

her wise good neighbours, and even by her medical adviser. And so she goes patiently on, like the doomed ox, to her fate. In the 24 hours more or less of labour, while finally expelling the child, the uterus in its upheavals has had all its sinuses opened to receive the poison which may have lain dormant during the foregoing months.⁹ And now the woman has an attack of "idiopathic puerperal fever." That is all. It may not be the gonococcus itself which produces all the turmoil, indeed, very likely it is not, but the toxins, which are quite capable of finding their way either by the tubes or preferably by the uterine lymphatics to the peritoneum, where they have things all their own way. I have notes of 30 cases of primiparæ, with history of infection, who suffered from this form of puerperal fever. These were all personal cases and the writer can vouch for the aseptic management of the labours. In the after light thrown by more recent studies I can recollect many others not noted as such who undoubtedly belonged to the same category. The patients in these cases often do not die, they only become lifelong invalids. In their after-life these martyrs generally suffer at each menstrual period. There is always some trouble; even a fresh urethritis has been seen to occur month after month at that epoch. These patients rarely become pregnant a second time, but if they do they are liable to abort¹⁰ and a hard time they have, running the same gamut as in their confinement at full time.

In my notes in prostitute cases there are several of *puella* with chronic endometritis who followed their vocation without giving disease to their "friends" during the most part of the month, but who invariably infected those men who had the fortune to enjoy their embraces on the nights immediately succeeding menstruation. These are the cases, too, where men are infected by women while menstruating. It is not the menstrual fluid (I am not forgetful of M. Gautier's researches on that liquid) which gives the urethritis, but the stored-up gonorrhœal pus or its equivalent which at that time comes down into the vagina together with the blood. Cases of urethritis contracted from women with leucorrhœa will likewise stand investigation many times. That such cases are of a benign type as a rule simply indicates that the virus has been attenuated *more homœopatico*. It is taught that when an organism has lived a saprophytic existence for some time its virulence is greatly diminished. Still, some of the most virulent cases I have ever had in my practice were said to have been derived from "whites."

I am, Sirs, yours faithfully,
Guatemala City, Central America. J. H. ARTON, M. D.

THE ADMINISTRATION OF CHLOROFORM TO MAN AND TO THE HIGHER ANIMALS.

To the Editors of THE LANCET.

SIRS,—Will you permit me to reply briefly to Dr. J. Blumfeld's criticisms, in THE LANCET of Jan. 9th, p. 122, of some remarks of mine in your issue of Dec. 19th, 1903, p. 1757? I must have very imperfectly expressed my meaning since Dr. Blumfeld attributes to me the view that the patient's safety does not depend on the care and skill of the anæsthetist but upon the particular instrument he uses. This amounts to an almost Gilbertian suggestion that the anæsthetist's responsibility would be limited to turning on the supply tap of his apparatus at the commencement of the operation and turning off the tap at its conclusion, while in the interval he allows his attention to wander. Dr. Blumfeld very rightly, as I venture to think, controverts this view with an

⁹ Menge and Krönig say: "Gonorrhœal puerperal fever is to be regarded as due to an infectious process, already existing in the body, not as an example of autogenesis."

¹⁰ The tendency to abort observed in the gonorrhœal woman comes from a "placental decidua" (Vial). It is worthy of note that it is suspected, if not quite proven, that many cases of puerperal convulsions are the result of a nephritis produced by the uterine gonorrhœal infection, conveyed probably by the circulation. It will be remembered that the terminal branches of the left utero-ovarian vein empty into the left renal vein. To connect our former empirical ideas of the pathological condition underlying these cases (pressure on the renal veins) we have only to go a step further and, given a stasis from pressure, it is easy to understand how the gonococci from the womb reaching this point and being detained and finding a suitable culture medium (human blood being the best) should spread and in colonies invade the kidneys. This may be considered fanciful but I leave it so.

admirably reasoned argument in which the suggestion is carried to its logical conclusion—absurdity. Unfortunately, however, I do not hold such a view and did not, as I think, advance it. The middle paragraph of my letter contained evidence of my recognition of the existence and importance of "that uncontrollable factor—the character of the respiration."

My contention was, and is, that the anæsthetist is in a much more scientific position when he supplies to the patient a vaporous mixture the exact quantitative composition of which he not only knows but can vary with precision as the necessity of the case requires and when he can completely control the quantity of such mixture supplied than when he is administering a mixture the quantitative composition of which he can only guess at and can only vary its composition by the same inaccurate means. It seems to me that in either case every whit as much care and skill is required during administration. Since of those two elements of danger in the production of anæsthesia, the character of the respiration and the strength of the vaporous mixture, the former cannot be controlled, while the latter apparently can, then the extent to which this control can be exerted is a measure of the extent to which the danger from this element can be minimised. Such a view does not imply that all danger can be removed from the operation of administering an anæsthetic.

Dr. Blumfeld's remarks on the limitations of the various forms of apparatus mentioned are valuable, particularly so as they are probably the result of his experience. An apparatus which does not allow sufficient variation of dosage would indeed be inefficient. With his concluding remarks on the necessity of experience and teaching in the use of anæsthetics for all who may have to administer them I am in entire agreement. I am, Sirs, yours faithfully,
Bromley, Kent, Jan. 9th, 1904. W. J. LINDSAY.

GLANDULAR FEVER.

o the Editors of THE LANCET.

SIRS,—In connexion with Professor J. W. Byers's article on Glandular Fever in THE LANCET of Jan. 9th, p. 84, it may be of interest to give a short note on a somewhat similar epidemic which occurred here in September last. Seven cases came under my notice and common symptoms in all were as follows: (1) rise of temperature; (2) slight sore-throat; (3) a unilateral swelling of the glands in the neck situated in front of the sterno-mastoid muscle and just below the angle of the jaw; and (4) a discharge of pus and mucus from the nose. Five of the cases had a purulent discharge from the ear on the same side which commenced at the same time as the swelling of the glands; these were the first five cases that occurred, the last two not having any discharge from the ear. The first two cases were the most severe, an intense cellulitis forming round the swollen glands. This was incised but practically no pus escaped. A swab was taken from the throat of the first case and sent to be examined for diphtheria but proved negative. Films were made of the pus from the ears and wounds and on being examined microscopically I found that the only bacterium present was the pneumococcus. I got Dr. F. W. Eurich, the city bacteriologist, to grow cultures from the pus and he confirmed my result as to the condition being a pure pneumococcal infection and remarked that he was struck with the fact that the swab previously sent to him had also given an almost pure culture of the pneumococcus. As several other cases of pneumococcal infection, such as abscesses in the vicinity of the knee-joint, a case of mastoiditis, &c., have since come under my notice here, it would be of interest to know if the abnormal atmospheric conditions of the last few months have had any effect in causing this coccus to take a special action like the above.

I am, Sirs, yours faithfully,
J. KENNEDY MATHESON,
House Surgeon, Children's Hospital, Bradford.
Bradford, Jan. 11th, 1904.

RADIUM IN THE BATH WATERS.

To the Editors of THE LANCET.

SIRS.—So much public attention has lately been drawn to the presence of radium in our thermal waters that I should like with your kind permission to say a few words as to the bearing that this substance may have upon the hydrotherapeutics of Bath. Into the general facts as to the way