alone this operation is not to be recommended, unless it is clearly causing the patient great inconvenience and suffering, or possibly, as in my case, danger to life. A suitable truss would, of course, too, in any ordinary case be tried and found wanting prior to the consideration of any operative procedure.

As might have been expected, after the removal of an organ of the apparent importance of the spleen, though very little is certainly known of its functions, ultimate ill-effects have been recorded in some cases. After the patients have made a good recovery from the operation, they have begun to lose ground and to manifest symptoms of weakness, emaciation, thirst, and drowsiness. Thus, of the three cases brought before the Clinical Society in 1896 by Messrs. Ballance and Pitts, two suffered in this way. The unfavourable symptoms were successfully combated by the administration of sheep's spleen, raw bone marrow, cod-liver oil, and arsenic, and the patients were eventually restored to good health. All three of them, however, exhibited enlargement of the superficial lymphatic glands. In the case recorded here nothing except ordinary food, and no medicine whatever, has been administered since the operation. One year has now elapsed, and the patient has suffered no inconvenience at all from the removal of her spleen, nor does she appear to miss it in the least. There is also no enlargement of any lymphatic glands that can be palpated. A report on the blood kindly made for me by Dr. Leon on February 8th, 1905—that is, three months after operation—showed a decrease of white corpuscles (7,000 per cm.), and the lymphocytes relatively decreased. Possibly in cases where no after-effects have followed splenectomy, spleniculi may have existed which were not observed. None were observed in this case, and they were certainly not present in the situations where they have been usually noted—namely, the gastro-splenic and great omentum. At the same time, as no definite search was made for them, they may have been present and have escaped notice. The Malpighian bodies, situated on the minute branches of the splenic artery, are closely packed with lymphocytes, and their removal, along with the spleen, seems to throw increased work on the other generators of lymph corpuscles—the lymphatic glands, which consequently become hypertrophied. The other chief supposed rôle of the spleen is the purification of the blood by the destruction of effete red corpuscles, and the numerous extractions such as formic, butyric, lactic acids, etc., which are obtained from a chemical analysis of the spleen, are probably derived from the destruction of these worn-out constituents. It might be anticipated, therefore, that the removal of this great purifier, so to speak, of the blood would be followed by some such toxic symptoms as have been enumerated above, owing to the introduction into the system of poisonous matter, of which the spleen normally cleanses it. The ultimate disappearance of these symptoms may have been due to the remedies given. More probably they were owing to compensatory action becoming established in some other gland, or to the presence of spleniculi, which have stepped into the breach and gradually restored the normal balance. In the case recorded here no remedies have been given, which might have anticipated, so to speak, the want in the economy produced by the loss of the spleen. Yet its removal has been followed by no apparent deviation from normal health, nor is there evidence of any other organs of the body—for example, the lymphatic glands—being burdened with increased work. Whatever be the explanation, it is remarkable that the spleen, which, *a priori* from its size, its large blood supply, its structure—which alone of all organs in the body allows the blood to come into actual contact with the living cells of the tissues (Starling)—can be removed without the patient apparently suffering any inconvenience or deterioration of health. The probability is that though perfection of health requires the presence and normal working of every organ of the body, a degree of it which is by our imperfect methods of observation satisfactory, may be maintained on a much shorter allowance; and that by some compensatory action which may serve the purpose, but be less than perfect, the human organism may be able to do without some of its component parts apparently without serious inconvenience or disability. I say apparently, because the question as to whether these mutilations eventually shorten life cannot be shelved, and the after-histories of such operations as gastro-enterostomy, removal, or, what amounts to the same thing, exclusion of the large intestine, etc., has yet to be written. It is well to remember, however, that there is an after-history, of which complete thyroidectomy and cophorectomy have afforded

instructive and somewhat disconcerting object-lessons in the past. The mere ability to show a patient apparently in perfect health after the entire removal of some organ which *a priori* might be assumed to be essential to its well-being, though it may flatter our pride, should not in our surgical efforts lessen our respect for every part of the body, or diminish our consideration for any part, because we happen to be ignorant of its functions, or because our patient seems able to do without it. The assumption, for instance, that the large intestine is a cesspool is, to say the least of it, not a bashful one, and is, moreover, contrary to our experience of its uses. On theoretical grounds, I think, every surgeon would treat such an organ as the spleen at all events with respect, and would not remove it unless absolutely obliged to do so.

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SOME OBSERVATIONS ON PUERPERAL ECLAMPSIA.

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WHILE house-surgeon in Glasgow Maternity Hospital, I had an opportunity of studying 8 cases of eclampsia, and of analysing 42 others which had been treated during the three preceding years. As much interest has been manifested of late in this condition, I should like to draw attention to a few points in its course and treatment which particularly impressed me, and especially to the use of saline infusion and *accouchement forcé*.

Premonitory Symptoms.—Oedema was present in 30 of the 50 cases, but it is no criterion of the imminence of convulsions. It may be present for weeks or months—a symptom of chronic renal disease—but an intense toxæmia may occur so rapidly that convulsions forestall oedema. The swelling is more significant when affecting the cellular tissues of the face. It is a fairly frequent history that oedema of the legs and feet has been present for weeks; then, without increase of this swelling, the face becomes oedematous, and eclampsia supervenes within forty-eight hours. This sequence is well illustrated in 2 of the 8 cases. In the first, oedema of the lower extremities was present for eight days; the eyelids then became swollen, and convulsions ensued in twenty-four hours. In the second, oedema of the feet commenced three weeks prior to the attack, became gradually worse, and was succeeded by swelling of the face. Only eight hours later eclamptic seizures followed.

One of the most important, because one of the most suggestive and unequivocal, of the prodromals is pain in the epigastrium. The pain is at first slight, or there may be only a sense of discomfort; but when this symptom is well marked, it becomes later so intense that the woman is rendered prostrate and is forced to cry out. It may be associated with pain in other regions of the abdomen or in the loins, and may be accompanied by vomiting or difficulty and oppression in breathing. These concomitants are most frequent when the pain has become intense and continuous, and betoken the imminence of convulsions.

Epigastric pain was present in 6 of the 8 cases, but either this is a larger proportion than usual, or less attention has been paid to this symptom than its importance deserves. Munro Kerr¹ in an analysis of 80 cases found it mentioned thrice. Dührssen² in 200 cases found it mentioned once. Parvin³ called it the "least frequent" of the prodromata. It is similarly mentioned in other obstetric works as one of the least important and most inconstant of premonitory symptoms, or is not mentioned at all. Of these 50 cases, 33 are carefully reported as to the presence or absence of prodromals. In 17 of these 33 epigastric pain is mentioned as present, and in 16 is either not mentioned or said to have been absent. In my own 8 cases, when present, it became continuous and very severe shortly before the onset of convulsions. To my mind its significance is very similar to that of headache: both may be present for some time intermittently and not very acutely, but when either becomes intense and constant it is unmistakable evidence that the patient is far advanced in toxæmia.

Duration of Paroxysm.—Herman⁴ says that the duration of a fit is seldom longer than a minute, and that the interference with respiration is such that a fit which lasts longer than two or three minutes must be fatal from asphyxia. He explains

fits reported to have lasted for many minutes as really instances of a succession of rapidly-following paroxysms. One of my patients, a 2-para, aged 24, had had twenty-four seizures before admission; she then had five more at gradually lengthening intervals, the last, comparatively slight, at 4 p.m. She was delivered by forceps at 8 p.m. Next morning at 11.30 she had another convulsion, which I did not see, but which the nurse said lasted five minutes. In the afternoon at 3 o'clock, while I was in the ward, she had a fit differing from any eclamptic seizure I have seen. In the order of parts involved it exactly resembled the previous fits, thirty in number; but the progression of the spasmodic wave from one group of muscles to another was very slow, while the contractions were not more frequent than 30 per minute. Clonic spasm continued for eight minutes by the clock. Respiration during this time was short and gasping, but there was a hiss at each contraction, which meant that air was issuing from the chest. After we had watched the convulsion for eight minutes, chloroform was administered, and the movements ceased within a minute. This does not quite prove that chloroform was inhaled, for the fit may have been on the point of stopping in any case; but it is certain that efficacious respiration must have gone on during those eight minutes to some extent, otherwise the patient could not have lived. I am equally certain that during the whole period of clonic spasm the movements were quite continuous and regular, and that there was no interval at any time long enough to allow of natural breathing.

Another point which makes improbable Herman's explanation of long-continued fits being in reality a series with respiration active in the intervals, is that after an ordinary eclamptic seizure the patient does not immediately breathe, but for a few seconds (sometimes ten) lies absolutely motionless, with the face pale or cyanotic. If, then, a patient were to have a series of convulsions so rapid in succession as to be mistaken for one continuous paroxysm, the interval could not be made use of for respiration. It is almost certain that during a seizure oxygenation of the blood must go on to some extent, and, if that be the case there is nothing to prevent our acceptance of cases reported to have had very prolonged convulsions, lasting even to twenty minutes.⁵

Rapid Involution of Uterus.—In all my own cases a very rapid involution of the uterus took place, the fundus reaching the brim of the pelvis in one case on the fourth day of puerpery, and in the slowest case on the sixth. I do not know what is the cause of this, or whether it occurs in all eclamptic patients. Parvin³ says that the eclamptic is more liable to *post-partum* haemorrhage, but this does not seem to be the general experience. Jardine,⁶ in reporting a case where there was an "alarming" *post-partum* haemorrhage, says:

In my experience of eclampsias, which has been a fairly large one, I have only seen one other case of *post-partum* haemorrhage, although many of them had been under chloroform for a long time, and the uterus had been purposely left to relax if it would.

This tendency to immediate retraction was remarked in all my cases. Even without manual control of the fundus during the third stage and after, the uterus at once retracted firmly, and the bleeding was less than usual. This natural "good management" of the placental stage may partly account for the rapid involution.

Treatment.—Although we cannot, from want of knowledge, employ a method of treatment calculated to influence the primary cause of eclampsia, our efforts may be so far rational that we can improve some of the morbid conditions present, which, although not the first step in the disease, tend to increase the general breakdown of the economy, resulting in epileptiform seizures. For example, the failure of the kidneys to secrete urine is not the original cause of eclampsia, but we know that if suppression of urine cannot be speedily relieved the patient will die, hence treatment directed to the promotion of diuresis is to some extent rational, and certainly it is scientific and will be helpful to recovery.

Now, in Glasgow Maternity Hospital the death-rate from eclampsia prior to 1898 was over 40 per cent.; since then it has been less than 25 per cent. The class of patients and the general circumstances are the same, the obstetrical treatment is practically the same, yet a great fall in the death-rate has been established, and that over a large number of cases. The only great change in the treatment has been the introduction of saline infusion. Is it not, then, unhealthy scepticism to assume that a new method, introduced for the first time just prior to the improvement in the results and continued ever since, has not been largely instrumental in the improvement, or to say that "no treatment has been

proved to do good,"⁴ and that "there is no treatment of eclampsia from which benefit can be certainly predicted"?⁷

The intercellular infusion of saline solution was introduced into Glasgow Maternity Hospital by Dr. Jardine. The solution now used is 1 drachm each of sodium chloride and acetate of soda to a pint of water. This solution, sterilized and at 100° F., is run into the areolar tissue beneath the breast, or after delivery, into the lax abdominal wall. The object of the treatment primarily is "to get the kidneys to act."¹⁸ In Herman's article on eclampsia in *Allbutt's System of Medicine* (vol. vii) there is this statement:

The secretion of urine begins to be re-established in from six to twenty-four hours after the cessation of the fits . . . but it takes from two to four days for the secretion to become as abundant as in health.

If we compare this with the accompanying table of cases where saline infusion was used, it will be seen that diuresis was always equal to, or even in excess of, the normal quantity within twenty-four hours of the cessation of fits.

Case.	Hours from Cessation of Fits.	Amount of Urine.
1	First 24 hours	Two pints.
2	" "	" "
3	" "	" Passing urine copiously." [*]
4	First 18 hours	Three pints.
5	" 12 "	" Fair quantity."
6	Second 12 hours	" Passing urine freely, probably about two pints.
7	First 12 hours	Urine was lost.
8	Second 12 hours	One pint.
9	First 24 hours	" Nearly two pints."
10	" "	Two and a half pints.

* When the exact amount is not stated the collection of the urine had been interfered with. The words in inverted commas are from the nurses' reports.

Analysis of the urines of eclamptics during convalescence⁹ shows a marked increase in the elimination of urea and uric acid, and probably other bodies are washed out which are even more harmful to the patient. Besides this eliminant effect saline infusion dilutes the toxins still in the blood stream, and this may be observed in other diseases—for example, diabetic coma.

There is the further benefit to the patient of the stimulation which immediately follows this treatment in collapse. This effect is of course best seen when the collapse is due to loss of blood, but even in other cases it is often marked, for at such times the disposition of the quantity of blood in the body makes the same circumstances for the heart and brain as an external haemorrhage—for example, the collapse subsequent to a lengthy operation when there has been practically no bleeding.

Infusion of saline solution may, however, be overdone. If the patient be very oedematous, if there be marked cyanosis with high arterial tension, and if there be much pulmonary congestion, it should be practised only to a moderate extent (say 1 pint) unless the patient be bled at the same time. There is more than a slight risk of increasing the work of the heart by too large infusions, and the result will be, if not an immediate cardiac failure, an oedema of the lungs which will be no less certainly fatal.

Obstetrical Treatment.—It is generally admitted that peripheral irritation is a bad thing in eclampsia, whether caused by uterine contractions or obstetrical operations, hence we should adopt that line of treatment which will subject the patient to least irritation. If the labour be well advanced and the os sufficiently dilated, it cannot be thought that a forceps delivery under chloroform would be as great a source of irritation as a succession of labour pains in the second stage, becoming more frequent and forceful, and probably lasting for an hour or more. The real difficulty in deciding on the proper obstetric treatment will only arise when the convulsions have occurred before or during early labour.

Herman¹⁰ has shown that after delivery, in about half the cases, the fits do not cease; and he points out that the disease runs its course in less than forty-eight hours, and that in most cases where convulsions began in pregnancy or early labour a cessation of the fits would occur about the time of delivery, independently of whether the uterus were empty or not. In my 8 cases it appeared that in 1 case delivery stopped the fits, in 2 cases mitigated the severity of the fits, and in 4 cases had either no effect or a bad one. Of the other

42 cases, 2 died undelivered, 7 occurred in the puerperium; the effect of delivery on the remaining 33 is here shown:

No effect, or fits ceased three hours before delivery ...	21
Fits ceased at delivery, though frequent up till delivery ...	4
Fits mitigated after delivery, though severe just before delivery ...	3
Fits more severe and frequent after than before delivery ...	5

These figures strongly support Herman's contention that delivery has no definite influence in cutting short the convulsions. But, besides the effect on the convulsions, we must consider the effect of *accouchement forcé* on the patient. One cannot adopt a line of treatment to benefit the fits if it is shown to be harmful to the patient. Herman found that the chances were about 2 per cent. better if delivery were hurried than if an expectant attitude were maintained. In my 50 cases I find the death-rates with *accouchement forcé* and with expectant obstetrical treatment exactly equal; but of the 7 patients in the above table, where *accouchement forcé* had a good effect on the convulsions, 6 died within a week from sepsis, cardiac failure, or oedema of the lungs. This very strongly suggests that the good effect on the fits may have been secured at the expense of the patient, and it is sufficient to show that *accouchement forcé*, even when the patient is "deeply" under chloroform, is a very severe shock to her, and an extra risk.

In these 50 cases, of the 42 which occurred before the birth of the child, only 4 were during the expulsive stage. It is important to note the difference in the two clinical types: (1) those occurring in pregnancy and early labour, and (2) those occurring during the second stage. In the first class the patient has probably been in the pre-eclamptic condition for some time, and the slight additional irritation of the first pains of labour is sufficient to determine reflexly convulsions which were already impending. In the second type the toxæmia may not have been well marked, and the strong stimulus of the expulsive pains in the second stage is required to bring about the loss of balance of the cortical centres.

If such a case be immediately chloroformed and delivered by forceps she will probably have few succeeding convulsions or none, but to apply the same treatment to a patient whose labour has not begun, or is in an early stage, is adding another risk, and is not only useless but harmful treatment.

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THE SURFACE TENSION OF URINE IN HEALTH AND DISEASE:

WITH SPECIAL REFERENCE TO ICTERUS.

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IN recent years the chemical examination of physiological fluids has been considerably supplemented by an application of the newer physico-chemical methods. Amongst these may be mentioned determinations of freezing point, electrical conductivity, refractive index, viscosity, etc. Thus the freezing point of urine has been employed in the clinical diagnosis of renal disease, whilst the freezing point of milk affords valuable information to the analyst and the medical officer of health. Extensive researches have also been carried out both in this country and abroad on the electrical conductivity of urine. In the regions of physiology, as distinct from those of applied pathology and clinical diagnosis, reference may be made to the brilliant researches of Paul and his pupils on the equilibrium of uric acid and the urates, to the electro-chemical investigations on the question of the so-called alkalinity of the blood, and to the innumerable kinetic investigations on the mode of action of the salivary, gastric, and pancreatic ferments. With regard to the vital question

of immunity from disease, there is every hope at present that the modern methods and results of physical chemistry, more particularly those pertaining to the nature and mutual action of colloids, will be of great service. It is, however, with the direct employment of physico-chemical methods in the clinical diagnosis of disease that we are more particularly concerned in the present paper.

I. OBJECTS OF INVESTIGATION.

Without attempting any analysis of the already very extensive literature in this department of science, the subject in hand may be illustrated by a reference to the work of Ph. A. Guye and St. Bogdan.¹ The methods referred to above are here considered in relation to urine. The nature of the results obtained will be immediately evident from the following table:

	Normal Urine.	Pathological Urine (Tuberculous).
Electrical conductivity (18° C.) ...	0.01937	0.00929
Lowering of freezing point ...	1.39	0.39
Coefficient of viscosity (18° C.) ...	0.0113	0.0109
Index of refraction (n_{18°) ...	1.3397	1.3355

The pathological urine exhibits a very much smaller conductivity and freezing-point depression, thus pointing to a great diminution, not only in total dissolved matter, but also in those constituents (salts) which conduct the electric current.

Of course, variations such as these can only acquire value for the physician when they are considered in the light of an extensive experience of similar measurements made on urine under varying conditions of health and disease.

Whilst considering this subject, it occurred to us that it would be of interest to carry out an investigation on the surface tension of urine, as it is already known that, under certain pathological conditions (icterus), the surface tension of urine is liable to be greatly lowered. In this connexion the simple and easily carried out sulphur test of Matthew Hay will probably be familiar to every physician. Special experiments have shown that Hay's test is of very considerable delicacy—for example, we found it was easy to detect an addition of 0.1 per cent. by volume of ox bile to normal urine, the limit being apparently reached at about 0.04 per cent. This method, however, gives only qualitative results, and is therefore not adapted to the purposes of scientific investigation.

Many methods are available for the measurement of surface tension, but the majority of these are unsuitable for the purposes of clinical diagnosis. In the choice of a method we have been guided by two main considerations: (1) The method must be simple and easily carried out, and must not involve the employment of delicate or elaborate instruments; (2) the method must be such that the results are not affected by accidental contamination of the surface of the urine to be examined.

As is well known, the surface tension of water is greatly lowered by a very small amount of greasy or fatty matter. Since it would in general be difficult to guard against such contamination under ordinary circumstances, it will be seen that the second condition referred to above is sufficient to exclude the majority of the methods commonly in use, since in these methods there is no constant formation of a new surface from the bulk of the liquid, although the contaminated surface can be purified by adopting special precautions. This condition is, however, satisfied by the method in which the liquid to be examined is allowed to drop slowly from a pipette and the number of drops contained in a given volume of fluid counted. As this method, moreover, requires only very simple and easily-constructed apparatus, it is clearly the one which is best adapted to the purposes of clinical work.

The general principle may be easily explained. The hanging drop is supported against the action of gravity by the tension acting in the surface of the liquid. It has been shown that for an orifice of given diameter the surface tension is proportional to the weight of the drop.² If v be the constant volume of liquid delivered from the pipette (as determined by two marks on the stem), d the density of the liquid, and n the