through the manifestation of their suffering, the number of successful cases of amnesia was not anything as large as in the case of those who were confined alone in private rooms well protected against impressions of sight and hearing.

The experience on which I base my report has been gathered in 1,700 cases. The statement published by my assistant, Dr. Gauss, in connexion with the first thousand cases, that the method presents no drawbacks for mother or child, has been confirmed by recent experience. If other authors mention numerous instances of atonic hæmorrhage after confinements under scopolamine injections, we can with certainty attribute them to incorrect dosing. We continually measure the loss of blood in the period of the after-birth, and it has always been proved that this loss does not surpass the physiological quantity. We must also attribute to incorrect dosing the disorders of the heart and breathing, which have been mentioned in other quarters; otherwise we should have observed them at least once in so large a number of cases as 1,700. I may here remark that we have even in cases of organic heart trouble always and unfailingly applied our method of scopolamine injections. Out of these 1,700 cases, two women have died during or soon after confinement. Neither of these deaths, however, can in any way be attributed to our method as such. In one of these cases it was a question of a rupture of the uterus, in a case of an absolutely narrow pelvis, where the husband refused to allow any operative treatment. (The husband was an Italian, who tried to emphasize his determination upon the assisting doctor by means of a knife which he held in his hand.) The second case was one of fatal hæmorrhage due to placenta prævia.

Scopolamine is certainly not a remedy which can be used without the strictest control in its dosing. I believe that all cases of death following upon the administration of scopolamine in surgical operations are entirely attributable to overdosing. In general surgical practice we have for many years made use of scopolamine before every chloroform-ether narcosis, as well as before every spinal anaesthesia; the number of operations, which already surpasses several thousands, has shown us, as it has other operators, that in scopolamine we possess not only the most humane, but also the safest narcotic.

The length of labour is only immaterially increased by the injection of scopolamine and morphine. But even should it be increased by half an hour (which is not yet proved by figures) I think that this factor cannot outweigh the benefit conferred upon women by the relief from pain. (Although I have administered scopolamine to women of the better classes with delicate nerves in about 350 cases, it is hardly necessary to say that I never had to apply the deliverance forceps.)

The former opponents of the method have, therefore, been obliged to admit, in the face of 1,700 confinements, that this method is certainly without danger for the mother. They now have recourse to the objection that, although it is harmless for the mother, it is yet dangerous to the child, because they declare that under scopolamine the number of children who die during birth is larger, and, secondly, that the surviving children experience bodily and mental defects in after-life. We are able, through a physiological reaction on the eye of a frog, to detect scopolamine in incredibly small quantities. Holzbach, in our clinic, has by means of this reaction been able to establish the fact that the fresh urine of a newborn child does contain scopolamine in extremely minute quantities. At the same time he proved that it is totally excreted after the lapse of a few hours. The quantity of scopolamine which passes from mother to child is, however, in the vast majority of cases so small that it has no



ascertainable effect on the organism of the latter. Only in about 10 cases out of 100 do children come into the world in a state we call oligopnoea. In these cases, if one is not prepared to wait, a simple external mechanical stimulation—such as massage, etc.—will suffice to make the child breathe properly again. The statement that oligopnoea might lead to mortal asphyxia has been refuted by the large material of our hospital.

Independently of the fact that, owing to the technical improvements in our method of dosing, the cases of oligopnoea have diminished now from 20 per cent. to 10 per cent., we have ascertained, by comparing statistics of children who died *intra partum* before, and after, the introduction of our method, that their death-rate has diminished in a not inconsiderable degree. For instance, taking the last 500 births under scopolamine, we have only to deplore the death of one single child *intra partum*. Only three children died in the first three days, if we deduct as premature births those infants weighing under 2,500 grams. According to Ashoff, this strikingly small mortality of children *intra* and *post partum* is perhaps to be explained by the fact that the moderate inactivity which is produced in the respiratory centre during birth by very small quantities of scopolamine is of advantage to the child. Due to the influence of scopolamine and morphine the child is made slightly drowsy, and when slight disturbances of the placental circulation and small accumulations of carbonic acid happen in the blood, this drowsiness prevents it from reacting at once by premature intrauterine respiration. The air passages remain free, and in cases of slight asphyxia, for example, prompt and more certain revival is to be expected.

The last objection raised by the opponents of this method, namely, that children will later on suffer ill effects from the minimal quantities of scopolamine they have absorbed, is difficult to refute by figures. We can only prove this much at present, that the mortality amongst the children in the first year is not greater now than it was before. The objection that the evil effects will become apparent in the mental development of the subject between his twentieth and thirtieth year can never be conclusively proved either way. But, if I may quote a nerve specialist of distinction, Hoche remarked, when these objections were discussed, that such an assertion was distinctly opposed to common sense, and could not be seriously entertained.

Looking upon this objection as disposed of, I think I can sum up by asserting that in "scopolamine-dawning-sleep" as practised by us, we have a method which, without danger for mother or child, attains the desired end—namely, completely suspending the perception of pain during labour, or, at any rate, reducing it to a minimum.

I have now arrived at the end of my lecture. As yet my experience on the application of scopolamine is confined to 1,700 cases. Although, as I must admit, this number does not carry with it the weight of absolute proof, I still consider it sufficiently large to justify me submitting our method for trial to this enlightened meeting.

## THE USE OF HYOSCINE-MORPHINE ANAESTHESIA IN NATURAL LABOUR.

By ROBERT COCHRANE BUIST, M.D., C.M.Edin.

It is a great pleasure to follow Professor Krönig, to whose example my own use of this method of alleviating the distress of childbirth is due, and to be able to confirm from point to point the favourable account he gives. Soon after the appearance of Gauss's papers I gave my ward sister instructions that in any case when the labour became distressing the patient should have an injection of hyoscine\* and morphine. In the earlier cases I was summoned to each case, and of the later cases I have seen a large proportion at some stage of their progress. The practical conclusion is that I use the method freely in private practice, that when I am in charge of the maternity wards it is used as a routine method, and that the house-surgeon uses it when I am in charge of the out-patient department. For the best results it requires a little experience in dosage, but properly employed it is capable of saving the practitioner many a worrying day and weary night,

\* Hyoscine and scopolamine are respectively the British and Continental names of the same drug.—R. C. B.

and the patient much exhausting restlessness and some operative deliveries.

A few case notes will perhaps give the best notion of the method, and the following are quite typical of the results we produce.

1. I was called at 8 p.m. to a primipara who had been in labour all day, and found her calling for the nurse to hold her back during each pain. The pains were recurring at three-minute intervals and the head was well down. After the injection, within fifteen minutes the pains were recurring each two minutes and there was no distress. The child was born half an hour after the injection, a suture placed in the perineum twenty minutes later, and the placenta expressed an hour after the child. The bleeding was moderate. Patient, nurse, and I were all satisfied with the relief given.

2. I was called at 2 a.m. to a primipara who had been in labour twenty-four hours. The pains were good and the distress had got beyond the patient's endurance. The os was dilated to about 4 cm., and the membranes were intact. Had I not had this new method, I should have had to produce obstetric anaesthesia with chloroform. I gave an injection of hyoscine and morphine, and soon after was able to go back to bed. At 5 a.m. I was called again and found the os fully dilated and the membranes still intact. To allow time for the dilatation of the vagina and perineum, I repeated the injection and went back to bed. At noon the head was at the vulva, and I decided to deliver. The child weighed 9 lb., the perineum was intact, and the placenta followed in half an hour. The patient was awake almost immediately, and, though drowsy, made inquiries about the child before she went to sleep. Asked next day whether she had suffered much pain, she replied, "I felt nothing after the first injection except the prick of the second."

3. A primipara was admitted at 11 a.m., having been ill nine hours, during which, she said, she had passed urine eighty-seven times. The os was open to 3 cm. and the membranes intact. She was retching and her face turned blue during the pains. At 2.40 she had hyoscine  $\frac{1}{10}$  grain (0.00065 gram) with morphine  $\frac{1}{4}$  grain (0.01 gram). She went to sleep in ten minutes. At 4.30 she was moaning occasionally and answering questions indistinctly. At 6.35 she was awake during the pains. At 7.7 the membranes ruptured. At 7.20 she asked if the labour had begun. At 9 she was still sleeping between the pains. The child was born at 10.10, and cried at once.

The series of hospital cases is still short, numbering only 65, and amounting to about half the cases admitted during the period of observation. I select it because the records have this in common, that they were all made by pupil nurses whose one task was to observe the uterine contractions. The respective numbers of primiparae and pluriparae are 31 and 34, and, in addition to variety of position, we find as complications: Albuminuria 1, accidental haemorrhage 1, accidental haemorrhage and pyonephrosis 1, hyperemesis 1, mitral stenosis 1, aortic stenosis 1, contracted pelvis 2. Two children were born macerated, the others all alive; 2 born on the same night had artificial respiration, and 2 were stimulated to breathe regularly by the use of hot and cold water. In 2 the bleeding in the third stage and in 2 *post partum* is described as excessive, and was doubtless alarming to those who were present; but we must note that in the worst case the pulse-rate recorded afterwards was 96. Curiously enough, the first two hospital cases which occurred after the closure of my first quarter had atonic bleeding *post partum*, and they had no hyoscine or morphine.

The uterine contractions seemed essentially to be unaffected. In case after case their interval is the same before and after the injection, and if in some cases the patient goes fully to sleep she is found to have made progress when she is again examined. In many cases the contractions become more regular. In some of our cases the injection was made at too short an interval from delivery to give the relief desired, but, as a rule, in from ten to fifteen minutes an influence, increasing for about an hour and wearing off again in from three to four hours, was noticed. The restless, tired patient would drowse at least between and sometimes even during the pains, and of these she had so little memory that one patient who was four hours in hospital before delivery said she had two pains after she came in.

In hospital the initial dosage has been morphine 0.01 gram ( $\frac{1}{10}$  grain), hyoscine 0.00065 gram ( $\frac{1}{100}$  grain), but in private practice I have usually found morphine 0.016 gram ( $\frac{1}{4}$  grain) a more satisfactory dose. The method is especially useful in cases that are to dilate slowly or have a long head-moulding, and if I had to summarize my treatment in a rule, it would be, "Whenever your patient is distressed by the pains and you expect the labour to last more than an hour, give an injection of hyoscine and morphine; if

you expect it to last a considerable time, return in from three to four hours and, if necessary, repeat the injection." The second dose may be of morphine alone or of hyoscine alone or of both, as you think necessary to reproduce the mental condition you desire. If you think from examination that the contractions are really violent, lay stress on the morphine, if you think that the patient is reacting excessively to what contractions she has, emphasize the hyoscine.

In the district work my present house-surgeon has used the method about 30 times in 190 cases, and his results run parallel with my own experience. It is difficult to describe the attractions in private practice of a method which relieves the patient's sufferings while it allows labour to progress regularly, and which does not require the constant personal presence of the medical practitioner as the obstetric anaesthesia with chloroform does.

**SCOPOLAMINE-MORPHINE AND CHLOROFORM ANAESTHESIA.**

By H. MACNAUGHTON-JONES, M.D.

I HAVE availed myself of the scopolamine-morphine-chloroform method of anaesthesia in a number of serious coeliotomies since my return from Professor Krönig's clinic in Freiburg at the close of 1906, and find that it has been resorted to in 28 cases at St. Ronan's Medical Home. I have specially reserved it there for those operations in which I anticipated that prolonged anaesthesia would be demanded, and in several instances where the condition of the patient was such as to make me anxious regarding the low vitality, and the power of resistance to the shock of operation.

These operations included 3 hysterectomies, 2 appendectomies with pelvic complications, 12 salpingo-oöphorectomies or resections of the ovaries, some prolonged vaginal operations, and 2 very extensive post-operative hernias. The ages of the patients were:

Over 70 ... .. 1	30 to 40 ... .. 10
60 to 70 ... .. 2	20 to 30 ... .. 3
50 to 60 ... .. 4	Under 20... .. 1
40 to 50 ... .. 7	

The duration of the operation in three cases was over 2 hours; in six, over 1½ hours; in five, over 1 hour; in the remainder, under the hour.

I have also availed myself of this method in a few operations other than gynaecological, as in the case of a young lad whose pulse was habitually 48 to the minute, and who was very anaemic. Here abdominal exploration revealed diseased glands and adhesions encircling the intestine. The pulse rose to 55 during the operation, but there was not the least shock from it, nor any sickness afterwards. Again, in a lady for amputation of the breast, who had been operated upon some years before for carcinoma, and had general disseminated carcinoma of the glands and skin nodules. One of her lungs was practically useless; she had a rapid compressible pulse. The operation lasted forty minutes. There was not the least shock, nor any sickness. I have not referred to details of the other operations, but perhaps I may mention a few to show the character of some of the cases in which I used scopolamine.

A patient, aged 65, was brought after a long journey to London in such an exhausted condition from pain and discharge that an examination at the time was impossible. She was sustained with strychnine injections and by rectal feeding for some days before I ventured to operate.

The growth was a columnar-celled carcinoma of the vagina. The operation lasted an hour. She made an excellent convalescence, leaving the home fourteen days after the operation.

A patient, aged 39, weighing barely 5 st., had a kidney displaced into the inguinal region, and a dermoid cyst of the left ovary. The kidney was anchored by the lumbar incision; and the dermoid cyst removed. The time occupied in the two operations was fifty-two minutes. There were no post-operative effects.

Three cases operated upon under this method died subsequently in the home. The first, aged 23, was brought in with a tympanitic abdomen and obstructed bowel. The condition on operating was proved to be due to a solid and inoperable growth, which completely blocked the lumen of the rectum, and involved the sigmoid.

The second death occurred from jaundice and phlebitis in a patient with enormous varicose veins in both legs, and from whom a large semi-solid ovarian cystoma was removed. She had previously suffered from repeated attacks of phlebitis and

gall stones. The symptoms did not supervene until after the stitches were removed on the sixth day. The immediate recovery from the operation was excellent.

The third death occurred in a patient admitted in a practically hopeless condition, suffering from (as was proved by exploration) inoperable carcinoma of the omentum and adnexa. There were no symptoms due to the exploration.

For some years, since reading the experiments of Professor Schäfer and Mr. Scharlieb, I have given an injection of strychnine ( $\frac{1}{100}$  grain to  $\frac{1}{50}$  grain) and atropine ( $\frac{1}{100}$  grain) an hour before operation in all cases of chloroform anaesthesia for any serious operation. The physiological effect on the pulse fully bears out the conclusions they arrived at as to the influence on the blood pressure. In the great majority of the cases, I have had administered the night before operation  $\frac{1}{2}$  grain of morphine and  $\frac{1}{100}$  grain of scopolamine. The following morning (operating at 9.30 a.m.), when the bowel has been thoroughly emptied, a second injection of the same quantity is administered, and from half to one hour before operation the strychnine and atropine is given. In every instance save one the chloroform was administered with a Vernon Harcourt regulator.

There is no necessity to refer to the influence of the scopolamine and morphine addition to ordinary chloroform anaesthesia. This has been so frequently written upon that it is universally understood. In some of the lighter cases a morning injection only has been given, and in some strychnine alone, without the atropine, beforehand.

I have never resorted to spinal analgesia, as I am quite satisfied with the general method. When in Heidelberg and Freiburg in 1906, I was surprised to find that every gynaecological operation was performed under spinal anaesthesia, and with complete success—in the latter clinic with the additional use of scopolamine and morphine.

In my own operations the patient is brought into the theatre in a quiet and tranquil state, sometimes slightly narcotized; she quickly goes under the chloroform, and remains in a most satisfactory condition during operation. The post-operative results, as regards immediate pain, sickness, and general discomfort, are, as a rule, better. With regard to sickness, there are exceptions, and in these latter the vomiting is not influenced.

There are no disagreeable post-operative effects. Only in one case—that of an abnormally obese woman with weak heart action—in which the abdominal incision went through nearly three inches of fat, had the chloroform to be discontinued for a time. This operation, which was a most severe one, owing to serious bowel complications, was completed in two hours, and there were practically no after-consequences.

I have asked Dr. Dudley Buxton, Mr. Scharlieb, and Mr. Bakewell, who have mainly anaesthetized the patients at St. Ronan's, to write me a few lines giving their experience generally of the results.

Dr. Dudley Buxton says:

Through the courtesy of Dr. Macnaughton-Jones I have been able to adopt this method in a number of cases, and have also employed it on various other occasions. The character of the narcosis differs from the familiar combination of chloroform and morphine. The patient passes very quietly into what resembles placid sleep. Even when the conjunctival reflexes are not abolished true anaesthesia appears to be present, and muscular relaxation complete. There is no stertor, but if the chloroform is pushed duskiness supervenes with slow and shallow respiration. As I have worked in all cases with Vernon Harcourt's regulating chloroform inhaler I have been enabled to mask this condition and lessen the percentage of chloroform. The amount, whether judged in percentages or in bulk, required for even severe operations is very small. If more than the minimal percentage or amount is given the profound and, I think, dangerous degree of narcosis supervenes. I habitually use oxygen with chloroform, and it is very valuable in the sequence I am discussing. In nervous, excitable women the antecedent injection of scopolamine and morphine is most useful. It has the effect of inducing a calm, happy, and hopeful state, and they inhale the general anaesthetic more easily and readily. The dose of morphine is important, and should be varied according to the general state and physique of the patient.

As to after-effects, they are, I think, less severe. The dangers are those familiar to surgeons who have employed morphine and chloroform. To avoid them the utmost care is necessary to limit the amount of chloroform used, and, as very little is really needed, this is readily done, especially if a regulating inhaler is used. The patient should not be pushed to the depths of chloroform narcosis essential when that anaesthetic is used alone, as the danger arises from paralysis of the respiratory centre. Fortunately the gradually-deepening



narcosis and lessening of depth of respiration, with the duskiess of colour that appears, give timely warning of any peril.

Mr. Herbert Scharlieb writes:

It is impossible to argue from a few cases, but, judging from those I have seen, I should say that patients under the influence of scopolamine and morphine need less chloroform to put and keep them under than those who have not had a prior injection of these drugs. I have never seen any ill effects in patients to whom scopolamine and morphine have been given, and I do not think that the modification of the usual conditions and indications of the pupil during anaesthesia matters in the least.

Mr. Bakewell says:

I have only given chloroform by the scopolamine-morphine method for Dr. Macnaughton-Jones. Most of the cases have been coeliotomies, and in some instances as severe operative procedures as I can well imagine. There has been a curtailment of the initial stages of the anaesthesia, and the total quantity of the chloroform administered has been smaller. There was less excitement during the induction period. I have never seen any alarming symptoms arise during administration by this method.

### DERMOID CYST OF THE JEJUNAL MESENTERY.

BY

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MESENTERIC cysts of any kind are very rare in the experience of all surgeons.

In 1892 Braquehay<sup>1</sup> collected 104 cases. In 1897 Moynihan<sup>2</sup> brought the number of reported cases up to 113. Dowd<sup>3</sup> in 1900 added 22 more cases from the literature with one of his own and increased the total number of known cases to 136. Since then other papers by Broca and Daniel<sup>4</sup> and E. P. Bauman<sup>5</sup> have appeared, and isolated cases such as those of J. Basil Hall<sup>6</sup> and Arnold Lea<sup>7</sup> have been reported. The previous literature has been reviewed by most of the above-mentioned writers so that it is hardly necessary to analyse it further.

It is probable that at the present time about 150 cases of mesenteric cysts could be found scattered through the literature, but of all these cases there are certainly not more than twenty described as dermoids. This is sufficient evidence of their rarity, and is the reason why the following case, which presented features not only of pathological but also of clinical and surgical interest, is reported.

The patient, Mrs. R. B., was a young lady aged 24 years, who had been married for six months. She had been a patient of Dr. Yeoman for many years, and her antecedent history was of interest.

She commenced to menstruate at 16 years of age, was somewhat irregular, and the discharge profuse, lasting seven days. There had been constant backache for several years, which was more marked at the menstrual periods. When she was 20 years old she was seen by an orthopaedic surgeon for the pain in the back, and he advised rest in the dorsal position for ten months. This treatment was carried out without benefit. For the last three years there has usually been only a ten-day interval between the catamenia, but the discharge has been less profuse, lasting two or three days. Since marriage the menses have continued in this way, but the pain in the back and loins has been getting worse.

There has been no marked constipation, nor has there been any symptom connected with the urinary apparatus. On examination it was found that there was marked tenderness over the symphysis pubis, and a sense of indefinite fullness and resistance was observed, but nothing more definite could be made out by abdominal palpation.

A combined vaginal and rectal bimanual examination revealed the presence of a large, round, tender, semisolid and movable "lump," equal in size to a large orange, between the uterus and bladder.

It was thought that this could be separated from, and moved independently of, the uterus, which was slightly retroverted, and it did not appear to be connected with the bladder. Both ovaries were easily felt, as they were prolapsed, swollen, and tender. This state of affairs made the case something of a diagnostic puzzle; here there was a movable pelvic tumour between the uterus and bladder unconnected with the ovaries, and if attached to the uterus it was by a small pedicle.

In the circumstances we came to the conclusion, although the patient was young, that the "lump" might be a pedunculated fibromyoma, and advised operation. This advice was taken, and the operation performed on March 8th, 1908, Dr. Yeoman administering ether, and Dr. Carlisle assisting.

When the abdomen was opened, with the patient in the Trendelenburg position, we soon recognized with what we had to deal, for the tumour was easily brought out of the wound. It was a heavy, semisolid, cystic growth, the size of a Jaffa orange, and lay between the layers of the jejunal mesentery with a loop of bowel curving round it and separated from it by about 1½ in. of mesentery, which at this part was very thick. The growth protruded more from the upper than from the lower surface of the mesentery, and the great omentum was firmly glued on to the upper surface of the tumour.

After packing off the abdominal cavity, the adherent omentum was ligatured and cut through and an attempt made to enucleate the cyst from between the layers of the mesentery—a proceeding which proved impossible. In the attempt the cyst was ruptured and the whole of the semisolid sebaceous material escaped. This rapidly became solid upon the dabs on which it was collected.

The important question then arose as to what should be done, and we determined to excise the whole cyst. This procedure was rapidly carried out, the vessels secured, and the hole in the mesentery closed. The appearance of the bowel, however, gave us some anxiety, for it immediately became much congested; nevertheless, excision of the loop of gut was decided against, and the subsequent restoration of the blood supply hoped for.

The ovaries, which were very swollen and oedematous, but otherwise healthy, were next fixed in position and the abdomen closed. Subsequent microscopic examination of the cyst wall showed that the growth was undoubtedly a dermoid.

The course of the patient's recovery was far from uneventful. Hiccough and vomiting gave us much cause for anxiety; but when at the end of forty-eight hours the abdomen was soft and flat and the pulse 80 we thought the bowel would recover. For several weeks, however, the patient suffered greatly from acute "colicky" pains, which gradually lessened in severity.

The wound healed by first intention, consequently it was somewhat of a surprise when, three weeks after operation, a large quantity of offensive fluid suddenly discharged through a small hole in the line of incision. This came from beneath the aponeurosis, but there was no real evidence of faecal fistula. This sinus closed completely, but a fortnight later another large quantity of brown offensive fluid escaped. This time the silk aponeurosis suture was found loose, and was removed a day or two later. We had now to consider the question as to whether the small bowel had become attached behind the wound and was leaking.

We came to the conclusion that, as the discharge did not continue, this was not the case, but that probably the bowel had become attached and was infecting the tissues in the immediate neighbourhood.

Two months after operation the wound appeared to be soundly healed and the patient was well enough to be sent to the seaside.

But our troubles were not over. Soon after her return home there was a further collection and discharge, this time of an undoubted faecal character, but as this is now very slight and very occasional we trust that it will soon close. The catamenia for the present remain as before, but this is a symptom we can easily control with calcium lactate when her other trouble ceases.

In view of the anxiety we had as to the fate of the loop of small intestine, with the blood supply of which we had so seriously interfered in excising the cyst, a brief consideration of the best methods of procedure in these cases may not be out of place.

The work of others and statistics must be taken into account in such a rare condition, for it is not within the power of any one surgeon to decide upon his own work, so limited are the opportunities for the formation of individual judgement.

Braquehay<sup>1</sup> gave the mortality in a large number of cases where excision was practised as 40 per cent.; when simple drainage was carried out there was a mortality of 8.5 per cent. Two years later Deffains<sup>8</sup> gave a mortality of over 38 per cent. in 18 cases in which the cyst had been excised, and in 16 in which the cyst was drained a mortality of 6 per cent.

It is evident, then, that even with the simplest procedure a high mortality obtains, and that when excision is practised the mortality is appalling. This last operation must therefore rank with the most fatal in surgery.

At the same time, each case must, of course, as Moynihan points out,<sup>2</sup> be judged on its own merits, and it is probable, as far as it is possible to judge, that the right thing was done for our patient. Nevertheless, it is not fair to judge from a

\* Since this paper was read an operation has been performed to close the fistula. This proved a tedious and difficult matter, but was accomplished with success; at least up to the present (September 12th) there has been no further escape.

single result—which after all was not entirely satisfactory—and having regard to the general experience the following are probably the best lines to follow in the treatment of mesenteric cysts:

1. If the cyst is a very large one, it should be sutured to the abdominal wall and drained.

2. When the cyst is of moderate size or small, an attempt at enucleation may be made. This may prove successful, as in Lea's case.<sup>7</sup> If this cannot be accomplished the cyst should be excised, *together with the loop of bowel and mesentery involved*. Excision of the cyst alone should not be practised, for at the present time excision of the tumour together with the bowel should not expose the patient to as great a risk as removal of the cyst alone is likely to do.

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## A CASE OF MISSED LABOUR.

By ALEXANDER DEMPSEY, M.D.,

Senior Physician and Gynaecologist, Mater Infirmorum Hospital, Belfast.

CASES of missed labour are so uncommon, that I thought I might venture to trespass on the time of the Section with the following notes.

What I understand by the term "missed labour" is that the fetus has been retained in the uterus beyond the natural term of pregnancy, though it may have been dead for a considerable time before the completion of that term; or the fetus may have been alive until the very end of the full term of pregnancy and parturition has not taken place.

Death of the fetus is probably the first event in all these cases. Then, some time after, there is usually an attempt on the part of the uterus to expel its contents. It is generally believed that the membranes rupture in this effort of delivery, and the temporary rest from pains which we often see in natural labour after rupture of the membranes for some cause or another becomes permanent and labour is arrested.

Among the causes assigned for missed labour are death of the fetus, and the changes consequent upon it; a too intimate connexion between the fetus and the uterus; unsurmountable obstruction to the delivery of the fetus, as in a case of carcinoma of the cervix; or some anomalous condition of the nervous irritability of the uterus. Barnes thought that some cases of presumed missed labour were cases of either interstitial pregnancy or of pregnancy in one horn of a bicornuate uterus.

I have recently removed a placenta in a case which must have been an interstitial pregnancy in the early months. The placenta was grasped firmly in the middle by the contracting uterus, one part of it being adherent in a sac outside the uterus and the other within it. I can easily imagine that such a pregnancy might eventuate in a missed labour. The length of time which the fetus may be retained seems to depend upon the entrance or exclusion of air from the uterus. If no air gains admission to the uterine cavity—and this is possible in narrow parturient passages when there is only a small slit in the membranes—the fetus may be retained for almost an indefinite time. A case is reported by Dr. Cheston in which the fetus was retained in the uterus for fifty-two years.

Here the fetus undergoes changes similar to those accompanying the formation of a lithopaedion, an event of not uncommon occurrence in sheep and mares.

When air enters the uterus decomposition and disintegration of the fetus sets in and an offensive and putrid discharge commences. Portions of the fetus after a time begin to escape, and this may go on until the entire fetus is thrown off. But before this result takes place the patient may die of septicaemia. Spiegelberg, however, does not consider the retention of a dead fetus very dangerous even when decomposition and suppuration accompanies it.

Severe septic infection, he says, is prevented by free drainage through the patulous os, while the internal wall of the uterus takes on a condition much like that of a granulating surface.

Others do not look upon it so lightly, because the

mortality in most of the recorded cases has been very high.

Regarding treatment, I think that in every case, when the proper term of pregnancy has passed and one feels assured of the death of the fetus, delivery should be accomplished. This course is the more urgent when suppuration is going on with an elevated temperature. Palliative treatment with repeated vaginal douching, as recommended by some authorities, and occasional digital examination to remove any fragments of bone presenting, in my opinion only serve to increase the danger of sepsis.

A bold effort to clear out the uterus, notwithstanding the amount of suppuration present, is in the end the safer practice to follow. This was done in the case I now relate:

The patient lived in Maghera, co. Derry, and I saw her on April 19th, 1907, in consultation with Dr. Mayberry of that town and Dr. Hegarty of Magherafelt. She was 35 years of age, and the mother of three children. Her last birth, which was twins, was in August, 1905.

In July 19th, 1906, she took ill with pain in the right iliac region and vomiting. She had a temperature ranging between 101° and 103°. She was then seven months pregnant. At the end of two weeks she was up and able to resume her household duties. During the course of this illness she had pains and a bloody discharge, and the doctor thought she was going to have a miscarriage, but the symptoms passed away. After getting up she felt in her usual health, and there was no discharge of any kind for three months. Then she menstruated regularly for three months. During these six months the abdominal tumour gradually became less. Dr. Mayberry did not see the patient from July, 1906, until March 28th, 1907. There was then a profuse purulent discharge occasionally mixed with blood, and some small bones had come away by the vagina. The purulent discharge had been going on for a month.

The patient declined to have an operation, and nothing was done beyond antiseptic douching until April 19th. She was then seven months beyond the normal term of pregnancy, and she had been carrying a dead fetus in her uterus for nine months.

Though she had become very much emaciated and extremely weak there was no elevation of temperature until a few days before my visit, and even then it only reached 100°.

I found the uterus in the middle line of the abdomen, and the fundus reached close up to the umbilicus. The os was dilated, and some bones were felt protruding through it. There was a most copious and thin purulent discharge with an offensive odour.

With Hegar's dilators and my fingers I opened up the os sufficiently to enable me to bring away the fetal remains. I did this with uterine forceps, with a sharply curved curette, and with my fingers. The neck and occipital region were the only parts covered with skin. The rest was skeleton, which I brought away in single bones.

After getting all removed I washed out the uterus with biniodide of mercury solution and loosely packed the vagina with gauze. The interior of the uterus had a rough, leathery feel.

The patient made a good recovery and has remained in good health since, but she has not yet menstruated.

The important thing to avoid in operating on such a case, in the presence of so much suppuration, is any bruising or tearing of the uterus or vagina. Should this occur the danger of septicaemia would be very considerable.

## THE TREATMENT OF ECLAMPSIA BY MEANS OF VERATRUM VIRIDE.

By Professor L. MANGIAGALLI,  
Milan.

THE treatment of eclamptic fits by means of veratrum viride is of Anglo-American origin, and this is therefore one reason for the choice of my subject, when speaking in a country where obstetrics has always been dominated by the most enlightened spirit of conservatism.

Veratrum viride is the so-called green hellebore, a plant belonging to the family of the Ranunculaceae. The studies published in the United States of America by Osgood and by Bullock in 1836 were among the first to make known the physiological and pharmacological properties of veratrum viride, common in North America, and it was also in the United States that its first application to the treatment of puerperal convulsions was made. In 1862 Dr. Baker, of Alabama, had already written in the *Lancet* that veratrum viride was evidently the most appropriate and useful remedy against puerperal convulsions. A numerous series of observations calculated to show its efficacy, to study the mechanism of its action, and to decide the rules for its administration were afterwards



published by Boyd, Brown, Colvin, Dunn, Fearn, Griggs, Kenyon, Kinne, Oatman, Osborne, Powell, Rushmore, Scott, Squibb, Thayer, Walker, Whitehead, Pearson, and more recently by René de Cohet, De Wet, Reamy, Mann, Hurst, and many others; and I am also pleased to call to mind the experiments made in Glasgow by Reid and by Sloan. I myself made known the American experience in my report to the International Congress of Obstetrics held in Geneva in 1893, and the communication made on that occasion by Parvin impelled me to experiment upon this method of treatment which came from across the Atlantic with so many favourable attestations, and I invited my colleagues to do the same. The invitation was readily accepted in Italy, where numerous observations were gathered, and many publications were made on the subject leading on the whole to optimistic views. To-day I intend to relate my personal experience, founded upon 100 cases noted during a space of a little over ten years, for my first observation dates back to April, 1897. The number of cases observed, the long period of time over which they extend, allow me, in the analysis of the results obtained, to eliminate any accidental influence of other series of cases possibly more or less fortunate; and the importance of the statistics is increased by the fact that they embrace, not the experience picked up here and there, but the experience of one clinic alone, guided therefore by conformity of judgement in the application of the treatment.

There are two chief fundamental bases for treatment: The first, that the convulsions are but an incident of the eclamptic poisoning, the nature and origin of which is still unknown. The genetic mechanism of the outbreak of the eclamptic fit is also still unknown, although the researches of the last ten years have thrown a light on both the problems, so that the solution of one and the other does not seem very distant. Still if the convulsive fit is but an incident, it is one of the strongest and most dangerous, not only because it represents the gravity of the poison, but also in itself, as the repetition of the fits is the cause of very serious circulatory trouble, constituting a new and serious source of danger to the life of the patient. I may cite cerebral haemorrhage, that finds its first genetic moment in the alteration of the vessels due to the poison, as shown by the researches made by Clivio in my clinic, but in the convulsions and in the increased vascular pressure caused thereby we find the efficient cause of the laceration of the vessels. Besides the cerebral haemorrhage, it is sufficient for me to mention especially the great frequency of pulmonary oedema, and of pneumonia from aspiration, which in the researches made by Roman was found to be the cause of death in about half the cases.

The second point to be considered is that whatever the hypothesis may be on the etiology of eclampsia, its connexion with pregnancy is undoubted, and is therefore a sound basis for treatment by the delivery of the woman. It is an old rule of Italian obstetrics which has always been maintained, and which more than a century ago our Borsieri synthesized with the words, *Sed protinus danda opera est ut a foetu expeditissime liberentur*. And it is not necessary for me to say how exclusively this precept guides the work of many eminent clinicians, so that the immediate delivery of the woman is procured at any stage of the labour, by deep cervical incisions, by forced manual or instrumental dilatation, or by the Caesarean operation, either vaginal or abdominal. Now, it is my conviction that to carry to its extreme consequences this principle, though I also consider it a fundamental one, may be not rarely pernicious, but the delivery becomes the more reasonable therapeutic postulate when it can be done under favourable, easy, harmless conditions, and one which is of especial value in obstetric practice outside the clinics. In my own practice for the last ten years my conduct has been guided by this idea: to lessen the frequency and intensity of the puerperal convulsions, or to suppress them, by means of veratrum viride while waiting for the favourable conditions which would permit the delivery of the woman. To obtain this, the experience of these last ten years compared with that of the previous twenty-five has convinced me that no treatment is better than veratrum viride.

However, before pointing out the results obtained, it is well that I should speak to you about the methods of the

administration of the drug, which are of great importance. The methods I adopt are similar to those indicated by the American authors whom I have mentioned. In limited administration it is necessary to keep scrupulously to the most minute rules. In the meantime let us begin by the preparations and the method of administering the drug.

In the beginning, in America, the tincture of Norwood and the fluid extract of Squibb were especially used. For these the extracts prepared by Parke, Davis and Co., Merck, Erba and Zambelletti in Italy are now substituted. I have used almost all the preparations mentioned, but the greater part of my experience is based on the Italian preparations. In regard to the method of administering, although the oral way is not to be excluded, it is obvious that the hypodermic one should have absolutely the preference, and that not rarely it is the only one possible. In regard to the rules of application, all are agreed the pulse must be our guide. As long as the pressure is high, as long as the pulse is full, strong, tense, it is necessary to continue the administration of the remedy; whence it is a fundamental precept with me—that *small and repeated doses are to be preferred to large ones given at long intervals*. With such a rule we are sure to avoid the inconveniences, and especially the vomiting, mentioned in some observations. It is, then, necessary that the doctor should not leave the patient and should administer the remedy, regulating himself entirely by the condition of the pulse. This may be ascertained by palpation of the radial artery or by measuring the arterial pressure. The pulse must be kept, if possible, below 80 beats a minute, but you must also consider the other characters of the pulse—the fullness and the strength. A pulse full, strong, above 80 beats, requires the use of veratrum. When the pulse is rapid and small and the arterial pressure but slightly elevated, the veratrum viride must not be used. Several times I have had to observe the exactness of Mann's observation—namely, that these conditions are especially found in weak, sickly, and badly nourished women.

As for the arterial pressure, I measured it with Riva Rocci's sphygmomanometer, which is simple and easy to apply, fairly exact, and also handy for a practical doctor. All admit that the arterial pressure is generally very high in eclampsia, but what is more important from the point of view of the treatment is that the fit of eclampsia is always preceded by a strong increase of the arterial pressure, so that one of the chief points of the treatment—so as to avoid the repetition of the fits—is to keep the pressure low. That shows the necessity of the continual observation of the patient, so as to regulate the administration of the remedy according to the condition of the pressure.

The highest pressure observed in my clinic was 280 mm. It is necessary, however, to determine the pressure which is dangerous and beyond which another fit becomes imminent. Now, in my clinic, every time the pressure has a tendency to exceed 150 mm. we lower it by administering veratrum viride, and this proves the justification of small and repeated doses. A hypodermic injection of  $\frac{1}{4}$  c.cm. or  $\frac{1}{2}$  c.cm. generally succeeds in lowering the pressure—not in keeping it low—but the efficacy of the treatment is all founded on this last element, whence the necessity of repeating the injection opportunely, pulse and sphygmomanometer in hand.

As regards the demonstration of the efficacy of veratrum viride, the statistics of the clinic do not deal with the matter in the most favourable circumstances. Putting aside the rare autochthonous cases which are discovered early, administration of the drug undertaken from the beginning of the eclampsia, guided by the rules adopted in the clinic, would be more favourable. The majority of cases are brought in from outside under conditions more or less serious, after a number of fits more or less numerous. Nevertheless, if the results in the statistics are good, that speaks still more in favour of its rationality and efficacy.

What I have said up to now shows that there must be a certain link, direct or indirect, between hypertension and the convulsions, for, by keeping the tension constantly low one succeeds, in many cases, either in stopping the convulsions, or in rendering them less frequent and intense. But after all the convulsive fit is not the total manifesta-

tion of eclampsia. The cases show, with greater frequency than with other methods of treatment, that when the fits were stopped by means of the veratrum viride the pregnancy continued for a time, more or less long, yet presented undoubted signs of auto-intoxication, albumen in the urine, disturbances of consciousness, post-eclamptic delirium, etc. Even now the conclusion to be drawn is that though veratrum viride diminishes the pressure, and with that or some other mechanism modifies or stops the eclamptic fits, it is not claimed to be an antidote to the poison. Nor need we think of a poisonous action of the veratrum on the fetus, for, on its death, one source, and according to some the principal one, of the poisoning of the mother, is eliminated. My statistics clearly show that cessation of the eclamptic fits is often possible with the continuation of the fetal life, without taking into account that against the therapeutical interpretation and the theory itself, one may cite not a few cases of eclampsia coming on when the fetus was not only dead but macerated. We shall see further on whether it is possible to offer a plausible explanation of the cause of the efficacy of the veratrum viride against the convulsions of eclampsia. At any rate, the objective study of the therapeutical results collected by impartial observation, independent of any preconceived theory, may lead us to conclusions which have the sanction of practice even when the causes and the intimate essence of the illness and the way of action of a certain medication are not revealed to us.

Let us, then, see what have been the results of my observation in the first 100 cases of eclampsia treated with veratrum viride in ten years—from 1897 to the end of 1907—and successively in the institutes directed by me: in the clinic of Pavia, in the old maternity of Milan, and in the new obstetrical and gynaecological clinic of Milan. They are divided as follows: 71 were in primiparous women—a percentage a little lower than that given by Büttner (73.7) for the Grand Duchy of Mecklenburg-Schwerin and that given by Meyer for the clinic of Zürich. Of the 100 cases, 3 were moribund when admitted, and died from a few minutes to two hours after their entrance, and no treatment could be applied except delivery *in articulo mortis*. Finally, in 3 when admitted there were evident symptoms of extensive cerebral haemorrhage found afterwards at the *post-mortem* table.

From the point of view of the argument that holds our attention, there remain to be examined the other 94 cases in which it was possible to begin a regular course of treatment according to the rules indicated. In the 94 cases mentioned we had 6 deaths; the mortality, therefore, was about 6.34 per cent.; but then, again, in 3 of these the fits were completely stopped, and death occurred several days after the complete cessation of the fits—in 1 case from sepsis, in 2 from pneumonia.

Now, if we want to make a comparison between the results obtained in the treatment with veratrum and other methods, one may immediately meet the objection that however logical it may be to deduct from our statistics the three cases of women who were *in extremis*, and the three cases of cerebral haemorrhage in which the treatment with veratrum viride was not instituted (and no treatment would have been efficacious, the other statistics may also be aggravated by a greater or less number of similar cases. Meeting such criticism, the total mortality becomes of 12 per cent., much lower than the one of 23.68 per cent. obtained by me in the previous ten years, before the introduction of the use of veratrum viride, though obstetrics had already benefited by the use of antiseptics.

Such a total mortality of 12 per cent. is, at any rate, inferior to those of several recent statistics, varying from 16 to 37 per cent.; of Esckelin, of 19.33 per cent. for 1897; of Zweifel, of 20 per cent. for 1897; of Bayer, of 24 per cent. for 1899; of Moran, of 25 per cent. for 1900; of Keen, of 37.5 per cent. for 1900; of Glockner, of 18.37 per cent. for 1901; of Goedecke, of 16.9 per cent. for 1901; of Büttner, of 34 per cent. for 1902; of Ortreil, of 21 per cent. for 1902; of 21 per cent. of Büttner for 1903; of Meyer Wirz, of 27.3 per cent. for 1904.

The great efficacy of veratrum viride in modifying and stopping the eclamptic fits, and thereby diminishing the mortality, is, at any rate, most evident. Certainly,

eclampsia is more than convulsive fits, but we must consider as precious a means which allows the most violent and dangerous manifestation to be controlled and allows us to gain time to deliver the women by operations of slight importance.

In my statistics there is a case of great clinical interest, one of extrauterine pregnancy in the eighth month. The fits ceased completely after the use of veratrum, and it was possible to extract a living child by laparotomy. This case is particularly demonstrative also that it is not by killing the fetus that veratrum shows its efficacy.

Let us examine, at any rate the results of my statistics as regards fetal mortality. Nine women affected by eclampsia fits were received who were already confined; from the other 91 women 93 fetuses were had, because 2 were cases of twins; but among these 6 were miscarriages, including the two cases of twins, and 4 were macerated. Therefore, there still remain to be examined 83 fetuses belonging to 83 confinements; of these, 47 were born alive, 36 were born dead—a fetal mortality of about 43.37 per cent., which, if somewhat above that of some statistics, is still far below that of others, so that we may certainly conclude that veratrum viride does not augment the fetal mortality. Besides, we must observe again that the death of the fetus is not to be held as an element of favourable prognostication. Of the 12 women affected by eclampsia who died, 2 entered already confined, and the fetus was already dead in 6 of the other 10.

Can we now try to find an explanation of the efficacy of veratrum viride? That it does not act by producing the death of the fetus we have already seen, as the clinical results contradict that hypothesis, and it is also excluded by the experiments made by Bertino on pregnant animals with doses very poisonous if not mortal. That it should act as an antidote is very improbable, although in some cases the convulsions cease and the pregnancy continues. I have always observed the persistency of other phenomena of poisoning, whence the conclusion that veratrum viride is not a remedy for the eclampsia itself, but only for its most violent manifestations—the convulsive fits. Meanwhile the fact is evident that the veratrum viride lowers the arterial pressure and diminishes the frequency of the pulse, and this is in conformity with physiological experience. But on this question two Italian experimenters have arrived at contradictory results on a point of great clinical importance. Gilardoni affirms, and Chidichimo denies, that with the continued epicritic administration of veratrum viride the single periods of low tension may be made continuous, and the arterial pressure maintained in this way permanently low. Now my clinical experience agrees with the former, for in many cases I have succeeded in that aim. I will add such is the hope which the physician must not lose sight of in administering the remedy, keeping in mind that the action of the drug is to lower the pressure, and that such an abatement coincides with the attenuation or cessation of the fits. We must now ask ourselves whether between these two facts there is a link, and if there is one, what is its mechanism. The pathogeny of the eclamptic fit is still obscure, and even if we accept the view that the eclampsia is owing to poisonous substances of ovular origin, or to poisonous substances produced by the altered metabolism of the mother, the mechanism by which they produce the eclampsia fit is unknown to us. Certainly such poisonous substances must be hypertensive, as hypertension is an almost constant note of eclampsia. But hypertension alone does not engender convulsive fits, as my assistant, Dr. Mirto, has shown, by artificially increasing the pressure in pregnant rabbits and dogs by the injection of adrenalin, with or without ligature of one or both the ureters. If it were shown that the before-mentioned poisonous substances, besides the hypertension, induce a state of cerebral hyphaemia, whence the convulsive fit, it would be easier to explain the efficacy of the veratrum viride, but while this is a hypothesis which has several considerations in its favour, it cannot be denied that it is contradicted by others. We must, then, be satisfied for the present with the affirmation of the fact that veratrum viride is efficacious in the treatment of the eclampsia fit, and that most probably such efficacy is in relation with its hypotensive action.



### UTERINE CANCER COMMITTEE.

DR. FREDERICK J. McCANN submitted the draft report on the early recognition of uterine cancer drawn up by the Special Committee appointment of which by the British Medical Association was urged at the Exeter meeting last year. In doing so Dr. McCann said that the Committee, of which he was chairman, spent considerable time in the consideration of the subject, and had divided its report into two parts: (1) An appeal to medical practitioners to promote the earlier recognition of uterine cancer; (2) an appeal to midwives and nurses. It was intended that the appeal to practitioners should be published in the leading medical journals, as it would be too expensive to send a copy of this appeal to all registered medical practitioners. The appeal to midwives and nurses would be sent to midwives and district nurses in England, Scotland, and Ireland, and in so doing it was hoped to obtain the assistance of the medical officers of health, nursing associations, and other governing bodies. The Committee had been selected on a territorial basis in order to obtain advice as to the best course to pursue in distributing this appeal. In Switzerland, Germany, and other countries similar attempts had been made to promote the earlier recognition of uterine cancer, and had been attended by useful results.

Observations on the report having been made by members of the Section present, the following resolution was unanimously adopted, on the motion of Dr. J. J. MACAN, seconded by Professor MALINS:

That this Section approves generally of the draft report on the early recognition of uterine cancer, and requests the Council to reappoint the Committee to complete its work.

## SECTION OF PSYCHOLOGICAL MEDICINE.

WALTER SMITH KAY, M.D., President.

### PRESIDENT'S OPENING REMARKS.

On the assembling of the Section for its first morning's work the PRESIDENT, Dr. Walter Smith Kay, welcomed those present, and then, after a few brief observations, called for the opening of the first discussion forthwith.

### DISCUSSION ON THE TREATMENT OF THE HABITUAL DRUNKARD (LEGISLATIVE AND OTHERWISE).

#### OPENING PAPERS.

I.—T. CLAYE SHAW, M.D.,

Lecturer on Psychological Medicine, St. Bartholomew's Hospital.

THE word "drunkard," as far as our immediate purpose goes, implies the drinking of alcohol in some form to excess, and does not include drugging by hypodermic medication, inhalation, etc.

Some people prefer the term "inebriate" because it is less opprobrious and more comprehensive, and applies to both alcoholic inebriates and drug habitués; but it does not appear that there is any particular reason for sparing the feelings of those who indulge to excess. We call a person who deliberately kills another a murderer, and we do not cloak our appellation under the euphemism "culpable homicide," so we will retain the term "drunkard," partly because there is a kind of rough brutality about it which concisely expresses the general feelings about this class of case, and partly because the word is well understood and recognized as comprehending the whole territory of excessive alcoholic indulgence.

It so happens that a Royal Commission is now sitting on this particular question, the date of this Commission having been fixed subsequent to the enunciation by this Section of our intention to discuss it at this meeting. The scope of this Commission is very large and will, I understand, be extended to take evidence from many societies and individuals, including even some who are not re-

cognized in medical circles as professional exponents of the subject. Not only is the British Medical Association itself, which has been for some time actively concerned with the question now under consideration, but the Society for the Study of Inebriety, the Medico-Legal Society, the Medico-Psychological Association, and even the Turveyites together with other bodies are to lay their views before the Commission. Let us hope that some practical result will be arrived at by this Section in the direction which the consensus of those best acquainted with the facts seems to point—the necessity for the authority to use restraint when it is required—and that our expression of opinion may be brought before the Commission as the deliberate conviction of a body of men who are intimately acquainted with the circumstances about which we are deliberating.

I do not propose to discuss the medical treatment of the habitual drunkard, not because I believe medical treatment to be altogether a failure—on the contrary, the records of some of the inebriate homes where medical treatment is carried out may be quoted to prove the great value of remedies in some cases—but because we have already as much power as we want for the application of medical agencies. What we require is the possibility of obtaining a *lengthened time* during which remedies (if necessary) may be applied. A discussion on the most efficacious treatment of inebriety by drugs is well worth the attention of any society; but we are all probably agreed that whilst the less legislative control there is on this strictly medical point the better (I am speaking now of properly qualified and registered practitioners), the time has arrived when, for the sake both of the patient and his social surroundings, it is necessary to have means at hand for the forcible control and detention of a person upon whom, whilst drugs have been tried and conspicuously failed, the curative elements of time and enforced abstinence from stimulants may have a reasonable chance of operating.

According to Section 3, Clause 3 (b), of the Act:

Habitual drunkard means a person who, not being amenable to any jurisdiction in lunacy, is, notwithstanding, by reason of habitual drinking of intoxicating liquor, at times dangerous to self or others, or incapable of managing himself or his affairs.

As practical men, conversant with the various altered mental conditions in which acute or chronic inebriety shows itself, we all recognize that, whilst there are actual insanities produced by alcohol, which require and are easily placed under certificates for asylum treatment, there are yet others which, though not certifiably insane, do require compulsory sequestration for their own and others' safety. We also know that mental troubles caused by alcohol have a way of recovering in a most surprising manner when all alcohol is withdrawn, and that under the present state of the law it is not possible to detain these people, though we know that to discharge them is to invite a speedy return to the deplorable conditions which before existed, so that, if legislation is to be of real value, it must be both corrective and preventive.

Our interference is required for persons who drink to excess—perhaps constantly, perhaps intermittently—who, though rarely drunk in the sense that they are absolutely incapacitated, are yet unable adequately to discharge their obligations, who squander the money which should be for the family in racing, betting, and gambling, who pass much of their time in bed when they should be following their business, who through absence of definite delusions or hallucinations cannot be certified as insane, who are ready to give promises of reformation which they do not keep, but who are always on dangerous ground, on the verge of doing something which renders them liable to legal penalties, though quite able to give plausible accounts of their conduct, and to sustain a certain amount of conversation on their actions.

This is the condition in which we want the law to help us, not only for the sake of the agent, but for those who are being slowly submerged and threatened with total extinction by a continuous current of banal excess which should be dammed up or diverted. Why should this destructive, grinding, and oppressive creature be allowed to desolate his home, to endanger the good name of his wife and children, and to pauperize his dependents, who ought to have the means of staying the destruction which they can see is gradually overtaking them. At present the law takes no notice of acts unless they cause harm to

self or others, not of continued evil practices which *may* do harm. There is apparently nothing to prevent an inebriate acquiring a licence to carry firearms and buying a revolver as long as he neither threatens to nor actually does use it. Only when he *has* used it does he come within the clutch of the law, though all the time he is in such a condition that the impulse to use it may arise at any moment, and the mischief be done before it can be prevented. Is such a possibility right either for the man himself or for society in general?

I come now to the legal processes which should be invoked, remarking in the first place that there is a dual control—namely, that of the Secretary of State, who regulates the management of the retreat and the duties of inspection, and the county and borough councils, which are the authorities for the granting of licences. I am able to state that the experience of those versed in the working of these retreats is that this dual control is for many practical reasons unsatisfactory, and that to the Secretary of State alone should be committed the power to grant, transfer, or withhold licences, and, moreover, that compulsory licensing of all retreats should be made absolute.

The question has been raised whether it would be of advantage to grant licences to lunatic asylums for the reception of habitual drunkards. I beg to suggest that it would, for the following reasons: In a paper read before the Medico-Legal Society on December 17th, 1907, I remarked that when a person insane from alcohol has been placed in an asylum and has so rapidly recovered that he can no longer be detained, it should be possible for the governing authorities to discharge the patient for a term of probation to an inebriate home, the term to be capable of extension for as long as was considered necessary. I still think that removal to the home would be a good thing for the well-to-do person who may have been placed during his attack of insanity in a private asylum, but in the case of the pauper patient the advantage of removing him to the part of the asylum licensed for the reception of inebriates would be a cheap and expeditious way of keeping him under supervision whilst it would free him from the company of actually insane persons, would prevent the stress put upon the accommodation at existing inebriate homes, and would lessen the expense. For a private patient whom it is not expedient, or even possible, to confine in an asylum, I would have the power granted to place them in a home upon the authority of two medical certificates, somewhat similar in form to lunacy certificates, but free from certain objectionable phrases which exist in the latter. These certificates should be available for at least six months, and should be capable of renewal for additional terms of six months if considered necessary by the authorities of the home. Whilst under these certificates the patients would be allowed to transact such business as he was deemed capable of by the home authorities and their superintendent, but special permission would have to be given for these acts of responsibility, and during the time of his detention all personal liberty would be denied, except by special permission of the superintendent. A question arises as to the propriety of having the order of commitment to the home signed by the next of kin. It would probably be better for the future family harmony if the immediate members could be kept clear from participation in the detention of the head of the house, but on the whole it is probably only right that the friends should undertake some responsibility, and therefore it may be concluded that some authorization from a near relative must accompany the certificates, either as a separate document or conjointly with those of the certifying doctors. The advisability of having the order of a magistrate is also a debatable point. Experience of certification in lunacy cases is not entirely in favour of having a magistrate's order, for in urgency cases it is not in the first instance required. If the order of the magistrate is understood to be nothing more than a formal official recognition that a certain process has been complied with, there would appear to be no reason, beyond the question of delay, why it should not be made compulsory; but if it is to involve discretionary power, then its possible harm is apparent, because the arbiter is not always in a position to judge the validity of the premises. The form of certificate would be on the following lines:

I,....., being a registered medical practitioner, hereby declare that I have visited and examined.....residing

at..... and am satisfied that the said..... is an habitual drunkard (or a chronic inebriate) and is unable to control himself and to manage his affairs. I accordingly recommend that..... be detained in the licensed home for inebriates at..... for a period of..... calendar months, at the end of which time this certificate may be, if necessary, renewed for another period of..... months, or for such time as is considered by the authorities and the friends of the patient to be advisable.

Signed.....  
I agree to the above certificate being issued.  
Signed....., nearest of kin.

The patient thus compulsorily secluded ought to have safeguards. Whilst under detention he would be visited by the Government Inspectors, and he would have his legal remedies for improper certification just as the lunatic now has.

I note that the Departmental Committee on the Inebriates Act has issued a series of questions for the guidance of witnesses submitting evidence before it, and from the scope of these questions it is evident that the whole subject of new legislation in regard to the taxation and licensing of retreats, the compulsory certification and seclusion of habitual drunkards or inebriates, their employment and treatment when in retreats, etc., is receiving the fullest consideration; but I venture to suggest that a resolution embodying the views of this Section of the British Medical Association on the question of the urgency of compulsory legislation in the cases under consideration might be presented to the Council, with a view to its being brought before the notice of the Departmental Committee with the object of reinforcing such testimony as is brought before it in favour of this proceeding. Section D of the questions says:

In view of the fact that the 1879 Act only empowers the detention of inebriates when they themselves desire treatment, and the 1898 Act only when the inebriate becomes criminal or degraded, are you of opinion that further powers are necessary to authorize the treatment, guardianship, or detention of inebriates who cannot be controlled or treated under any existing power?

Surely our answer can only be that we do think so, and that whether the means employed be by certification or by inquiry on commission (as has been urged by one very experienced prison physician), we must do all in our power to back up such efforts as are being made to attain this end.

I conclude by saying that we can teach people the necessity for moderation, but alcohol is always a dangerous instrument, and we cannot be surprised if some who do not know how to use it as a servant find that it becomes a tyrannous master. Drunkenness may not be the end for which alcohol is taken, but the social system which permits the insidious ensnarement, swelling up finally in a paralysing incompetence for economic duties, should also provide the means of escape from the net, if even by temporary restraint of another kind, and it should resolve that the possession of such a power is not a futile asset.

There is really nothing novel in legislating to effect the compulsory detention and control of these persons except that this country already lags behind numbers of others, whilst in some places (I am now quoting from particulars furnished by Dr. Hogg, of the Cedars, Rickmansworth) still more stringent laws are in action, for an habitual drunkard who is obliged to seek help from the authorities to support himself or his family is liable to arrest, and may be placed in a workhouse for a term not exceeding two years.

To carry out with all due safeguards the measures above proposed is not asking too much in the way of public expenditure, because the private patients will be able to pay, and if the present homes for pauper inebriates are not sufficient, it will be possible to make use of the machinery of the existing county asylums by adding a little to their accommodation.

II.—Commander CHARLES T. SCOTT,  
Chief Constable of the City of Sheffield.

THE INEBRIATES ACTS, 1879-1900.

It is no part of my present purpose to enter into any long discussion on these Acts, but to point out some of the causes which seem to militate against their successful operation. I will, therefore, merely explain briefly the objects of the two Acts with which we are mostly concerned, referring those who require further information to



the Acts themselves and to the report of H.M. Inspector under the Inebriates Acts, 1879-1900, for the year 1906, which can be obtained from any of the Government printers for 2s. 6d.

The Habitual Drunkards Act, 1879, authorizes justices to license "retreats" to which habitual drunkards may be admitted on their own application. It also defines the meaning of the term "habitual drunkard" in the appalling definition which appears later on. Rules can be made by the Home Secretary, and penalties can be inflicted for breach thereof.

The Inebriates Act, 1898, sets up State and Certified Inebriate Reformatories. The former, as the name implies, are run by the State. The latter can be run by private individuals, corporations, or similar bodies, and arrangements are made for their upkeep and for the maintenance of persons detained therein out of the rates, etc., with a subvention from the Treasury.

Section 1 of this Act provides that persons convicted on indictment for offences punishable by imprisonment or penal servitude—if these offences were brought about by drink—may be sent to the State or other certified inebriate reformatory. Section 2 provides that persons convicted summarily at least three times during the preceding twelve months of other minor offences, such as drunk and disorderly, drunk in charge of a child, etc., may be sent to an inebriate reformatory.

For some reason this latter Act of Parliament never received adequate attention. When it first came into operation the reformatories were not ready, and probably the South African war had a lot to do with distracting attention from what ought to have been, and is, a most important Act of Parliament.

It was the Licensing Act of 1902 which first reminded us of the Act of 1898, and referred us also to the Act of 1879. We realized that for the first time some one other than the inebriate was to be benefited. The husband or wife, as the case might be, of the inebriate was enabled to obtain a separation order and other privileges.

To exemplify some of the difficulties of the Act I cannot do better than relate the Sheffield experiences. The Act of 1898 says that any one

who within the twelve months preceding the date of the commission of the offence has been summarily convicted at least three times of any offence mentioned in the schedule (drunk and disorderly, etc.), and who is a habitual drunkard, shall be liable upon conviction on indictment—or, if he consents to be dealt with summarily, on summary conviction—to be detained for a term not exceeding three years in any certified inebriate reformatory the managers of which are willing to receive him.

We at once commenced drawing up lists of "our friends" who had been so convicted, and in a very short time we appeared before the magistrate with a case of a man who seemed to answer the above description. We proved the previous convictions, and were then met with the question: "Is he an habitual drunkard?" We said, "Oh yes. He has been convicted thirty-five times before, and is always drunk." We were then met by the following question: "Is he a person who, not being amenable to any jurisdiction in lunacy, is, notwithstanding, by reason of habitual intemperate drinking of intoxicating liquor, at times dangerous to himself or herself or to others, or incapable of managing himself or herself and his or her affairs?" (The definition in the Act of 1879.) We said we didn't know, and were not able to prove it. Case dismissed.

We set to work again and caught another—a lady this time, full of whisky and bad language, who, in an access of alcoholic mirth, had gone through the bedroom window. She also fully answered the definition. We thought we were right this time, as, if not dangerous to herself, she certainly might have hurt somebody passing by falling on them. Again we went into court and proved our case, as we thought, but were met by this: "Of course she is dangerous to herself or others when she is drunk; but what is she like when she is sober?" We had to admit that we had never seen her sober, and, consequently, did not know. The magistrate went on to say that Parliament even as long ago as 1879 must have known that all drunken people, when drunk, were at times dangerous to themselves or others, and their definition in the Act of 1879 which governs these cases must have been intended to mean "when they were sober," meaning thereby that the words of the definition

should be "at times, when sober, dangerous, etc." Case dismissed.

This decision is, whatever Parliament might have intended, logically correct, and the Sheffield magistrate is not alone in his reading of it. But it follows, then, that the ruffian who comes home drunk every night and beats his wife is, provided he is thereby not injured in his mind or dangerous during his sober moments, not an habitual drunkard at all, and this may have more to do with the comparative failure of the Inebriates Act than His Majesty's Inspector under the Act seems to think, and at once reduces the habitual drunkard to a very limited class.

There are other difficulties in the way, all of which tend to reduce the efficiency of the Act. Under the principal Act a person charged can only be tried on indictment at Quarter Sessions or Assizes, unless he consents to be dealt with summarily. It is one of the peculiarities of this class that they never know or believe that they are habitual drunkards, and consequently never, or rarely, will they consent to be dealt with summarily, that is, by the magistrates. The county or borough is consequently put to the expense of a trial at Quarter Sessions or Assizes, and frequently, instead of going to this trouble and expense a sentence of imprisonment is substituted. This is a fruitful source of failure of the Act. Again, when a person has once been sentenced to detention and on his discharge breaks out again, the whole of the above cumbersome procedure has to be gone through once more, thus losing all the benefits which he might have received from his original detention, which loss would not have been so marked if he could have been recommitted directly he broke out. The Government Inspector's report is full of this complaint.

The extreme difficulty of proof is another great defect, and it is pointed out that in places where there are many courts an inebriate can easily be convicted forty-five times altogether in one year, and still be outside the purview of any particular court. The inebriates explain their methods to the inspector.

Another fault is that the expenses appear on the rates, and the attention of the council is thus more particularly called to the fact that they are paying directly for the cost of a worthless person. The cost if incurred by the Government would still, of course, be paid by the public, but in its capacity as a taxpayer. The cost of an inebriate in a reformatory is, by the way, very little more than that of his detention in goal, which cost is likely to go on for ever, whereas that of detention in an inebriates' home may cease with the recovery of the patient.

But the greatest difficulty of all is the evident reluctance of the Bench to break up the home of the inebriate. They believe that, poor as the home may be, unhappy as the wife and children may be, it is still a home of sorts, and to send the breadwinner of the family away for three years means the break-up of the home, possible temptation for the wife, and certainly penury for all, with the gravest uncertainty as to the success or otherwise of the result. This is the real rock on which the Act splits. The effects are so drastic, the results so small, and in Sheffield so unsatisfactory.

The materials for the above remarks have been obtained partly from the law books, partly from my own observation, and partly from the report of His Majesty's Inspector under the Inebriates Acts, 1879-1900 for the year 1906, the last available report at the time of writing. This report is full of detail and worthy to be read a great deal more than it is likely to be. Reference is made therein to every one of the points noticed above. It shows infinite care, and is the report of a good man struggling with adversity and appealing for more and greater opportunities. The numbers he has to deal with are pitifully small; indeed, they ought to be ten times greater, and there is no mention of results. The thanks of the public are fully due to him and his colleagues who are working in this tremendous cause, but before concluding this portion of my paper I should like to quote from one part of the report which seems to be the keynote of the whole of the legislation on this important matter:

Every year's experience adds its quota of evidence in support of the necessity for the existence of special institutions for the control and treatment of inebriates.

Briefly stated, the accumulated evidence points to three definite conclusions:

1. That all habitual drunkards who render themselves liable to be dealt with under the Inebriates Act of 1898 have become temporarily or permanently diseased, and have passed beyond the limit of responsibility.

2. That any attempt to frighten such persons into sobriety by repeated fine or imprisonment has proved utterly useless; and

3. That some more satisfactory method is urgently needed to afford the victim a real chance of recovery, or if recovery is past hope, then to safeguard society from the malicious, criminal, or indecent results of his drunken habits.

4. For these reasons I have not hesitated to recommend as the only possible remedy the substitution of long-continued reformatory treatment for the prison methods which still largely obtain. . . .

Is this the last word that can be said? Have all the resources of medical science been exhausted? Have the medical men of the country really definitely considered the matter? Are the much-advertised "cures" that one sees in the paper nothing but frauds? Is it not just possible that there may be some truth in the statements that 80 per cent. of their cases are cured? Surely the names that appear in their prospectuses give some weight to these assertions? Canon Fleming is not a rogue, and he and his committee testify to the value of the "Keeley System of Cure." There is also the "Turvey System." I know nothing of either, but I do know of another which I am told is similar to the "Keeley Method," and I will as briefly as possible relate what I know.

A young man engaged to a charming girl had from a variety of causes given way to drink. The lady had refused to have anything to do with him until he gave up drinking. He tried times and again and failed. He came to me. He told me he could not resist "that little devil"—pointing to the whisky bottle. He broke open doors at night to get it, and when none was kept in the house he brought it home in little flasks and hid them all over the house—in old boots, behind pictures, etc. He was introduced to this treatment, which consists of the injection of some preparation of gold and numerous doses of medicine. Within one month he was cured. No doubt it may be said that this was a case of faith healing—that the mind was acting on the body. It may be so; but in the middle of the cure the patient suddenly broke out and had a whisky and soda, which made him violently sick. This was a physiological fact, explainable, in the opinion of the ordinary policeman who has not got any faith, only by the fact that either the liquor was doctored or the patient was—and that, too, with something more potent than faith.

The result of this experiment, and one other equally successful, induces me to accept with some confidence the statements contained in Canon Fleming's circular, and I venture to submit for your approval the following suggestion:

That Parliament be petitioned to appoint a Royal Commission to inquire into the efficacy of other methods of treatment for habitual drunkards, and if any of them are found to be of value shall direct that the inebriate on committal may be sentenced to detention as heretofore, and to undergo such treatment, and that the Home Secretary shall have power to release on licence such inebriate at any time on the certificate of the medical officer in charge, with power also to re-arrest immediately the terms of such licence are broken.

The question of the future of the people is of such importance that the closest attention of your great and important body cannot be directed to any better or more worthy object.

#### FURTHER DISCUSSION.

Dr. JAS. STEWART (Clifton), whilst supporting the views of Dr. Clay Shaw, did not agree with him that the term "inebriety" was merely a euphemism for "drunkenness," but thought that a scientific distinction should be drawn between them; drunkenness was a vice, inebriety a disease. This disease was one involving injury, probably of a destructive, irreparable character, of at least one portion of the brain—that is, of the "will centre," whose position, they might hope, would be located ere long.

Dr. WARD COUSINS (Portsmouth) referred to the notable change which had taken place in medical opinion with regard to the effect of alcohol on the body. If they went back to the end of the eighteenth century they found that disease generally was treated by severe antiphlogistic measures, blood letting, tartar emetic and purgatives, etc., and that all stimulants were excluded until the patient

appeared in danger of dying of exhaustion and depression. At that time John Brown, of Edinburgh, instituted a new system of treatment based upon the administration of stimulants and narcotics. Some years afterwards Todd also lauded the value of brandy, and induced many in his day to take the first steps in a fatal inebriety. To-day, however, the general conviction of the profession was that stimulants were only temporary necessities in the treatment of disease. Dr. Ward Cousins then strongly protested against the teaching of the late Dr. Norman Kerr that drunkenness was a disease, but maintained firmly that it was a vice. With regard to treatment, he considered that in early cases the outlook was favourable, but in long-standing cases, where a destruction of the neurons in the superior cortex and atrophic changes in the brain had occurred, the only treatment lay in kind protection and seclusion at home or in public institutions. He had great confidence in the ultimate triumph of universal temperance—not by medical science discovering a specific remedy for all drunkards and dipsomaniacs, but rather by the advance of home sanitation, the diffusion of hygienic principles, and the teaching of national health throughout the country.

Dr. JOSEPH S. BOLTON (Nottingham) said that Dr. Shaw had dealt with the legislative treatment of the inebriate. Unfortunately such treatment was necessary, but it was expensive and the results unsatisfactory—elaborate machinery, all too late. In the early stage the disease was eminently curable. If, however, the disease had already taken a firm hold and the patient could not throw it off, then was the time for treatment by drugs. All persons who had the craving for drink and were willing to be relieved were suitable for this treatment. He had never met with any condition of health which was a contra-indication, and not only those who came voluntarily were fit subjects. Sleep was the first desideratum. For that purpose a dose of apomorphine (hypodermic) sufficed. After that immense doses of potassium bromide, combined with small doses of chloral and other hypnotics. The remainder of Dr. Bolton's remarks were of the nature of a eulogium of the drug treatment of the inebriate.

Dr. T. N. KELYNACK thought it was most desirable that the medical profession should support those who, on the Departmental Committee of the Home Office, were endeavouring to base practical measures on scientific principles. The term "habitual drunkard" should be avoided when speaking of the true inebriate. In the recent legislative enactments of other countries the designation "inebriate" had been adopted. He then quoted the definition adopted by the Society for the Study of Inebriety, and urged the members of the Section present to use the term "inebriety" in such a way as to fall in line with the endeavours of this society and other agencies in their work of instructing public opinion on this matter, and as far as possible to see that legislation should be based on scientific principles.

Dr. JOHN Q. DONALD (Fife) was somewhat sceptical of the good done by the royal commissions. With regard to treatment, his experience had convinced him that the results of drug treatment were not only negative, but positively harmful. Out of 27 cases of narcomania which had come under his hands, 19 had learnt the use of a hypodermic syringe whilst undergoing Keeley and other forms of drug treatment. Compulsory detention of habitual drunkards was an absolute necessity. Their certification, however, was more difficult than in the case of the insane, and therefore something more than the certificates of two medical men was required. Further, the granting of licences to retreats should be centralized.

Dr. BEDFORD PIERCE (York) said that, whilst he had no doubt whatever as to the necessity for additional legislation in order to detain the habitual inebriate, he felt considerable difficulty as to the procedure which should be adopted to attain this end. The medical certificate suggested by Dr. Shaw contained a bare statement that the person was an inebriate or habitual drunkard without apparently any evidence on which that opinion was founded. It seemed extremely doubtful whether any such certificate would be approved by the general public. Moreover, the suggested certificate did not say whether it was founded upon single or repeated examinations. He agreed with Dr. Donald that there was no true parallel between certificates of insanity and of inebriety. In the



one case there were facts which the medical man could observe at the time of the examination, but in the other little or nothing could be elicited at the time if the patient were sober.

REPLY.

Dr. SHAW, in reply, expressed his willingness to substitute the term "inebriate" for "drunkard." He did not believe in any "will-centre," and, whilst agreeing with much that had been said by some of the speakers as to the treatment of early cases and of the prevention of young people becoming drunkards, these were not the class considered by him in his paper. In conclusion, he submitted the following resolution, which was carried unanimously:

That the members of the Section of Psychological Medicine of the British Medical Association are of the opinion that the time has arrived when legislation for the compulsory detention of inebriates in retreats, homes, or other licensed houses is urgently required in the interests both of the patient, of his family, and of society in general.

## DISCUSSION ON HOSPITAL TREATMENT IN INCIPIENT INSANITY.

### OPENING PAPERS.

#### I.—BEDFORD PIERCE, M.D., F.R.C.P.,

Medical Superintendent, The Retreat, York; Lecturer on Mental Diseases, University of Leeds.

In opening this discussion on the early treatment of mental disorders in general hospitals and private practice I must at the outset disclaim any originality in the remarks I venture to offer. The ground I propose to cover is so well worn, that I have not attempted to mention the names of those who have passed along it. If I have digressed a little, I hope you will bear with me. Some may be willing to sympathize with the wayfarer who is now and then tempted to pursue a beckoning bypath.

It will be generally admitted that, under present conditions, it is difficult, if not impossible, to secure effective treatment for early undeveloped cases of mental disorder. Even patients with ample means frequently fail to obtain appropriate treatment, although many more alternatives are open to them than to those coming from the wage-earning classes.

If the wife of an artisan shows signs of breaking down, little or no effective treatment is possible until the patient is bad enough to be sent to an asylum. At present no general hospital will admit such a case, at least not in this country. Convalescent homes are necessarily closed, nursing homes are too costly, and even if the patient is wishful to place herself under care somewhere, little or nothing can be done. Such a patient cannot even be admitted to the county asylum as a voluntary boarder. Thus, in the great majority of instances, the case drifts on until some decided evidence of insanity appears. The next step is to consult the relieving officer; he arranges in most cases for the patient to be removed to the workhouse infirmary, then after a week or two she is certified as insane, haled before a magistrate, and transferred to the county asylum. This institution is generally many miles away from her home and family, and though she may recover, it must be admitted that the procedure is most unsatisfactory, and there is serious delay in rendering effective treatment.

There is reason to fear that any change in procedure which rendered it easier to place patients under care in existing asylums for the insane would fail to meet the need. The objection to asylums turns largely on the special character of the institution and the certification and attendant loss of civil rights that is required before reception. We may deplore the prejudices that exist, and point out that they rest on superstition and ignorance, and yet they greatly influence us in giving advice. Though we may say that mental disorder is but a symptom of bodily disease, we know, and our hearers know, that insanity commonly degrades the person attacked, that the hereditary factor cannot be ignored, and that more than half those who are placed under care remain and become demented.

I cannot but think that our existing methods of dealing with the insane go a long way towards keeping up

prejudices in this matter. A person suffering from mental disorder cannot receive adequate treatment without the sanction of a magistrate, and there is no other disease which requires a justice of the peace to be consulted before remedial measures are taken; the patient forthwith loses civil rights; he is treated in a special institution far removed from his family, under conditions which are determined by questions of safety that concern only a small proportion of his fellow patients, and is subject to regulations widely different from those in a general hospital. The nurses belong to a special class; the medical officers are specialists of only too pronounced a type; the managing committee is not associated with or connected with any other body or organization that deals with the sick; in short, the whole government of the asylum emphasizes the fact that the insane are not as other men, but require special treatment under special regulations.

It is probably impossible to alter the present system—at any rate, as regards confirmed cases of insanity—yet it is evident that it tends to perpetuate existing prejudices and increases the difficulty of placing early incipient cases under treatment in the institutions best equipped for the purpose. Practically speaking, the effect of our present system is that patients are not certified and placed under care until every possible alternative is exhausted. In consequence of this much valuable time is wasted. All of us can remember, in not a few cases, the disastrous consequences of this delay. Even where no obvious untoward incident has occurred, there has sometimes been reason to think that the result of the case would have been widely different had the early treatment been more judicious.

Something must be wrong with a system which virtually deprives patients from securing efficient treatment in the early stages of an illness.

In order to facilitate the early treatment of insanity, it has been urged by some that hospitals of a somewhat different character from those we have at present should be established, whilst others recommend that special wards or pavilions attached to our general hospitals should be available for this purpose. There is much in common in these two proposals, since the special pavilion, if it be large and properly equipped, becomes in effect a special hospital. An important contribution to the subject has been published this year by the State Board of Insanity of Massachusetts in the special report as to the Best Method of Providing for the Insane.

In a small pamphlet of 36 pages is found a full and careful statement of the principles which should guide the State in the discharge of its duties to the mentally afflicted. As regards the City of Boston the State Board recommends the establishment of—

1. A psychopathic hospital of 120 beds within the city.
2. A voluntary and convalescent branch in the suburbs.
3. A custodial and infirmary centre within a 10-cent trolley ride.
4. A farm colony within a 25-cent trolley ride.

The last two proposals are outside the scope of our present discussion. With reference to the first two, I hope I may be permitted to quote freely from the report, as the arguments in favour of the erection of a special hospital could hardly be expressed more clearly.

The hospital should receive all patients for first care, observation, and examination, preliminary to suitable distribution to the custodial and infirmary branch and colony. It should have a reception house and other provision for classification and short treatment of all clinical types of acute and curable insanity. The distinctive characteristic of its residual patients would be probable curability. The hospital should be small, retaining not more than 10 per cent. of the insane of the whole district. It should be the centre of the higher medical and scientific work, with an adequate staff of physicians and ample facilities for research into the nature and causes of insanity. The training school for nurses should here reach its fullest development. The whole régime should be elevated to the plane of the general hospital for acute physical diseases. . . .

A psychopathic hospital, in a large city, should be located near the general hospital and medical school in order that disease of the brain may be associated with affections of other organs, its physicians stimulated by contact with investigators and teachers in other fields, and its facilities for investigation and abundance of clinical and pathological material supplement and complete the assemblage of general laboratories and clinics. They would be trained for the future teachers in mental diseases and physicians in the service of the institutions. In the wards of these hospitals, convenient of access from the general hospitals, students would become as familiar with mental symptoms as they now are with manifestations of physical disease. They would go into practice in the community able to recognize and

interpret the early indications of derangement of the mind at the time when they alone may foresee its possibilities, and, perhaps, forestall its development into confirmed insanity by preventive counsel and curative measures. Such exclusive opportunity is now lost, as a rule, because of lack of such knowledge and training, and because the scanty means of the poor do not allow home treatment and the general hospital for other acute affections shuts its door in the face of the mental patient. Hence there is imperative need of public provision for the treatment of incipient mental disease, especially while the patient and his friends are unconscious of its presence or shrink from the idea of insanity. The present lack precludes preventive treatment and lessens chances of cure.

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In the opinion of committing physicians throughout the State, 21.5 per cent. of their patients who are sent to insane hospitals might be treated under the voluntary relation in general hospitals. It is probable that some 500 insane commitments might be avoided every year in this Commonwealth if adequate provision of this character were available. The expense would be saved, the stigma of insanity escaped, and the right attitude of physician to patient preserved. Furthermore, every hospital, especially in the cities, should be a centre of instruction and counsel in mental hygiene, prevention of insanity, and after-care of discharged patients. The poor of the district should be encouraged to seek its advice and granted free consultation while they may properly remain at home.

It is interesting to note that the needful legislation has been obtained to give effect to this report, and the State Board is authorized to select a site and prepare plans. The hospital will probably be in the heart of Boston, in association with Harvard University and other medical schools and the general hospitals. Things are moving rapidly in this direction in America. Mr. Phipps has given £100,000 for the erection of a similar hospital in Baltimore, another is contemplated in New York, whilst Michigan has been a pioneer in this matter, and its psychopathic hospital was opened in 1906.

It will be necessary to consider next how far the proposals so actively taken up in America are applicable to our conditions here. Some difficulties, moreover, arise if the hospital be a building specially constructed for the treatment of insanity for periods longer than a few weeks.

1. If the usual legal formalities be necessary before patients can be received there is little doubt that the new institution will have few advantages over existing ones. Any prejudices now prevalent would soon attach themselves to it. Call the hospital what you like—psychopathic, psychiatric, or the barbarous term “mental hospital”; the fact that persons are detained therein, certified to be of unsound mind, will militate against applications for treatment for early incipient cases.

2. Another difficulty arises when any hospital for the insane is situated in a populous centre. How will it be possible to provide adequate facilities for open-air treatment and for exercise? The cost of land would necessarily limit the size of the garden, and though rest in bed is good treatment in a large number of recent cases, one must admit that some require outdoor occupations, organized games, manual labour, all of which are by no means easily provided in a city. At present the great bulk of the insane in Great Britain are living in country districts, with ample air space, abundant opportunity for exercise, and there is a danger that in respect to hygienic conditions the proposed hospitals will be less satisfactory than our present asylums. No doubt it is intended to draft patients requiring outdoor occupation to the farm colony or the convalescent sanatorium, but many of the recent cases will be likely to require more space than can be provided in a populous district. Walks abroad would have to be in streets or public parks, and outside the boundaries of the hospital would be little of that restfulness so greatly needed to promote recovery.

3. A third objection arises from the character of the patients admitted. They will be acute and recent cases with a prospect of recovery. Chronic patients will be removed to the custodial or infirmary centre, quiet dements to the colony, convalescents to the suburban sanatorium. It is to be feared that an institution only containing such patients as would remain would have an unduly high proportion of the troublesome, noisy, and depressed suicidal patients, and too small a proportion of those would be able to take an interest in general topics or follow useful occupations. I fancy new cases coming for the first time would find the hospital depressing and the behaviour and appearance of the patients far from encouraging.

4. Lastly, there is the financial difficulty. In the majority of English countries there will be little disposition to build new hospitals for the insane. It will be said with much force that modern up-to-date institutions have been provided. They are for the most part beautifully situated, and their equipment has been of the very best. It is, therefore, not reasonable to ask us to build over again fully-equipped hospitals in our cities and towns.

In fact, the very perfection of our present institutions will prevent any progress on the lines suggested. It must be remembered that connected with many of our asylums there are already detached hospital blocks designed for acute cases and fitted with every possible means of treatment. It is, therefore, probable that only in those districts in which overcrowding of existing institutions renders it necessary to make additional provision for the insane is it likely the local authorities will entertain the question of erecting psychopathic hospitals similar to those now being established in America.

The next point to consider is whether it is not possible to meet the difficulty by the establishment of special departments in our hospitals and infirmaries. One of the greatest obstacles to progress in England is lack of co-operation between the various persons concerned. This is eminently the case in respect to the institutions dealing with sickness.

We have a large number of entirely independent general hospitals, supported by voluntary contributions, managed by a committee, and responsible to no one but their subscribers; next we have Poor-law infirmaries doing the same work in certain departments, managed by the guardians and supervised by the Local Government Board. Lastly, we have the county and borough asylums, managed by a committee of the local council under the supervision of the Lunacy Commission. These are quite independent, and there is much waste of effort by overlapping. The Poor-law infirmary stands in an intermediate position, and in relation to insanity it forms a sort of receiving house. Although insane persons are received and detained in almost all workhouse infirmaries, they are not “institutions for the reception of lunatics,” and so the patients cannot be transferred to the asylum without a medical certificate and magistrate's order.

In certain districts the power to detain patients for a limited time has resulted in the formation of special wards in which large numbers of patients are annually treated until recovery without certification. This seems to suggest a solution of our problem, at least as regards the legal position of persons suffering from undeveloped insanity. If it be found that a great many cases of mental disorder can be dealt with satisfactorily in workhouse infirmaries, it is evident that under more favourable conditions there is no doubt as to the possibility of success. But it seems altogether wrong in principle that persons suffering from mental affections should have in the first place to apply to the relieving officer and become paupers in order to receive treatment. In my opinion the treatment of acute disease is not the proper function of the guardians of the poor, and it has only become so through the inability of the general hospitals to meet the demands upon them. This, however, opens up a very wide question, and it is to be hoped that the Poor-law Royal Commission will be able to suggest a remedy for this as well as for the many other inconsistencies arising out of our present system of Poor-law relief.

The treatment of the insane in general hospitals rather than in special psychopathic hospitals or in Poor-law infirmaries has been strongly urged by many authorities, in particular by Dr. Clouston of Edinburgh. This is in a sense a return to the lunatic wards of long ago, which were justly condemned and abandoned in favour of our present asylum system. There is, however, no danger of a repetition of the abuses of the past, and much can be said in support of the proposal to open special wards or pavilions for the treatment of the insane as an integral part of the general hospital.

In the first place, it emphasizes the fact that insanity is a disease—that the insane are patients needing medical treatment. After a time one can hope that it will be as natural for patients afflicted in mind to seek advice at the hospital as if they were suffering from any bodily ailment. Cases will, in consequence, come under treatment much



earlier than at present, many accidents and calamitous results of insanity will be averted, and there is good reason to think that in some cases timely advice and treatment will result in recovery without the necessity for certificates or legal care.

Secondly, this tends in the direction of bringing the practice of psychiatry more into line with general medicine, and will in university centres greatly assist medical education.

Thirdly, if this proposal be generally adopted, it would largely increase the number of places available for the relief of persons with mental trouble. No patient, excepting those from remote country districts, would in the first place be sent far from home and family. Patients could, when it was desirable, be readily visited by the relatives without great expenditure of time and money. Although the stay in the hospital may only be a temporary expedient in many cases, and in a short time transfer to an asylum may be needful, it will nevertheless have been of great value. It would lessen the blow that enforced removal from home and family often involves. Afterwards patient and friends alike would be glad that the formal certification was only undertaken after a period of observation and treatment in a general hospital.

It must be at once conceded that this proposal is beset with difficulties. Many hospital authorities would say that the funds at their disposal are insufficient to meet existing needs, and that it is altogether impossible to entertain the establishment of any new department. In some localities geographical considerations may prevent any extension of premises.

I do not, however, think that the financial difficulty should prevent the subject receiving careful consideration. It is one of great importance, and if a case be made out it is possible that in England as well as in America private benevolence may enable something to be done. Even if this fails, it is not impossible under a new system of Poor-law administration that hospitals which receive such patients might obtain help from the rates without losing their individuality and independence.

Another difficulty lies in the fact that there are many cases of insanity which will require a far longer period of treatment than can possibly be given in any hospital ward, if they are to be successfully treated to recovery. The causes which lead to a mental breakdown have usually been long in operation, and one cannot expect a speedy recovery. This is certainly true of many cases, but there is no doubt that a very considerable number of recent cases will be so decidedly benefited by a stay of three or four weeks, that they may be discharged as convalescent. The results obtained in Poor-law infirmaries abundantly prove this statement. No doubt a prolonged stay in a hospital ward would be undesirable in most cases, and unrecovered patients would have to be removed to the asylum; yet in a few there might be great advantage in extending the period of residence beyond the time named.

Next, the resources of the hospital as to treatment would be limited. Facilities for amusement, recreation, and occupation would be out of place in a general hospital, even if it were possible to provide them. This would greatly restrict the treatment available, and practically speaking those patients would do best who would be benefited by rest in bed.

But a course of treatment in bed is of great service in the great majority of recent cases, and some authorities prescribe it as a matter of routine. In bed the bodily condition is more easily examined, and remedies are more easily administered. Patients in bed are under more effective observation than if up and about, and at the outset of a case this is an important matter. Although certain patients may not derive much benefit by the "bed treatment" and will do better under the "leg treatment," as it has been called, it is not likely that a short stay in bed will be injurious to such as these.

Special lines of treatment, such as packs, baths, massage, intestinal lavage, restrictions in diet, are all of them as easily secured in a general hospital as in an asylum.

There are, however, more serious limitations in that the resources of a hospital in dealing with noisy, destructive, and violent patients will be small. It is only too likely that such patients would be subjected to mechanical restraint of some kind or other. Restraint is frequently used in a general hospital in a way that would not be per-

mitted in any asylum. Whilst to some extent temporary needs justify extreme measures, this difficulty must be satisfactorily safeguarded. Seeing that the patient is not restrained when he reaches the asylum he must not be restrained in the hospital. It is largely a matter of nursing and management. But in any event there will be a small number of cases which cannot remain long in the hospital ward, for example, the indecent and obscene as well as the impulsive and dangerous, and such will have to be transferred without delay. Yet it is my conviction that the number of these will be very small when the arrangements for treatment are completed.

To sum up the advantages from a patient's point of view:

1. They would be able to obtain treatment earlier, and in some cases this would prevent the development of serious illness.
2. Many would escape certification and attendant civil disability.
3. In the case of those who failed to recover, certification would be delayed and the possibility of mistake could be excluded.
4. There would be less separation from home and family.

There are, moreover, signs that it will not be long before we shall be able to distinguish between our cases with greater accuracy than at present. As it is, an experienced man can pick out with considerable certainty the cases likely to recover soon. In the near future the selection of cases for temporary care in a general hospital will be very less difficult than at present.

Some reference is required to the effect of this proposal upon medical education. At present in this country the training of medical students in psychiatry is inadequate. The general practitioner has a special duty to the public in relation to insanity. I allude to the signing of certificates of lunacy, and only too frequently he is not properly qualified to discharge it. Moreover, he alone sees the early cases, and is called upon at the outset to give advice.

During the course of training required in almost all our medical schools the student hardly ever sees a recent undeveloped case of mental disorder. Such patients as he sees are already under asylum care, and he has little or no opportunity of coming into personal contact with insane patients.

The proposal to form special pavilions for nervous and mental cases, or a psychopathic hospital attached to the general hospital, will go a long way to remedy this deficiency in medical education; and if there also be established an out-patient department, this will provide an opportunity of examining early unconfirmed cases and of studying the perplexing problems that arise when one approaches the borderland between sanity and insanity. It is, moreover, clearly undesirable that the study of mental diseases and the researches into their pathology and etiology should only take place in asylums situated as a rule in remote districts far from medical schools and a scientific atmosphere. It would be well for the students as well as for those engaged in original research if the laboratories were a part of a university. There can be little doubt that the establishment of special hospitals in our cities with attendant departments for clinical research would result in a more rapid increase of knowledge in psychological medicine.

We must regretfully admit that our system of teaching psychiatry compares unfavourably with that in most European countries; yet there is good reason to think that, in London at any rate, a great step forward will shortly be made owing to Dr. Maudsley's generous donation to the London County Council.

Time will not permit much being said as to the treatment of early cases of insanity in private practice or among the wealthier classes. Cases constantly arise for which existing methods of treatment, such as a nursing home, or in a doctor's family, or hydropathic institution, are found to be unsatisfactory. Some of these might be sent to the special hospital pavilion as paying patients, but I think in many cases treatment under conditions of greater privacy will be required.

I therefore suggest that existing institutions for the insane should erect special departments or set apart detached houses for the reception of incipient cases. I see

no reason why the special facilities existing in Scotland for the temporary care of incipient cases should not extend to the care of patients in such detached houses. If a private individual can be trusted to detain such a patient for six months, so can a recognized hospital for the insane, which is subject to regular inspection by Commissioners and other authorized visitors.

Such a temporary means of detention is urgently needed. I have in mind a public man, who became suddenly confused, depressed, and suicidal, and came to the Retreat under certificates, as I was unable to receive him as a voluntary boarder. In a month this gentleman recovered, and then found that, owing to the certificates, he had lost his position as managing director of a private company. Can we be surprised that prejudices exist against the present system if the incidence of temporary medical disorder produces such results? Can we wonder that pressure is put upon physicians to evade the law and consent to illegal uncertified care?

There is no doubt in my mind that the alteration in the law to meet this difficulty would be a simple matter, and though it would undoubtedly mean an increased area requiring supervision by the lunacy authorities, it would certainly prevent the abuses so widespread at the present time, and at the same time render it much easier to place undeveloped cases of insanity under effective treatment without serious loss of time.

#### CONCLUSIONS.

1. Under our present system early and undeveloped cases of mental disorder, especially those derived from the poorer classes, are unable to obtain proper advice or treatment.

2. That it is most desirable to provide in or attached to our general hospitals special wards or pavilions for such cases, and that out-patient departments be established in connexion with them.

3. In university centres and other large towns the hospital ward might become a special psychopathic hospital, provided it was affiliated or closely connected with the general hospital and medical school.

4. At present the opportunities afforded to students for obtaining clinical instruction in mental disease are inadequate, and the above proposals would greatly assist in remedying this deficiency.

5. The present system of dealing with the insane tends to separate psychiatry from general medicine, which is hurtful to both the general public and the medical profession. It certainly has the effect of rendering it less easy to effectively treat early and incipient cases.

6. Poor-law infirmaries attached to workhouses are unsuitable places for the treatment of acute disease, whether of body or mind.

7. It is desirable to devise some means of co-ordinating the functions of hospital, infirmary, and asylum to avoid overlapping.

8. The facilities for treating patients from the wealthier classes are also inadequate. It is suggested that existing institutions should be encouraged to open special departments for the reception of early incipient cases, and that the present vexatious legal requirements be suspended for a limited time in the case of such patients.

9. An alteration in the law is urgently needed, so that early undeveloped cases may be placed under treatment without lunacy certificates or loss of civil rights for a limited time, say not exceeding six months. A medical statement setting forth the facts and a notification to the Commissioners in Lunacy should be required in every such case, and possibly a right of appeal to a magistrate might be considered desirable as a safeguard against wrongful detention.

10. It is further suggested that this principle of notification should be applicable to all cases so detained, whether it be in asylum, hospital, or private house, provided always that such house or institution should be open to inspection by the Lunacy Commissioners.

It is believed that a change in the law on the lines indicated would render it much easier to secure effective treatment of mental disorders in their early stages, and it would at the same time tend to prevent much of the illegal detention and irregular practices so prevalent at the present time.

II.—GILBERT E. MOULD, M.R.C.S., L.R.C.P.Lond.

#### A MENTAL OUT-PATIENT DEPARTMENT.

At the Royal Hospital, Sheffield, there has been in existence for the past seven years a mental out-patient department. As such a department is apparently not common in connexion with a general hospital, perhaps a short account of its experiences may be of interest to the Section. Previous to this one of the general physicians to the hospital was Dr. Crockley Clapham, who was practising privately as an alienist. This naturally led to his colleagues sending to his medical out-patient department all out-patients manifesting mental symptoms, and also to their calling him in consultation to any cases with such symptoms in the wards. On his retirement it was determined, largely owing to his belief that the treatment of insanity should be associated with the general hospitals, to appoint a physician for mental diseases to the hospital, who should have charge of an out-patient department, but should not be entitled to any beds in the wards, except for such cases already in the wards as should be from time to time assigned to him by the courtesy of his colleagues, or for certain of his out-patients admitted on the same terms.

Now, it is manifest from the very nature of these cases that the number of them suitable for out-patient treatment must be limited, and accordingly we find that one morning in the week, and a time of from one and a half to two hours, is sufficient for their treatment. Also as might be supposed the great majority of the patients are as uninteresting to the alienist as are to the keen house-physician the shoals of gastric ulcers that arrive with in-patient letters of recommendation. That is to say, there is a large proportion of cases of neurasthenia, hypochondriasis, and hysteria, with perhaps an equal number of chronic epileptics. In fact, any case that has been making a nuisance of itself to the other departments, is very likely to be relegated to this. Even a new case of ringworm of the scalp on one occasion managed to evade the vigilance of the porter and sister, and present itself as having something the matter with its head.

Nevertheless there has been an encouraging number of the cases for whom the greatest hopes may be entertained from a mental out-patient department—namely, cases of incipient insanity. The greater number of these have been cases of melancholia; but there have been also cases of mania, both on first attack and recurrent, adolescent and climacteric insanity, general paralysis (perhaps not so numerous as might be expected), dementia due to syphilis, lead, alcohol, and senility, and delusional cases.

There have been also a large number of congenitally deficient children, from the very earliest ages brought for an opinion as to their future prospects in life. There must also be added rarer cases such as those due to myxoedema, chorea, Graves's disease, etc.

As may be imagined, most of the relatives are anxious to keep the patients out of the asylum as long as possible; and this brings forward the question of the responsibility attached to one in charge of a mental out-patient department in continuing to treat cases therein. May it not be granted that if the relatives are told that a patient ought to be in an asylum, and that they ought to see the relieving officer with that object, and if this advice is recorded in the case-book, then the responsibility rests with the relatives, and the physician may continue to treat the patient as an out-patient? It is evident that he is, at all events, under some supervision, and advice refused one week may be taken another. Cases with suicidal or dangerous tendencies, of course, give rise to the most anxiety from this point of view. It may sometimes be the duty of the physician to take the initiative in communicating with the relieving officer. Yet this has only once been necessary in the history of this department, and then the patient, one with delusions of persecution, had found his way to the private dwelling of his medical adviser, and expressed his intention of doing for his enemies, among whom, perhaps, he might have included the former.

Most cases of incipient insanity have to be sent eventually to an asylum, for, with regard to the poor, we have no doctor anxious for a resident patient, no lady with large asylum experience, no sea voyage prescribable—if any one still prescribes a sea voyage. Yet some suitable cases have been taken into the wards, by the kindness of the



in-patient physicians, under the charge of the alienist, and have made a recovery after a few weeks' rest in bed, or have, at all events, postponed their transfer to an asylum. And here may we not see the growing embryo of the mental ward of a general hospital, the idea of which is so much in the air at present? The small isolation ward is already in existence where lies the casual lunatic waiting his transfer to an asylum, too often bound to the bed, as though he were the man who on the stage defies all comers to bind him so that he cannot escape. How easily might this be enlarged and transformed into the small ward of three to six beds, which would, we believe, soon justify its existence.

Would it not pay local authorities to contribute to out-patient departments and to hospital mental wards? There can be no doubt that they would save the maintenance of many patients in the asylums. Some cases may be treated throughout to recovery or chronicity as out-patients. For instance, a case of acute hallucinatory insanity in an adolescent made a complete recovery in a month. With the exception of hypnotics and aperients, we are accustomed to think little about drugs in an asylum, and yet we cannot read any article about any particular form of insanity without seeing that under treatment it ends up with "Such and such, strychnine, phosphorus, bromide, etc., may be found useful." The out-patient department is *par excellence* the field for experiment with drugs, and as a man never knows what he can do with his toes until he has been deprived of his fingers, so it is astonishing what one can do when one has nothing else but drugs and suggestion to work with. The belief of the lower classes in drugs is marvellous, nor less efficacious should be found the personal influence of an educated man exercised at the weekly visit.

Is it not the case that in the practice of our branch of the profession talk comes more into play than in any other? The patient talks, unless he is of the sort that will not talk at all; the doctor talks, and the relatives talk more than either. At the general out-patient department there is neither time nor appreciation for the amount of talk required for a mental case. Epileptics are obviously amenable to drug treatment, also cases consequent on myxoedema, lead, syphilis, chorea, Graves's disease, etc.

There are many epileptics who can continue their bromide treatment for years with variations according to their weekly requirements, receiving advice as to their habits of life, diet, and sleeping. Anaemia and constipation can be treated in all cases, and hypnotics may be entrusted to judicious relatives. Every known analgesic may be tried for every possible form of headache, of which the neurasthenics will supply a plentiful crop. There will evidently be a great call for tonic treatment. It will be found that a few of the commoner hospital mixtures will suffice for the majority of the patients—iron and magnesium sulphate, iron and quinine, gentian and soda, bromide mixtures, Easton's syrup, acid mixture of nux vomica, arsenic, and calumba.

Obviously discrimination will have to be exercised in handing poisonous medicines to some of these patients. But behind all these there lies the knowledge that the whole *British Pharmacopoeia* is available in the well-equipped hospital dispensary. There are at hand, too, all the other special departments for the correction of defects in eyes, ears, teeth, etc.; the physicians can be consulted for possible disease of heart, lungs, or kidneys; the neurologist should be available for his proper quest, the determination of the presence of organic disease of the nervous system, and the surgeons may be relied on, if any relief may be given by operation. In return for this the alienist at the hospital will be called in to all cases showing signs of insanity in the wards; and at this hospital this has been done with cases of mania after abortion, chorea, and lead poisoning, of general paralysis, delirium tremens, hysteria, and malingering. In hospitals with lying-in wards, puerperal mania would be included, and post-operative cases might be expected.

Advice has been asked as to the probability of speedy recovery, if the patient remained in the hospital, or whether an immediate transfer to the asylum would be preferable. Cases of this nature have been handed over to the care of the alienist in the wards. Advice has been asked by the surgeons as to the probability of benefit from operation in cases with hypochondriacal delusions,

with hydrocephalus, and with Jacksonian epilepsy. Lead poisoning, as might be expected in Sheffield, where many of the trades, such as file cutting, involve working in this metal, has been found to be a frequent cause of insanity. It has been noted in, and believed to have been the chief or an important factor in the causation of, cases of general paralysis, epilepsy, mania, melancholia, dementia, and delusional insanity. Most of these cases due to chronic lead poisoning are depressed and suspicious with a fear of impending evil, and they often are troubled with noises in the head and may have auditory hallucinations. There have been also in the Sheffield district cases of mania and stupor in women due to acute lead poisoning produced by taking diachylon as an abortifacient. As might be expected, these latter recover under medicinal treatment. The after-care treatment of the discharged insane has been felt to be a matter of the highest importance; and nothing could be more suitable, considering that many cases have admittedly to be discharged from the asylum too soon on account of the pressure of more urgent claims, than that they should be advised to attend an out-patient department for a considerable period, where they would be under some supervision and advised as to their habits and mode of life.

Such, then, have been the experiences of this department. Perhaps it may be claimed that they form a justification for other hospitals to found a similar one.

#### DISCUSSION.

Dr. MAURICE CRAIG (London) said that physicians working in psychological medicine were not uncommonly accused of doing nothing for their patients except making them as happy as possible. To a certain extent this was true, the reason being that it was impossible to treat many of these early cases in the recoverable stage. Pauper-patients could not be treated until the disorder was so marked that it was obvious even to a layman, to wit, a justice of the peace. He was strongly in favour of special wards in general hospitals, as by that means only could the public learn to view mental disease in the same light that they did disease in general. At present, slowly emerging through the superstitious era of public conceptions of insanity, we should make more rapid progress if the Legislature and official supervisors discontinued the use of such obsolete terms as "lunatic" and the like. Similarly, the term "alienist" was open to objection.

Dr. OSWALD (Glasgow) thought that cases of incipient insanity were best treated, not in special pavilions, but in the ordinary wards of general hospitals. He believed that special pavilions would only intensify the too prevalent belief that there was a real difference between mental and ordinary physical disease. He suggested that the restrictions to persons undergoing voluntary treatment should be removed in the case of pauper patients.

Dr. R. H. COLE (London) was in favour of special wards in general hospitals rather than in special psychopathic institutions which were now being advocated in some quarters. He was also agreed that the admission of voluntary boarders to county asylums would be a step in advance.

#### DISCUSSION ON SCHOOL LIFE FROM THE POINT OF VIEW OF PSYCHOLOGICAL MEDICINE.

##### OPENING PAPERS.

I.—FRANCIS WARNER, M.D.Lond., F.R.C.P., F.R.C.S.Eng.,

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SCHOOL life has many aspects, each with a special interest for students and practitioners of psychological medicine, and presents many problems that need scientific solution. The school is a valuable field of clinical material where we may observe the children and study their physical constitution and mental hygienic surroundings.

To those engaged in medical practice it may appear that the hygiene of school life falls more directly under the supervision of the medical officer of health, and that the duty of observing the individual children and their mental development is restricted to the school medical officer, but for the work of each the scientific observation

of individual children is necessary. Let us consider first the children to be educated, and then pass on to the groups in which they may be classified, and their progress during school life.

In studying school children it is essential to notice certain marked differences between the boys and the girls. Let me point out some of the characteristic differences in the constitutional make, brain status, and life history of boys and girls. Evidence collected as to children seen individually in school has shown that well-developed girls are not more delicate than boys, and may work hard and play vigorously with advantage.<sup>1</sup> But girls with any degree of physical developmental defect or brain disorderliness (nerve signs) are more apt to acquire harm and less good from their environment than boys of similar constitution. It is satisfactory, however, to point out that there is more normality among the girls. Fewer girls than boys are of subnormal development; this is particularly the case in the younger age groups.<sup>2</sup> It is the children with some degree of subnormal development who form the pathological portion of the school, and present social and educational problems of importance in psychological medicine.

Mental ability is indicated by certain objective signs in the children. Brain action may be studied by observing certain "nerve signs" (normal or subnormal), while the physiognomy and growth of the child afford evidence of his constitutional development. In thus observing the school child it is convenient to fix the pupil's eyes by getting him to look at an object held before him. We may then study the physiognomy from different points of view, noting especially each separate feature, any overaction in muscles of the forehead and about the mouth, as well as the tone of the orbicularis palpebrarum and the general facial expression. Next, by moving the object upon which the eyes are fixed, we may note the eye movements, which are very important. When the child holds out his hands in front, their balance and any movements of the fingers are noteworthy, while the pupil's imitation of the observer's hand movements gives useful indication of the capacity for co-ordination and control. Symmetry of balance of the head, spine, shoulders, and feet are helpful as indicative of brain status. Response in action, and co-ordinated movements guided by sensory impressions, afford important trustworthy evidence as to the health of a brain. In a young child the power to control action accurately is a better mental test than the shortness of the time occupied by the action. I find it useful to test response, both in imitation of my finger movements at sight, and by making passive movements of one hand of the child while his brain moves the other hand, the eyes being closed.

It is useful to recognize certain types of childhood and study their average progress during school age. Let us describe a few groups:

Healthy normal children form the largest proportion, but they vary much in character and in mental potentialities. At 7 years of age the head circumference should be 20 in. to 21 in.; the palpebral fissures of sufficient size and horizontal, the nose, especially in its bony portion, well grown, the ears complete in all parts and not outstanding. Examination of the mouth should show it to be of due size, the teeth sound, and the palate well arched and of good breadth. In other respects we look for signs of good health. On examination of the nervous system, the general balance of body should be erect and symmetrical, facial expression bright, with no fullness under the eyes. The hands when held out in front should balance straight, on a level with the shoulder, and the elbows straight.

*Children with Defective Physical Development*, such as stunted growth, head small or ill-shaped, narrow palate, small mouth and eye openings, nose too small, with insufficient growth of the nasal bones, ears without an antihelix, etc. These cases are more frequent among boys. As a class they tend to ill health, brain disorderliness, and mental dullness unless well trained. This is especially true of the girls, who readily degenerate towards anaemia and hysteria.

*Children of Nervous Temperament* are commonly well grown and of good development, but are sometimes of

subnormal weight. With a lively disposition and bright expression, the child is usually over-mobile, with wandering eyes, twitching fingers, and the general balance of the body erect, but asymmetrical. The hands, when held out, are often unequally balanced; usually the left is lower, with a drooping of the shoulder, and the fingers and thumb over-extended at the knuckles. Such children are often bad sleepers, tooth grinders, nail biters; they have a variable appetite, and are liable to headaches. They are quick pupils, but fidgety and forgetful. In habits they are gregarious, and often fond of both work and play.

*Children showing Brain Disorderliness and Defective Nerve Power.*—The physiognomical development usually shows defects such as small head and other defects of cranium, palate, nose, ears, and facial apertures. There are also "nerve signs," such as lack of facial expression, the forehead puckered with both horizontal and vertical creases, eye movements slow and inaccurate. Response is usually difficult to obtain, and there is but little capacity for control and co-ordinated action. The gait is slouching and the movements slow, often accompanied by uniformly repeated actions such as protrusion of the tongue or grinning and frowning.

*Children Mentally Feeble.*—These are distinguished by the defects of all the other classes, and are the dull and backward pupils in the school, with many physical defects and nerve signs indicating brain disorderliness, often accompanied by a subnormal weight, or an adipose tendency, or some other result of defective nutritive processes.

It is the boy in whom we see many indications of low physical development, together with imperfect power of co-ordinated action as controlled through the senses, that is likely to pass through the ordinary school curriculum without benefit.<sup>3</sup> But there are other children who are stupid, dull, and lazy in school work, but who have a fair physical development and good power of co-ordination; they are active in play, and quite able to do mechanical work. For such children to be classed as "mentally deficient," and sent to the special school, is only too likely to prove disastrous to their moral, mental, and physical well-being. The importance of carefully diagnosing all children classed as "mentally deficient" cannot be over-estimated.

That part of school life which deals with the training and teaching of children also falls within the scope of psychological medicine, especially when the condition of the pupil is in any particular disordered or sub-normal.

When trying to cultivate the power of attention in a child of weak mental capacity I usually begin by controlling eye movements and fixation of sight upon an object presented. It is important to practise the pupil in receiving each class of sensory impression separately—for example, by sight only; by hearing only; by touch (cutaneous sensibility); by muscular sense, as in feeling a succession of graduated weights; or by the sense of movement, as in feeling size and length without weight. Then in subsequent teaching he may be led to compare one visual impression with another, one impression of sound with another of the same kind, and so, too, with the impressions of length, weight, etc. Thus, he becomes trained in methods of observation which aid mental growth and give the experience necessary for the appreciation of the value of coins, the length of lines, and the proper use of figures.<sup>4</sup> The child may thus be trained to learn the value of money and the comparison of the sides and angles of a geometrical figure.

Thus, in studying school life we have to observe the children individually, then classify them, and note the effects of the mental and hygienic environment upon their progress, while suggesting any modifications necessary in the case of individuals. I have spoken about training, but said nothing about the school staff; still, the teachers are worthy of study as well as the children. A teacher who is tired, languid, or nervous is often irritated by the pupils. The reflex of this on the children is obvious, and disastrous in but too many cases.

<sup>1</sup> Report on Examination of Children in Schools, 1895. Published at Parkes Museum.

<sup>2</sup> The Study of Children, ch. xiii. Macmillan and Co.

<sup>3</sup> The Study of Children. See cases, ch. ix.

<sup>4</sup> The Nervous System of the Child, ch. vii and viii. Macmillan and Co.



## II.—RALPH P. WILLIAMS, M.D., D.P.H.,

Professor of Public Health, Sheffield University; Medical Inspector of Elementary Schools and Assistant Medical Officer of Health to the City of Sheffield.

## FEEBLE-MINDED CHILDREN.

THE Sheffield Education Committee have established three special schools under the Defective and Epileptic Children's Act, 1901. The examination of 147 feeble-minded children now in attendance at these three centres has been carried out, much on the lines suggested by the recent memorandum of the Board of Education, and I venture to put before you some of the results I have obtained.

*Family History.*

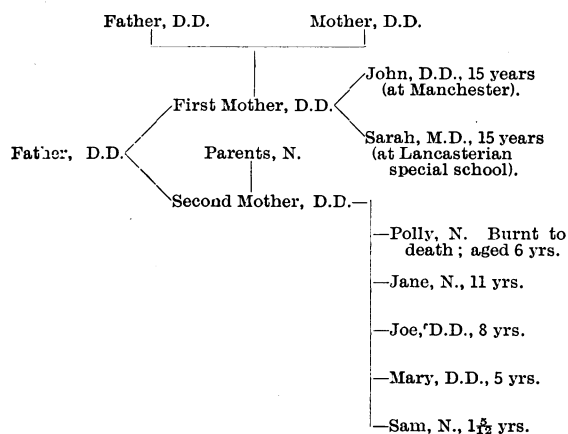
As a predisposing cause of feeble-mindedness, heredity has always occupied a most prominent position, and under this heading I have investigated the family history of 297 children recorded in the medical officer's notebooks at the schools.

In 103 cases the family history showed nothing of importance.

I have tabulated those in which a predisposing cause was found (that is, 194 cases). (See Table I.)

In many cases the family history is very bad. It was, for example, in the following :

CASE A.—(D.D.=Deaf and dumb. M.D.=Mentally deficient. N=Normal.)



The cost to the public of educating John, Sarah, Joe, and Mary (provided the father pays £2 a year towards the deaf-and-dumb ones) will be over £850, whilst if the children were normal it would amount to £90.

CASE B.—Mother deaf and dumb; her brother is insane. Father's sister is insane. Child's brother is feeble-minded.

CASE C.—Father's sister and mother's brother insane; child's brother feeble-minded.

CASE D.—Mother alcoholic; brother phthisical; uncle committed suicide.

In an investigation of this kind one is also much impressed by the great infantile mortality which occurs in many of the families. For example :

CASE E.—Eleven children, six of which died under 3 years of age.

CASE F.—Eighteen children; eleven died under the age of 14 months.

CASE G.—Twelve children; nine died in infancy.

N.B.—No symptoms of syphilis were found in these cases.

Such histories as these point to feeble-mindedness on the part of the mother, and also suggest that neglect in the early months of life has contributed in all probability to the mental deficiency of the child itself.

*Physical Condition.*

*Height and Weight.*—The children were weighed in their ordinary clothes without boots; the height was also registered without boots. The figures show the following characteristics :

(a) The physique of the feeble-minded children is found to be considerably below the average; this is accentuated by the fact that a very great percentage of the children are drawn from very poor districts of the city.

(b) In certain children the physical development was excessive.

(c) Small number of children examined.

*Nutrition.*—In noting nutrition, they were classed under the headings "good," "medium," or "bad," and the figures came out as 42, 56, and 49 respectively. In making this estimation, the weight, the presence or absence of anaemia, and the condition of hair and skin were chiefly relied on.

*Cleanliness.*—The absolute necessity of school nursing was most apparent, only one of the girls having clean hair, 63 being found with nits and 10 with lice. The boys, with short hair, compared most favourably, 46 heads being clean.

Eight children had verminous clothing. The homes of all the worst cases were visited by the lady health visitors, and temporary improvement has taken place.

One of the three schools has a bath which is regularly used with the greatest benefit.

Parental neglect is unfortunately often found in connexion with the feeble-minded. I believe in Sheffield the Health Department acts in rather an unusual way; we put in force Section 10 of the Prevention of Cruelty to Children Act, 1904. The following case is an example :

Two boys—J., aged 14, and C., aged 10—are brothers attending a special school. They have been continually reported by me during the last year for neglect and verminous clothing. The chief lady sanitary inspector has had them under observation for years, and the mother has been repeatedly warned. Six weeks ago I visited the home (which was found to be filthy), and I personally warned the mother. On July 18th, in consequence of a subsequent report, I again visited the house; the room where four children slept had a broken-down bed in it; a feather bed with half the feathers out, saturated with urine and excrement, was scattered about the floor. I at once applied to the magistrates for a warrant for the removal of the children "to a place of safety," and they were removed that day to the cottage homes. The mother was prosecuted, and received six months' imprisonment. She is a chronic drunkard, totally incapable of looking after children. No bodily harm had been done the children, and they were fairly nourished, but they were kept in such a way as was likely to cause detriment to their health. The necessity for prompt action in such cases is most urgent.

*Head Circumference.*

The maximum circumference of the head has been measured in all the children. This measurement for boys is stated by Holt to be 45.9 cm. (18 in.) at the age of 1 year, and 53.5 cm. (21 in.) at the age of 10 years (the girls being about 1 cm. less at each age than the boys).

In the feeble-minded the highest average I obtained with the boys was at the age of 15, when it was 51.6 cm. With the girls the highest average for any age was 51 cm. at 13 years.

Head circumference is not looked upon as of any anthropometric significance, yet, excluding cases of hydrocephalus, there appears to be some relationship between mental ability and this measurement, as the following result suggests: In examining 226 infants, aged from 4 to 6 years, in a good district elementary school, I measured all the head circumferences. The head mistress was asked by me to put down her estimate of each child's mental capacity under five headings—namely, Excellent, Good, Fair, Poor, and Dull (see Table II).

*Palate.*

A high and narrow palate has always been associated with feeble-mindedness. Figures I have obtained bear out this relationship :

Normal infants: Number examined, 433; high and narrow palates, 11 per cent.

Feeble-minded children: Number examined, 147; high and narrow palates, 32 per cent.

*Teeth.*

Three children out of 147 had a perfect set of teeth, and 82 had more than four carious. In no case was any trace of stopping observed. I hope in the near future that dental treatment will be provided, for, as Dr. Kerr states, in the treatment of this class of child there is no question

of competition between private dentist and municipal officer.

*Heart and Lungs.*

A careful examination of heart and lungs has been made in 102 cases.

- In 1 double mitral disease was found.
- In 7 the heart's action was weak.
- In 2 there was prolonged first sound.
- In 2 the apex beat was diffuse.
- No lung disease was found.

*Feeding.*

This is necessary as the children come some distance. Midday meals are provided at two of the centres, and at the third the children bring their own food, which is warmed up. The charges are 1½d. or 1d., the poorest having free meals. The arrangement is practically self-supporting.

*Curriculum.*

Great stress is laid on manual training—carpentry, gardening, cobbling, and housewifery being taught in addition to mat-making, clay modelling, etc.

Speech training is also made a special feature, it being found on giving a test sentence to all the children that the speech of only thirty-six was passable.

The value of money is taught with real coins, so that not only size and colour but also weight may be observed. The teaching of arithmetic is practically limited to playing at shop (weighing, measuring, and paying).

In order to ascertain the results obtained by the schools, the homes of sixty-one children who had left school were visited by two of the lady sanitary inspectors, Mrs. Franks and Miss Mary Sutton. The results were as follows :

- 16 at home, engaged in housework.
- 17 at home, doing nothing.
- 3 in domestic service.
- 7 errand boys.
- 7 various trades.
- 9 in asylum or workhouse.
- 2 dead.

One boy, aged 15, is earning 10s. per week by selling papers. Others less fortunate are selling pot-mould for rags and bones, hawking sticks, selling matches, etc.

With regard to the whole system, the necessity for permanent after-care is most urgent. When assistant medical officer at Broadmoor Asylum, I came in contact with many feeble-minded men who had committed crimes of violence, and on referring to Dr. Brayn's last report it is seen that out of 780 patients in the asylum, 112 were congenitally mentally deficient, and of these 46 had committed murder, 16 attempted murder, and 7 manslaughter. There is no doubt that the feeble-minded girl drifts with the greatest ease into a career of vice, and, if she does marry, becomes a neglectful mother. Dr. Bonnhoffer of Breslau finds that 30 to 40 per cent. of the beggars, loafers, and prostitutes of Breslau consist of mental defectives, whilst the number of mentally-defective persons compared with the population was only 1 per cent.

It is hoped that the report of the Royal Commission, when it comes out, will be followed speedily by strong legislative action, which will result in the feeble-minded being placed in colonies, the sexes being kept separate, where they will be kindly treated, provided with suitable manual occupation, and kept away from intercourse with the rest of the community during the whole of their lives.

TABLE I.—Showing the Family History of certain Feeble-minded Children.

	Alcohol.	Epilepsy.	Tuber- culosis.	Insanity.	Weak- minded- ness.
Father ... ..	30	3	16	3	2
Mother ... ..	24	6	13	1	4
Brothers ... ..	—	3	25	2	5
Sisters ... ..	—	—	17	2	3
Aunts and uncles ...	2	12	35	13	2
Grandparents ... ..	2	2	17	7	—
Totals ... ..	58	26	123	28	16

TABLE II.—Showing the Head Circumference (measured in Centimetres) of certain Children.

Mental Capacity.	Dull.	Poor.	Fair.	Good.	Excel- lent.
Boys ... ..	49.85	50.93	50.41	50.88	50.98
Girls ... ..	48.33	49.37	49.41	49.54	49.75

DISCUSSION.

Dr. SHUTTLEWORTH (London) thought that the Section had been fortunate in having the discussion opened by Dr. Warner, whose investigations and writings had done so much to draw public attention to the necessity of differential educational treatment in accordance with the varying physical and mental conditions of school children. As long ago as 1888 a committee had been appointed by the British Medical Association, at the instance of the Section of Psychology, to investigate these conditions, and the work done by Dr. Warner and others associated with him on this committee had borne fruit in practical measures, notably in the legislative provision of special schools for epileptic and defective children. Much, indeed, remained to be done, and there were hopeful signs that discriminative treatment of individual children, rather than the mere wholesale prescription of subjects to be crammed alike into all, was beginning to be recognized as a function of the authorities controlling public education. The dull and backward and nervous children, referred to by Dr. Warner, needed special consideration in the scheme of elementary education; for, strictly speaking, they would not be included in the legal definition of mentally defective, though they required a curriculum quite different to that of the ordinary school—that is, more of the physical and manual training method and less of the "three R's." With regard to the neurotic children, it seemed to him that, through lack of power of concentration, they had no chance of benefiting by the instruction given in the ordinary school, and in many instances, such as those in which there was a tendency to choreic symptoms, it was quite justifiable to consider them as subjects of functional defect of mental action and, as such, to enrol them as requiring special instruction. As a matter of fact he had known children of this class derive much benefit from a period of tactful training and more individual care than was possible in the comparatively small classes of the special schools. He had been much interested in the facts and statistics adduced by Dr. Williams. The terrible family histories traced out certainly pointed to the desirability of prohibiting marriage between obviously defective persons; but to stem the torrent of degeneracy it was needful to trace it to its source and endeavour to deal with its springs and contributory streamlets. Dr. Williams had stated that as many as one-sixth of the criminal lunatics at Broadmoor were probably the subjects of congenital mental defect, and Dr. Donkin had recently stated, from his experience, that from 15 to 20 per cent. of convicted criminals were of this class, as were also a large percentage of the inmates of inebriate institutions. Surely it would be more economical in the long run for the State to extend such protection to the feeble-minded who had passed through special training, so as to prevent their swelling the ranks of the criminal classes. The institution of industrial colonies for such as were not capable of employment in the outside world seemed the most hopeful means of meeting the difficulty, and it seemed not unreasonable to suppose that segregation of those unfit for parentage would tend to diminish the proportion of degenerate offspring in the future.

Dr. CROWLEY (Bradford) said that what appeared to him to be of special urgency was the paying of special and individual attention to that class of child who was neither normal nor mentally deficient. Special classes should be formed where every attention would be paid to nutrition, cleanliness, and the physique generally, and where the whole training would be based on handiwork, using the term in its widest sense. He was of the opinion that great care should be taken in accepting records of family histories, especially such as had been taken by teachers. Data with regard to alcohol were especially fallacious.

Dr. HELEN BOYLE (Hove) thought that possibly a certain



fusion of different types of defective children might be good.

Dr. STEWART and Dr. MAURICE CRAIG also took part in this discussion, the latter dwelling upon the importance in later life of early habits.

#### REPLY.

Dr. WARNER, in reply, said that it was advisable in taking family histories to see that they were complete, and included not only the defective but the normal members of the families. Collections of family histories too often drew attention to the failures, and failed to notice the brilliant successes springing from the same stocks. It was not uncommon to find mental evolution and mental devolution in the same family. Also feeble-minded women might have normal children. As to the size of children's heads, Dr. Warner pointed out that Dr. Williams's cases appeared to be hospital cases, and that he, Dr. Warner, had found that the average circumference in well-developed children was 17.5 in. at nine months, 19.0 in. at twelve months, and 21.0 in. at seven years.

### VERA AND PRAESENILIS MELANCHOLIA AT THE FEMALE CLIMACTERIC.

By LEONARD D. H. BAUGH, M.B.Edin.,

Senior Assistant Medical Officer, Gartloch Mental Hospital, Glasgow.

PHYSIOLOGISTS consider the evolutionary and devolutional stages of life as the periods at which the greatest expenditure of force is demanded from the organism. Alienists further recognize that at these epochs there is a tendency to morbid mental reaction, particularly in those with hereditary instability. In the literature of mental disease much has been written about the psychoses of decadence, and a large number of cases classified and described under the term "climacteric insanity."

Writers on climacteric insanity mention melancholia as the typical illustration, but their statements as to the percentage of cases that should be regarded as climacteric show differences of more than 10 per cent. This divergence of opinion may be explained as resultant from:

(a) The variations in conception as to what constitutes melancholia.

(b) Undue importance being attached to the prodromal "stadium melancholicum" first noted by Guislan and lucidly described by Griesinger.<sup>1</sup>

(c) The fact that, influenced by the age of the patient and the stress laid on the far-reaching consequences of the involuntal changes which occur at or about the climacteric in the female, observers have classed under this term attacks of insanity which recurred or incidentally started during the period.

To illustrate the variations in percentage and support the explanations (a, b, c) offered, a list excerpted from Lewis<sup>2</sup> is appended:

Reid, Hanwell, 1.1 per cent.; Tilt, 3 to 4 per cent.; Skae, Edinburgh Royal, 11.1 per cent.; Clouston, Edinburgh Royal, 12.6 per cent.; Merson, West Riding, Yorks, 14 to 15 per cent.; Bevan Lewis, West Riding, Yorks, 4.4 per cent.

The percentages quoted were calculated on the total female admissions. Although very valuable and instructive, percentages calculated in that manner do not impart information as to the proportionate incidence of the forms of climacteric insanity. A classification of these forms is submitted to you, and special attention is devoted to melancholia, which is subdivided into vera and praesenilis melancholia.

The observations are based on a study of females between 40 and 59 years of age, admitted to the Gartloch Mental Hospital for Glasgow, during a period of four years. For the investigation the age limit had to be arbitrarily fixed, the decade from 45 to 54 is regarded as the climacteric, and the preceding and subsequent periods of five years as the pre-climacteric and post-climacteric epochs. A comprehensive view of the mental illness is taken in each case. Recurrent insanities with attacks before the pre-climacteric epoch are excluded from consideration as climacteric, and regarded as incidental, as are also cases of organic brain disease, confusional insanity, epilepsy, and paranoia.

In the four years under review there were 510 female admissions; 145 women admitted were between 40 and 59 years of age; of these only 16 are climacteric, the other

129 being incidental. Stated in figures, 3.1 per cent. of our total female admissions, and 11 per cent. of the women belonging to the selected period are cases of climacteric insanity. On apportioning the cases to the three periods we find:

Pre-climacteric (40-44), 54 women, of whom 1 is climacteric.

Climacteric (45-54), 61 women, of whom 10 are climacteric.

Post-climacteric (55-59), 30 women, of whom 5 are climacteric.

On reviewing the 145 cases, two facts stand out prominently with regard to those considered as incidental: (1) The prevalence of previous attacks which occurred before the age of 40 is striking; this history of previous mental illness is most frequently met with in the pre-climacteric epoch. (2) The colouring of the attacks by epochal characteristics. We realize that this epochal colouring is what might be expected if we recall that mental changes accompany the physical. It is accepted that in normal people, at the climacteric, action tends to be replaced by reflection, and that the outlook on life becomes narrowed and less roseate, or as Clouston<sup>3</sup> aptly puts it, "Much of the go is out of the person; life becomes slower." We know also that what under ordinary circumstances are normal characteristics are often abnormally accentuated in morbid states. Emphasis is laid on the points recalled, as depression is present in more than 50 per cent. of the cases. In the majority this depression appears to be a typical example of epochal phenomena, influencing to some extent the character of a recurrent mental illness, and in some appeared, as G. A. Rorie<sup>4</sup> found, to impart symptoms of an acute nature to those labouring under permanent derangements, such as epilepsy and the higher grades of imbecility. For the incidental cases which without being melancholias are coloured with depression, the figures in the three epochs are:

(a) Pre-climacteric, out of 53 incidental cases 26 showed depression.

(b) Climacteric, out of 51 incidental cases 32 showed depression.

(c) Post-climacteric, out of 25 incidental cases 16 showed depression.

To pass to the climacteric cases: Three are diagnosed as paranoidal. The term "paranoidal" is selected, for, although the symptoms are systematized they lack the slow evolution of paranoia. The hallucinations and delusions are those of persecution, and are accompanied by depression. That they were placed under institutional care practically at the same age (two at 46 and one at 47) is noteworthy, though probably only a coincidence. Other points of similarity are—they had been self-supporting, well conducted, showed mental symptoms for the first time in the pre-climacteric epoch when labouring under menstrual derangement, their delusions are systematized and centred round the reproductive system. Two were married and had had children. Recovery cannot be expected in this type, and the nature of their delusions precludes their being given over to home care.

By a process of exclusion we have arrived at the thirteen cases of melancholia. The dictionary definition of "melancholia"<sup>5</sup> has been somewhat circumscribed by Kraepelin and others. The term is used here in the restricted sense. In addition to "the feeling of misery" our cases show "apprehension," considered by Kraepelin<sup>6</sup> to be an essential characteristic; suicidal tendencies are also marked. Farrar,<sup>7</sup> in a paper read to this Section at Toronto, illustrated some types differentiated from the broad category of devolutional melancholia.

His definitions of vera and praesenilis melancholia are accepted for this classification. Our seven illustrations of true melancholia showed apprehension, introspection, somato-psychic delusions, fairly active, though limited, thought processes with a tendency to dogmatism, retained sensation and orientation. In only one did the mental illness start before the age of 45. The 6 praesenilis cases showed, on the other hand, apprehensive fear, subjective uncertainty, disorientation, sensory misinterpretations. In the graver illustrations of the praesenilis type there were also noted, verbigeration, rhythmical movements, motor excitation, and a tendency to self-mutilation. Arterio-sclerosis is clinically detectable in these cases. Gaupp<sup>8</sup> of Munich attaches so much weight to arterio-sclerosis that he bases his classification of depressions on it. If not prepared to accept it as a causative factor of such importance, we should at least recognize it as a valuable aid in

diagnosis and prognosis. The prognosis, judged by our results, is more favourable in vera melancholia. Out of 7 cases 3 recovered, 1 discharged relieved to home care, 2 are chronic, the other, removed by her friends from our care, committed suicide shortly afterwards. Of the praesenilis cases 2 recovered temporarily, but relapsed.

While in a short paper like this it is impossible to approach the subject of mental illnesses at the climacteric from more than a few points of view, I trust that those brought to your notice may have been of interest, and in conclusion, I must thank the Medical Superintendent of Gartloch, Dr. Parker, for permission to make free use of cases and records.

REFERENCES.

<sup>1</sup> Griesinger *Mental Pathology and Therapeutics*. <sup>2</sup> Bevan Lewis, *Textbook of Mental Diseases*. <sup>3</sup> Clouston, *Mental Diseases*. <sup>4</sup> G. A. Rorie, *Insanities of Decadence, Journal of Mental Science*, July, 1905. <sup>5</sup> Hack Tukey, *Dictionary of Psychological Medicine*. <sup>6</sup> Kraepelin, *Clinical Psychiatry*. <sup>7</sup> C. B. Farrar, *Types of Devolutional Psychoses, Review Neural and Psychiatry*, October, 1906. <sup>8</sup> R. Gaupp, *Die Depressions Zustände des höheren Lebensalter, Münch. med. Woch.*, August, 1905.

DISCUSSION.

Dr. MAURICE CRAIG thought there was no such disorder as climacteric insanity, though it was a useful term to denote those insanities which occur during the climacterium. There were many types of mental disorder occurring at this time, arising from many causes. The subjects were frequently working women, of whom, perhaps, governesses formed the largest number. He then referred at length to the several forms of mental disorder occurring at this time; to the various causes of mental disorder, including autointoxication; to the sources of intoxication, particularly the alimentary canal, and in this relation said that women, as a rule, drank too little water, a point which he commended to the careful consideration of the general practitioner.

SOME STATISTICAL POINTS IN CONNEXION WITH THE STUDY OF THE INHERITANCE OF INSANITY.

By W. PALIN ELDERTON,

Fellow of the Institute of Actuaries, etc.

I.—Introduction.

THERE is little doubt that at the present time the systematic study of insanity is of very great importance, not merely from the point of view of curing the maladies from which particular individuals are suffering, but also because of the sociological aspect of the disease.

I propose to refer only to a part of this large subject, and shall attempt to show from an actuarial point of view how solutions could be reached to the problem whether the disease is handed on from generation to generation or is merely accidental in its incidence. At present the statistics available are in some respects unsatisfactory, and I feel that perhaps it may be of interest to show how the information at present available might be supplemented in order to enable us to form more accurate opinions. I would preface my remarks, however, by saying that I do not blame the medical profession for the occasional statistical shortcomings that may be laid to its charge. I no more expect a doctor to be an actuary than I expect him to be a clergyman or a banker. Of course, if he is fortunate enough to have had time to study statistics, so much the better; but it is really outside his province, and logically I feel that statisticians and doctors should join forces when medico-statistical problems have to be faced, and should arrange their statistics in collaboration. This opinion of mine is my excuse for accepting your kind invitation to me and intruding myself at a medical meeting, in the hope that the actuarial side may help the medical side in the particular problem before us.

I propose first to give the date required for the investigation of the problem of heredity and to show the result we wish to obtain; then we will try to see where existing information is unsatisfactory; then I shall attempt to show how one can approximately remove some of the unsatisfactory features, and suggest some information which is necessary if the statistical study of insanity is to proceed on scientific lines. I may remark that the term "insanity" is throughout used in a wide sense; at present, at any rate, it seems useless for a statistician to try refinements.

II.—Data, etc., for Problem of Heredity.

If we record for each of a large number of persons taken at random a certain character, such as eye colour, height, presence or absence of a certain disease, and also record similar information for the parents of these people, we have, I think, the ideal data for the investigation of heredity in the community. Details will have to be adjusted in some cases, but with such information we can measure the intensity of the relationship (correlation) between the character in the children and the character in their parents. In order to give you an example of this kind of data, we will take some statistics of insanity and assume for the moment they were obtained in this way.

TABLE I.

	Insane.	Not Insane.	Total.
Parent :			
Insane ... ..	49	361	410
Offspring :			
Not insane ... ..	149	20,000	20,149
	198	20,361	20,559

In 49 cases the disease was present in both father and son. In 149 cases the father had the disease and the son had not. In 361 cases the son had the disease and the father had not. In 20,000 cases neither had it.

You will notice that all the possible cases that can arise are given in this table.

Now let us see how to estimate the intensity of heredity in such a case. It is sometimes a help to put the matter in a more definite shape, and ask, Is the correlation between disease in parent and offspring as great as it is in eye colour? Is it as great as the correlation between vaccination and immunity from small-pox, or antitoxin treatment of diphtheria and recovery?

In order to enable one to contrast such relationships, statisticians use a scale which runs from -1 to +1. If the correlation is perfect, its coefficient on this scale is unity, while if there is no relation at all its value is zero. The sign is used to give additional information; if you compare the heights of parents and offspring, you will notice that the tall children are generally the offspring of the tall parents, and in this case we should say the coefficient measuring correlation is positive; while if the tall parents generally had small children, and the small parents generally tall children, the coefficient would be negative.

In order to give some idea of the value of coefficients the following table has been prepared:

TABLE II.—Showing the Use of the Coefficient of Correlation as a Scale for Measuring Correlation.

THINGS COMPARED.	COEFFICIENT.
Age of wife and age of husband in population (Yorkshire) ... ..	0.96
Left cubit and left middle finger ... ..	0.85
Height and left foot ... ..	0.74
Effectiveness of vaccination and resistance of small-pox ... ..	0.64
Eye colour in parent and eye colour in offspring ... ..	0.50
Diphtheria—antitoxin and recovery ... ..	0.32
Size of family, mother and daughter ... ..	0.21
Length and height of skull (French series) ... ..	0.13
Length and breadth of Parisian skulls ... ..	0.05

It is to be remembered that these coefficients give a result from a large number of cases; they tell nothing about what will happen in a certain case.

III.—Inheritance of Insanity: Difficulties.

Now let us turn to our particular case—namely, that of insanity—and consider the particulars that are available. When a patient is brought to an asylum it is, I imagine, often possible to find out whether his parents are then insane or have been so at some previous time, and also how many brothers and sisters he has, and their condition as regards insanity. In this way we can apparently fill up three out of the four divisions of the small table that we require for measuring correlation; but you will notice that even these three divisions are not altogether satisfactory. Suppose, for instance, the patient is the eldest of a family of five, and is only 32 years old, then one might ascertain that all his brothers and sisters were sane. We should then conclude that the insane brother had four brothers and sisters, all of whom were sane. But fifteen years later



all the rest of the family may have shown insanity, and a rearrangement would be necessary. In other words, if the experience is taken from the present inmates of asylums, one is sometimes left in doubt as to the tabulation, because one is dealing with incomplete data, and to make it complete one needs to prophesy what is going to happen. You will notice in this connexion the importance in statistical work of finding out the ages of all the persons on whom your investigations depend.

There is also another objection—namely, that we have so far been unable to fill in one division at all; for we have no data to tell us the number of sane children born to a sane parent. I propose to deal with this question a little more fully, as it is often difficult to obtain statistics, and, judging from the frequency with which such a question is ignored, one is inclined to think that its importance is not fully realized. I will attempt to explain the effect of neglecting this item by taking two numerical examples and showing the results of filling up one division with different values. Turn to the table with which we began, and, instead of 20,000, put in first 2,000 and then 40,000, and we shall have these two Tables A and B in Table III.

TABLE III.

(A) Parent.

	Insane.	Not Insane.	Total.
Offspring insane ... ..	49	361	410
Offspring not insane ... ..	149	2,000	2,149
Total ... ..	198	2,361	2,559

Coefficient of correlation ... .. 0.17

(B) Parent.

	Insane.	Not Insane.	Total.
Offspring insane ... ..	49	361	410
Offspring not insane ... ..	149	40,000	40,149
Total ... ..	198	40,361	40,559

Coefficient of correlation ... .. 0.61

Now, if I work out the coefficients of correlation, I get the values shown under each table, and they differ considerably; but as some of you may not be used to working with such statistical coefficients, I will suggest another way of seeing the importance of the division with which we are dealing. If you take Table B, you will see that  $\frac{49}{410}$ , or 12 per cent. of the insane offspring, came from insane parents, while  $\frac{149}{40149}$ , or one-third of 1 per cent. of the sane offspring, came from insane parents. Here there is clearly a distinct correlation. Now turn to Table A, and we find 12 per cent., as before, for the insane offspring from insane parents, but we have as many as 7 per cent. of the sane offspring coming from insane parents, and the correlation is therefore less. If the pairs of percentages were the same, there would be no correlation; this would be reached if we had 1,093, instead of 2,000, in Table III A. This will show you the importance of the particular division, for we may, by estimating it incorrectly, conclude there is a large amount of correlation when there is really none at all, and it also gives us the practical lesson that we must not only record the members of the affected families, but must also consider the number of people in families which are entirely free from a disease.

We will now see how difficult it is to get the information necessary to enable us to fill in the particulars correctly. If the 49, 149, and 361 are got from family histories of a certain part of the community (say, collected by a single asylum),\* then we ought to take a figure  $N$  such that if we were to collect a large enough number of family histories at random to give 49, 149, and 361 in the three divisions, then the same histories would give  $N$  in the fourth division of the table.

A moment's thought will show you how difficult it is to guess the figure. It would not do to say that there are 3,000 insane people in the community to each 1,000,000 of

the population, and we have considered only 410 insane people, so that the number in our fourth division is  $\frac{410 \times 1,000,000}{3,000}$ , or about 137,000, because some people may

be sane this year but have been insane before, and these ought to be included in the divisions relating to the insane of the table, not in those relating to the sane. This means that 3,000 is an underestimate.

## IV.—The Force of Insanity in the Community.

Let us see exactly what is wanted to remove the inaccuracies. We require to know (1) the number of people who develop insanity at an age later than that which they have attained when they come under consideration, and (2) the number of people who never develop the disease.

For our fourfold table the former would enable us to get rid (approximately) of incompleteness in data, provided we knew the ages of all the people considered, and the latter would supply us with the fourth division.

It seems to me that the only way to get the results we want is by tracing a large number of people from the time they are first admitted into an asylum and seeing what happens to them. This would necessitate tracing those people who are discharged from asylums, and might be done in much the same way as I understand the authorities of sanatoriums for consumptives try to keep in touch with their discharged patients. One could, by writing each year to a discharged patient or his relatives, find out the death-rates of the discharged patients at each age, and by using these rates we could evolve the functions with which we want to deal. This could, of course, only be done by the asylum authorities, and may be difficult in practice; but I mention it because it seems the most exact method available, and is being attempted for at least one other disease.

We can, however, make some approximations to the proportion of the "permanently sane" in a community in one of the following ways:

1. By using some particulars collected several years ago by Sir Arthur Mitchell, originally published in the *Journal of Mental Science*, 1877 and 1879, and reprinted in the *Journal of the Institute of Actuaries*, vol. xxviii, pages 425 to 436. Mitchell found that 1,297 persons were admitted to Scottish asylums, for the first time, in a certain year (1858), and he traced these for twelve years, recording the number who died insane, the number discharged, and so on. These statistics relate to a rather ancient date, but are useful as a rough guide for our investigation. I attempted to use Mitchell's figures to find out how many people will become insane out of a certain number at age 20 (that is, the chance of a person becoming insane). Consider that there are 50,000 people, all aged 20, who have never shown insanity, then if we know the chance of a person becoming insane for the first time between 20 and 21, we shall find how many of the 50,000 become insane before 21, and shall therefore know the number who are sane at that age. Similarly, if we know the chance of a person aged 21 becoming insane, we can find the number sane at age 22 and so on. In this way we have reduced our problem to one of finding the chance of a person aged  $x$  showing insanity for the first time. But Mitchell has given us the number (1,297) who were admitted for the first time into asylums in 1858, and, by comparing this number with the population, we can form an idea of the chances required—only approximately, because we do not know how many persons in the general population have been insane. One can make some allowances, but they are technical, and cannot conveniently be explained in full detail in the present paper.

Here, as in many other cases of statistical work, the age-distribution was unfortunately not given, and I had therefore to approximate it by using Dr. Urquhart's recent data.<sup>1</sup> With these approximations I estimated that the chances of a person developing insanity for the first time were 0.44 per mille at age 20, and 0.92 per mille at age 30; they then gradually fell till they are about 0.34 per mille at age 80. You will understand that these figures are mere rough estimates based on a series of assumptions\*; they are not to be taken as final.

\* This was actually the case with the statistics given; they are due to Urquhart (see references post).

\* I may remark that a rate of mortality of a part of the community is involved.

Using these chances, I find that 2½ per cent. of the people aged 20 show insanity at some time during their lifetime.

2. Some figures due to Dr. John Macpherson are mentioned by Mr. David Heron in a recent paper on the inheritance of insanity,<sup>2</sup> in which similar material to Mitchell's is traced for thirty years. It is not clear, however, that the number of cases dealt with refer to all the first admissions over a given area, and an alternative method must therefore be tried. This method consists in tracing the people through the rest of their lives, and assuming that the rate of mortality for the discharged lies between that for the insane and that of the general population. This gives a series of numbers which show the number of people living in the community corresponding to those in the asylums, and by proportion one can estimate the true number of insane (past and present). "Student" at the Biometric Laboratory, University College, London, used this method and found values varying from 1 per cent. to 2½ per cent., according to the mortality assumed. As he did not know the age incidence the problem of working out mortality rates, etc., was very awkward, as age groups have to be assumed and the calculations become very complicated.

With this evidence before us, I think we may put my 23 per cent. as an upper limit, and we may now refer to the application of these results to measuring the correlation between insanity in parent and offspring.

V.—Statistical Measure of Heredity in Insanity.

In Mr. David Heron's memoir, to which I have already referred, an analysis is made of the correlation between insanity (1) in parent and offspring, and (2) between brother or sister and brother or sister (sibship). Using the percentages of 1 per cent. to 2½ per cent., he found a parental correlation varying from 0.62 to 0.52, and a fraternal correlation of from 0.55 to 0.45. While if 2½ per cent. be used as the proportion of adults who become insane the coefficients of correlation are reduced to 0.50 and 0.40 respectively.

In order to show you how these figures compare with those relating to the inheritance of other characters the following table has been prepared:

TABLE IV.—Showing Intensity of Inheritance.

	Parental.	Fraternal.
<i>Physical:</i>		
Stature ... ..	0.50	0.53
Span ... ..	0.46	0.55
Eye colour ... ..	0.50	0.48
<i>Psychical:</i>		
Intelligence ... ..	0.52	0.45
<i>Pathological:</i>		
Tuberculosis ... ..	0.50	0.48
Deaf-mutism ... ..	0.54	0.73
Insanity ... ..	0.57	0.50

It must be borne in mind that some of these values are based on "incomplete" data, and the results form only a first study—self-confessed—but one of much interest.

In the appendix to a paper published this month<sup>3</sup> (July) on Tuberculosis mention is made that there is a marked relation (a correlation coefficient of about 0.30) between insanity in husband and wife. "It would appear, therefore, that stocks which are insane, epileptic, markedly eccentric, or alcoholic in a degree amounting to mania do tend to mate together." This result is based on a small number of cases, and requires confirmation, but it is a matter bearing on heredity which future investigators must consider.

VI.—Conclusion.

You will probably feel that all this leads us but a little way. True, but we on the statistical side want your help just as you sometimes want ours. My friend, Professor Pearson, has been trying for some years to collect family histories in order to find out more exactly the real measure of heredity. He has issued (a) a general disease and temperament schedule intended for the intelligent layman; (b) a special family disease schedule for medical men; (c) a special schedule for sanatoriums for the tuberculous, accompanied by a card for purposes of mortality

investigations. If work similar to that being done on tuberculosis could be done on insanity, we should be better off. The collection is a long process; one requires thousands of families.

And now I must conclude. In offering you this paper of difficulties I feel that I am really giving you the thing which appeals to you, for the medical profession has always attempted to tackle unsolved problems with splendid courage. It is my knowledge of this quality of yours that has encouraged me to write my paper, and I can only hope that its many shortcomings may not obscure the importance of investigating the statistics of a difficult subject.

Postscript.

May I mention some data I came on by chance recently? In a book of odd papers which had been collected by the late Dr. Robert Cory I found one nearly connected with our subject. It is a Report on Seventy Cases of Epilepsy, by Dr. J. Leech, of Hulme Dispensary, and R. Dacre Fox, Resident Medical Officer, Manchester New Workhouse. There is no date, publisher, or any such helpful information on the pamphlet, and I do not trace either doctor in the *Medical Register*, but I fancy from the position of the paper in the series that the date is 1875. On the sixth page of this memoir the following table is given:

	Number of Known Relatives.	Number of Cases of Epilepsy in Relatives.	Number of Cases of Nervous Disease (including Epilepsy).	Percentage of Cases in which Some Form of Nervous Disease may be found in Twenty Years.
First series of 20 non-epileptics ...	453	3	8	35
Second series of 20 non-epileptics ...	512	1	10	30
Twenty cases of epilepsy ... ..	290	9	17	55

The "non-epileptics" were really almost random samples as they were taken from the family histories of forty non-epileptic patients. Clearly the information as it stands even when taken with much else that is interesting in the paper, is not of very great value, but if only we had found full information of those sixty full family histories we should have felt wealthy! However, I mention the data because the authors appreciated the value of taking cases "at random" to test their results.

REFERENCES.

<sup>1</sup> *Journal of Mental Science*, April, 1907; or Heron's *Memoir on Inheritance of the Insane Diathesis*, 1907. <sup>2</sup> *First Study of the Statistics of Insanity and the Inheritance of the Insane Diathesis*, by D. Heron. London: Dulau and Co. 1907. <sup>3</sup> *A Second Study of the Statistics of Pulmonary Tuberculosis: Marital Infection*, by the late E. G. Pope, edited and revised by Karl Pearson. London: Dulau, 1908.

THE PSYCHOLOGY OF NEURASTHENIA AND HYSTERIA.

By A. T. SCHOFIELD, M.D. BRUX.

I PROPOSE in this short paper to confine myself strictly to these two functional nerve diseases. Between them and true insanities there is a great distinction, which it is well to emphasize as much as possible with patients, both for their comfort and cure. While there are connecting links, there is not so much likelihood of neurasthenics becoming insane as of the healthy to become neurasthenic.

Neurasthenia simply means "nerve weakness," and the name came to England from America and France about 1886. Bouchet in 1857 described neurasthenia as a distinct disease. For a long time after hysteria had been accepted as a pathological entity neurasthenia was looked upon with grave suspicion; but to-day its claims for distinct recognition are generally accepted, the only survival of its dubious condition being the apologetic manner which sufferers often assume in the consulting-room.

Neurasthenia is, primarily, due to heredity and malnutrition, while it is brought on by excessive strain or poison. Over-education is not the important factor it is supposed to be. If its true order of body first and then brain be observed it seldom is a cause of neurasthenia. Neither is a busy life, for neurasthenics abound in country places,



such as Iceland, and farming districts. It is more common in the single than married.

This last point is an interesting one. Neurasthenia often arises in men from sexual excesses, in women more commonly from celibacy, or the strain of child-bearing, etc. In the eighteenth century this disease was called the vapours, and was almost exclusively attributed to suppressed sexual passions. Nowadays masturbation is looked on as a most frequent cause. When, however, we remember this is found in some 99 per cent. of boys at some time or other, and yet that few are nervous, we see it is not such a common factor; and when it is, it is probably due to psychic rather than physical causes.\* If this cause be overestimated I am convinced that enforced celibacy amongst women is much underestimated, and in dealing with any case in unmarried women between 30 and 45 this must never be forgotten. There can be no doubt, however moral and unconscious the victim, of the great brain strain due to constant suppression of those nerve forces in women intended for marriage and maternity. In these, however pure and blameless, silent tragedies are for ever being enacted for want of any opportunity of entering upon the marriage state. Surely there are features in marriages abroad and in the East (apart from polygamy) worth consideration in this connexion. Every marriageable woman certainly ought to have a chance of matrimony, both for her own sake and that of the nation. Nowhere (save in the States) are so many potential wives and mothers doomed, against their wish, to perpetual celibacy as here. Let me add that in thus speaking I do not allude in any way to undue sexual impulses or to anything but perfectly pure and normal womanhood.

Neurasthenia is largely due to hereditary alcoholism. The influence of the heavy drinking of the last century is still seen in our offspring. Hereditary syphilis is also a little suspected but an undoubted cause.

Neurasthenia is found largely amongst factory girls in London, Birmingham, and Lancashire, from malnutrition and monotony of work. It is rarely due to ordinary work, but rather to care and worry connected with it. It is more common in colleges, and hysteria in schools, as the unbalanced mind (unconscious) at puberty in the school time produces hysteria, whereas overstrain and other factors act in college life. Of course, when there is any underlying mental taint, there is a psychic instability that is easily made active.

The leading causes of neurasthenia may be summed up as—

1. *Poisons* from influenza, syphilis, enteritis, dyspepsia, alcohol, drugs, zymotic and other diseases.
2. *Malnutrition*.
3. *Fatigue* from overwork, worry, insomnia, pain, sexual excesses or suppression.
4. *Emotional strain* from shock, grief, accidents, religion, love, etc.
5. *Indirect causes*, as enteroptosis, floating kidney, eye-strain, also bad environment, suggestion, and other psychic factors.

The principal predisposing cause is heredity. Psychologically, neurasthenia is a disease of exhaustion of the higher nerve centres, since poisoning produces toxic effects similar to those from overwork. To weakness of these centres by fatigue or poison from some of the above causes we may attribute even the most psychic symptoms of this disease, such as "phobias" and "obsession," provided always that the patient be free from any underlying mental taint.

"Hysteria" is a word that covers symptoms so various that we may almost speak of one class of them, neuromimesis or the mimicry of disease, as a distinct disease.

Hysteria as elaborated by Charcot and largely accepted by America and England means a disease of narrowed common (anaesthesias, etc.) and special (contracted vision, etc.) sensation, and by convulsive seizures. But I find in the majority of the cases here these symptoms often absent, while neuromimesis is strongly marked. In this many varieties of disease are simulated in a way wholly beyond any voluntary powers, producing local swellings and effusions, paralyses, high temperatures, etc.

\* It must be remembered that this vice is quite common amongst girls and women.

An analysis of 350 cases of these functional diseases in my own practice shows that 13 per cent. are hysterics and 87 per cent. neurasthenic. The women neurasthenics were fewer than the men, while in hysteria they were nearly double. In neurasthenia debility was the leading symptom, then sexual hypochondria and depression. In hysteria I had no well-marked case of Charcot's symptoms.

To understand the psychology of hysteria we must very briefly consider these two points in mental physiology. The central nervous system not only acts voluntarily and by ideas of which we are conscious, but is continually vibrating with memories and trains of thought of which we are unconscious. A very small proportion of the afferent currents produce conscious sensations of any kind, but nevertheless unconscious sensations equally produce psychic and physical effects. Take, for instance, a simple illustration: Pain, said to be in the little finger, is really the result of some change in the central termination of the ulnar nerve in the brain, and whenever any such change takes place, whatever its cause, we say there is pain in the little finger. Similarly in a house, if the hall-door bell rings, you say there is some one at the hall door, although (1) the wire may be pulled inside the hall, (2) the bell may be struck in the basement, (3) the wire may be entangled with another, so that the wrong bell rings. In the little finger also the "pain" may be caused (a) by pressure on the ulnar nerve; (b) by irritating the stump after amputation, when the pain is still referred to the lost little finger; (c) by pressure on the brain centre by tumour, etc.; (d) by transference of sensation from ideal centres by suggestion, or memories (of-past pain) and other associations.

In health, of course, we can generally distinguish each of these varied causes of the same pain, and all of them from local injury; but the peculiarity is that in hysteria, as a rule, the local symptoms (pain, etc.) are nearly always due to a central cause. It must be remembered that the pains, etc., though started in ideal centres, are as real and acute as if due to a local injury, and may produce real illness, or even death, so that a disease of the imagination is not an imaginary disease—a truth generally ignored. The fact that the pseudo disease is unconsciously produced is vital to the psychology of neuromimetic hysteria; for if produced consciously, and therefore voluntarily, it ceases to be a disease at all, and becomes shamming or malingering. Even when we get Charcot's "narrowed consciousness," it is produced (1) unconsciously, and (2) by some sort of suggestion, for it is found (1) to be produced against the will of the patient and without his knowledge, as in contracted visual fields, and (2) the anaesthesias, etc., are not permanent but change with ideas, and are capricious in area and locality, following no known nerve distribution.

From these and similar facts I found myself shut up to the following proposition: "That hysteria is a mental disease, and yet one in which the conscious mind is not involved, and in which the patient is absolutely ignorant of the cause of the symptoms." The obvious conclusion to me was that "a disease due to mind and yet not to conscious mind postulated 'unconscious mind';" and this gave rise nearly twenty years ago to my theory of the "unconscious mind," which has given offence to some English psychologists, though finding rapid acceptance elsewhere.

If mind be defined, as by Professor W. James, as "the power to pursue definite ends with the choice of means," I cannot for one by any ingenuity regard the functional life of the body and its organs as a mechanical process, such as the working of a motor car. I regard its action as vital—that is, mental—and the body as a psycho-physical unity, instinct with mind, of which the highest parts are illumined by consciousness, while the lower part that controls physical and organic action acts unconsciously, but none the less psychically.

The "unconscious mind" of which I speak is not the "cosmic mind" of Myers or the "mystic mind" of von Hartmann, but simply that part of the one mind we possess that is not illumined by consciousness.

In hysteria, the conscious mind being sound, the patient is not insane; but there is profound disorder of the unconscious mind, or, as we sometimes call it in this connexion, the *vis medicatrix Naturae*. In health this force presides over the nutrition and repair of the body, but in hysteric

neuro-mimesis it produces ailments and disorders of all sorts, and injures the individual instead of benefiting him. It seems to me, therefore, that while disorder of the conscious mind is called insanity, that of the unconscious mind is called hysteria.

Though I believe that this psychological concept of hysteria is new, I find nothing to contradict it in authorities on hysteria, such as Wilks, Ormerod, Sir R. Reynolds, Möbius, Charcot, Paget, Eichhorst, Briquet, Erichsen, and Erb.

To conclude, therefore, this brief monograph, I regard the psychological distinction between neurasthenia and hysteria as profound. In neurasthenia we get an exhaustion or poisoning of the higher cerebral or spinal nerve centres. In hysteria there is no question of exhaustion, but there is aberration and morbid action of that part of the unconscious mind (*vis medicatrix Naturae*) which presides over the functions and nutrition of the body, and which produces in hysteria, unconsciously and against the patient's will, morbid phenomena which the mind consciously is wholly unable to produce even if it wished—as, for instance, reversed peristalsis, high temperature, local tumours, etc. Hysteria is thus parallel to insanity though quite distinct from it; the radical difference being that the sphere of the former is the unconscious and that of the latter the conscious mind.

### ASYLUM DYSENTERY AND ULCERATIVE COLITIS.

By F. G. BUSHNELL, M.D.Lond.,

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THE subject of dysentery in asylums is of interest to psychologists and to pathologists. It is related to, if not identical with, the acute and ulcerative forms of colitis which are seen in general hospitals and practice and of which the following five cases examined bacteriologically are examples.

They were all cases of from six weeks' to six months' duration; two were in males and three in females. The ages of the patients were from 25 to 49 years. The women had all suffered from constipation severely for years (an action once in three days to three weeks). Severe anaemia and moderate leucocytosis (12,000 to 15,000) were found. Four of the cases ended fatally and the fifth is not improving. Perforation and peritonitis occurred once.

At the Royal Society of Medicine opinions were expressed in March of this year as to the relationship of this disease to (1) English dysentery, (2) tropical bacillary dysentery, (3) swine fever or dysentery, (4) "kala-azar" of India. Drs. Mott and Gemmell are of the opinion that asylum dysentery is a form of true dysentery, and Professor Osler agrees with this view. Drs. H. H. Tooth, H. P. Hawkins-Dalton, and A. M. Elliot consider the acute forms of colitis are identical in their morbid anatomy with dysentery, which was endemic in England to the middle of the last century.

The object of the bacteriological examinations of my cases was to determine whether dysentery bacilli, assuming the disease was bacillary, could be recovered from the stools during life and in the viscera after death. If not, what micro-organisms were most constantly present in the stools? and their identification culturally, by serum reaction and animal experiment was necessary.

*Method of Isolating Bacteria.*—Two loopfuls of the faeces were transferred from the (sterilized) collecting apparatus to a tube of sterile peptone beef broth and an emulsion made. From this two or three loopfuls were smeared at once over the surface of two litmus lactose agar and two Conradi-Drigalski plates, and incubated at 37° C. overnight. On the next morning samples of the lactose and non-lactose fermenters were picked off and transferred to peptone broth and their cultural characters determined. The (smaller) streptococcal and staphylococcal colonies were also examined from time to time. The bacteria isolated were: *B. coli communis*, *B. alkaligenes faecalis*, non-lactose fermenter I, *B. pyocyaneus*, *Streptococcus faecalis*, staphylococcus. In no instance were dysentery bacilli of the nine described types present.

The smaller colonies of streptococci were constantly present, and were, roughly speaking, more numerous than the *coli* types of bacilli. The latter were constantly present, and lactose and non-lactose fermenters were in about the same proportions.

Among the latter was a bacillus (I), causing death in guinea-pigs in a few days, with peritonitis and septicaemia. It was a rod-shaped bacillus, of peculiar and vigorous motility, not staining with Gram's method, and tending to cohere spontaneously in clumps.

In broth it is characterized by a flaky growth; it does not ferment lactose though it may decolorize it, it splits glucose, levulose, galactose, maltose dulcit, and mannite, with formation of acid and gas. Sucrose it alkalizes in 24 hours constantly, milk it acidifies within 24 hours, but does not clot (26 days). (These characters were present in cultures from the heart, liver, and peritoneal exudate of a guinea-pig dead four days after inoculation of three agar tubes of growth.) This bacillus is distinguished from that of summer diarrhoea of Morgan by its reactions with mannite and dulcit.

*Agglutination Reactions.*—At least twenty observers have recorded agglutination with various strains of dysentery bacilli and the blood serum of immunes. Thus, with a dysentery bacillus obtained from Flexner, Hewlett obtained agglutination within one hour in 1 per cent. solutions of serum of two patients with asylum dysentery. Hawkins records a case of ulcerative colitis which agglutinated with Shiga's bacillus; again, Dalton states that a multiple papilloma of bowel also gave a positive reaction with this bacillus.

This bacillus I of our cases was repeatedly tested in 2½ per cent. and 1 per cent. dilutions of the patient's serums and always gave a positive reaction. The reaction was not, however, conclusive owing to the ease with which well-formed clumps appeared spontaneously in the (filtered) control. Its reactions with the serum of animals immunized against *B. enteritidis* and paratyphoid is to be tested.

The agglutination reactions of various lactose and non-lactose fermenters and of *B. pyocyaneus* were inconclusive with the patients' serums but suggested some specific clumping.

*Pathogenicity.*—Up to the present Bacillus I has been inoculated twice and produced death of a guinea-pig in about four days with a septicaemia and peritonitis due to the bacillus. Dr. Henderson Smith, who kindly made an autopsy for me, found the spleen congested, the liver pale and friable, the suprarenals bright red, the mesenteric vessels, intestines, and kidneys congested, but no ulceration of colon present.

The *B. pyocyaneus* isolated was fatal to a guinea-pig, but the virulence of the *coli* bacilli and other organisms has not yet been tested.

#### CONCLUSION.

The result of this examination of 5 cases of colitis (4 fatal) was that no dysentery bacilli were found present. The type of pathogenic organism may be different from that found in tropical dysentery, and it may be one of the colon group—a view supported to some extent by the serum reactions. The pathogenicity of some of these bacilli is undoubted in guinea-pigs, and the test of feeding animals upon cultures with the object of reproducing the disease and then recovering the bacillus from the lesions is to be carried out. Türk of Chicago has reported the formation of gastric ulcers in dogs under certain conditions after ingestion of *Bacillus coli*, and there is evidence that the saprophytic bacteria of intestine, etc., may assume pathogenic characters when their environment alters. Such an alteration may result from prolonged constipation. Enhanced virulence occurs in streptococci, staphylococci, and *coli* bacilli in the peritonitis following perforation of the bowel. It is suggested that a superficial colitis, eventually resulting in progressive ulceration, may be caused by this alteration of their biochemical characters.

A preparation said to contain living lactic acid bacilli was given with the object of crowding out the bacteria present in the bowel, with an apparent increase of lactose fermenters in the stools.

Treatment by vaccines prepared from the virulent bacilli in the stools seems well worthy of trial.

ON the occasion of the opening of the 67th session of the School of Pharmacy of the Pharmaceutical Society of Great Britain, Bloomsbury Square, the Pereira Medal will be presented by the President, and the Inaugural Address will be delivered by Mr. F. Harwood Lescher, F.C.S.