Since the establishment of the obstetric art on a basis of science, more than a century ago, various methods have been used for protecting the maternal soft parts from injury during the passage of the fetal head. And yet, the contrariety of facts and opinions, which have found expression at different times and in different places, well calculated to suggest the thought, that theory and practice are not yet in accord on the subject.

It remaining to others to judge of the merit of the several suggestions at this time, I shall judge myself, with reservation that the desires of others, because they are desires, are not necessarily the desires of the sick. This, I am well aware, is a hazardous course of reasoning. If the patient did not resist, if the wall of the abdomen were not far enough along, if the patient were not conducted with sufficient care and to the best advantage, the resistance of the perineum would not be complete, and the soft parts would be injured. For a full half hour the head of the child was upon the floor of the pelvis, distending the perineum sufficiently to its utmost capacity during the paroxysm of pain.

When the head was born, I was conscious of a slight laceration of the fourchette, but flattered myself that it was not more
serious than what frequently, and almost necessarily occurs in first deliveries. But between the delivery of the head and that of the shoulders, there was considerable delay, owing to the absence of pain, and I became concerned for the safety of the child, as there was a loop of cord around his neck, and his face became livid and convulsed. This diverted my attention from mother to child just at the moment when the uterus resumed its action with renewed energy, and expelled the shoulders and body of the child quite precipitately. After resuscitating the child, for he was asphyxiated, and separating the cord, I was proceeding to deliver the afterbirth, when the patient complained of pain and smarting at the vulva, and on examination I found that the perineum had been torn entirely through.

The surfaces were immediately brought together and stitched by three silk sutures, one deep and two shallow. But the operation failed, and a second operation was performed three or four months later by Prof. Hodgen, which, though not a complete success, was a great improvement, as it succeeded in restoring the septum between the anal and vulvar orifices, over which the patient has subsequently borne two children without injury.

At the time of this accident the patient was lying on her back with her knees well drawn up and abducted so that her feet came near to and a little outside of the nates, and she was supported in this position by two female attendants, one on each side and down on the knees.

This accident so impressed me with apprehensions of its liability to occur, especially in the primipara, as to have frequently impressed the question of its operation upon my mind as a matter of serious study and reflection, and the possibility that the sharply flexed position of the patient's limbs at the time of the accident might have favored the result, often presented itself.

Case second.—In 1875 while I was in charge of the city hospital, a girl about sixteen years old was sent to me from police head-quarters with the request that she be examined for indications of recent child-birth. The examination revealed, among other evidences of recent delivery, a fresh laceration of the perineum. The patient confessed her crime, and, by careful questioning, I drew from her the fact, that at the time of the birth she was sitting over the privy vault, in which the child was sub-
sequently found, with her body leaning forward, so that the abdomen rested upon the thighs, a position which must necessarily have put the perineum greatly on the stretch.

Case third.—In the Autumn of 1877 I attended Mrs. T., about thirty years of age, medium height and of slender form, in her third confinement. The two first, both girls by a former husband. The second stage of her labor was somewhat tedious, and just as the head began to distend the perineum, the patient, as a matter of choice took the left side position, with her thighs and knees sharply flexed. The fetal head, which was unusually large, soon became impacted in the soft parts, uterine contraction became stronger and refused to intermit, the perineum appeared to have reached the point of its utmost distention short of laceration, being pushed down so that the fetal occiput rose from under the pubes, and yet the head was so completely enveloped by the expanded perineum that a rupture of the latter seemed inevitable. At this moment, however, the patient, perhaps involuntarily, extended her left limb to a line nearly parallel with that of her body, and coincidentally with this movement, the fetal head passed through the vulva without any perceptible injury to the parts.

This circumstance, which may have been a mere coincidence impressed me still more fully with the idea that sharp flexion of the thighs upon the pelvis, during the last throes of parturition is not conducive in all cases to the safety of the mother, nor the speedy and safe delivery of the child.

Reflecting upon the different results in the termination of these cases, and the possibility of that difference having been determined by the different degrees of flexions of the limbs of the patients at the moment of the passage of the fetal head through the vulva, I became seriously impressed with the belief that Mrs. T., owed her escape from serious injury to the accidental extension of her left thigh, by which, not only the perineum, but all adjacent tissues were relaxed and made susceptible of a greater degree of expansion. The incident was not forgotten, and I confess, it has not unfrequently influenced my conduct in subsequent practice.

Case fourth.—Last Spring, 1880, I attended Mrs. H., a primipara, in which the fetal head became so tightly impacted at the vulva as to cause considerable delay, though the uterus was
contracting with great force, and I began to fear that a laceration was inevitable. The patient was on her back with her knees sharply flexed and abducted, so that her position was very nearly that known as the lithotomy position. I requested her to straighten her left leg, and as she did so, the vertex became more prominent and the forehead slid over the perineum without causing even so much as an abrasion at the fourchette.

**Case fifth.**—In October last I attended Mrs. S., a multipara twenty-seven years of age, in her second confinement at term, her first about seven years ago. It was a face presentation, that is, the face presented to the symphysis pubes. The face would not rise into the vulva until the chin had passed out from under the pubic arch, which caused the vertex to dig down and become completely encapsulated by the over distended perineum, where it remained stationary for more than half a minute, although the womb was contracting continuously and with tremendous force. The patient was on her back with her limbs strongly flexed. I requested her to straighten them a little, but her energies were so completely subordinated to the expulsive efforts that she failed to comply, but one of the lady attendants caught hold of the left ankle and drew it down a little and as she did so, the child's head was delivered without any injury to the mother.

These cases will suffice to show how the idea was suggested, and ultimately matured into a conviction, that a sharply flexed and abducted position of the thighs, though convenient and necessary in some cases, is not conducive of the greatest degree of security to the maternal soft parts at the moment of the passage of the child's head through the vulva, nor to the speedy and safe delivery of the child. But if I fail to present a satisfactory reason for my conviction, I shall be accused of allowing a few unimportant coincidences to assume, in my mind the relationship of cause and effect and to form thereby, the basis of a delusion. But I maintain that the conclusions to which I have arrived, and upon which I have acted are corroborated and sustained by the anatomical relations of the parts concerned. The central axis of the gravid uterus is on a line—or nearly so—with that of the pelvis, and extends from the umbilicus in the direction of the coccyx, and crosses the line of the central axis of the body obliquely. A line drawn through the central line of the vulva,
would impinge on the sacrum, crossing the central axis of the pelvis nearly at right angles. Now, if it were possible for the human female to be delivered at term in the erect position with her knees bandaged together, it is more than probable, I think, that the delivery would be effected on the posterior aspect of the thighs. The only obstacle to delivery in that direction would be the perineum, which, in that position would be so completely relaxed as to be easily pushed back by the pressure of the fetal head, especially when antagonized by the resistance of the muscles of the internal aspect of the thighs.

Some experience in gynecological practice enables me to assert that, with a Sims speculum, the perineum can be drawn back several lines further without pain or injury to the patient, if she be either in the prone or semi-prone position with her thighs extended and adducted, than it could if the limbs were strongly flexed and abducted. We find the reason of this in the fact that, in the ratio that the limbs are extended and adducted, the perineum, as also the nates is relaxed and its capacity to expand increased and vice versa, in the ratio that the limbs are flexed and abducted, the perineum and nates are stretched, and their capacity for expansion abridged; and hence it follows that extension or moderate flexion and abduction only are compatible with the greatest security to the perineum at the conclusion of the second stage of labor.

The question of the management of the perineum is truly a hackneyed one, but so long as the laceration of this part is admitted to be among the possible, and not unfrequent accidents of the parturient effort, the dread of its occurrence, and the terrible consequences which it entails, must continue to obtrude themselves upon the attention of the thoughtful practitioner, by whom it is impossible to contemplate such consequence to the young wife and mother, just at the threshold of her duties and obligations as such, without feelings of the deepest concern, and he is not likely to view with indifference, or as devoid of interest, any new thought or suggestion on the subject which may be offered for its prevention or amelioration.

I regard the instructions usually found in text-books on this subject as of little and uncertain value. Such, for instance, as a napkin folded and pressed against the perineum with the palm of the hand. I have never been able to discern the practical utility of the napkin, and have often wondered if it did not sug-
gest to the patient the idea that it was intended to prevent the
attendants hand from becoming soiled.

Tyler Smith—lectures on obstetrics, second edition, page 365—
objects to pressure on the perineum in any form, and advises
that it be made directly upon the foetal head so as to retard its
progress and prevent a too rapid expansion of the perineum.
But to this procedure there is the objection that, in the exact
ratio that the perineum is protected by it, the uterus is endan-
gered, and we avoid one accident at the risk of producing another
and still more dangerous one. For we should never lose sight of
the fact, that among the causes of rupture of the uterus—which is
always fatal—obstructions, either natural or artificial, to the free
passage of the foetus stand first.

I concur with this author, however, in condemning all efforts
for the direct support of the perineum with the palm of the
hand as not only useless, but liable to do harm. I also concur
with him in the importance of keeping the foetal head pressed
well forward against the pubes; though in my experience it is
only occasionally necessary to make efforts for that purpose,
and when necessary, I do not think the method proposed by
him to be the best, for the reason that in any case where there
is immediate danger of rupture of the perineum, any effort to
insert the fingers between it and the foetal head would certainly
increase the danger.

But the points which I wish specially to maintain are, that
the perineum is not only relaxed by the extension of the limbs,
but the degree of its inclination is increased so as to impose less
resistance to the passage of the foetal head, and vice versa; in
the ratio that the limbs are flexed, and abducted, the perineum
and contiguous parts are put upon the stretch, and consequently
its resistance and liability to rupture proportionately increased;
and while some degree of flexion and abduction are convenient;
and, in fact, necessary, I do not believe, after a careful study
of the anatomical relations of the parts, that it would be possible
for the human female to be delivered at term with her thighs
completely flexed upon the abdomen and abducted, without
more or less laceration of the perineum, moderate flexion, there-
fore, is all that should be allowed where the danger to these
parts is imminent during the last thirds of the parturient effort.
But before I dismiss this subject I must report briefly, the facts
of a case which came under my observation more recently and
which, though not an obstetric case, is nevertheless, a case of
injury of the perineum, and though somewhat unique in charac-
ter, is, I think, corroborative of my views.

Daisy H., a little girl aged 5 years, an exceedingly intelligent
child, and as sprightly and elastic as a rubber ball, was, on the
evening of the 20th of February last, playing with her little
sister, still younger, in her mother's sitting-room, when she fell
and ruptured her perineum. I saw her in a few minutes after
the accident, and found a rent in the margin of the raphé on the
right side and parallel to it, about ten lines in length, in fact,
nearly the entire length of the perineum, though not extending
into either of the passages, and from what could be ascertained
without resorting to the probe, the wound was judged to be
something more than half an inch in depth. The parents were
greatly alarmed and distressed, fearing that some irreparable
injury had been inflicted, and that an operation would be neces-
sary. But as both the natural passages remained intact, I was
prepared to assure them that rest and position with a bandage
over the nates would be all that was required, and that the ulti-
mate consequences would probably be trifling. The cure was
quite rapid and satisfactory, and it was found impossible to keep
the little sufferer in the recumbent position for more than a few
days only.

This little patient fell on a smooth and well carpeted floor,
and though both her parents were present at the time of the
accident, neither of them saw her fall nor were they aware of
the injury until the child complained to her mother that she had
hurt herself. Neither of them had seen her fall, and supposed
that she had fallen over one of the rockers of the rocking chair
which stood near the stove on one side, but the child insisted
that she had fallen on the floor near the foot of the lounge, and
in order to produce such an accident, she must either have fallen
on her buttocks with the thighs abducted in opposite directions,
or, which I think is the more probable, that she fell on the nates
of the right side, while the weight of the body under the peculiar
motion which had been given it at the time of falling, or
while running, brought such a strain on the integuments of the
perineum as to force them to give way.
Essays in Obstetrics and Gynecology.
Contents.


II On the Convulsions of Reproduction, called Puerperal Eclampsia, and their Congeners. page 58.

III On an Indicating Ascis-traction Forceps. page 194.

IV On Dr. Alexander's operation for Backward displacements of the Utterus and for Prolapsumus Utteri. page 202.
The Straight-bodied Position in Labour.

Position in labour has for its objects the convenience and comfort of the patient and attendant and the conservation of the patient's powers, parts, and products. That position is the best which is most compatible with these conditions.

Position is not a matter of routine but of careful thought and attention especially during the second stage, and it is to position at the conclusion of this stage that my remarks will be almost entirely restricted.

Defining the position to be assumed by the patient, Dr. Playfair
at page 330, volume I, second edition of his Science and Practice of Midwifery, says (and I am not aware that the words have been altered in the recent edition): "In this country it is usual for her to lie on her left side, with her nates parallel to the edge of the bed, another body lying across. This is the established obstetric position in England, and it would be useless to attempt to insist on any other, even if it were advisable." The underlining is mine. That dictum contains absurdity on the face of it; for, if another position were advisable, the patient herself would see the desirability of adopting it; it is inconsistent with fact, for, with two or three exceptions, all the patients whom I have attended in confinement have been sufficiently decided to obey directions as to
position when they were explained to them, and if the dictum were accepted it would inevitably shut out labour position for ever from all scientific progress. The dictum must therefore, with all respect to so eminent an authority on most obstetrical subjects, be regarded by those who wish to decide this question of position, in the best for themselves.

But—the position above-defined is assuredly that in which obstetric operations are most conveniently commenced: it is not however the best position in which to terminate either operations, or those cases, the vast majority of all labours, where the natural powers suffice. Besides, the patient usually places herself more longitudinally in bed, and occasionally from the situation of the bed or room to the form of the room, the right-sided position is most convenient to the patient, and have found examination thus to be practically as easy as when using the left fore-finger.
but forceps I have never attempted to apply to, and once I had difficulty in removing a placenta. The accustomed side is naturally most convenient to the practitioner.

During the first stage of labour, before the membranes rupture or are ruptured and the liquor amnii escapes, there are few exceptions to the rule that the patient may assume whatever position she pleases; if rupture has taken place prior to full dilatation of the os uteri a delicate judgment as to force and resistance of the cervix will guide us in concluding when the patient should be in the recumbent position.

During the second stage, previous to the descent of the head or other presenting part of the foetus upon the perineum, we may determine the amount of permissible
flexion of the thighs by observing the amount of apparent reflex flexion after straightening the limbs, which takes place at the onset of an uterine contraction. This is my guide to position during the greater part of the second stage, and in this way, I believe, the accessory powers of parturition are thus far exercised to the greatest advantage.

We come now to deal with those parts—more relevant to our inquiry—forming the sacral segment of the pelvic floor.

Dr. Garland Hurst did a service to obstetric science and womankind when in a paper on "Position in relation to injuries of the perineum during labor," published in the St. Louis (Missouri) Medical Surgical Journal for August 1881, (produced herewith)
he pointed out that "a sharply flexed and abducted position of the thighs, though convenient and necessary in some cases is not conducive to the greatest degree of security to the maternal soft parts at the moment of the passage of the child's head through the orif, nor to the speedy and safe delivery of the child," and that (pap) the perineum is not only relaxed by the extension of the limbs, but the degree of inclination is increased so as to impose less resistance to the passage of the fetal head and vice versa. The "vice versa" evidently refers to the effect of flexion of the limbs. That is the kernel of Dr. Hurst's paper: the shell consists of five obstetric and one surgical illustrative cases. It is to be borne in mind that Dr. Hurst is writing of the American dorsal position.
Flesion of the limbs towards the termination of the second stage is never necessary in the British positions.

My attention was directed to this subject by reading a paragraph in the British Medical Journal of April 8th 1882, and since that date I have adopted the principle to our British positions in all the labours which I have attended. Thus while subscribing in the main to Dr. Auri's statements my contention is that, in every case when the presenting mass (be it head, breech or placenta) has at most reached the perineum, and we have arranged for its delivery, the patient should be placed in the straight-bodied position, that is, with the lower extremities as far as possible adducted and in the same straight line with the body. If classic Greek be preferred to
plain Saxon this may be called the orthodox position (optos, straight, and to Sepas, the living body).

The British left-sided position is most of all positions suited to this adaptation or rather modification.

My first convert to that modification was a very practical obstetrician of twenty years experience—lately my employer—Mr. J. Fox Morrish of Dingle Hill in this city, who previously had been a very strong supporter of the perineum. and I observe that Dr. Arthur Coles, now obstetric physician to the Middlesex Hospital, at a meeting of the Obstetrical Society of London reported in the British Medical Journal of 6th January 1883, state that "rupture might often be prevented by straightening the limbs while the head
is emerging.

It is true that one great effect of this position is to prevent the sacral segment of the floor being converted into a valvular lid for the pelvis box in its coccy-pubic plane. Hence a source of speedier delivery, and avoidance of perineal rupture.

But it is not true that this position relaxes the perineum; the fact is that artificial tension is not put on.

Now in what I must designate the sigmoid position an artificial tension is induced by the dragging of the skin of the nates and posterior aspect of the thighs on the perineal tissues through the skin and subcutaneous tissues over the base of the perineal body and fourchette; and also I am inclined to
believe that some amount of resistance to distension of the pelvic outlet is produced by the tension or traction of the stretched glutei maximus on the coccyx and posterior surface of the great sacrosciatic ligaments tending to prevent the backward displacement of the coccyx at the sacro-coccygeal articulation. I have noticed abundant adipose tissue act as a contributor to tension in primiparae. In this acquired tension, then, the powers of parturition, or the obstetrician's arm, or both, as the case may be, find an additional element to be overcome, which is also met by the presenting part at an angle nearer a little to the right-angle than occurs naturally, so that increased power is demanded, and the
force not only overcomes the resistance, but
does so by more or less solution of continuity
of the resisting body - the vulvo-perineal tissues.
And this is the key to very many cases of
failure of attempts to prevent perineal
laceration whether by forceful or gentle
"support", Goodell's anal plans, or incision.
The obstetrician first has placed the patient
in a position of which artificial tension is
a result, and then has made more or less
unsuccessful endeavours to undo his own
work.
Whereas
in the straight attitude the natural
obtuseness of the angle of inclination of the
perineal plane is preserved, and whatever
laxity of parts is present is not interfered
with, thus by diminished resistance expediting
labour and helping to maintain the integrity
of the pelvic floor.
Both these points are illustrated in the process of placing a patient in the lithotomy position, and may be simply observed by placing two fingers in the vagina and causing the patient to flex and adduct acutely, then extend and adduct her thighs.

In endeavouring to prove the practical benefit of the straight-bodied position I must hold laceration or non-laceration to be a test by results so far as the perineal body and "skin over the base of the perineal body and fourchette" are concerned.

Perineal skin is, I think a sufficient term to express the idea enclosed by quotation points.

This opens up the subject of the causation and prevention of rupture of the perineum, with which I shall briefly deal.
I. Causation.

Nearly all that can be said on this subject is to be found in Churchill's Midwifery, 2nd Ed., pp. 445-446, and in the recent Manual of Gynecology by Drs. Hart and Barbour, p. 5-13. The causes are to be classed according to the influence of what Professor A.R. Simpson used to designate as the three P's of labour—the powers, passage, and passageways. To these I would add a fourth P—position.

1. Powers (of patient and practitioner).

   a. Violence of pains, causing a too rapid passage of the head through a canal neither prepared by "vital" nor mechanical relaxation or dilatation.

   b. Too early passing of the hand to turn, (Hart & Barbour) especially in primipara.

My version has been limited to four
multiparous cases where there was no laceration on passing the hand.

(c) Careless use of forceps. When the blades are not passed sufficiently far over the child's head, the narrow portion between the fenestrum and channe may, and have observed them do, cause rupture of the fourchette or cervico-perineal margin anterior to the perineal body; but particularly if traction be not made exactly in the axis of the outlet (which I define as that axis which the foetal head would follow naturally—all the structures retaining their integrity), there is almost certainly more or less laceration produced apart altogether from carelessness. On the other hand too rapid delivery, and retaining the blades in position when they might have been
removed, support the statement that a want of due judgment in the use of the forceps contributes to rupture of the perineum.

(2) Passages —

(a) Narrowness of pubic arch and thickening about the urethra. Obviously the effect is to direct the passage more backwards and so the powers are more expended on the perineum. I am under the impression that thus slight differences of breed or race constitute an important difference in various experiences of rupture of the perineum.

(b) Straightness of sacrum — as in ricketty pelvis. Here a want of direction forwards of the presenting part produces an effect equivalent to that of "narrowness." In W
Flockhart's case incidentally mentioned at page 92, there was ricketsy pelvis, and, in a previous labour, total rupture of perineum through the external spincter ani.

(c) Weakening (by disease) of perineum, syphilis, and also I think plaitosis. They are at least contributory causes.

(d) Rigidity, and occlusion of lower outlet by hymen, are examples of rupture by the force employed destroying resistance.

(e) Pelvic exostosis altering the direction of the head.

(f) Excessive breadth of perineum by receiving the force in its centre. This would be an efficient cause if coupled with acetabulum laceration.
(3) Passengers—

Our attention is to be fixed on these chief points— the child's passing occiput-in-occipito-posterior presentations; its sinclavicular and clavicular in occipito-anterior cases; its shoulders, hips, and the placenta in all presentations; and the back in rare cases of expulsion, or of spontaneous evolution.

(a) The specification, large head, occipito-posterior presentations, shoulders (Hart-Barbour, loc. cit.) and the generalisation of Churchill (loc. cit.) mal-presentations and mal-positions of the head—the one being almost equivalent to the other, includes a large number of cases, and yet do not require lengthy notice. Professor Simpson at a discussion on Dr. Matthews-Duncan's paper (Obstetric Journal Vol. V p. 129) laid great—
stress upon the passage of the shoulders as a cause of rupture. I had an unfortunate experience of this in the case of Mrs Mason (No 25 of list annexed): the child's head was born without causing any visible laceration, and I was beginning to think this another case in support of straightening the links, when my anticipation was shattered by both shoulders being literally "shot down" together and the perineum torn for half its breadth. That result, however, it is usually not difficult to avert.

(b) Want of correlation in development between foetal head and maternal passages.
In a paper read before the Obstetrical Society of Edinburgh by Dr. Matthews Duncan (Obstetrical Journal Vol. IV p. 42) the following sentence occurs: "It would appear that in the Darwinian progress of the species the head of the foetus has increased in size more rapidly than the orifices through which it has to come have increased in size or dilatability; and this is supported by an hypothesis to the effect that tear of the cervix uteri, vagina, vaginal orifice, and perineum cannot be a "final arrangement". I trust the species have made no Darwinian progress at all, but apart from that assumption, the theory profound in that sentence possesses an extraordinary fascination for the imaginative powers. So extraordinary is it, that
after perusal and re-perusal, as a criticism I have concluded that should Darwinian progress according to Dr. Duncan unfortunately continue, the recognized mode of delivery must inevitably come to be induction of premature labour or basixys, and that Macaulay's New Zealander will prove to be a medical gentleman native of the antipodes and so of lower species than we—having a longie and basixys in one fold of his toga, and perhaps a cephalotribre in the other!

(c) Too rapid passage of the head—
from small size (Churchule). From small size I believe there is little or no danger; this is constantly observed in premature births; but, unless retarded, a medium sized head with
sharp pain frequently causes some rupture in this way.

What takes place during an ordinary occipito-anterior delivery of the foetal head?

Hart  &  Barbour state (op. cit. p. 573) that when the foetal head is passing through the vaginal orifice it distends it – all round, but of the vulvar orifice the lower half only is distended i.e. it does not stretch those parts of the vulvallying above the level of the urethra or urinaries. From a consideration of the anatomy of the external generative organs, and from observation I can concur in that statement. It implies that the greatest pressure is expended in the direction of the least resisting structure, and points to the perineal skin for traces of its results.
to the movements of the foetal head itself.
the paper of Dr. Duncan already referred
(OBSTETRICAL JOURNAL Vol. V p. 121) says:
"With its sub-occipital region pressed against
the middle of the pubic arch and advancing
wheeling around a point—somewhere about
the centre of the symphysis pubis, the
region of its vertex is advancing rapidly
in a larger circle wheeling around the
same point—and distending the
perineum to its utmost." We
shall take with that view the
measurements of the foetal head given
on the previous page:

Sub-occipito-vertical (3/4" inch behind
anterior fontanelle)
Diameter 3½" inches. Cir. 11¼" ins.

Sub-occipito-bregmatic
Diameter 4½ ins. Circumference 12½ ins.
Sub-Occipito-Frontal (1 inch in front of anterior fontanelle).


The most important of these measurements, so far as the present subject is concerned, is the last—the sub-Occipito-Frontal circumference (and that I have verified in a few instances)—that is the maximum circumference of the series—the thick end of the wedge.

Now the only method which I can conceive as enabling us accurately to determine the curve of exit pursued or followed by the fetal head is to record a pencil-tracing on a thin piece of wood covered with white paper by a pencil fixed over the occipital end of the occipito-frontal diameter, and of such slope might the word be that it would
have for points d'appui the upper margin of the symphysis pubis and the sacro-coccygeal joint—the coccyx itself being movable; the relations of the curve could thus be easily determined. But so far as I am aware it has not yet been tried; and my observations lead me to differ slightly from Dr. Duncan. The movements appear to me to be advance of the occiput as far as about the plane of the sub-occipito-vertical circumference with the above point as the focus of a curve, as Dr. Duncan states; but then the sub-occipital region becomes fixed so that the centre of curvilinear motion of the now passing vertex and sinciput is in or near the sub-pubic ligament, at which moment the power
is applied chiefly to the anterior portion of the head—the sinciput—we might say. This produces what I have frequently noticed, viz., distinctly more rapid rotatory movement of the sub-occipito-frontal circumference. As soon as this circumference, too, has passed, there must be added the retractive resilience of the perineum. In fact, the movement of the sub-occipito-frontal plane is like that of a lever of the 3rd class—the wedge is no longer the form or type—the sub-pubic ligament—is the fulcrum, the powers are the force, and the perineal resistance—the weight. Hence the greater liability to laceration at this moment. Therefore I write as a sub-cause to (c)

(d) rapid passage of the sub-occipito-frontal
circumference. It is clearly apparent that when the foetal head is advancing as well as "rotating" there is less danger. But frequently, when the thighs are drawn up and the larger-circumference passing, there is inevitably "more or less laceration."

The fourth P is Position, and I need scarcely repeat what I mean.

(1) Acute flexion of thighs with abduction as a cause contributing to the production of laceration. Churchill (loc. cit.) speaks of "awkwardly placed" patients being more liable to lesion, but his meaning is certainly not the same as that of Dr. Hurst and myself.

Seldom,
if ever, does any one cause operate in the production of laceration of the perineum; there are many factors and plenty of variety in their combination. The etiology is pretty fully before us. What then is the prophylaxis?

II Prevention.

This necessarily consists in attention not to one, but to a variety of points. (a) Is support of the perineum necessary? Foresee support is to be wholly condemned as (1) "it tends to damage of the vestibule"; (2) it produces reflex greater uterine contraction, just as pressure on the internal aspect of the perineal body augments the pains. As a matter of fact, I have seen more laceration under forcible support with tough flexion than under any other
Gentle support is necessary and beneficial, by the method of receiving the glabella in the palm and pressing with the thumb and forefinger (of the right hand usually) towards the central line of the perineal skin. What is the effect of this method? Simply to supply the distending parts with tissue which may save over-distension. But this cannot be done to the full when acute flexion of the thighs, and perhaps some abduction, have already stolen tissue by the tension they have induced. In some instances any support is unnecessary in the straight-bodied position.

(6) Retardation of head. Churchill (loc. cit.) says that this course is erroneous in theory
and miscellaneous in practice. I venture to think that retardation of the head is of much and decided benefit, especially in preventing the sub-occipito-frontal diameter when rapidly passing from ploughing up the perineum. I have seen nothing but benefit from the practice of allowing the head to glide from the orbit rather than push through. Churchill's opinion appears to arise from undue fear of uterine rupture. But what is retardation of the head as a cause to obstructions in the pelvis where no rupture of the uterus ever takes place? In the one case interference prevents uterine rupture; in the other, that of the perineum. I cannot therefore see the error in theory or mischief in practice alluded to.

(c) Guidance of the head. This should be in that direction in which we conceive
the line traced by the mettus
have already indicated to lie.

(d) Incision frequently prevents serious laceration, it is said; but I do not practise it. Perineums incised are practically equivalent to those slightly lacerated, and the ideal is to have no lesion whatever present after the delivery has been accomplished. 

(e) An arm should be delivered before the shoulders pass. This, if possible, by reducing bulb, would greatly diminish the importance of the shoulders as a cause of laceration. Both shoulders should be prevented from emerging together.

(f) The necessary powers of parturition should be suspended in those cases where we judge laceration to be impending.
This is of course a means of retarding the head, and should be the complement of the retarding (literally) or in front.

In forceps deliveries, the traction should be slow, cautious, and intermittent, and that every attention should be given to extraction. When we judge that sufficient power remains to propel the fetus through the vulva over the perineum, we should remove the forceps as soon as the head begins to expand the skin over the fourchette. Obstetricians have long directed their attention to these and other perinatal saving points, one point excepted, and that the position of the perineum itself. I therefore note lastly as a preventive of laceration:

(h) The straight-bodied position.
Over thirty years ago Professor Sir James Y. Simpson (Selected Obstetric Works p 152) drew attention to the common occurrence of lacerations, and to their beginning as "slight roughness of the mucous surface of the perineum", which is simply a brief description of the "inevitable laceration" of primiparae pointed out only with somewhat greater precision a quarter of a century later by Dr. Matthew Duncan (Obstet. Journ. VII p. 40). Dr. Duncan says that an assistant and a candle are required in order to investigate that lesion. Accordingly, I have not investigated it further than to take the opportunity of placing the tip of my forefinger in the quadrangular depression left by the laceration of the triangular ridge of tissue forming the inferior
margin of the vaginal orifice.

Furthet he remarks at page 642 of the same journal that "when the anterior edge of the perineum alone is referred to, as for instance in a laceration not amounting to half an inch in linear extent, it is called (in his paper) the fourchette."

That limitation or definition of fourchette I adopt—below, with this proviso that the estimate of extent is made at the time of delivery when stretching of parts increases the real extent of lacerations in appearance only, otherwise half an inch of laceration would mean from one-third to two-fifths of the whole perineum, and so could scarcely belong to the "fourchette."

Our subject will be elucidated somewhat by a comparison between
the results of twenty-five primiparous cases delivered by Dr. Duncan's deputies at the Edinburgh Maternity Hospital, which form the foundation of his paper already referred to, — all presumably in what I have designated "the usual sigmoid position," and twenty-five primiparas delivered in the straight-bodied position. Vide list, page 50.

The series is not large on either side, — and if too narrow for founding firm generalizations, yet they serve well to illustrate the results obtainable from the two positions.

The figures regarding rupture of the perineum in the usual position are high. Dr. Duncan in the paper mentioned quotes Schroeder as putting them (for the German dorsal position)
at 61 per cent of injury to the "perineum," whilst—in his own twenty-five principal cases there was experienced on the most-favourable view 52 per cent of perineal laceration. These were natural deliveries. Analysis of these cases shows that in eight there was "no" laceration (of the perineum or fourchette); in four the fourchette was torn; and in thirteen the perineal body was involved by the lesion in the following manner:

In 2 — laceration to verge of anus,
— 3 — 1 inch in linear extent,
— 1 — 3/4 "
— 5 — within 1/4 of anus,
— 1 — "slight"
— 1 — simply stated "of perineum."
That is a category of lacerations of rather more perhaps than average severity.
It is of interest to note that the average weight of the thirteen children is 7 lbs. 6 oz., the heaviest, which brings up the average considerably, weighing 10 lbs. 9 oz., and the lightest 6 lbs., and further that the heaviest child produced (or rather the delivery was completed with) less laceration than the lightest, but the lightest child of the whole series weighed 5 lbs. 2 oz., and with this there was no laceration.

Now, as every attention would doubtless be given to the permanence in that series, it seems to me that part of the causation of these lacerations is to be found in the sigmoid position adopted. As further examples of
what that position can accomplish, I may cite two cases of primiparæ and one secundipara coming under my own observation, but where I was not present when the child was born. (Nos. 26, 29, 30 of list annexed.) In fact, three cesarean experiments on the subject were ready made for me. The first had gone to stool during the second stage, when a storm of pains came on, by which an average sized male child was born - the comparative density of the perineum being ruptured to the external sphincter ani, but fortunately not through it. The second had assumed the American dorsal decubitus (and this is about the only experience I have, or wish to have, of that position) with knees well drawn up & thighs abducted, and the perineum was similarly injured.
The child was syphilitic. The third was the second para, who was in bed partially uncovered by the nurse and in a doubled up attitude. Just as I entered the room the child was being expelled in an occipito-posterior position, and the perineum was as in the two previous cases. It may be objected that the perineum in the first was thin, that the second patient was syphilitic and that the third was an occipito-posterior case—all of themselves causes of laceration. But seldom is any one cause efficient, and granted that these conditions predisposed to laceration, I take it that that event became inevitable from the positions in which the patients were delivered.

Thus we see that the flexed-thigh
attitude is not altogether conducive to the patient's comfort and the conservation of her parts. That is the one side of the question; let us now look at the other side. The results do not form a perfect contrast to those now stated (it is not to be expected that they should), but it will be admitted that they exhibit a substantial improvement.

The analysis of the twenty-five promiscuous cases in my list shows that 12 were forceps deliveries, in 5 of which there was no laceration, in 4 the fourchette was lacerated, in 3 the perineal body was injured. Thirteen (13) were natural labours in 9 of which there was no laceration, 3 of the fourchette, and
1 of the perineal body. Added together we find:

- Forceps 12
- Natural 13

\[ \frac{12 + 13}{25} = \frac{25}{25} = 1 \]

1 laceration was within 1/4 inch of anus, forceps case.

2 — 1/6 inch of anus.

If the perineum is not as torn one-half.

All were full time, occipito-anterior cases — save one.

Regarding the children, I may state that one was weighed for me at 9 1/2 lbs., and there was a case of small premature twins — the exception referred to — a complex case of footling cord and back presentation where by evulsion of the second fetus (the only mode in which I could deliver it) the doubled up back of

*Note: In my paper read at the last meeting of the British Medical Association and published in the Medical Press and Circular of August 22nd, 1883, I had, in error, classed one delivery with short forceps in which no laceration
presented an object to the perineum large enough to bring the case within the range of this argument. I weighed eight of the children myself and the average of five males and three females was 7lbs 8½ ozs. one of these only causing slight rupture of the perineum. The others were not under the general average of 6½ lbs. circumstances such as the superstition of parents that ill-luck follows the weighing of children, the absence of my Salter’s balance (which I had tested as to accuracy), and other considerations prevented my ascertaining their exact weight. The occipito-frontal circumference averaged 13 3/4 inches, the diameter 4 7/8 inches, in nine instances: in four cases the average sub-occipito-frontal circumference was slightly over 12 3/4 inches.
We thus see that the passengers were fully up to the average in all cases, and that the conditions of labour were such as to supply a test.

But the contrast of the two series of cases is unequal. Including aided and unaided deliveries I have 16 per cent of lacerations of the perineum, against Dr. Duncairn's 52 per cent. Yet it is clear that, as 2 of my lacerations arose in forceps cases which constituted twelve twenty-fifths, or nearly half of the total number, if all had been unaided as were Dr. Duncairn's I should have had only 8 per cent of laceration of the perineum. He refers to forceps deliveries at page 647 (op. cit.) as accompanied by severe and frequent perineal injury, and says that instrumental
influence is not yet determined. Opinions, however, may be formed before exact determinations by a Baeomian method are arrived at.

And the opinion which I have formed as to forceps deliveries is this, that, with the exception of cases of rigid pereum or obstructed outlet, accurate attention to the prophylaxis which has been indicated previously, and especially removal of the blades of the instrument before complete extraction of the head, will place these cases on the same level as regards rupture of the pereum as natural labours.

I am free to confess that a desire to avoid the Charybdis below the public arch only vet strike against the Scylla of the pereum may and did partially account for my three forceps lacerations, as traction was probably not made.
exactly in the axis of the outlet — a fertile source of laceration in instrumental cases. But I hope to obtain even better results than 25 per cent. of laceration in these cases.

The general deduction from the two series of cases is that they form good illustrations of the influence of position in labour, and prove that practical benefit arises, ceteris paribus, from the adoption of the straight-bodied or orthodromic position.

Corroboratively, and as leading up to an universal affirmative proposition I may mention in multiparae an occipito-posterior, face becoming occipito-posterior, an occipito-posterior forceps case, a breech, and two cases of podalic version, all delivered in the
straight-bodied position without laceration. These cases only go to a certain extent to show that mal-presentations are not always perverse causes of rupture, and that the straight position assists in avoidance of that accident.

As to the method of procedure: in unmarked cases it is simple and obvious, the patient is placed longitudinally in bed to begin with (unless an extraordinary vaginal examination be requisite), and the limbs are straightened as the presenting part descends upon the perineum. Pressure of the foetal occiput on emerging against the maternal thighs is to be avoided—occasionally by slight abduction of the upper limb, still keeping the limb straight, however. In instrumental
delivers the patient should first lie on his back, placed in the "established English obstetric position"; then the forceps blades, for example, are to grasp the head (or breech) firmly, while the nurse lifts the patient's shoulders so that the body is brought round parallel to the margin of the bed. A little additional assistance places the patient further from the edge but within convenient reach, and the child is delivered, the accoucheur standing in a procedure in which there may be theoretical but in my experience no practical difficulty.

In conclusion, I will point out that non-laceration of the perineum materially influences the convalescence of the patient by removing an acknowledged source of septic poisoning, by preventing
that badly recuperative languor of feeling sometimes resulting from septic absorption in its slightest degrees, and by avoiding a source of irritating sensations of discomfort. The pelvic organs too, receive that measure of support which naturally they are intended to obtain from an unbroken pelvic floor, thus excluding retroversions, prolapse, and other leylears of possibilities from this cause from the category attending early resumption of ordinary duties and the erect posture. So that we may in truth say that a great measure of the health and happiness of woman depends upon the integrity of the sacral segment of the pelvic floor. Any plan, therefore, which can be shown to tend to produce laceration is to be avoided, and that which tends to prevent laceration, even if a little more
inconvenient to ourselves, is to be practised. I have endeavoured to show that the "established" position produces, whereas the straight-bodied position tends to avoid laceration, and I have indicated how the latter position may help to expedite labour. Ought we not, therefore, to abandon the "established" in favour of the straight-bodied position as being more in accordance with the upright arrangements of Nature, and as, on the whole, answering more fully to the requirements of a good position—the convenience and comfort of the patient, the attendants, and the conservation of the patient's powers, parts, and products—all of which aims are as fully as possible attained under this modification of the British right or left lateral position.
List of cases referred to.

Principal

I. Forceps used. (x) No laceration.

1. Mrs. Lewis, 38 Jacob St., Redfern Park.
2. Mrs. Holland, 15 Leura St.
3. Mrs. Edwards, 4 Mornington St.
   Note. Delivered this patient in a second confinement without laceration.
4. Mrs. Stevenson, 10 Beresford Road.
   Note. Dr. Morrish operated, and believed the damage occurred from the position.
5. Mrs. Browne, 39 Blackburn Rd., Belva Dr.
   (x) Laceration of fourchette.
6. Mrs. Murray, 18 St. Johns Rd.
   Note. At the two last noted, Dr. Morrish was present. The three cases 4-6 were those which caused him to acknowledge the value of the straight position.

8. Mrs. Snider, 120 Park St.
9. Mrs. Laird, 26 Lusanne St.
   (x) Laceration of perineum only.
10. Mrs. Ralph, 43 Moses St.
12. Mrs. Birchi, 87 Portsea Rd.
II. Non-instrumental. (a) No laceration.
13. Mrs. Lee, 22 Malta St.
15. Mrs. Wilmumon, 58 Arthurs St.
16. Mrs. Waterhouse, 36 Whitelby St.
17. Mrs. Dyer, 39 Hurry St.
18. Mrs. Boote, 8 Beamish St.
19. Mrs. Davies, 4 Lorenc St.
20. Mrs. M. Nicol, 34 Locksdales St.
21. Mrs. Dodd, 20 Dunchill St.

(b) Laceration of Fourchette.
22. Mrs. Jones, 144 Mill St.
23. Mrs. Antrobus, 8 Denton St.
24. Mrs. Iwills, 46 Smith St.

(1) Lack of permun.
25. Mrs. Mason, 45 Locksdales St.

Cases furnishing negative evidence.
26. Principal, Mrs. Townsend, Uppercross St. 3 delivered
27. Miss Jane, 11 Providence Buildings 7 oppressed
28. Secundipara, Mrs. Greengs, 3 Emerald N. 3 scapito post-
Corroborative cases.

Mrs. Henderson, 42 Willis St.

Mrs. Whornby, 44 Sackett St.

Mrs. Edwards, 10 Shearers St.

Mrs. Simpson, Park Road

Mrs. Bradmore, 89 Moses St.

Mrs. Neil, 4 Bruce St.
On The Convulsions of Reproduction called Periperal Eclampsia, and their Congeners.

Prefatory Note.

It has been my fortune—shall not say whether good or bad—to have attended a group of cases which illustrate the occurrence of convulsive phenomena before, during, and after parturition. The leading case was one of what is usually called Periperal Eclampsia, there was one of Epilepsy, one of Hysteria, and also an example of the affection termed
Incurpital Albuminuria, unaccompanied by convulsion—short memoranda of which last have already been published. These cases will be most conveniently discussed in the reverse order to the above enumeration of them. My plan is to introduce each case, narrate it, discuss its bearings and finally gather up the points of contrast between these four types of affection among themselves, nephritis with uraemia, apoplexy, and the convulsions of post-partum haemorrhage under a general differential diagnosis.

There is one point with which I can best deal in this note, that is—**Nomenclature**. First, as regards a group of cases wherein albumen is present in the urine of gravid women, I agree with those who avoid the name
Incurporal Alburninuria, and prefer to designate this symptom more exactly as The Alburninuria of Gestation, which might not inelegantly, be abbreviated into Gestatory Alburninuria— for the rule is that the albumen begins to disappear as soon as gestation is terminated by parturition, therefore the affection properly belongs to the period prior to parturition, namely, gestation. Further, than this, evidence does not clearly lead, but as the state of the kidneys is in all likelihood one of Congestion (the indications point that way) I am disposed to rank the affection as only the most important and prominent symptom in a disease bearing a pathological name—the Renal Hyperaemia of Gestation.

Secondly,
Secondly, when convulsions supervene, we read, among other synonyms, of puerperal eclampsia, puerperal convulsions, and eclampsia gravidarum, parturientum et puerparum, terms which ought to but often do not indicate a fairly well-defined disease. This last designation seemed to me so ponderous that at a discussion at the Liverpool Medical Institution on a paper by Dr. J. S. Burton, I ventured to abbreviate it into The Eclampsia of Reproduction, as including the three states above referred to in Latin terms. Now, however I am inclined to go further and eliminate the word eclampsia as well. It is only a metaphorical term for convulsive affections, apparently dependent upon the fact that flashes
of light or circles of colour are among the premonitory symptoms of some epileptic attacks. Derived from ἕξαλλος, I lighten or shine forth - it does not in the least convey to the mind the idea of involuntary bodily movements. Professor Carl Brau's thinks the derivation from ἅρμανω appears the more correct, but I do not find support in Liddell & Scott's lexical 2nd Ed. for his rendering (zusammenraffen, sich schütteln, to roll together or shake oneself). The derivation ought to be from ὀφρασία, wriggle or contort the body - ephadasma, a convulsion, and to avoid unequal emphasis, we must use the Greek word γενετικός, of or belonging to the birth of a child, genetic, as equivalent to the words reproduction, or the
phrase of Athen or Ptolem. There is then produced an expression - Genetic sphadasmata - equally classic with that much objected to, almost as unattractive, but more concise and accurate. This would use to describe the symptom were I inclined to part with the plain English term (provisional until the disease be named) No Convulsions of Reproduction, i.e., the convulsive symptomatise of a particular disease arising in connection with a propagation of the human species.
I. The Albuminuria of Gestation.

The Albuminuria of Gestation or Gestatory Albuminuria is a symptom of that condition of kidneys which gives rise to it, and is of two genera, one of which may be termed (not in some cases where a precise diagnosis is impossible must be) Albuminuria Accidental to Gestation. In this sense there have been included cases of chronic Bright's disease occurring before or continuing during pregnancy, - cases of acute inflammatory Bright coinciding with gestation, - cases of albuminuria produced by sudden hyperemia of the kidneys said by Dr. Playfair to result from causes such as sudden exposure to cold, and impeded cutaneous action, with the altered condition of the blood as a
preceding factor, - cases of the species with which Professor Graniger Stewart appears to identify puerperal albuminuria as a whole when he says: "It certainly seems reasonable to suppose that among the peculiar tissue changes which go on during pregnancy, morbid products may under certain circumstances be evolved which are capable of leading to serious irritations of secretory and other structures." That is an opinion couched in very cautious terms. However, from a consideration of the state of the urine in my case of The Convulsions of Reproduction, I am inclined to admit the possibility of that explanation being applicable to a certain proportion of cases of the Albuminuria of Gestation. "Physiological" albuminuria, when definitely shown to exist, will fall under this genus and, lastly, it includes all those cases whether
belonging to the above or not, where the albuminuria is cured, or is recovered from during gestation.

The remainder of the cases constitute the second genus of this Albuminuria. I place my typical case in the foreground for two reasons, first, because the particular condition has been viewed as furnishing the key to "puerperal eclampsia," which I believe it does not, and, secondly, in relegating it to another class, by the process of exclusion I shall arrive a step nearer to the truth as to the causation of the Convolusions of Reproduction.

Case.
Mrs. Lloyd, 3 Elm St., Islington Park, a
principal, aged about 26 years, engaged
Mrs. J. Fox, Morish of Dingle Hill, whom
I was then assisting, on 5th April 1882,
for her confinement which was expected
in the middle of July. She is a woman
of the nervous type of constitution, temperate
in habits, and with comfortable domestic
surroundings. She never had suffered from fits
and was generally "healthy", having only had
what was called an attack of erysipelas in the
leg about a year previously from which she
completely recovers. No history of previous
erysipelas was discoverable, and the temperature
was apparently normal. Her lower extremities
were edematous, and this symptom had existed
for sometime—two or three weeks—previously.
On the 6th I examined the urine and found
a trace of albumen present.
were no observed premonitory symptoms of this condition. Mr. Morrish prescribed diuretics.

These symptoms increased until, in the beginning of May, the patient was confined to bed, being, from the oedema of the legs and labia and debility, unable to walk. There was no puffiness of the face. I thought there was a slight-mitrall systolic murmur; Mr. Morrish thought not. At this time I saw the patient in bed and reported to Mr. Morrish my opinion that induction of premature labour would require to be done to save the patient. On May 10, I saw her again and found her suffering from headache, eyesight not complained of, pulse 120, thirst not pyrexic to the hand. The urine had a Sp. Gr. 1041, contained urate, and was of a drab-like opaque colour. The quantity was 16 ounces in 24 hours.
It was so albuminous as not to be capable of being poured from the test tube after boiling. I had no opportunity of examining for casts. Acetate of Potash, Digitalis, and Decoction of Broom were prescribed by me, and Souther’s tube and puncture were used to reduce the labia majora.

Induction of premature labour was now resolved on.

On the 11th, at 12.30 we introduced a firm elastic bougie, which I had previously carefully rendered aseptic, into the uterine cavity between the membranes and walls, and pains set in about 10 p.m. on the 12th, i.e., 33½ hours afterwards. She had fifty grains of chloral in two doses during the night. At 8 o'clock morning a Barnes' modification of Kellier's bag was introduced, and shortly after ten
we delivered a living female child, of about
seven months characters. The presentation
was normal, in the first position. There
was no convulsion. The child was care-
fully wrapped in cotton wadding, and
packed in a basket— which was placed
by
the side of the fire leaving the face alone
partially exposed. We prescribed
the Jarnier—Donkin diet—shrimp milk.

On the 15th (forty-eight hours after delivery)
the oedema was gone. The urine was of
Sp. Gr. 1031, of more normal colour, with
less deposit, neutral in reaction, albumin
a half of column, quantity 3 points
in 24 hours. A little milk peel
now allowed.

May 24th: the patient was out of
bed. Usual diet being gradually resumed.
June 9th. The urine was of Sp. Gr. 1031, acid in reaction, and the albumen had become reduced to 1/6. Both mother and child were doing well. The patient through her husband was warned that there would be great risk if they did not limit their family to the one daughter.


Nov 27th. Urine paler than straw color; reaction acid; a faint doubtful trace of albumen. Quantity fair average. Slight mucous deposit.

Dec 9th. Patient complains of a little lumbar pain; otherwise feeling well and
performing her ordinary duties, keeps healthy, no cardiac murmur. The child is "a fine baby. She had removed to I Alder Street."

That is the record of her first pregnancy and parturition. Warning being unheeded, I am fortunately able to present the sequel in an account of the second pregnancy and delivery.

On 26th April 1883, the patient was supposed to be in the fourth month of her second pregnancy. The urine contained no trace of albumen to heat and nitric acid or to picric acid. June 4th. Labour is reported to be somewhat swollen: no albumen in urine. Otherwise well.


Quantity twenty ounces in 24 hours but variable. Oedema of labia remains.

The urine was examined every four days (with similar result—save that there was not even a doubtful trace of albumen found) until 20th September 1883 when labour for the second second time began at 5 p.m.

At 10 p.m. I found slight oedema of labia, pains regular, fairly strong; 0 or 1 inch diameter; membranes entire: head presenting.

At 3.20 a.m. of 21st there was full dilatation of the os, but pains short and feeble. The head was in the first position. I ruptured the membranes but neither internal rotation nor advance followed. A draehm and a half of Extractum Bryotae Liquidum strengthened
the pains, but without effect on the progress of the foetal head. Patient was becoming heated, and begged "help", which was at once given by Sir James E. Simpson's long forceps (my own being then in London) with the result that a male child weighing 8 lbs. was extracted at 4.40 a.m. in the straight-bodied position with rupture of the fourchette only, which remained entire during the first labour. No anaesthetic was administered. I expressed the placenta as usual. Pulse 85.

11 a.m. Pulse 72. Has micturated twice. Moderate lochial discharge. No pain whatever, and felt comfortable after having slept a little. Child also doing well.

22."—Favourable state continues.

Oct. 4.—Patient rose from bed. She said she delayed getting up in order to make sure.
Next day I examined her urine which was of a pale straw colour. There was a flaky white precipitate on boiling, dissolved by dilute nitro-hydrochloric acid - phosphates. Patient well.

On Oct. 17th Mrs. Lloyd was enjoying her usual health and the child was in a thriving condition.

It is not necessary to multiply examples of this condition. I shall only refer to Dr. Angus Macdonald's three cases (Obstetrical Journal Vol. IV pp. 139-140).


2. M. K., 29, unmarried, primipara. Dropsy about 2½ months before delivery. Urine
as in Mrs. Floyd's case. Supposed nephritis 5 years previously. Juxta.
Recovery. The serena went down, and in 15 days there was no albumen.


An example of a case which I hold could not have been of mechanic origin, but "accidental" to the pregnancy, is Dr. Arsoy's from the Bulletin Général de Therapeutique, of a principal where there were "premonitory symptoms of eclampsia", oedema & albuminuria at the 8th month. But after giving 80 grains of Bromide of Potassium daily there was no fit, and both mother and child survived.
The Diagnosis in Mrs. Lloyd's case proceeded upon five plain facts: (1) the clear previous history of freedom from Bright's Disease; (2) the presence of undoubted signs and symptoms of pregnancy; (3) of albumen in the urine; (4) of dropsy of the lower extremities; and noteworthy absence of facial oedema; (5) and the absence of pyrexia. Thus it was simple and easy enough to reach the opinion expressed in the words "puerperal albuminuria". But—this is rather broad knowledge. Let us therefore look a little more closely at the details under the headings of (1) Pathology and (2) Treatment.
I. Etiology. To many minds a conclusion as to the etiology of albuminuria has been largely equivalent to a determination of the causation of puerperal eclampsia, they have regarded the two subjects as inextricably intermingled; their belief is comprised in three words of a descending series of conditions, pregnancy, albuminuria, eclampsia. (Witn. Dr. Athill's dictum in discussing Dr. More Madden's paper read at the Dublin Obstetrical Society, 9. May, 1874: "albuminuria and eclampsia are synonymous.")

Dr. Lever, to whom belongs the Guy's Hospital Reports 1848, homage of being one of the first-two observers (Sir James Simpson, as we shall see, was the other) to connect albuminuria with convulsions in the puerperal state, was of this class. He was distinctly of opinion (p. 514 loc. cit.) that the pressure of the
pravid uterus on the emulgent veins is the cause of albuminuria, and having arrived at a similitude of Morbus Brightii he makes one futile search for urine in the blood, then, without thoroughly spanning the gap between albuminuria and eclampsia, leaves the impression that puerperal convulsions are caused by the uræmic condition only. This was the too extensive generalization of first-light. What followed? I shall only call two or three of the leading authorities as witnesses on either sides, and comment shortly afterwards.

Pressure or no pressure is the issue raised. On the one hand, we find that thirty years after Seely wrote, Dr. George Johnston divided puerperal albuminurias into four classes—

I. Those with Chronic Bright's disease.

II. The pressure class in the later months.
of pregnancy, which he declares are applicable on no other theory. IV. Where the albuminuria comes on early; and IV. where it appears a day or two after delivery.

Dr. Playfair (Midwifery p. 223) believes in pressure + altered blood.

Dr. Dickenson (Pathology and Treatment of Albuminuria p. ) categorically affirms the pressure view.

Dr. Angus Macdonald, writing of Albuminuria without Eclampsia (Abstetrical Journal vol. IV pp 139-144) points out that one of his cases in particular supports this view, adding that over-distension led to renal engorgement.

In the course of a discussion on Dr. Macdonald's paper (loc. cit. p. 328) Professor Simpson held to the pressure view and pointed out that the ovarian cyti-
was not completely analogous to pregnancy. On the other hand, I may cite Dr. Matthew Duncan's cautious demand (loc. cit.) in the course of the same discussion (to which Professor Simpson's opinion was a reply) for proof of pressure on the renal veins in pregnancy, as causing albuminuria. He had seen albuminuria with and without convulsions occurring, and also recovery from albuminuria taking place during pregnancy, and he put forward what I may briefly designate as the abdominal humour objection. The nature of this objection is defined very clearly by Professor Bright. Dr. Graniger Stewart. "That (Dr. Lever's) view," he says, "has been accepted by many, but it is disproved by the facts that the renal veins are often subjected to much greater pressure in cases of ovarian and uterine
tumours, without renal disease, and that albuminuria occasionally occurs early in pregnancy, but yields to treatment while the pregnancy goes on to its natural term. Bartels (Concilius Canis Vol. XV) under the heading Acute Parenchymatous Nephritis of Pregnancy, concludes that “while we must reject the mechanical explanation he must admit— all the other explanations advanced to be untenable.” Bartels obviously means that obstetric medicine is not yet an exact science, and that we cannot write J. E. D. after everyone of our reasons from clinical or even pathological observation. He alone, in the places cited, of the authorities mentioned distinctly takes into consideration before giving the above judgment, the subsidiary question whether the renal veins may in this “nephritis” of pregnancy, be abnormal
in their distribution. He states (loc. cit.)
that the position of the renal veins protects
them from pressure, and in a note he
that Heller found the left renal vein crossed
the second lumbar vertebra, he having examined
a large number of bodies. This
proves nothing, as it may be assumed from
the fact of the large majority of primiparae
escaping without, or without serious,
albuminuria, that the veins are usually
in them, as we should expect, in a
normal position; and besides, it is possible
that the very next case Heller might have
examined, would have upset his conclusions
by presenting a very abnormal distribution
of the veins.

Now, and herein lies the answer
to Professor Simpson's question as to eclampsia,
(which is quite as appropriate to this
subject, and I am bearing in mind that)
much of what I have quoted has been called forth by the subject of eclampsia gravidarum etc.) why does one woman become affected rather than another? viz., as regards a large proportion of the cases, that the veins are abnormal in the few, normal in the many, and so are subject to pressure sufficient to produce renal hyperemia and consequent albuminuria in the one case, and exempt in the other. However I must briefly sum up these opinions.

Dr. Seaver's ideas were too general. Had he said that there is a class of cases of albuminuria occurring during festation, caused by uterine pressure on the renal veins, and terminating in uremic and uremic convulsions, I would have entirely agreed. Still his general
proposition so far includes my particular one, and so supports it.
I may point out that Dr. Johnston's class I falls under my division Accidental albuminuria; that his Classes IIases must be amalgamated to meet the conditions of mechanical pectoral albuminuria, as here the albumen appears as early as between the 5th and 7th months at all events, and persists late, until gestation is terminated. Class IV does not come under "gestation but parturition being over, puerperium. The "plus something else" of Dr. Playfair is not essential, as he grants pressure, and that is acknowledged by pathologists to be an efficient cause of albuminuria. Jones & Schering's Pathology p. 671 referring to Dickinson's
work: Greer's Pathology, p. 381, to a certain extent, but not directly, corroborate.

With Dr. Dickinson and Dr. Angus Macdonald, I agree, but would make this form of albuminuria stand out even more distinctly as a genus only.

Professor Grainger Stewart and Dr. Duncan stand on the same opposition from the abdominal tumour and yielding to treatment "objections, and their opinions on such a subject are of very great weight. Still, I answer them in this way: (1) I admit that if an albuminuria yields to treatment other than emptying the uterus, that particular case has not been of mechanical origin; it belongs to the genus Accident to Pregnancy; (2) it is obviously irrelevant to apply this objection to any case in which the albuminuria continues until then
termination of gestation, which albuminuria of mechanical origin must do, and we look to the facts of such a case as Mr. Lloyd's to remove that "if." The objection, therefore, only amounts to this that a proportion of the cases of Albuminuria of Gestation are not of mechanical origin, a proposition which I not only admit, but, at the outset, plainly aver. The "abdominal-tumour" objection is dis异地 of by Professor Simpson who pointed out that there was no complete analogy between it and the gravid uterus. Its form, mode of origin, direction of growth are more or less different. Then, with Dr. Duncan, I might demand proof of the much greater pressure exerted by these tumours on the renal veins without renal disease appearing.
Barter's is over-exacting; we can see that he requires the most obvious proof, such as might have possibly been afforded by an examination post mortem of Dr Angus Macdonald's patient who died of peritonitis, had circumstances permitted it.

Which side does my case lead me? Consider (1) that in the fact of proncipality there are the elements, firstly, of rigidity of the abdominal walls partially preventing the uterus from expanding upwards and forwards leading thus to counter-pressure downwards and backwards, i.e., in the direction of the large pelvic and abdominal veins which are occasionally abnormally distributed (Gray's Anatomy p. 446, top).

(2) The progressive nature of the edema and albuminuria. They might be said to increase almost concurrently with the
rise of the uterus in the abdominal cavity. First the legs were affected early— they were oedematous before she was seen at the sixth month nearly. A month later both legs and both labia were very oedematous, but puffiness of the face or lower eyelids was not noticeable. Then the albumen which was only at first a trace gradually became very large in quantity. (3) Medicinal treatment proved of no use: the urine continued diminishing in quantity and increasing in albumen in face of it.

But (4) oedema, albuminuria and other symptoms began to disappear with astonishing rapidity after the uterus was emptied by the induction of premature labour. This observation is a very prominent feature of such cases,
and is very well indicated by Dr. Cup's Macdonald's cases 2 and 3 cited at pages 70 and 71.

The balance of these opinions, and the facts of Mrs. Lloyd's case, then, oblige me to hold, as I have already stated, that there is from pressure a mechanical gestatory albuminuria, and to reject the theory that all cases of the albuminuria of gestation are explicable by circumstances arising out of the pregnant-condition itself whether from the production of morbid products or otherwise.

On the general subject, I shall only refer to the recent discussion on the Pathology and Clinical Significance of Albuminuria just terminated in Glasgow, the last report of which appears in the British Medical Journal of 12th April 1884 p. 119.
It led, as the President remarked, to very great diversity of opinion regarding the immediate pathology of the affection; and any one can see that no generalisation as to the mode of escape of the albumen is possible—even the leading theory of secretion or escape in the glomeruli and reabsorption from the tubules finding little acceptance.

Treatment.

Clearly the treatment applicable to acute and chronic Bright's disease is that to be decided upon for those forms of albuminuria which are "accidental" to pregnancy. Success may result from local applications, with the Tannier-Dorhkin skin milk, plan of diet, or with Digitato...
or one or a combination of the other
afflictions. In some of these cases it
might even be advisable to terminate
the pregnancy.
But in the mechanical form medicines
acting on the kidneys are of little use;
indeed raising the blood pressure on the
arterial side probably does harm owing
to obstruction on the venous side of the
renal circulation. Two alternatives in
such obstructed cases lie before the patient—
labour or uraemia. Mrs Lloyd seemed to
one to be on the borderland of these alternatives.
Some threaten uraemia early, as she did,
and require the induction of premature
labour; in others the kidneys will be
relieved either by an abortion or by
delivery at term. Treatment, here,
therefore resolves itself into a decision of the
question. Am I to induce premature labour in this instance or not, and if so when, and how? The chances of life and health, present and future, of the mother, as well as the immediate and remote chances of life and constitutional strength to the child have to be considered. And it is to be noticed that a kind of inverse ratio of probabilities obtains. Should labour be induced at the 7th month, for example, the probabilities are that the mother's life will be saved now, and shortening of it from chronic Bright's avoided: she will fully retain her usefulness, but the child will have all the diminished chances of life involved in premature birth and mal-presentation. I need not point out how Mrs. Lloyd conforms to the above probability in
actual fact—beyond emphasising the statement that she remained well and free from kidney disease after a second and moderately severe labour. The first child's state turned out to be exceptionally favourable. So that the result furnishes the highest attainable standard in such cases or nearly so.

Should we resolve to permit the pregnancy to run its own course, and, instead of an abortion, which would be equivalent to an induced labour, occurring, it went to term we permit our patient to face the dangerous possibilities of uraemia or intercurrent complications, and the probability of chronic kidney disease, especially lighter up in the event of subsequent pregnancy, whilst as regards the child, if it be born alive at all, its life chances are greater
than those of the premature infant.

Some idea of the immediate result to the mother is gathered from the figures given on page 163; what the condition of some of the remainder would be may I think be fairly gathered from another source—Dr. Dickinson’s case of Amelia Icale (op. cit. p. 163) where there had been oedema of the legs and other symptoms of renal affection in six pregnancies, premature labour supervened about the 7th month, and the patient died of exhaution two days after; and the case of the servant girl, a pinnipar quoted at page 166 of the same work, where there was albuminuria of gestation and death from anaemia three years after first appearance of dropsy. Permanent alteration of the
Kidney structure had taken place in these cases. We wish to avoid such a result, and the induction of premature labour enables us to do so with very little risk, if carefully performed. Churchill gives Vulpian's statistics as 115 children saved out of 161, and eight mothers dying, but 5 of them from causes not connected with parturition. One mother in 53 was the death rate of parturition in which the operation was performed, compared with 1 in 19 of Litman's albuminurics, above referred to, dying of "eclampsia"—a comparison, however, which yields only little light on account of dissimilar conditions. But Churchill also (loc.cit.) states the views of various obstetricians, and the consensus of opinion is, that if
properly performed, the operation is practically a safe one. This I believe to be nearer the truth: what an advantage as regards the mother at all events does the operation give us then! My experience of it is limited to two cases, Mrs. Lloyd's, and another for contracted pelvis in which I took an active part. This patient was Mrs. Storckhart, 47 Aberdeen St., Stockton, Park, a dwarf multipar, aged 32 years, who was delivered of her first child by craniotomy, of her second by version, and of this her third by induction of premature labour, in which the child presented with the right arm over the head. I therefore picked it out with the forceps, placing the upper blade between the arm and head. The mother had no bad symptom, and the child survived three hours.
three and a half hours in Mrs Lloyd's 12½ in Mrs Fishburn's were the intervals before labour began.

The best methods of induction of premature labour at present devised are well represented by these two cases - in the one by the introduction of a foreign body between the membranes and uterine wall, in the other by dilatation of the os. In the one a gum-elastic bougie, powdered thoroughly aseptic by separate washing in liquor potassae, carbolic acid, and iodine was used: a catheter ought never, in my opinion, to be employed. It has two surfaces to be rendered aseptic, causing difficulty, and its canal cannot but convey some air, bearing, in the absence of the antiseptic spray, living microbes giving rise to a possibility in the circum-
stances of a septic metritis. Dr. Angus Macdonald's case on page 70 is a pointed example. A bougie, however, of just sufficient softness to be easily guided overcomes these objections, and, as a rest, if the operator should be unfortunate enough to push the instrument into or through the uterine wall, the operation cannot altogether have that laid to its account.

In the other case the multiparous cervix yielded soon to Jaits dilators followed by Barnes modification of Keillor's bags also all similarly rendered aseptic, and they answered well. I see little objection to the commencement of dilatation by a sponge or laminaria tent (the latter mostly for primipares) as the stage of the cervix might indicate. Continuing dilatation by Jaits dilators, [as I did in a case of incomplete abortion (McLachlan, 5) [Machin]).]
and finishing it, if necessary, by preferably Steele's improvement of the indiarubber bags. This should seldom be delayed beyond the seventh month, and where the symptoms are severe should be done at the earliest possible moment.

As regards a repetition of the pregnancy, I would retreat somewhat from the position which I originally took up in Mr. Lloyd's instance (experience compels most men to retreat a little now and then) and hope that, in a similar case, the physiological as well as mechanical relaxation produced by the first pregnancy might permit of a second misapplication and repetition as soon as the urine was free from albumen and the patient's general health established, without evil results following.

We conclude then that many of
the cases of the Albuminuria of gestation are of mechanical origin - that the remainder originate variously - that they all tend to terminate in delivery, or in uraemia, when, if convulsions do occur, they should be called Uraemic Convulsions, and not be nick-named Periperal Eclampsia although occurring in a periperal woman. It is most difficult to do this, but we shall find it is to a great extent possible. Thus the subject of the Convulsions of Reproduction is so far narrowed. These convulsions have been divided into the groups hysterical, epileptic and apoplectic periperal convulsions or eclampsia. That division is misleading and erroneous. Accordingly, I shall endeavour further to narrow the
circle of the subject of convulsions by eliminating one by one, all the three
divisions, and reducing the Convulsion of Reproduction to a disease sui
generis.

I begin by narrating a case wherein hysteria manifested itself after
delivery.

Case II. Hysteria in the Puerperal State.

Mrs. Helston, Weston Gr., Isalteth Park, a midwife was attended by a certified
midwife of ten years experience who sent an urgent message to say that her
patient had a convulsion immediately after an easy delivery.

On arrival, I found the patient speechless, with convergent strabismus, medium pupils,
placid face, cool skin, pulse 42, and a peculiar jerking of the right-lower extremity. She was evidently not comatose. The uterus was contracted, but not very firmly; there was no post-partum haemorrhage. No oedema anywhere.

The previous history included hysterical fits in which there had been quivering of the eyelids. Being now questioned sharply as to a "ball in the throat," she pointed to her throat.

She would take nothing from the attendant or friends. So I commanded her to swallow (for the uterine case) 3 i Extracti Epsrat Liquidi in water, which she easily did to the surprise of relations and midwife. Ordered Chloral Hydrate grains XV and Potassius Bromide grains XXX in Syrup of Orange.
and water to 7 ½ glass, and
informed the persons present that the
case was one of hysteria, that it was
nothing serious, and that they could
let me know if she required to be seen again.

In presence of the patient, I asked the
midwife to draw off some urine with
a clean silver catheter and bring it to
me that evening for examination, my
object being to prove a negative, as I
did not of course expect to find a
morbid quantity of albumen present.

This specimen I never got for, at the
sight of the silver catheter, an hour
after taking the draught, the patient
made a somewhat sudden recovery.

Remarks. Here the power of suggestion was
no doubt stimulated by the prospect of
what seemed to the patient an "operation"
being performed upon her, and doubtless also recovery was aided by the draught administered. I must, however, explain that I did not prescribe "a clean silver Catheter" as an adjuvans to the chloral and bromide draught, and did not anticipate its singular efficacy. My reason for the specification was that, knowing the midwife (for whom this one attended and from whom she would have got the catheter) had catheterised a case of Metric which I had seen for her, with a soft-rubber instrument, I was particularly anxious that that rubber instrument should not come near this patient. Hence emphatically asked a clean silver catheter to be used.

This patient exhibited an attack of that protracted disease, hypertonia, chorea gravidarum, was out of the question...
under the circumstances of delivery. Cases of the kind have been recorded as puerperal eclampsia; an instance is found in m.35 of table page 141. But that is not only a misnomer, it is a distinct clinical and pathological error. I shall afterwards (p. 138) refer to the differential diagnosis, and meantime I shall only say that in a case like this the previous history, aspect, symptoms and signs presented are indicative of hysteria alone — on one symptom especially must I lay considerable stress — the skin was cool, i.e., in hysteria the temperature is normal. Lately we heard something of 'nervous temperatures,' but in them the fallacy of vindicating is present or at least they have not been proved free from the fallacy, on the contrary, a
temperature of 12.9° F. has been admitted to have been produced by a hot-ray secreted under the pillow. Body temperature therefore greatly assists the formation of a correct diagnosis and prognosis. We may thereby much relieve the minds of all concerned (ourselves included) of anxiety cast, the result in such a case.

One of my objects in bringing forward this case was to show that Dr. Angus Macdonald was leaning on a reed when he largely relied on the statement of an 'intelligent nurse' in the case to which I shall afterwards refer, narrated at p. 243 of his book on Heart Disease in Pregnancy, 1878 edition, as a case having the balance of evidence in favour of puerperal eclampsia. In Mr. Kelston's case even a comparatively
cool certified midwife of long experience made panic observations. Of how much less value then under more grave circumstances is the testimony of a monthly nurse.

But this argument against too much stress being laid on the statements of an attendant is too-edgy; it tends also to detract from the value of the evidence of my next-narrated case being one of epilepsy.

Nevertheless, I believe the diagnosis will be justified, and accordingly present.

Case III. Epilepsy before and after Parturition.

Mrs. Butterworth, 16 Carter St, Liverpool, was a principal, aged 23 years and 9 months, with a refined pale complexion,
well-nourished, and in fairly comfortable circumstances.

Previous History. She had suffered from bronchitis and congestion of the lungs. Two points are of special importance.

(a) When 14 years of age, in trying to save a child from being run over by a "car" (cab) she got a blow on the head and had five fainting fits within three weeks after. She is strongly attached to the euphemism "saints". I only mentioned the word "fit" to her once, but took the precaution to explain to the friends that she might have a seizure of the kind indicated, and which I suspected was epileptic, in the course of her confinement. Patient is not much inclined to speak of these attacks within the last nine years, but mentions one
as occurring a year ago.

A friend of the patient informed me that about three months previously (June 1883) when in the Isle of Man she took a fit, and that the medical man who was called to her had said on seeing her: "you have sent too late for me," and bade them good morning so like death was she!—a somewhat singular incident.

On 21st Sept. last (1883) she had a prolonged fit. I afterwards examined the urine and found no albumen in it. Otherwise she was healthy.

(b) Also in the year of the above accident she was assisting a servant to clean a window, when the step ladder gave way, and she fell to the ground—descending on the glutens region.

Labour
Labour began on Friday, 28th Sept., and I attended her late that evening. The presentation was left occipito-anterior. Progress was slow until the coccy-
public plane was reached, when in consequence of narrowing of the pelvic outlet, the descent of the head was arrested: there was an incurve coccyx and ankylosed sacro-coccygeal articu-
lation. The alternatives before me were culdotomy or forceps, and I decided that the mother must run
the risk attending fracture of the coccyx for the sake of a living child.
Accordingly I applied Sir James G. Simpson's long forceps. On making pretty firm traction a slight, then a louder snap was heard, and the head was delivered with ease thereafter.
the patient being in the straight-bodied position, and not anaesthetised. The
perineal body was slightly tender to pm. The child, a male, weighed 7 lbs; its
sub-occipito-frontal circumference immediately after birth was 13 to 14 inches; and
the cord was over its shoulders. Parallel to the sagittal suture and near to the
posterior superior angle of the right parietal bone there was a depression ploughed
in the bone apparently before internal rotation had fully taken place. The scalp over this
depression seemed uninjured. Placenta and membranes were expressed in five minutes after the child's
delivery. Discharge free: ergotae
extracti liquidi 3 ii prim. Friday August 7: feels fairly comfortable. 
Shrewsbury
Spergularia. She remained well until 2nd Oct., when she had an attack at 7:30 p.m. in which she was described as turning her head to one side, "working" with the mouth, clenching the hands, and being wholly unconscious of her nurses clapping or her surroundings. I was sent for in haste, but on arrival in about an hour found that she had regained consciousness. There was a slight flush of cheeks, tellin of rise of temperature which was 102.6°F. pulse 120; skin moist; tongue normal; breast engorged with milk. The bowels had moved under 1/2 Al Ricini, Glycerini an 3 of Al. Cinnamonorum pt.iii, taken in the morning. Lochia present; red; had urinated freely with no abnormal colour. Complained of abdominal pain, which she said felt as if proceeding from
below upwards before the attack, also of some coccygeal pain on motion.

Ordered turpentine stipes to abdomen, and by Sir. Ammon. Aq. 3 fp, A. Acast. 3 fp, Sir. Pulv. 3 iii Aq. aquae 3 iv. Sir. 3 p 4½ houria summand.

Oct. 3rd morning. She had ½ pain solid opium every two hours for the pain.

No albumen in urine of previous night. Sp. gr. 1021: Acid. Symptoms improved.

4½ a.m. Pulse 100. Temp. 102. 4½. Pain felt over lower half of sacrum from sacro-coccygeal articulation. No abdominal pain on pressure. Discharge, milk, and urine as before. Ordered Comp. Belladonnae to sacrum & coccyx.

5½ a.m. Pulse 84, Temp. 100. Sacral pain relieved by the plaster.
temperature and other symptoms varied between this and the 13\textsuperscript{2} when she was still in bed with slight pain on pressure in the left inguinal region. Otherwise feeling well, but still somewhat weak. Pulse 86. Temp. normal. No recurrence of convulsion. Child doing well: head depression almost gone. She got up from bed on Oct 21st, and subsequently made a good recovery.

Remarks. Whether the diagnosis of epilepsy in this case is an exact one depends to a certain extent upon the accuracy of the observation and evidence of those about the patient. The description of the fit, however, tallies with that of a moderate attack of the grand mal, and I see no reason to doubt its substantial
accuracy. Then the history of a severe shock, if not actual injury to the nervous system at a susceptible age, followed by attacks which formed a chain from that time until after her confinement, combine to form a body of the strongest circumstantial evidence that the case was one of epilepsy. As regards temperature manifestly in a case such as this we cannot hold it as important to the diagnosis of the nature of the convulsion. The rise took place at the time of establishment of the lacteal secretion, and we may also see a reason for it in the results of the injury to the coccyx. Note that there was no albumen in the urine passed during the night succeeding the fit of Oct 25, and none found before labour. Albuminuria is frequently
found in the urine of epileptics immediately after the fit, as pointed out by Feoni of Cagliari quoted by Dr. Burton in the paper already referred to. And it is found after any convulsive seizure—Finlayson's Clinical Manual p. 189. Here, the absence of albumen was not a point affecting the diagnosis as the urine examined was not the first passed after the fit— but I believe it favourably affected the ultimate prognosis of the case.

The other aspect of this case is found in the state of the coccyx, and in the events of the forceps delivery. It is only relevant to my subject so far as it illustrates the fact that severe injury may be sustained by or inflicted on a paralysed epileptic, presumably in a condition of "metabolic nervous equilibrium."
without producing an attack of epileptic convulsions. It bears upon the question, whether and how much interference is warranted in puerperal neuroses?

The prognosis depended almost entirely upon the effects produced by the injury and counter-effects of treatment. The fit or a recurrence of it gave very little anxiety.

The treatment was mainly prophylactic, inflammation. I did not deem it necessary from the long intervals and moderate severity of the attacks then to prescribe the time-honoured Bromides, recent Hydrobromic Acid, or nervous tonics, and the result justified my anticipations in this respect. Indeed, the patient made ultimately what is called a good recovery.
We have now arrived at this position that all cases of mechanical, and many of Accidental Gestation Albuminuria, if terminated by convulsions, are purely uraemic, and that so-called hysterical and epileptic puerperal convulsions are simply cases of the diseases Hysteria and Epilepsy occurring in connection with reproduction. Apoplectic and other convulsions will be considered under the differential diagnosis. Here I will only say that those cases are cases of Apoplexy still—minute sand-grain apoplexies apparently from long continued vascular spasm, as found in cases of uraemic convulsions, for example, excepted.

What then is the disease Puerperal Eclampsia or Convulsions or the Convulsions of Reproduction or whatever
we choose to designate it? and why, repeating Professor Simpson's question is one female attached by them rather than another? Can we discover the logical "sufficient reason" why it is so rather than otherwise?
Intraperal eclampsia or The Convulsions of Reproduction.

That the occurrence of eclampsia in connection with the reproductive state is a complication fraught with tremendous issues and appalling to any mind which contemplates the circumstances in which a new and a mature human being are involved are points upon which all who have witnessed such cases are agreed. Beyond even this there is the utmost diversity of opinion. What the "reason" is, is answered by various theories, treatment is very varied, and even the "necessary" symptoms of the condition are often misconstrued. There is, in short, a welter of opinion on the subject.
Puerperal Eclampsia has been called a disease. Dr. Playfair, for example, refers to it four times as such at p. 289 of his work on Midwifery. But plainly it is only a symptom of a pathological condition which causes or permits a discharge of nervous impulses to the various muscles affected, and finds some analogy in ascites as a symptom of cirrhosis of the liver. Until that pathological condition has been clearly demonstrated we must be contented with applying the name of the leading symptom—convulsions—as the provisional name of the disease, until the pathological condition is more definitely settled than by balancing well-constructed theories.

I shall define the disease itself as follows: A disease of the nervous
system, connected with child-bearing, manifesting itself chiefly by tonic and clonic muscular spasms, accompanied by a rise of temperature, and frequently by albuminous urine.

In following the line indicated by the case to be narrated, we keep in mind that considerations as to pathology and etiology are only subsidiary and entirely subordinate to those of diagnosis and treatment: time presses in such cases. Accordingly, I shall take events in the following order.

I. The History or Statement of Facts.
II. A table of 89 cases reported as "eclampsic". Analysis.
III. Diagnosis.
IV. Prognosis.
V. Treatment
   A. Natural.  B. Artificial.  The line pursued in my case.  Other treatment per Table.

VI. Some of the Clinical Features of the case.

VII. The pathology of the disease, and Etiology of its chief symptom.

VIII. Differential Diagnosis.

IX. Conclusion.

I. History.

Case IV. Mrs. Browne, 1 Blackburn Terrace, Beloe St., Toxteth Park, Liverpool, was a principal aged 24 years, about 8½ months pregnant, to whom I was called on \( 18^\text{th} \) September 1882 at 9.10 am, with the information that she had had a fit a few minutes previously.

At 9.15, found her...
conscious, with pale face and half open mouth, pupils dilated, intelligence obscured, but able to recognize me as "the doctor".

Before any further inquiry could be made she had another convulsive seizure of which the following are the principal features: First, there was a slight un-easy toss about; she called "mutter, feebly; the fingers of the right hand lying outside the bedclothes twitched; the pupils dilated; mouth closed but not clenched, then within a second, clonic contractions of the voluntary muscles generally, marked by the orbicularis palpbralium, scleritis exhibited; but then with the clonic contractions there came tonic contraction of the respiratory muscles during which there was no perceptible respiratory act,
and cyanosis supervened. The aspect could only be described as ghastly. There was frothing at the mouth with apparent attempts at spitting to clear it. There was no general juncture and no twisting of the head and neck to either side. The tonic spasm of the respiratory muscles passed off after a clinch during which death seemed threatening from asphyxia, then came a slowly drawn and sobbing respiration, disappearing of clonic contractions and of cyanosis. Tranquil asthenic followed, and coma was deepening. Position on the right side throughout the convulsion which lasted 45 seconds. Pulse 108 after. Lungs and heart were healthy. I now procured a little of her urine, which had been passed before.
the first seizure, and at once boiled it in an iron spoon. It was so albuminous as to resemble Devonshire cream.

 Ordered a draught—containing 20 grains of Chloral Hydrate and 40 of Bromide of Potash. A piece of wood given to place between the teeth was not hard bitten, but there was a slight tinge of blood from the tongue having been nipped in the first fit.

 Having done this, I turned to Mrs Browne's mother in order to ascertain one or two facts regarding the patient's previous history. Mrs Browne had suffered from muscular headache and stupidity for a fortnight previously attributed by her to want of regular motion of the bowels for which castor
oil was several times taken. She had never had "fits"; had been "wild," and was now only four months married.

Subsequently the chemist who dispensed the medicines for the patient voluntarily informed me that she had asked him previous to her marriage for "Bitter Apple," which, suspecting its pernicious use, he refused to supply.

From the second fit she was in a condition either of obscured intelligence or absolute coma.

The draught producing no apparent effect, I gave Nitrite of Amyl by inhalation, the ordinary dose of five drops being given from the usual twisted paper filler. This distinctly warded off two fits while
I was present, but I found that between the warming twitch of finger and administration of the drug there was no time to turn to the piece of furniture on which the bottle was standing, unhook it, and drop the Nitrite on my filler, in order to produce any effect as indicated by bright flushing of the face. The pineapple perfume pervaded the premises. Chloroform was tried intermittently, being inhaled about the expected onset of a convolution, but could not be given effectually.

I therefore saw that if the idea under which the Nitrite was administered was to be further acted upon something with more lasting effect would have to be given. Consulting with my then chief, Mr. Harrow, at 1:30 p.m. I
suggested Nitro-glycerine which had
been reported in the British Medical Journal,
22nd July 1882, p. 573, as giving very good
results. This was agreed to, and, a 1 per
cent. solution being procured from town,
at 3:55 she had one minum in water,
at 4:25 another minum, and at 6:25
a third. Suffusion was produced—the
tint of complexion being a neutral between
bright-arterial and venous congestive hues.
At 6:20 the co-admitted the firefinger.
During the first four hours
there were five fits; an average of one
in 48 minutes. From this time until
5:40 seven more; and from that
hour until 7:45 there was, the friends
informed me, first a single fit, then two
following each other, and for ten minutes
by the clock (7:34-45) a chain of at
least three convulsions marked by very ill-defined intervals.

By the time I had witnessed three or four fits, I began to hail the asphyxia as ending the fit.

In the evening, we resolved on aiding labour by puncture of the membranes and insertion of Barnes' adaptation of Heill's bags. At 8.55 this was done. At 9.15 there was a recurrence of a double fit: the duration was now 1 min. 10 seconds. Pulse 132-156 per minute—accelerated to latter rate during the seizure. Twenty fits since morning. The average interval during the last eight hours is reduced to nearly half an hour. 'Timed' one or two intervals at 2'
minutes.

10.30 p.m. again two fits - there was less jerking of eyelids but more clumsiness of other muscles. Increase was exhibited during labour pain now coming on. Large bag inserted at 10.45. The temperature was now noted as being 105.2°F, pulse 144. It was a day of tremendous strain and anxiety to me, I may add.

14.5. 6.25 a.m. Pulse 138, Temp. 102.4°, Resp. 32. She had had two fits during the night - but twitching of the hands was frequently present. The presentation was left occipito-anterior; internal rotation had not occurred: os 3/4 - dilated - thinned posteriorly. From this time there was rapid progress until 6.55 a.m., when I returned
from another portentous and formed the head almost on the perineum, which structure from its resistance to the touch foreboded delay. Therefore at once Mr. Morriish being now present I applied Sir James Simpson's long forceps with greater ease than in any case before or since, and delivered the patient of a male child, without chloroform, and without convulsion, in the straight-bodied position, and with no laceration of the perineal body, chin, or fourchette. This was 2½ hours, 55 minutes after the first fit, and 10 hours 10 minutes after the onset of the extra evidence and rather than force was commenced.

The child was male as was conjectured beforehand from the painted
and cornual form of the abdomen - dead.
Blood stained serum oozing from its
more and congested in appearance. The
placenta also congested, and membranes
were extracted from the vagina five
minutes afterwards: the uterus felt small
and low down so that I could not get a
grasp sufficient to express the remains.
Pupils semi-contracted: pulse about
140; respiration more regular, but
patient was still almost comatose.
No stethoscopic signs of pulmonary
congestion.
Twenty-four
fits in the 24 hours.

At 9.10 am. another fit - the first
since delivery. 9.15, Mt of 170

Nitro-glycerine solution again.
11 am. Temp. 103°. Pulse uncountable.
11.30. Four leeches to one temple and five to the other. The latter wounds bled very profusely. She had a fit about 12 o'clock during which the blood was said to have "spouted" from the leech wounds. At 1.40 p.m. I thought it proper to stop them, and did so by pressure and puff-ball which acted well as a local haemostatic. (The puff-ball was suggested some time ago, by an Irish medical man, I think). 2 p.m. Another fit: patient still perfectly unconscious.

3.30 p.m. Temp. 102.8, Pulse 136. Little or no lochia since delivery: urine passed in bed. Bowels not moved, and no purgative was given on account of previous free evacuation.

Aspinum to loins and nape
of neck was ordered.
6:25. Pulse 138—two quavers of face. Pupils responding to light. In same position as when last seen. Brown fluid had been vomited. The mustard had acted well. Four ounces of urine drawn off. Sp. Gr. 1025; very acid reaction; albumen 3/5 =; slight blood-staining. I examined the deposit on the 76 along with John R. Logan, M.B., C.M., of the Northern Hospital, and we found uric acid crystals, red blood corpuscles, blood casts, and casts chiefly composed of an hyaline basis with some debris on their surface.

At 9:55 p.m., coma persisting. I changed front in the matter of treatment, and suggested Pilocarpin sub-
cutaneously, which was agreed to. But, in default of Phlœcarpin, we gave MXXX of mixture of Laborandi
10:45. Temp. 100.4. Pulse 126.
Resp. 27. MXXX more of the Laborandi as no physiological effect of sweating, salivation, sickness or secretion of mith arose from the first dose.

15-8:45 a.m. I learned that about 2 a.m. i.e. 3 hours fully after second dose there had been profuse perspiration only. During sweating and while the skin was deadly pale, she is reported to have "worked" somewhat.
Pulse 144. Lies on back now with knees temporarily drawn up. Tongue still slightly protruded. Moves head
a little: opens eyes, yet quite unconscious. 
Had a good deal of discharge, and water 
said to be passed with it.

Respiration regular. She seems some-
what more conscious. 3/4 of the 
Jaborandi mixture given.

1:40 p.m. Slight freely acting: pulse 
soft and quick. Condition otherwise as 
before.

8:45 p.m. Temp. 104.8. Pulse 132. 
Respiration occasionally had been sighing 
all afternoon. Skin still very moist. 
Head has been turning from side to side 
and occipito-frontalis elevating the brows. 
The look resembles a conscious stare. 
Lochia have been fairly abundant 
since Jaborandi was given. There 
has however been no galactagogue action,
or even natural secretion of milk. Only a little milk now and again has been taken as nourishment.

10.45 p.m. Drew off 3 of urine: op. 1021: reaction very acid: albumen 1/5 of column.

10½ 9 am. Discharge abundant during night. There was free sweating and she was more conscious. Pulse 144. Two fits— the last she had— since the late evening visit, when 3 of Liebig's Extract of Meat in 3 of water given per rectum was retained. Breath non-ammoniacal but free Hydrochloric Acid. The head is not swaying. She makes as small howling noise apparently voluntary.

Shin moist. The swelling of the tongue has gone down, and it is withdrawn within the mouth. Could speak some words, and had an aspect closely resembling that of idiocy.
Discharge plentiful, and urine said to be with it. Had taken small quantities of milk and beef tea by the mouth. A little brandy taken at intervals. Does not move the right arm.

17th Morning. Temp. 102.6 Pulse 154. Resp. 34. She is in a condition of semi-coma with half lucid delirium. The patient's friends in attendance state that at times they could scarcely control her in the delirious frenzies. Moves the right arm. 3½ oz urine were drawn off: Sp. Gr. 1.024: very
acid in reaction, and with uric acid
ear crystals adhering to the sides and
bottom of the bottle, on standing, dis-
solved by Liquor Potassae. D. J. H.
Logan kindly computed the area of
this specimen for me at 1.4916 per
cent.
At 11.45 a.m. we discussed the next-
step to be taken. Dr. Horsfall proposed
to give Carbonate of Ammonia; but-
I raised Fricelli's -thesis against this, in
the belief that it was well to avoid what-
might be an increase of a quicksand,
and we agreed to give, in view of
the delirium, Potassii Bromidi pro. XX,
Hyoscyami Jucutuæ 3 g. This
was repeated at 3.30. I may
here state that since noon yesterday,
we had agreed to what is euphemistically
termed expectant treatment, in other words to "let well alone."

12.45. Temp. 102.6°. Pulse 132. Resp. 34. There were intervals of delirium and apparent partial consciousness. Discharge of urine and lochia. Skin moist. A little wincing on pressure over the left vaginal region. Liquor Jodi was painted over the abdomen.

At 5.50 she was quiet. Pupils of medium dilatation, there was divergent strabismus; pulse and respiration as before. Able to swallow, and moves un-easily when urine, which had similar characters to that of the morning, was drawn off.

At 9 pm. Temp. 104.2° in the axilla.
pulse about 138, waves small; feet—cold; pupils not widely dilated, but more than medium; face pale and covered with cold sweat; body sweat still warm. Aetheris Mxii subcutaneously, and some also by the mouth. 9. 15, cold had reached the knees and nose. We now left. A minute before midnight, she died quietly.
### Table of Cases reported as Eclampsia

<table>
<thead>
<tr>
<th>No.</th>
<th>Affirm. Initials</th>
<th>Attendant</th>
<th>M.</th>
<th>Age</th>
<th>Disease of Pregnancy</th>
<th>Duration of Hysteria</th>
<th>Allowness</th>
<th>Treatment</th>
<th>Result</th>
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<tr>
<td>1.</td>
<td>Jan. 450</td>
<td>Hospital de la Pieté</td>
<td>M</td>
<td>38</td>
<td></td>
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<td>Cesarean sec</td>
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<td>2.</td>
<td>817</td>
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<td>P</td>
<td>26</td>
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<td>Alb. after.</td>
<td>Chlor.</td>
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<td>3.</td>
<td>247 M. C.</td>
<td>D. Madden</td>
<td>P</td>
<td>18</td>
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<td>No record</td>
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<td>D. P.</td>
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<td>Foreeps x.</td>
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</tbody>
</table>

**Note:**

- Second confinement - no fit
- Second pregnancy - aborted
- At end of 8 months no preg - no albumen
- Complete evacuation post-midwifery
- Multiform cholera
- Died of cholera
- Living
- Died
- Died
- Died
- Died
- Died
- Died
- Died
- Died
<table>
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<th>No.</th>
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<th>Etc. in All.</th>
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<tr>
<td>35</td>
<td>Mr. A. McHenry</td>
<td>D.</td>
<td>1 yr. 1 mo.</td>
<td>20 yrs</td>
<td>1st stage 1st month</td>
<td>NO report: Phenol.</td>
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<td>36</td>
<td>Mr.</td>
<td>2 yrs. 8 mo.</td>
<td>Delivered by accouchement force about 16 hours after labor</td>
<td>Was given 40 grains Picrotox. Brown.</td>
<td>Choral, Bleeding 3 days.</td>
<td>Lived Depression</td>
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<td>2 yrs. 8 mo.</td>
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<td>Choral, Bleeding 3 days.</td>
<td>Lived Depression</td>
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<td>Lived Depression</td>
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**Lanced 1874 to 1879 no reports.**

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<th>43</th>
<th>Single</th>
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<td>7 yrs.</td>
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**1880 Oct. 1**

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<th>30</th>
<th>Mr.</th>
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**British Medical Journal Vol. 1874 (Searches from 1873)**

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**American Journal of Obstetrics Vol. 17, 1869**

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<th>Treatment</th>
<th>Result</th>
<th>Mortality</th>
<th>Chills</th>
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**American Journal of Obstetrics Vol. 17, 1869**

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<td>Attendant</td>
<td>P. M. Age</td>
<td>Type &amp; Years</td>
<td>Disease &amp; Pallium</td>
<td>Treatment</td>
<td>Result</td>
<td>Sex</td>
<td>Notes</td>
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<td>Dr. James P.</td>
<td>44</td>
<td>Drying &amp; Conv.</td>
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<td>—</td>
<td>Died</td>
<td>M</td>
<td>—</td>
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<td>BM</td>
<td>—</td>
<td>25</td>
<td>After</td>
<td>—</td>
<td>—</td>
<td>Died</td>
<td>M</td>
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<tr>
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<td>GB</td>
<td>De. Brown P.</td>
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<td>—</td>
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<td>—</td>
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<td>M</td>
<td>—</td>
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<td>—</td>
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<td>M</td>
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<td>MM</td>
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<td>MM</td>
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<td>Died</td>
<td>M</td>
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Analysis:

Average age of 15 married Primparae 25.7 years, stated 29. Age of 4 Married Multiparae 31.8 years.

— 7 single — 8 —

Of 15 non-descript — Do — 21.9 — 23. Of 5 not noted, but probably married — 6 —

Majority of latter 15 probably not married.

Average age — 22.4 years — 34.7 years.

Number — 60 — 61.

Add Mrs. Brownie's case — 61. —

2. Juice
2. **Diseases of Seizures.**

<table>
<thead>
<tr>
<th></th>
<th>Before labour was observed to commence</th>
<th>During labour, cap 1st stage</th>
<th>After labour</th>
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<tr>
<td>Patient</td>
<td>10 + 1 = 11</td>
<td>24</td>
<td>13 = 51</td>
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<tr>
<td>Multi.</td>
<td>0</td>
<td>6</td>
<td>4 = 10</td>
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<tr>
<td></td>
<td></td>
<td>33</td>
<td>17 = 61</td>
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</table>

3. **Duration of Eclampsia.**

- **Fatal cases.**
  - 3 about 24 hours.
  - 1 about 2 days nearly.

- **Recoveries.**
  - 1 about 2 hours
  - 3 - 6-12
  - 6 - 12-24
  - 5 - 24-36
  - 2 - 2 days
  - 3 = 11 cases
  - 1 - 5

4. **Oedema.**

- Before all: 15
- With albumen: 3

5. **Albumen found.**

- Before: 15
- After 1st fit: 3

6. **Temperature noted in 9 cases.**

- 3 cases where it was 102.9°; 102°; and 104°. Fully recovered.
  - One "high" and one "up" - recovered.
  - One died of "peritonitis" (peros.)
  - One died of "puerperal fever" i.e. (?) puerperal eclampsia.

- 1 died of hyperpyrexia where temp. ran from 103° to 107°.
  - Mrs. Browne died - temp. fluctuates between 105° and 106°.
  - Five recovered: four died.
### Treatment

<table>
<thead>
<tr>
<th>Operative (done)</th>
<th>Medicinal (given)</th>
<th>Operative (performed)</th>
<th>Medicinal (administered)</th>
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### Result — to

<table>
<thead>
<tr>
<th>Mothers</th>
<th>Children</th>
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<tbody>
<tr>
<td><strong>Died</strong></td>
<td><strong>Live</strong></td>
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<tr>
<td>Prim. 12+1 = 13</td>
<td>46</td>
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<tr>
<td>Mult. 4</td>
<td>11</td>
</tr>
<tr>
<td>2</td>
<td>7</td>
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</table>

22.9 per cent of these mothers died. 36.3 per cent of these children were stillborn.

There are many points of very considerable interest contained in the fore-
posing Table and Analysis; but we shall allow most of them to convey their own lesson some of them, however, we shall notice shortly.

III. In the Diagnosis of my case of the Convulsions of Reproduction, which was first made under the name "Puerperal Convulsions", three main facts at once presented themselves - a primipar,
convulsive seizures, and albuminous urine albuminous before the first-convulsion. These were supplemented by the considerations that there was no observed previous oedema of the legs, the premonitory symptoms were of short-duration, and occurred late in pregnancy. But there was another symptom which was not at nearly throughout, but the weight of which I was unable to estimate until I observed in the Obstetrical Journal, Vol. I, page 359, a notice of Dr. Bournivelle's
works. "The Clinical and Thermometric Statistics of Diseases of the Nervous System", where his opinion is stated that in "eclampsia" the temperature is high; in uraemia it is low, and in hysteric it is unstable. These are important points in the differential diagnosis which have not been sufficiently insisted upon.

The observations of temperature in the published cases have been very meagre. Dr. Burton, in the paper before referred to (Liverpool Med. Surg. Jour. 1883 p. 557) read at the Liverpool Medical Institution—the very latest review I have heard delivered or read on this subject—makes no reference to temperature at all. Churchill quotes a case of "apoplectic" convulsions occurring in 1833, wherein "the heat of the brain was much greater than natural". If we look at—
our analysis page 144, we find in one patient the temperature "high", another "up", one dying of "peritonitis" which however, was I think due (apart from any supposed liability of a convulsed patient to abdominal inflammations) to the use of the forceps with insufficient dilatation of the os; one died of "puerperal fever"—probably the high convulsive temperature of which Dr. Bournville speaks; these are thermometrically observed to be pyrexia, and one dies in a state of hyperpyrexia. Lastly, Mrs. Brown's first taken temperature was 105.2°F.; it then fluctuated between 100°F. and 103°F. and finished at 104.2°. A course of temperature which also bears upon the prognosis of the disease. It is true that intercurrent complications such as lactation, slight or severe pelvic inflammation—
tions might tend to vitiate the result as between hysteria or epilepsy (e.g. Mrs. Butterworth's case), and Convulsions of Reproduction, of Temperature observations. But when we take the history of the case, we obtain light, and may be able to say: The convulsions are not the cause of the temperature, but the local condition. Temperature then, with fallacy eliminated, should be taken as an important factor in the diagnosis—the chief points in which are: (1) Pregnancy, ascertained frequently prior to parturition; (2) the convulsions are tonic and clonic; (3) elevated temperature; (4) often albuminuria, and may have altered urine, as hydric acid in my case; (5) tendency to coma without paralysis early; (6) history of absence of early oedema of the lower extremities and albuminuria, of epilepsy and hysteria, and
of strychnia or other convulsant poisoning or traumatic tetanus, or in short, of convulsions from toxemia other than renal already alluded to.

IV. The Prognosis. Here M. Bouvenelle is quoted from the Archives de Zoologie, April 1845, as saying:

1. The temperature increases from the commencement to the end of the attack; 2. in the interval it keeps elevated; and 3. If a fatal termination is likely it attains a high degree.

Mrs Brown's case practically supports the first of these points, certainly supports the second - and, unfortunately, is a sufficiently striking example of the third. But there are other important points in the Prognosis, for example, the constitution of the
patient, the amount of the albumen, if any, the profundity of the coma, the duration of the intervals between fits, the treatment employed and its effects. In Mrs Brownie, on the third day (Sept. 16) there was a small gleam of hope, but the symptoms, whether considered jointly or severally, almost throughout warranted the grave prognosis which was communicated to the friends.

Levi's mortality (Guy's Reports 1843) was 14.2 percent of the mothers, and 4 out of 15 children.

Churchill (2nd Ed. Induring, p.413) finds, in a very large collection of cases from various sources, a maternal mortality of 29 percent.

Braun (Op. cit. p.501) says that the prognosis in eclampsia is always to be considered dangerous to life, since as yet 30 percent prove fatal.
Dr. More Madden, going partially over the same ground as Churchill, from the Rotunda Hospital Reports found that out of 115 cases wherein the result was accurately noted 26 died or 22.6 per cent.

Litgman (Lancet 1/1880 p.912, Dr. Murphy's paper) found in 131 pregnancies, 324 albuminurics of whom 42 had convulsions, and as about 1/4 of eclampsias die, 1 in 19 would be his mortality, although this is not definitely stated.

And lastly, my analysis shows 22.9 percent of a mortality in 85 cases — which may be estimated as occurring in 50,000 labours. Children 36.3 percent. Lever's death rate was below, Brains above, the average which may be taken at about 25 per cent, or one death nearly in four cases, and we may also accept it as a cheering fact (in face of Lever's percentage).
that the prognosis of to-day is an improvement on that of 30 years ago.

But our analysis brings out this point that the mortality is not always proportionate to the duration of the attack. Indeed only refer to the figures given, and draw attention to the case of Mrs. D. (No. 16) wherein Dr. Angus Macdonald used the proverbial sledge-hammer, saying: "No case was ever treated with more attention and less brains." The case was one of the most serious character; yet the patient recovered, which caused Dr. Macdonald to pause and remark that there must be a good deal of pros or ergo propter in the treatment of eclampsia usually pursued, after all. What then must be said about the influence of treatment generally upon the prognosis? Just as much as may, in the absence of a "specific", believe, be said of other classes of disease —
Some cases, apparently the most severe, recover under little or no treatment, some are "cured" by treatment, and some yet well in spite of it. Occasionally, I fear, treatment adds to the mortality, and this often through too much "brains" being concentrated on the case in a high endeavour to save a human life.

V. Treatment.

A. Natural.

(a) Asphyxia for each fit.

Asphyxia was in my view the natural terminator of each convulsive seizure, and I found confirmation of this opinion in the Londonian Lectures of Dr. Barnes, who quotes Lances—May 1873, p. 619.

* See case from Vierteljahreschrift—Prague, where forceps, bleeding, ice, morphia, chloroform, cold douches, and transfusion of blood to finish utli, were used, and the woman recovered. Thursday Jan 21/1880. p. 912.
sequence of the asphyxia which itself produced.

The congestion of the face is deepest at the climax of each attack. It arises from a combination of causes—the tonic spasm of the respiratory muscles suspending respiration and thereby preventing or rather not causing access of atmospheric air to the blood circulating through the lungs (as seen by the nitrite of amyl test) — the irritation of the vasomotor centre, which I believe to be present, giving rise by constriction of the arterioles, to peripheral resistance, such as impedes the right heart in emptying itself (Foster, A Text-Book of Physiology 3rd Ed.) and causes heaping up of blood in the venous system (the leech wound of W.S. spouted during a fit), initiating cerebral congestion and intensifying that of the kidneys, in most—
cases already existing. It is well that respiration is arrested for the time as otherwise there would be a greater risk of syncope from further overloading of the right heart (Foster loc. cit.). We have from asphyxia then, I believe, an exhaustion of the vaso-motor centre which removes its previous morbid influence over the the vaso-motor nerves distributed to the arteries of the cerebral ganglia and body generally, relieves the tension in the arterial system, so permits a termination of the sequeiries, and between them allows the balance of the two circulations – venous and arterial – to be more equally maintained.

(B) Delivery for all the fits. It is a generally recognized fact (Playfair’s Midwifery p. 224) that where albumen shows itself in the urine during pregnancy there is a strong tendency to the occurrence
of premature labour. This is a natural treatment; for the fact of pregnancy, upon which the kidney disease in those cases depends, is not itself tolerated by the effect of the disease itself. An argument on this head is unnecessary. One point, however, I may deal with, viz. that the nerve-state causeth convulsions, very frequently also induces the labour, rather than labour the convulsions. Obviously this cannot refer to the small number of cases in which convulsions appear, wholly disappear, and labour afterwards independently supervenes, or to the larger number in which convulsions supervene after delivery is accomplished, but only to the class noted as convulsed "before" (in part) and "during" (wholly) labour. Now as it is very difficult to draw the line precisely at the commencement of
of labour, it must be just as difficult to determine the point raised by a reference to time alone. I will refer to one or two facts:

1) Labours with convulsions frequently occur from two to three or even four weeks before the expiry of the normal period of intra-

2) When convulsions occurred as supervening during labour, the period is often the first stage.

3) Reference to Table p. 139, and to Mrs.

Brown's case supports each of these points. But Mrs. Brown's case goes further. The doctor only admitted the tip of the fore-

finger nine hours after convulsions ap-

peared, and the labour was premature by a fortnight of time. My limited experience of primiparous labours leads me to believe
(allowing for the effects of treatment in this case) that 9 hours are not generally occupied in accomplishing the dilatation indicated. The inference then in Mrs. Brown's case, at least, is that the storm in the nervous system endured by the convulsions caused labour to set in.

(4) That convulsions are competent to effect this, I quote Kussmaul & Jenner on Epileptiform Convulsions (New Sydenham Society's Select Monographs 1859), p. 70:— experiment on a female (gravid) rabbit— wherein, after removal of the brain up to the optic thalami
tons, they found

(2) general convulsions ensue from "cerebral anaemia" (1) on ligation of innominate 
(2) compression of left subclavian (2) 
(3) in the fourth series of induced convulsive attacks two foetuses were expelled.
and the act of parturition was continued until ended by an haemorrhagic death. The conclusion which they state contains the assumption that often thus the "eclampsia gravidarium becomes eclampsia parturientum," which is just the contention which I have been endeavouring to sustain.

We now pass to 

B. The Artificial Treatment of the disease.

The drugs administered were Chloral and Bromide of Phosphine, Chloroform, Nitrite of Amyl, Nitroglycerine, and the mixtures of Laborandi and Henbane. The accouchement-aide was employed; the patient was carefully nursed; she had a little brandy occasionally.
blistered, leached, and was finally given
ether hypodermically.

(11) Chloral + Bromide. Regarding the former
Chloral. Fothergill says: "By its combination of qualities
as a powerful vascular depressant—a sedative
to the nervous system chloral is indicated
in all cases of cerebral irritability with en-
cephalic vascular activity, especially with a
thick radial pulse, and (loc. cit.) Chloral
too... is distinctly indicated when the above
condition is associated with pyrexia.

In the lowering of arterial tension admin-
istration of albumen in the urine is often
obtained. Chloral, therefore, is
to be given a prominent place, and, in
another case I would try it more freely.
It was administered in 24 of the 64
recoveries (p. 145—table).

Pringle (Therapeutics p. 146) mentions that
Brown-Séquard and others found the bromides contract all the blood-vessels, producing anaemia of the brain and spinal cord, thus diminishing the excitability of these organs. These actions arise from large or moderate continued doses. The draught I gave produced no visible effect: the dose was too small. Given, therefore, that Dr. Angus Macdonald’s view that anaemia of the cerebral motor ganglia through irritation of the vasomotor centre is correct, I may safely assume that had neither diminished the discharge of nervous impulses from the vasomotor centre, nor intensified an already, at least during the seizures, existing cerebral anaemia. I cannot reconcile the theory that both these drugs produce cerebral anaemia, and yet subdue convulsions the outcome themselves of an
anaemia of the brain — yet my Table gives statistical indication of the benefit of chloral in practice. The effects, however, of diminished reflex action and sensibility (Ringer op. cit. p. 354) are easily appreciated.

(2) Chloroform could not be administered continuously, and was too slow when given intermittently, even with watching the interval between the seizures, to arrest the convulsion.

(3) Nitrite of Amyl was inhaled from a paper funnel — dose 5 drops. The idea of giving this drug occurred to me independently when attending Professor A.K. Simpson's class in 1879. He informed me that it had been already given. Therefore
resolved to try it in the first case I should see of perpetual Edampain. The main object of this treatment was to prevent or relieve arterial spasm. The uneasy motion and twitch of the patient's finger was the signal for its administration, and twice I distinctly stopped the onset of a convolution by the inhalation. As soon as the flush rose, which was within 3 or 4 seconds, there was an end of the seizure. On the other hand I found that the interval between the warming twitch and the tonic respiratory spasm was too short to allow of my lifting the bottle, uncorking it, and pouring out the drops of Nitrite. Were compression of the cerebral arterioles by oedema the immediate cause of the convulsions, I fail to see that the Nitrite could have any good.* See Hummelth-Jenner's paper p. 81.
effect.) Manifistly that was only proof of
the suspension of respiration just as the
previous observation is a proof and test
of the presence of vascular spasm being the
proximate cause of the convulsions.
After the seizure and on the first respiration
the Nitrite did not seem appreciably to affect
the effect—there was purplue suffusion
only, due to the yet imperfect oxygena-
tion of the blood.
The great objection to the Nitrite was its
very temporary action, and its inefficacy
in anticipating the fits. It is not
generally useful in these convulsions,
(Brit. Med. Jour. II/1882 p. 546; and Dr-
Burton's paper referred to—he mentions my
case as the first he had known), and is
never likely to be much used, as it is
not a practical remedy. The hypodermic
injection or the administration of pure Nitrite of Sodium might give better results, and deserve a trial.

(4) Nitro-glycerine M of 1% solution as a dose—was therefore looked to as giving a more permanent action of a somewhat similar kind. The noteworthy feature which followed was a change in the rhythm of the convulsions; they were now curiously linked instead of being quite simple; the intervals were longer, and so was the duration of the seizes individually and collectively.

Mr. W. E. Green (Brit. Med. J. 51882, page 573) acts on the principle of "dilating the arterioles"—a practical way of saying 'dilating the arterioles', and within ten minutes of taking the first dose the patient became conscious, ultimately
recovering, a result which he thought fully supported his theory. Had Mrs. Browne similarly recovered under the Nitro-glycerine I daresay I should myself have been inclined to give the drug the whole credit.

(5) The next step taken was the emptying of the uterus. The argument is: The disease depends upon the fact of pregnancy; this tends to be removed by abortion; ergo, aid nature in that cure. Mrs. Browne's labour was only aided, there was no "accouchement forcé" in which I do not believe as it is attended with many risks even in the non-convulsed parturient, and does not yield commensurate benefit to the patient. Case 48, if not also case 10, are among the somewhat numerous illustrations of the risks attending force
labour in such cases. I think that labour should be aided, as, for example, by applying the long forceps as soon as the os interi is fully dilated, and the patient under chloroform. Note that Mrs. Brown had 18 fits before and was commended, six during its progress, and only five distinct seizes after 29 in all. With however, one imperfect and various minor manifestations of disturbed nerve action. No chloroform was given during forceps delivery, and no convulsion occurred for 2 hours 15 minutes after. The point cannot be decided arithmetically, but here we have a case which we may take as affording rebutting evidence to opinions against obstetric manipulations of all kinds.
(6) Leeches were applied to the temples in order to relieve venous congestion. The cerebral condition remained unaltered. Two points are to be noticed, the spouting of the blood during a fit, and the treatment of the wounds by puff-ball.

(7) Sinapisms were applied over the loris and mape of neck, but neither the kidney condition nor the coma were perceptibly relieved or altered.

(8) All efforts having failed, the case was attacked from the uralmic side of its supposed pathology. Mixture of Jaborandi of which 10 to 40 minims was a dose, were given. No symptoms followed for one hour, when 30 minims more were administered. About three hours after the second dose there was profuse perspiration, without
salivation, and no galactagogue action, but a point was observed which I am not aware of having been previously noticed, and which was too marked in sequence to allow a non causa pro causa objection, namely, that the lochia non-existent before the administration of laborandi became of considerable quantity during the night on which the drug was given. There was a fall of temperature an hour after the first dose. The patient was said to "work" somewhat during the sweating stage and became deadly pale. This pallor and "working" were probably correlated, as this probably is a detrimental effect of the drug. The authorities I have at-hand point out beneficial actions of the
drug, beyond elimination of urea. Sangharam (Therapeutics p. 285), says, this action on the skin is considered due to vasomotor paralysis and consequent dilatation of the cutaneous arterioles. Frigeri (Op cit. p 478) says Jabourandi seems to be slightly narcotic, for patients often fell asleep during our experiments. This, however, might be due to the dilatation of the cutaneous arterioles producing a corresponding cerebral anaemia.

We see after the drug was given, less albumen in the urine, which was beginning no doubt to disappear at any rate, but also an improvement in the cerebral and general condition for a day, giving rise to a ray of hope in a case which from the outset was clouded enough.
(9) The use of stimulants will in the circumstances, I think, be unquestioned. Should the stimulation not have been freer? The ether at the close was useless.

(10) It is an open question whether she should not have been freely purged. Obstetrical Journal V p 622, Dr. Kidd stated that his practice when coma continues with fits at short intervals is first-hand purging, then emptying the uterus.

(11) The nursing was done by friends and was constant and careful. Unfortunately, however it was also futile.

(12) The mixture of Hypoxyamins with Potassium Bromide was given after a discussion as to the effects of other hypnotics and drugs, at a stage when reaction had reached a crisis beyond which
the patient's strength was unable to carry her. In similar circumstances, I would not advise a repetition of the treatment, although it may be justified by such a case as No. 37 (table), which recovered.

Other treatment for " eclampsia."

In the analysis of my table at p. 145 the other treatments of a very varied nature are enumerated. I shall mention two here.

Colchicum. (D. Murphy's paper Lancet I/1880) "Balfour looks upon colchicum as almost--a specific."

Dr. Madden's paper referred to read at the Dublin Obstet. Soc. 9th May 1874--prenummary symptoms are "similar to what are occasioned by the circulation of little acid in the blood, and which precede..."
an attack of gout."

Now in Mrs. Brown's urine there was free uric acid; therefore, had I known of the use of colchicum, it would have been given.

Morphia, Atropia & Acromine together, subcutaneously. Dr. Bowles of Caistor. (Brit. Med. Jour. 11/1882 page 90.) says he "cured" a patient by two subcutaneous injections at half an hour's interval of Morphia Hr. Atropia M III Fleming's Acromine M III in solution; and three others similarly. "The injection acted like magic," he says. So the doses of Morphia & Atropia respectively, I have no objection, but I should scarcely be bold enough to inject subcutaneously.
four minims of Hennings mixture of Acornite within half an hour. With a more cautious use of the Acornite this, I have no doubt, is a good combination in "sthenic" cases; but a physician speaks too confidently when he uses the words "cured" and "injection acting like magic" in cases of the convulsions of reproduction. My four years experience leads me as yet, to employ the word "recover", instead of "cure", in most cases.

VI. Two clinical features of this disease.

(1.) Primiparity. It is a striking fact that nearly all cases of genuine convulsions of reproduction occur in connection with first pregnancies. Thus Sir James Simpson (Selected Obstetric Works)
(p. 340 and 342) in quoting Dr. Collins' statistics says: "It is to be recollected that the standard proportion of first pregnancies in the general sum of labours amounts only to 30 per cent." These statistics showed 30 cases in 16414 labours, 29 cases being primiparae. That is 1.8 cases per 1000 women of whom 300 were primiparous, but 29 cases happened among 4987 primiparous or 5.8 convulsae per 1000 primiparous labours.

My small table shows 61 primiparae to 14 multiparae having "convulsions", and these from extended and scattered sources.

Figures only tell us the broad fact, and primiparity is so clearly established as to be beyond doubt as
a primary factor even in true puerperal convulsions. As bearing on this factor let me remark that the cases occurring in multiparas are frequently reported to be cirrhotic Bright's disease, or albuminuric retinitis is noted (e.g. Case 11 p. 243 Dr. Angus Macaulay's Op. cit., Case 18. table). It seems to me that habits leading to cirrhosis are far more likely to have been formed in multiparae than primiparae, and if so, they suffer from an uræmia in which the pregnancy is a concurrent accident, and not from puerperal convulsions. Thus we are not far from the truth if we say that true convulsions occur practically in primiparae alone.

Now it has been assumed that rigidity of the abdominal wall constitute the
chief difference between primiparity and multiparity (e.g. Dr. Goodfellow, Med. Jour. & Gaz. 1860, ii, p. 127). That is a difference, but if this rigidity did lead up to convulsions not essentially uraeemic, why are true convulsions comparatively so rare? Why is the proportion of effectual rigidity only about 6 cases to 99 4 where no visible result ensues? Adopting in the main as I do, the theory of my namesake Dr. A. Macdonald, I am inclined to believe that the reason is to be found that the tendency to develop inflammation about the vaso-motor centre by pregnancy only exists in the few, and in these the causative agent has been at work for the first time.

(2) Fretting.
(2) Fretting has been named as a predisposing cause, and its opposite, a contented or stolid disposition, has been mentioned as exempting from convulsions in an albuminuric case — D. A. Macdonald's cases 2 and 3 quoted here at pp. 90 and 91. We have seen that 5.8 per 1000 is about the number of eclampsia cases occurring in primiparous labours alone, or 1 in every 173 primipars, married and single. Now taking 7 per cent as the illegitimacy in all labours, say 5 per cent will be primipars, that is 50 per 1000 of all labours in which as above 300 will be primipars, i.e. 250 legitimate + 50 illegitimate primipars per 1000 labours. Among these primipars less than 2 eclampsias (1.94) will occur. We
may safely assume that "fretting" is not at all uncommon among the single
praid women; and if we add to this the "fretters" from married life, we
have a load enough of human misery
in the one side of the balance, but where
is the eclampsia in the other?
Hence this fretting seems to me to
have so little practical influence as to
be not much more than an idea of
the Irish school. Its bearing is that,
after the disease sets in, it finds a less
resistant condition of the nervous system
body generally on which to expend its action.

VII. Pathology and Etiology.

There are three directions of search
for structural alterations (1) the ureters
(2) the renal veins and kidneys,
(3) the medulla oblongata. We are
here treading on very uncertain ground. But we may set aside the view of Haldena (Murphy, Burton, papers cited) that pressure on the ureters causes the kidney disease which leads to eclampsia, as not according with the teaching relating to hydrenephrosis, and not proven in fact.

Dr. Lever (Guy's Reports cit.) is usually pointed to as the first who connected Intrauterine Convulsions with kidney disease, by assuming pressure on the renal vein as causing the kidney affection, but this class I have disposed of by placing it under the heading of Mechanical Gestation Albuminuria. Sir James Simpson (Op. cit.) pp. 298-299 seems to me entitled to rank.
with Dr. Le ver, as he had lectured on
the association of puerperal eclampsia
with granular kidney”, before Le ver
wrote, and the publications of both
appeared in 1843—Guy's reports, and
St. Monthly Journal Nov. 1843, p. 1015.

Then we have Bra user (Op. cit.—p. 475), telling us that the kidney
of eclampsia is one of hyperæmia
and commencing exudation passing
through a second stage to cirrhosis.
This view is supported by Dickenson

But it is not essential to the occurrence
of puerperal convulsions that there should
be kidney disease at all. (Professor
Graniger Stewart, Bright's Diseases p. 54;—
Dr. Angus Macdonald, Abst. Journ. IV p. 139;—
Dr. Madden, paper cited, — and Ta ble p. 140)
Case 20.

Dr. Angus MacDonald (Heart Disease during Pregnancy, etc. Case II p. 43 remarks) propounds the view that there is an inflammation about the vasomotor centre, and supports it by Dr. Hamilton's microscopic examination of Case I—an unmarried primipar, where the medulla at the level of the ovarian bodies had vessels peculiarly prominent—abundant vascular pleats in the corpora lutea, 't between, dispersed leucocytes.'

Dr. Barnes (Lumbar Lectures on the Convulsive Diseases of Women) Lancet I 1873 p. 515, simply begs the question, when he tells us
that the "accumulated irritability of the nervous centres determined by pregnancy" is called into action by an eccentric stimulus from uterus to centres, and when toxæmia is superadded "the danger is great—that the excess of irritability will run into pathological action." That is a state of "inmutable nervous equilibrium." At p. 516. he adds: But—convulsion in pregnancy or labour may occur without "albuminuria. In this case we must—"invoke an equivalent tertium quid "something which lends intensity to the "normal physiological cæthrum of the nervous "system"—a blood affection, altered nerve substance, struma, syphilis, "or some "indefinable hereditary taint." He condemnns strongly Dr. Lees's assertion of "the universal as to the connection of "albuminuria and eclampsia." But it is
difficult to decide whether the dogma of Dr. L. or the painful groping in the dark speculation of Dr. Barnes as to this "tertium quid" is the more illogical. Evidently, the paragraph only means that regarding the tertium quid he knew nothing. Now have we not—rather more than wildly speculative ground for saying that this tertium quid is inflammatory action about the vaso-motor centre leading to that vascular spasm which produces convulsions by cerebral anaemia? (Eusem & Jenner, op. cit. pp. 101 & 102.)

The Dranle-Rosenvic theory finds many supporters. One admires its brilliant fancy, but I do not accept the view of edema of the brain, as Bartels (Lemmnsic Cyclopaedia...
Col. XV, p. 124 mentions Rommelacz's experiments of tying the 4 jugulars, injecting water into the blood of animals, without convulsion, and also a case with excess of urea in the blood, excessive anaemia, but no oedema of the brain post-mortem. And I do not think that nitrite of amyl or nitro-glycerine would have effect against such compression. The statistics also are against. Thus the pathology is narrowed to the kidneys and the medulla.

My belief, and I can only state that, is, that both are affected by excreentitious products as Dr. Angus Macdonell and also Professor Granger Stewart (op. cit. p. 53) indicate. But the convulsions have their proximate origin in the state of the vaso-motor centre, and its neighbourhood.
VIII. Differential Diagnosis.

Professor Carl Braun (op. cit. p. 491 et seq.) distinguishes "The uræmic eclampsia of the pregnant, parturient & lying-in woman" from chloræmic eclampsia, hysterical convulsions, epilepsy, apoplectic convulsions, convulsions in meningitis and encephalitis, acute tuberculous meningitis, from convulsions at the onset of Typhus, uræmic convulsions, toxic convulsions i.e. from various poisons, chorea gravidarum, and from faintness.

That enumeration is useful as shewing the direction in which the mind must travel. I shall only indicate a few points of distinction between Periperal Eclampsia or the Convulsions of Reproduction truly
so called and

Hysteria. Sir James Simpson (op. cit. p 57) pointed out that before the seventh month the convulsions are almost always hysterical. It is then a consideration.

Braun (op. cit. p 494) enumerates the symptoms of hysteria — globus, disorders of sensation, urine free from albumen and tube casts.

Bouneville — ut recta — the temperature is normal. Recovery invariable.

Epilepsy. Braun, p 495, is habitual, chronic, and most often there is an aura. Chiefly recovery.


Post-partum haemorrhage.

Kussmaul & Jenner (op. cit. p 93),
Dr. Angus Macdonald's Case II p. 243
op. cit. was to my mind one of haemorrhage (internal), although chronic interstitial nephritis was found after death. Reticence of manner: cold: strange feeling over heart: rushes in the ears: convulsions: free haemorrhage when the cord pulled or belly squeezed, are symptoms almost exactly corresponding to those of internal haemorrhage. Princess Charlotte's case (Playfair op. cit. p. 17), apart from the presumed pulmonary embolism, of which I may say I have seen an instance, exemplifies internal haemorrhage. I have been called to two cases of haemorrhage externally under midwives' care, one of whom died with anaemic convulsions. The risk, however, of confusing internal haemorrhage with convulsions, and the true convulsions of hyposthenuria,
Uraemia - with or without) chronic interstitial nephritis. The only absolutely diagnostic point of which I know is that (Bournville - ut ante) the temperature is low, and this is indicated to apply to the commencement; whereas in Eclampsia - the temperature is high, declining in favourable cases, keeping up in unfavourable cases, such as Mrs Brown's.

In conclusion, let me point out the position, not by any means I admit unassailable, which I think best explains the greatest number of cases of Puerperal Eclampsia or the Convulsions of Reproduction:

1) That it is an acute motor-neurosis
arising from inflammatory action about the vaso-motor centre— the convulsions being the symptom: the inflammatory action, the disease, and that the cause of this inflammation most frequently but not invariably also suffices to produce a renal hyperaemia.

(2) Where no albuminuria is present— the case is explained by the existence of the vaso-motor centre—itis alone.

(3) That the convulsions are neither hysterical, epileptic, apoplectic nor anaemic, but entirely sui generis; and that temperature forms an important factor in their diagnosis.

(4) That the treatment from which the most successful results have, as yet, been obtained, and on which
I would rely is
(Operative) Accouchement-aid, Counter.
irritation to neck and loins, and
bleeding which might be had recourse
to in "sphagic cases" with full pulse,
but I would not abstract 122 ounces
of blood in 17 hours, as Lever tells
us he did from "a large muscular Irish-
woman".
(Medical). Colchicum might have
had a trial in Mrs Brown's case.
But Nitro-glycerine, Chloral Hydrate,
Morphia, Aconite and Veratum
Vulvae, are the agents which have
been shown to be the most efficacious
either as supplemental to the operative
treatment above-mentioned or otherwise

The general lesson
conveyed by the series of cases which
I have presented is that the mechanical
Albuminuria of Gestation is a curable
affection at little risk, when properly
treated; that Hysteria and Epilepsy
stand in similar positions to these
diseases in the non-pregnant condition,
but that Pelureal Epilepsy or
the Convulsions of Reproduction is a
disease of the most formidable nature
and difficulty of treatment - the
pathology and treatment of which
can not yet be said to stand on
a satisfactory footing, and will not,
in my opinion, less, until (if ever)
the mysterious movements of nerve molecules
and atoms be, to some wondering observer,
revealed.
An Indicating Axis-traction Forceps.

There are two classes of axis-traction forceps, those with, and those without additional traction rods. To the former belongs Professor A.R. Simpson's modification of Garnier's forceps; to the latter, the instrument now shown.

Attention is directed to the blades, which are broad in the steel and narrow in the fenestrum. They are 6 7/8 inches long, 2 1/2 in. apart at the widest, 1 inch apart at the extremities which are not so broad as usual. They will yield half an inch at the point—and all forceps will do this more or less according to the temper of the steel. Thus the grasp of the instrument is enlarged on making traction, and we
must make allowance for "spring." The points are so formed as to prevent marking of the foetal head; and in practice there has been no "digging in" of the blades. Their breadth is 1 3/4 inches; fenestrum 7/8", steel on each side 7/16" in.

The fenestrated portion is almost straight, and the pelvic curve lies in the lower portion of the blades and upper part of the shank, where, in my opinion, it should lie when the blades are passed well over the foetal head.

The shank measures 2 7/8" ins. (with the blades 9 7/16"), and this prevents lodging in the vagina, when the high operation is required to be performed (Case 4). The lock allows scissors-action necessary for the purpose of indicating the grasp; and the act of locking is easy—the upper
blade slipping into the lower. We must reverse this order if we wish to remove the blades before the head emerges, and take away the upper blade first. The lower blade is easily recognized by the hooked part of its lock.

The binding-screw has an inch scale on its convexity which shows how far the extremities of the blades are apart when in position, and by which an idea is given of the head diameter grasped (scale 1½ ins. = 33⁵⁄₈ in. grasp, 2 ins. = 35⁷⁄₈ in., and 2½ = 4 inches — to which add ⅛ inch for spring), and a warning against slipping of the blades obtained. An objection has been made to the use of the screw that it helps to crush the foetal head. Now I have found that by holding the handle with the left forefinger and thumb so
as just to feel the same grip as with the ordinary long forceps then tightening the screw to that point, and relaxing it between pains, no mark whatever need be made on the head, a statement which cannot always be applied to the usual types of long forceps. The screw is not at fault: its user may be.

The handles are in two portions hinged together for portability's sake, for ease in manipulation, and to allow of axis-traction by passing backwards the moveable halves past the perineum, at the end of which halves a transverse bar for traction is passed into suitable oval holes. That bar, which is also oval to present a broad side to the fingers, admits of being screwed into
the head of the binding-screw when not in use. The line of traction passes through the centre of the transverse bar, and reaching midway between the blades, to their points, in a plane drawn through the long diameter of each fenestrum. With the head at the brim, therefore, the traction-bar must lie opposite the tip of the coccyx.

There is only one point—in the mode of application to which I need to draw special attention, viz., it is obvious that the blunt-handle must not be drawn so far back towards the perineum as is the case with, e.g., Sir James Simpson's long forceps, otherwise the points will be too far tilted forwards. The mission of
the screw and traction-bar and raising of the handles are points too obvious to mention. It is occasionally of advantage to pass the upper blade at first horizontally, then to turn it upwards and forwards into its proper position.

The advantage of axis-traction as a principle will be conceded by all who have practically applied it, or even given it serious consideration as a mechanical question. At least, that was very apparent to me the moment I had tried Professor Simpson's axis-traction instrument on the dummy. But as a Liverpool physician observed to me sometime ago, there are men in our profession who have "passed the climacteric," and reject improvement...
because it means to them—change.

The index in my instrument gives a degree of precision, and the hinges admit of its occupying comparatively small space. In this, the first pair, there is the disadvantage of the handles being rather heavy. They might be made an inch shorter and considerably lighter with advantage. They are dark-brown in order to prevent formidableness of appearance. I have applied these forceps with ease in the eight following cases, where the indicator shewed from 1½ in the first, to 2 inches separation of the points of the blades, in the last. The words cavity, brim, outlet indicate the position of the fetal head when the instrument was applied.
1. Mrs. Singer, 120 Park St. - Cavity.
3. Mrs. Lewis, 38 Jacob St. - Cavity.
5. Mrs. Burch, 81 Ponsdale St. - Cavity - A post-mortem pelvis (?) : posterior fontanelle very low, 0.5 half dilated. Application of forceps very easy.
6. Mrs. Joyce, 39 Lewis St. - outlet.
7. Mrs. Wolfechurch, 7 Claribel St. - outlet.
8. Mrs. Thomas, 48 Willburn St. - Walton. outlet.

They were all primiparae, save Mrs. Nelsby - a secondipara (delivery of a stillborn child by the long forceps in her first labour attended by another practitioner) all were occipito-anterior presentations, and all made good recoveries, save Mrs. Lewis, who did not get up from bed until the third week, but subsequently regained her strength perfectly. She was a weakly constitutioned stumpy patient with caries of one of the metatarsal bones.
On Dr. Alexander's Operation for Backward Displacements of the Uterus, and for Prolapsus Uteri.

The fact of my having assisted Dr. Alexander of this city (Liverpool) in the performance of his operation on the round ligaments for the remedying of an uterine displacement, will, I trust, justify me in giving some explanation and criticism of it. Dr. Alexander has kindly permitted me to peruse the manuscript of a little work on the subject, which is now almost ready for publication; so that I have the advantage of having his views fully before me. Dr. Adams
of Glasgow had designed and performed such an operation on the dead body short-ly before Dr. Alexander had performed his first operation; but as the latter had thought out and mentioned his plan to his friend Dr. Irvine, with whom I am acquainted, and also performed the operation on the living person before the former had done so, he is entitled to the credit of having introduced it into practice, and the operation may, with this explanation, be fairly called Alexander's operation. It is only another example of two scientific minds being almost simultaneously engaged in the study of the same subject.

The case was that of a patient, Mrs. Carter, 23 Wilson Street, &c.
suffered from retroflexion and retroversion of the uterus, and from "fits," and was also occasionally the subject of suicidal impulses, in the treatment of which conditions I had taken part. In the seizures she was suddenly projected forwards to the floor or ground where she happened at the moment to be, became unconscious for a very short time, and then, after resting a little, when she was able to again move about as usual. The impulses were "safe" ones: she on one occasion dropped herself from the back bedroom window to the yard a distance from her feet of about seven or eight feet. She did not try to leap; and was unhurt when I saw her immediately afterwards.
The fits were supposed to be dependent on the condition of the uterus, which indeed was the subordinate (if it were not, as it appears, the causative) aspect of Mrs. Carter's case. They were called hystero-epileptic.

The position of the uterus is represented in a diagram of the work (No. 1) of which the following is a reduced copy.

(a) is the origin of the right round ligament, which is shown in black ink.

I recommended Battey's operation, in 1882, when I was not aware of Alexander's plan having been carried out, as there was sufficient justification for double ophrectomy in the circumstances found in Professor Simpson's
article on the subject-in his Contribution to Obstetrics and Gynecology (Table p. 308). But the operation on the round ligaments was performed: it promised to be best for the uterus—it might keep it in position, and even remedy the sterility of nine years duration, and possibly also remove, in that way, the lead symptoms, and oophorectomy held in reserve. This expectation was warranted by the case of a Mrs. 13, narrated by Dr. Alexander, who was a sterile epileptic with distinct retroversion of the uterine, whose vertebral arteries had been ligatured without benefit of the epilepsy—but both epilepsy and sterility disappeared after shortening of the ligaments, and after parturition was accomplished the womb remained in the normal position.
- a typically perfect result.

We shall here merely quote a passage in Mrs. Carter's case.

Mode of Operation. I give it as a précis of the author's own words, and then point to one or two incidents in Mrs. C's. case.

Bowels and bladder to be emptied and patient anaesthetized. Pubes to be shaved from pubic spine outwards. Incision to be made from pubic spine upwards and outwards for half an inch in thin to three inches in very stout subjects, in the direction of inguinal canal. Increase depth of wound until the tendon of the external oblique muscle is reached. Tie the small superficial external pudic artery or twist it...
cut across. Look for the external abdominal ring, guided by oblique fibres crossing it, and by a small morsel of fatty tissue issuing from its inner end. Cut across these oblique fibres in the direction of former incision, and a reddish tissue—the end of the ligament—just before it spreads out in the mons veneris—where some fat appears bulging out of the ring. Raise the end of the ligament, with the fat, out of the canal, by an aneurism needle passed below it; grasp it with the fingers (not forceps), and gently pull and dissect out the ligaments. If this has been done without breaking the ligaments (an accident which gentle handling avoids), and there has been no interfering pelvic cellulitis—parametritis—or uterine adhesions posteriorly, then the ligaments "run" easily.

This ascertained, the uterus is placed in position by the sound, for backward displacement as far as possible in the normal
position, in prolapse the ligaments capability for being pulled out is the limit, and so maintained by an assistant whose finger also touches the cervix uteri. Then pull out the ligaments until they are felt by the sound to control position of uterus. In prolapse they are drawn up as far as possible.

To retain the ligaments: a needle threaded preferably with strong catgut is passed through skin at lower and outer side of wound, through outer pillar of ring round ligament, on emerging through inner pillar, and opposite point of skin to entrance. Knot in 1st coil. Then use 2nd and 3rd sutures similarly if necessary. Sponge wound; pack redundant-ligament into its inner end; if frayed out, cut it off; sponge again; knot the sutures and dress (aesthetically). In Mrs C's case the right-round ligament ran easily, it was as thick as a goosequill; the left ligament was like a crow quill, and was pulled out with difficulty.
I was holding the handle of the sound and found this fact not only noticeable to the eye by the ligaments themselves but also by deflection (to the left) of the sound handle, and the touch through the instrument conveys the idea of the right-border of the uterus being advanced beyond the left-border. Finally, control of the position of the organ was very perceptible and was equalised in both ligaments.

The position of the organ after operation is figured in diagram 2.

(a) origin of right-round ligament  (b) direction of uterus.

That diagram at once shows the radical defect of the operation in this
instance, and furnishes a hint for all retroflexion operations. The uterus must first be straightened, as well as replaced, by the forceps, and maintained so before the ligaments are shortened and as long as necessary after this is done. Mrs Co's uterus was only straightened on the fourth day by a galvanic stem pessary. The position then was as in diagram B. (a) right round ligament (b) stem pessary.

The sound was at first passed by Dr Irvine with some difficulty; and the operation had to be revised. Then on second passing Dr Irvine failed. I tried, but could not pass the sound beyond the flexion, and there B.
Alexander himself did so, and it went with a jerk, apparently as suggested by diagram 2, not along the uterine cavity beyond the flexion, but into the substance of the organ. Her temperature was as high as 102°. 5° F. afterwards accounted for I thought by the passage of the sound, for the operation itself is one of the mildest in systemic effects. The careful reposition of the uterus by the sound then is a most important step in the operative procedure.

How the ligaments (when shortened) help to replace and retain the uterus in position is easily understood by a study of Savage's Plate 11 - On the Female Pelvis Organs - figure 2 (prolapse), if figure 3 (retroversion), of which the following are sketches wherein I have added arrows.
to show the direction of the force employed in pulling out the ligaments.

Plate XI, Fig. 2.

Fig. 3.

The organ is retained in position, doubtless partly by the shortened ligaments. According to Dr. Alexander, the uterine is placed by tightening of its "mooring ropes"—the round ligaments—"outside and below the current of force" tending naturally to drive the pelvic viscera towards the perineum. But—anyone who has looked at the diagram from nature, produced in Dr. Berry Harte's paper on Sacro-Pubic Hernia in the Edinburgh
Medical Journal for August 1880, will see that what takes place is that the uterus is drawn into another and more favourable situation of pressure, represented in the case of operation for prolapse by lines in diagram p. 105 which show a direction of pressure towards the pubic segment 1 to 9, and in the case of retroversion as figured by Savage, the almost normal direction of pressure represented by all the lines is substituted for direction indicated by lines 9 to 14, which I have inserted in my sketch of Fig. 3.

Diagram of the directions of intra-abdominal pressure within the pelvis.

Indications
Indications and Contra-indications.

The operation is indicated in cases of retroflexion and retroversion of the uterus, singly or in combination, where pessaries cannot be worn with bodily, or with mental comfort, or neither, to the patient, and where there are symptoms demanding relief. These symptoms may either be directly referable to the uterus or belong reflexly to the nervous system. (Eg. Dr. Macfie Campbell's cases, Liverpool Med. Chir. Jour. 1883 p. 235. He describes four cases — the first of extreme retroversion and prolapse with premenstrual asthma, wherein the displacements were cured, the asthma benefited, the 2nd of prolapse in the 3rd degree cured; the 3rd of prolapse "externally" and cystocele in which owing to adhesions postoperatively...
posteriorly the uterus could not be
moved by the ligaments. There was
no improvement; and the 4th case
retroversion and prolapse was retained
in position as near as possible to the
normal.

It is also
indicated in Prolapsus of any
degree, to keep the uterus within the
pelvis, in an improved position.

It is contra-indicated in very old
persons; in those laden with fat; or
small chested with large abdomens;
of broken down constitution.

Adherent uterus not-replaceable by
the sound. (This attitude should
always be determined before operating,
not afterwards, as in Dr. Campbell's case.

3.) Pelvic cellulitis of both sides pre-
venturing the ligaments from "running," but not of one side only.
During the child-bearing period the symptoms must be severe before operating: caution is required to be exercised.

"The operation may therefore be performed in all cases in which the state of the womb & of the patient allow it to be performed," and where symptoms demand its performance.

The wounds in Mr. Carter's case healed by granulation. The ligaments could be seen dragging the wound, and this probably was the cause of the delay in recovery of the wounded tissues. She lay on her back, with knees supported.
by a pillow. I suggested the prone position with support to the fronts of the thighs as calculated to remove the dragging, and to keep the uterus in better position during the wound healing and formation of the artificial insertion of the round ligaments. But it was not considered feasible, and was not therefore enjoined. I still, however, remain unconvinced, until I have seen it tried practically and found wanting.

Its effect. Ultimately in Mrs. C's case, says Dr. Alexander, the uterus assumed the position which is a slight improvement on that shown in Diagram II. She had, as she herself informed me afterwards, lost
her uterine symptoms, but "the fits and feelings were as bad as ever," which goes (to a certain extent only, owing to the flexion remaining) to show that the head symptoms were not dependent upon those of the uterus. Dr. Alexander had proposed Battey's operation, but, owing partly to her experience of the former operation and partly to her horror of the idea of being unseated, she has not yet consented to having it performed.

There can be no doubt that the operation is almost—without danger to the patient—and that it not only is calculated to improve many cases of the kinds to which it is applicable, but, on the testimony of Dr. Alexander himself, and of Dr. Macfie Campbell.
in the paper already mentioned, Dr. Imbachi of this city, and Dr. Leonard of Carlisle among others, it has already been the means of considerably improving the conditions of miserable patients. The example I have reviewed shows a result the least favourable; but the case was one beset with difficulty; on the other hand the case (5) of Mrs. B., the epileptic and sterile, must be regarded as one of its greatest triumphs.

In conclusion, I will only draw attention to one fact that it must not be forgotten that "the chief support of the pelvic contents is the compact unbroken pelvic floor." - Harr & Barbour Op. cit. p. 59. Harr article cited p. 103 - Therefore I would suggest that the
operation on the round ligaments should, in cases where the tip of the sacral segment—the perineal body has been much lacerated in labour, be supplemented by that for repair of the perineum.

We should, then, as far as we can at present see, have reached almost to perfection in the principles upon which proceed the operative treatment of Backward Displacements of the Uterus and Prolapsus Uteri.

It has struck me myself when a student in 1879-80, student-like, that relaxation of the round ligaments was a great 'cause' of retroversion of the uterus, as I have recently noticed by a marginal note in my Playfair's Midwifery—but it neither struck me, nor apparently anyone else, that tightening of that
relaxation would lead to 'cure' of that and other conditions, until Dr. Adams and Alexander solved the problem by seizing on the principle and supporting it by practical demonstration. They therefore, and as I have shown at the outset, Dr. Alexander especially, deserve all credit due to them for placing this important operation on the list of feasible contributions to practical Gynecology.
I, Archibald Drummond Macdonald, Bachelor of Medicine and Master in Surgery of the University of Edinburgh, residing at Number Twenty-six Spellow Lane, Liverpool, declare that the thesis entitled "Essays in Obstetrics and Gynecology" accompanying this declaration was wholly composed and written by myself alone. And further I declare that my age was thirty years on the Ninth day of March last, Eighteen hundred and eighty-four.

Archibald Macdonald

2nd May 1884