to be but a floating kidney, inflammation of the surrounding tissues would in all probability have developed at no distant date, greatly increasing the difficulty and danger of any future surgical procedure. I do not deny that the disease might have remained quiescent; the experience of Bruce Clarke, Murray (of Capetown), and others, have shown how people manage to keep alive with very bad kidneys. In my case, it must be remembered, the kidney was movable, and thus much exposed to injury.

2. Acting on Mr. Knowsley Thornton's principles, I reached the diseased organ through a Langenbuch's incision, and was thus enabled to gain a fair idea of the state of the opposite kidney. I admit that there was danger, in this instance, of letting pus escape into the peritoneal cavity; but the abdominal incision enabled me to enucleate the kidney with ease, keeping both aspects in sight during the whole process, and I could clearly distinguish the state of the hilum and ureter at a very early stage of the operation.

3. This clear view of the kidney and its relations afforded by Langenbuch's incision demonstrated that the diseased kidney could not possibly have exercised its functions for some time, and could hardly ever do so again. As Noble of Philadelphia² has observed, in reference to a case somewhat like my own, "Previous experience in somewhat similar cases had shown that when the system was relieved of the presence of one suppurating kidney, the other, even though not perfectly normal, did its work better." The ureter, atrophied below the pelvis, did not require Howard Kelly's very radical nephro-ureterectomy, which is not very difficult in a female subject, and is good and thorough surgery when the ureter is extensively diseased.³

The Absence of Pain.—It is chiefly because pain was entirely absent in this case, the main subjective symptoms being tenderness and nausea caused by the pressure of stays as in any movable kidney, that I publish this record. Most authors admit that painless pyonephrosis may occur exceptionally, but find that as a rule it is painful. In Morris's tables⁴ all the 8 cases⁵ of pyonephrosis are reported as "painful"; in one (No. 13) the kidney, as in my case, was movable. Here I may note that the clinical history affords explanation why the kidney disease was purulent; it was clearly not a case of a movable kidney becoming first hydronephrotic through obstruction of its ureter due to kinking, and then suppurating. The mobility had little or nothing to do with the disease.

To return to the question of pain, it is generally admitted that a calculus does not cause suffering when impacted; in my case there was both a loose and an impacted calculus. Mr. Bruce Clarke, who perhaps more than any other authority, has brought forward evidence to prove how long extensive kidney disease may be borne, informs me in respect to the present case that he does not know whether he has observed a case of painless calculous pyonephrosis specially, but painless kidney containing calculus is common enough if the stone be fixed, and painless pyonephrosis without stone is almost the rule in his experience. Surgeons must never forget his remarkable researches in the *post-mortem* room at St. Bartholomew's Hospital from 1874 to 1884. "24 kidneys containing stones were discovered; 11 of these had well-marked symptoms during life and 13 had not."⁶ He quotes Dr. Murray's case (Capetown) where there was double calculous pyonephrosis⁷. I have looked up the original report, and find that the case was somewhat like mine, but was bilateral, whilst neither ureter was totally obstructed. As for pain, there was not even tenderness. The patient had been weak all through her life. Shortly before her death she suffered from attacks of faintness with fever and diarrhoea. A tumour was detected in the left loin, free from tenderness; it grew bigger and pus was passed freely. Finally, the patient died delirious, after being three months under treatment. Murray's report of the necropsy is summed up in the sentence quoted by Bruce Clarke. The two kidneys "were simply bags of uriferous pus, very little indeed of gland structure remaining."

Mr. Morris has recently informed me that only last month he operated within a week on two cases of renal calculus, the first intensely painful, the second painless. In the first, where pain was constant, he found a small stone packed away in a recess in an upper calyx. The second case bears striking points of resemblance to my own. The patient was

a lady, aged 47, who had never suffered from any pain or local discomfort, beyond aching on the right side at night when she lay on the left side, and this symptom was attributed to rheumatism. For two months before operation she had passed bloody urine almost uninterruptedly. The right kidney was opened. It was riddled with calculi and pus sacculi. Nineteen calculi were removed; but the kidney was simply drained, and the case is doing well. In this instance there was painless calculous pyonephrosis with a distinct urinary symptom; in my case the same condition with no urinary symptom, as the ureter was blocked.

The Absence of Fever.—My patient declared that she had never experienced anything like feverishness or rigors. She had attended to her domestic duties without a day's rest for years—and, as she had six living children, the eldest hardly 14 years old, those duties, I presume, were heavy. We know, however, that in chronic suppuration there may be little rise of temperature, and that even a temperature of 103° or 104° is not inconsistent with little loss of strength and a sensation of uneasiness soon forgotten. I dwell on this symptom—or absence of this symptom—because we are all apt to put down absence of fever or of any history of fever as evidence that, in any kidney case, suppuration is improbable. More often, however, as Morris and Bruce Clarke have already pointed out, we persist in thinking that stone in the kidney is always painful, though we ought to know better. Plenty of cases have been recorded where pain was absent. My own case shows that it may be absent, and that a patient may go about with one kidney converted into a bag of pus, yet appear in excellent health.

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THE TREATMENT OF PUERPERAL ECLAMPSIA BY SALINE DIURETIC INFUSIONS.

By ROBERT JARDINE, M.D., ETC.,

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In his criticism of my views on the treatment of puerperal eclampsia by saline diuretic infusions, Dr. R. P. Rankin Lyle says I make a great mistake in considering diuretic infusion the treatment. A wider experience than I had when I wrote the paper makes me more confident than ever that the infusion is the essential part of the method. My aim is to dilute the poison, and get rid of it as quickly as possible—that is, to treat the cause and not a mere symptom.

I have recently had under treatment some half-dozen cases of marked dropsy and albuminuria of pregnancy. As they had all reached the stage of severe headache, I think one was quite justified in concluding they were within an ace of having fits. By prompt purging with salts, and the use of diuretics and a milk diet, they all except one escaped eclampsia, and were safely delivered some time later. The excepted case was in the second stage of labour when seen, and her urine was loaded with blood. She had five fits *post partum*, but recovered after saline infusions. There was no time to establish diuresis by drugs in the ordinary way. If I had seen her a few days earlier I have little doubt I could have prevented her having fits.

If by the establishment of diuresis one can prevent the fits, surely it is reasonable to suppose that they can be cured by the same method. When the fits are occurring diuresis cannot be established quickly enough in the ordinary way. Absorption by the stomach is almost in abeyance, and, even if it were normal, a good many hours would have to elapse before the action could be set up. Hence my method of giving the drugs subcutaneously. Besides the diuretic effect, we get a dilution of the poison and a stimulation of the

patient. We get the same effect as in the treatment of septicæmia by saline infusions.

Dr. Lyle says that the infusion "has no immediate effect in controlling the convulsions, nor has it any effect on the action of the uterine muscle." As I have treated 25 cases of my own, and have been associated with others in the treatment of at least a dozen more, I may fairly claim to have had not a little experience in the matter. Whatever Dr. Lyle's experience may have been, mine is that there is a very marked influence on the fits. I have now treated a fair number of cases without giving anything to check the fits. The following may be taken as an illustrative case:

In the end of December Dr. Ramsay of Parkhead sent in a primipara who had been delivered under chloroform after having had one fit. The fits recurred about an hour after delivery, and she had had twenty by the time she reached the hospital. She had two more before the nurses had finished washing her. She was deeply comatose. The excretion of urine was almost suppressed. The albumen stood slightly above the U mark in Esbach's albuminometer. Four tablespoonful of salts were poured into the stomach through a tube, 2 pints of infusion given, and a hot pack applied. Two and a-half pints of infusion were given six hours later, and 2 tablespoonful of salts. She never had another fit. In the first nine hours 16 ounces of urine were drawn off, so the kidneys had quickly reacted to the infusion. In the next twenty-four hours 37 ounces were obtained, and the bowels had moved copiously seven times. In the following two days 168 and 212 ounces were collected.

If it was not the infusion which stopped the fits in this case, what was it? This is not an isolated case. As to any action on the uterine muscle, there may be none directly, but in so far as the woman's general condition is improved, there must be improvement in the uterine condition.

Dr. Lyle is to be congratulated on his results with morphine. I have used it frequently, but my results have been very unsatisfactory. He claims certain advantages for it. He says "it prevents excess of waste products being thrown into the blood." On what authority does he base such a sweeping assertion? That it has no effect on the kidney I am not prepared to admit. I am firmly convinced that it lessens excretion.

As to the obstetric treatment of the condition, my experience teaches me not to interfere if labour has not begun. During the first stage if the fits cease dilatation may be left to Nature, but if they recur the uterus should be emptied as quickly as possible. During the second stage delivery should be effected at once. When any operation is being done the patient should be put deeply under chloroform to lessen shock. To say that all my fatal cases were cases in which I interfered, and to infer that they would have done better if they had been left alone, seems to me to be hardly warranted. I think he will find that all the cases delivered forcibly did not die. In every case in which I interfered I had reasons for doing so; and although I lost some of them I am quite sure I saved others. It is the bad cases one has to deal with in this way, and if one loses some of them the interference must not be condemned.

Dr. Munro Kerr has collected all the cases of puerperal eclampsia treated in the Glasgow Maternity Hospital during the last fifteen years. He informs me that the death-rate among the cases treated by chloroform, chloral, and bromide, veratrum viride, or morphine, etc., was 47 per cent. The reason of this exceptionally high death-rate is that most of the cases are brought in after they have been convulsed for many hours. We rarely see them within twelve hours of the first seizure. Since the saline infusion has been added to the treatment the rate has fallen to 17 per cent. If there had been only 8 or 10 cases one might imagine it was a coincidence, but this could hardly happen with over 30 cases. The first year I began it we had 8 cases and 5 died. The 3 which recovered were the only ones in which the infusions were used, and they were not by any means slight cases. I leave these figures to speak for themselves.

A CASE OF PUERPERAL ECLAMPSIA AND ITS TREATMENT BY MORPHINE.

By GEORGE ELDER, M.D.,
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IN the BRITISH MEDICAL JOURNAL for November 24th, 1900, there is a paper by Dr. Fitzgerald on Puerperal Eclampsia, showing the value of morphine as the remedial agent, and it is

in further illustration of this opinion that I venture to submit the following notes of a case of Dr. Bloomer, of Long Eaton, to whom I am indebted for them, as well as for the opportunity of seeing the case.

Mrs. C., aged 35, primipara, was delivered on October 29th, 1900, of a child which was alive shortly before birth, but was stillborn. For two months prior to her confinement there had been swelling of the legs and feet, yet she did not consider herself sufficiently ill to seek medical advice. An hour after delivery the patient suddenly became convulsed, the attack being said to have lasted from 5 to 6 minutes, but when Dr. Bloomer saw her shortly afterwards consciousness had returned. Hot applications to the loins were ordered, and a dose of 20 grs. of chloral hydrate and 30 of potassium bromide to be given every four hours. During the six hours which followed, she had three seizures of increasing severity, when morphine gr. $\frac{1}{4}$ was administered hypodermically with the effect of stopping the fits and producing two hours' sleep. On awaking, she was very excitable and delirious, but there was no recurrence of the fits, and excepting night draughts of bromidia no further medicines were required. Her recovery was complete. With regard to the condition of the urine, no examination of it was made before labour, nor could there be for twenty-four hours after, as there was complete suppression; but when examined the following day, it was found to contain one-third of albumen. Writing two months after, Dr. Bloomer states that the urine still contains a trace. So far as the previous history of the case is concerned, it was elicited that no member of the family had ever been subject to fits of any kind.

A disease which kills 30 per cent. of the mothers, and those usually taken from the young and the healthy, and 50 per cent. of the children is one which must always have an urgent claim upon our attention, more especially as, in spite of all that has been written and said upon the subject during the last fifty years, we are still very much in the dark as to its nature, its causation, or even its treatment. We know it is due to some form of auto-intoxication, and that in the vast majority of cases this follows upon renal insufficiency either as an expression of a passing congestion or organic disease; still, this leaves a goodly number of cases unexplained.

The late Dr. Angus Macdonald pithily summed up the situation when he said that there might be albumen in urine without eclampsia, eclampsia without albumen, and eclampsia with albumen; and his still more distinguished fellow townsman, Sir James Simpson, gives instances in his *Obstetric Works* (vol. i) of fatal cases in which from beginning to end no trace of albumen could be discovered. It ought not to be lost sight of that, as long ago pointed out by Trousseau, highly-strung, excitable women are more predisposed to this disease than others. Still, after making due allowance for cases having other than a renal origin, the fact remains that the majority have that origin, and it cannot be doubted that if it became the routine practice for the urine of pregnant women to be examined at short intervals from the fourth month to the end of pregnancy many valuable lives would be saved.

Prophylaxis is what is wanted in the treatment of eclampsia. Putting the patient upon a rigid milk diet as recommended by Tarnier with such uniformly excellent results as soon as there is evidence of kidney failure, combined with other measures for stimulating the functions of the skin, the kidneys, and the bowels would, in the vast majority of instances, obviate the necessity of even considering the question of the destruction of foetal life by the premature induction of labour.

When, as in Dr. Bloomer's case and in Dr. Fitzgerald's, the medical attendant is suddenly brought face to face with a patient in convulsions, whether these be before, during, or after labour, no remedy is so quickly and certainly sedative as morphine hypodermically administered.

A CASE OF MULTILOCULAR CYSTOMA OF THE OMENTUM: REMOVAL: RECOVERY.

By

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THE case is reported on account of its apparent rarity. We have been unable to find a full report of a similar growth. True, simple serous cysts of the omentum should not be designated rare in children, though they are by no means common, but the extent of the condition in this case, the