a month of the act. This act of intermission continued in operation for seven days.

The epidemic then subsided; but a large number of sporadic cases occurred in houses in which epidemic cases had been introduced, and in which it was afterwards found that serious defects existed in the house-drains. These defects seemed to expose the inmates to fresh sources of contagion; and others then suffered from the disease who did not in the first instance get a sufficient dose of the typhoid contagia to set up the fever.

The second epidemic, which followed upon the intermission of water-supply on the 16th of September, has not been at present succeeded by these sporadic cases. The larger number of the cases have occurred in the high level district, which includes a large number of the best houses in the neighbourhood, and those which have the best arrangements as to drainage. The first outbreak affected this district equally with the second. The second outbreak mainly affected this district, but including those parts of the low level which are in immediate communication with the high level water-mains. Both epidemics came to a climax five weeks after the act of intermission was committed; but the second has been more simultaneous than the first, because the act was committed for a few hours only, whilst in the first instance it extended over several days. The second, although it occupied a less extended area, has been more general and more severe, which may be easily understood, considering that the foci of contagia were much more numerous on the second occasion than on the first, and the chances of escape from their influence much less.

There are many other points of considerable importance which are worthy of record, and which may form the subject of another notice. One, however, may be mentioned here; namely, that in a certain street the houses are supplied with water from two different sources; those on the right by the Croydon Waterworks, those on the left by the Lambeth Company. The sewer is in the middle of the road, and is common to both. The fever affected only those who were supplied with water from the Croydon waterworks.

OBSTETRICAL STATISTICS.*

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THE importance of obstetrical statistics must, at the present time, be obvious to every one who is much engaged in the practice of midwifery. In no department of the healing art is there a greater need just now for clear and definite principles by which we may shape and guide our future practice. Within the memory of most of us, the changes in this department have been so strange and sweeping, that they constitute little less than a revolution. During the last half-century, the practice of medicine and the practice of midwifery seem in this respect to have changed places. The *medicina perturbatrix*, that heroic system of treatment which prevailed in the time of our fathers, is, in these days, superseded by a more careful and expectant plan; whilst, on the contrary, the somewhat timid *laissez faire* policy of waiting upon Nature, and giving her no assistance until she had proved herself incompetent to do her work, has, in the hands of the modern accoucheur, given way to an active anticipation of her wants, and sometimes, I fear, to an officious interference with her operations. To bring these changes to the test of time and experience, which alone can decide upon their respective merits, we require records of cases accumulated by a great number of independent observers during many years of private practice, in addition to the valuable statistics which we already possess from the various lying-in institutions. If every man who is in extensive midwifery practice would keep accurate notes of all his cases, and, on relinquishing the active duties of his profession, would follow the good example set by Dr. Churchill of Dublin, and publish a statistical account of them, what a mass of valuable information we should obtain! The records of lying-in charities are, in their way, most important contributions to obstetrical science, as all must acknowledge who have read, for instance, Dr. Johnston's masterly report of the Rotunda Hospital at Dublin for last year; but they are incomplete in one respect; viz., that they furnish statistics of labour amongst the lowest grade of society only; whereas every one who has been much engaged in private practice knows that the process of parturition is much affected by station and habits of life. It is only, therefore, from careful records of private practice that we can obtain statistical evidence of the manner in which the process is performed in the upper and middle classes of society.

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Most accoucheurs who have been many years in full practice have had a tolerably wide experience of all classes; for their career usually commences amongst the poor and ends amongst the rich. Hence they are well qualified to make a comparison between the mode in which the process is performed in one class and in the other. And all who are in a position to do this must, I think, be prepared to admit that those who work hard for their daily bread have an advantage in this respect, and that, by a natural rule of compensation, a hard life tends to produce an easy labour. That the function of parturition should be thus affected by habits of life is not so much to be wondered at, when we see how evidently the function of lactation is modified by the same conditions. We all know that, amongst the labouring classes, a woman who cannot nurse her child is a rare exception, whilst amongst the rich and luxurious it is equally uncommon to find a mother who can nurse properly; on the contrary, the milk is deficient either in quality or quantity, and we see the same course of events recurring, and hear the same old story repeated with tiresome reiteration; the mother has nothing, or next to nothing, for the baby, and the child, thus deprived of its natural food, cannot digest the artificial food which is substituted for it, diarrhoea sets in, and the doctor who is called in to prescribe for this state of things is obliged to recommend that the natural deficiencies of the mother should be supplied by a hireling substitute drawn from another grade of society.

In order to institute a fair comparison between the process of parturition amongst the rich and amongst the poor, we require the records of a large number of cases of each sort attended in private practice. By this I mean that, amongst the poor, the patients ought to be attended at their own houses, and not at a lying-in hospital, because there are certain special risks connected with these institutions which would render the comparison an unfair one. It is necessary also that all cases to which the practitioner has been called in in consultation should be carefully distinguished. These conditions being observed, we may class amongst the rich all those who can afford to pay well for the attendance upon them, and amongst the poor all those who are attended gratuitously in connection with some lying-in charity. Most men who have had much experience will have met with a considerable number of cases of each kind, the earliest patients belonging to the poor and the latest to the rich class; and a number of records of this kind will give trustworthy data for making such a comparison as that I have indicated. I have the means of doing so on a small scale from the records of my own practice. During the time I was a student, and during the first years of practice, I attended more than 250 poor patients at their own houses in connection with two lying-in charities. I am, therefore, in a position to compare the first two hundred of these cases with the last two hundred of my patients in private practice, dating back from the end of last June. In both sets of cases, I have excluded those to which I was called in consultation. I have not sufficiently correct notes of the duration of labour in my earliest cases to enable me to compare the first and second stages of labour in each set; my remarks now will be entirely confined to the third stage of labour and its attendant dangers and difficulties. And here I find that, in two particulars, there is a decided advantage in favour of the poorer class.

In the first place, with regard to *post partum* hæmorrhage arising from inertia of the uterus, I find that, in the 200 cases of the poorer class, there were seven cases of *post partum* hæmorrhage, whilst there were as many as twelve, or nearly double, in the 200 of the richer class. Again, none of the seven cases out of the first 200 were very severe, but were readily controlled by such means as cold, pressure, ergot, and the introduction of the hand. On the contrary, some of the twelve cases in the second class were very severe, and two of them required, in addition to the means above mentioned, the injection of perchloride of iron. I should mention that the first 200 cases were attended previously to the year 1846, and the last from February 1872 to July 1875. During the time when I was attending the first set, I was called in to one consultation case which was not severe; whilst I was attending the last 200, I was called in to two cases which were both very severe, one terminating fatally within six hours. These cases are not included in either list. These figures show, then, a percentage of $3\frac{1}{2}$ cases of *post partum* hæmorrhage in the poorer class of patients,* and 6 in the richer class: a fact which confirms the belief which I had previously entertained, that the latter are more prone to *post partum* hæmorrhage than the former. This hæmorrhage arises not so much from a want of contraction in the uterus immediately after delivery, as from a want of persistent contraction during the first hour afterwards. The uterus contracts and again relaxes, so as to allow a considerable accumulation of blood within its cavity.

* Dr. Johnston's report, above alluded to, shows a percentage of little more than 2.

I remember that when, in the early years of practice, I attended chiefly amongst the poor, I was constantly in the habit of leaving the patient in half an hour after the birth of the child, provided there had been no trouble with the placenta, and that the uterus remained well contracted; and I never once had cause to regret having done so. Moreover, I never took any particular trouble about the bandage. Now, however, I never dare to leave a patient within an hour after delivery, even after I have carefully bandaged and left the uterus apparently well contracted. Were I to do so, I should very probably be hastily summoned back to her, and find her at death's door on my arrival.

With respect to adhesion of the placenta, the difference between the two classes is still more striking. I have no single instance of this complication recorded in the first 200 cases, and not fewer than fourteen, or 7 per cent., in the last, besides four consultation cases, which are not reckoned. Dr. Johnston records only two instances in 1,236 in his report of the Rotunda Hospital for 1874. In a paper which I published in the BRITISH MEDICAL JOURNAL for June 19th, 1875, I endeavoured to account for this great disparity by the theory that the deposits of lymph which are always observed upon the placenta in cases of adhesion are due to a depraved condition of the blood, analogous to what is considered to exist in a rheumatic or rather gouty diathesis, and that this condition is caused by high feeding and insufficient exercise. Hence its prevalence in the richer class.

However this may be, my own statistics certainly show that two dangerous complications of labour are much more common amongst the rich than amongst the poor; and I firmly believe that, if we could obtain sufficiently extensive statistics of labours amongst the rich and amongst the poor (not attended in lying-in hospitals), we should find that the puerperal mortality of the former would exceed that of the latter, notwithstanding the want of good nursing and other disadvantageous circumstances accompanying confinements amongst the poor.

It is very difficult to obtain correct information as to the number of puerperal deaths which occur in private practice; but, if it could be procured, such information would be very valuable. There is perhaps no one point connected with midwifery practice about which so many loose statements are made. I have often heard men who have been in extensive practice for many years boast that they have never lost a lying-in patient. As Dr. Matthews Duncan remarked in his Address in Obstetric Medicine last year, "All such beliefs, reports, and statements are mere incumbrances of inquiry, and are to be thrown overboard, if not more ungraciously dealt with". By a careful comparison of the registers of births and deaths in Edinburgh and Glasgow during 1869-70, Dr. Duncan estimates that the mortality of lying-in women is as high as 1 in 120 within the month. And he further remarks that "even now, in 1874, we are only striving to reach a sound conclusion as to this mortality; and, after all our labour, official and private, have no statistics to be relied on for the comparison of the results of successive decades or of longer periods".

In estimating the amount of puerperal mortality in my own practice, I have excluded all cases which I attended as a student, and also all cases to which I was called in consultation by other practitioners. I exclude those which I attended when a student, because such cases were exposed to special risks at that time, when the causes of septicæmia were not so well understood as they are now. At that time, I was a dresser at a large hospital where erysipelas was very prevalent, and I was also much engaged in dissections and *post mortem* examinations. I exclude the consultation cases, because it is obvious that they must be abnormal cases of unusual severity. My whole number of cases is thus reduced to 1,069. In this number, there were five deaths, or one in 213½; viz., one from septicæmia, one from scarlatina, one from ruptured uterus, one from pneumonia (coming on before delivery), and one from convulsions during pregnancy.

This rather low rate of mortality is to be attributed more to my good fortune than to any other cause; for I have never had two consecutive cases of puerperal fever or septicæmia of any kind, nor am I aware of having ever conveyed any infectious disease except scarlatina, which I once communicated to a patient, but she happily recovered. In the death from scarlatina which I recorded, the patient herself caught the disease about a week before her confinement.

The cases which I attended as a student, and which are not included in the foregoing list, presented two deaths; viz., one from septicæmia, and one from phthisis. The list of deaths in cases to which I was called in as consultant is, as might be expected, a rather formidable one, and yet I have omitted in this list a good many deaths from puerperal fever, because my notes of the cases are very incomplete, as I saw many of them some days after labour, and not more than once, twice, or, at most, three times. Out of the consultation cases to which I was called during or just after labour, I have recorded 22 deaths; viz., 7 from septicæmia, 5 from convulsions, 2 from *post partum* hæmorrhage, 2 from ruptured uterus, 1 from hæmorrhage from placenta prævia, 1 from bronchitis, 1 from metritis, 1 from exhaustion after obstinate vomiting, 1 from jaundice and albuminuria, and 1 from Cæsarean section.

The relative frequency of the different cranial presentations has long been a *vexata questio* amongst obstetric authors, and we yet need more extensive statistics to settle the disputed points satisfactorily. Many believe with Baudelocque and the older authorities, that the second position of the head in which the occiput is turned towards the right acetabulum, is next in frequency to the first; whilst others, such as most British accoucheurs, hold Nægele's opinion that the second position is the most rare of all, nearly all the reputed cases of this position being in reality cases of the third position which had been thus converted during the course of the labour. Dr. Leishman, in his work on the *Mechanism of Parturition*, considers that Nægele's views have been too implicitly adopted by British accoucheurs; and the late Dr. Uvedale West, who investigated the subject very carefully, arrived at the same conclusion. I published a paper on this subject in the BRITISH MEDICAL JOURNAL in 1852, and I have just communicated another, based on more extended observations, to the *Obstetrical Journal*. The latter was founded upon statistical observations of 1,000 consecutive cases of vertex-presentation, dating back from the end of last June. My own views as to the relative frequency of the first and second positions are opposed to those of Nægele; and, contrary to the experience of most other accoucheurs, I find that the fourth position is more frequent than the third. The following figures will show the result of my observations.

First position	...	...	...	...	792
Second "	...	...	...	...	152
Third "	...	...	...	...	19
Fourth "	...	...	...	...	37

1,000

My own observations also show that it more frequently happens that the third and fourth positions remain unchanged throughout the labour, than that they become converted to the second and first.

The superiority of the practice of midwifery in the present time to that of the past is shown in nothing more signally than in the much greater use which is now made of the forceps in tedious and difficult labour. Up to the year 1844, according to Dr. Churchill's statistics, the forceps was used in British practice only about once in 342 cases. Now, however, as the report of the Rotunda Hospital for last year will show, it may be used as often as once in every nine or ten cases, and with the best effects, provided the operator be skilful and judicious. The above report shows a maternal mortality of about 1 in 27, and an infant mortality of about 1 in 7, which is a considerable improvement upon the mortality after forceps-cases in British practice up to 1844; for the maternal deaths were then as high as 1 in 20¼, and the infant deaths as 1 in 4¾. But there is one striking peculiarity in Dr. Johnston's report which would startle many an accoucheur of the past generation who believed and taught that one of the essential conditions for the application of the forceps was, that the os uteri should be fully, or nearly fully, dilated. Out of the 138 forceps operations already mentioned, Dr. Johnston gives the particulars of 42 in which the os was not fully dilated, and in the greater number of these the os was dilated to the size of a crown-piece only. Dr. Johnston has the best of arguments in favour of this practice; viz., its success. The deaths of but two mothers and four children are recorded. In by far the greater number of instances, the operation was resorted to on account of early rupture of the membranes, and escape of liquor amnii before the dilatation of the os, thereby allowing the foetal head to press injuriously on the soft parts of the mother. The os, however, must not be rigid or unyielding. I was called in very lately to two cases of this kind myself, and used the long forceps with the best results, although the os uteri was scarcely three inches in diameter. Beyond this, however, I have very little special experience of my own which will bear upon this question. I had never used the forceps before when the os was so little dilated, although I had many times employed it before the os was fully dilated, especially in those cases where the membranes have ruptured, and where the head, being from some cause arrested at the brim, does not descend upon the os uteri so as to complete the dilatation. I confess, however, that the bare possibility of the forceps coming into general use as a remedy for protracted first stage is enough to make one shudder. What has been said of opium may be well applied to this instrument also—

"Sacra vitæ anchora circumspectæ agentibus,
Cymba Charontis in manu imperiti."

The forceps is a powerful instrument for good or for evil, according to the skill of the operator, and its abuse in unskilful hands may lead to

much worse consequences than are likely to ensue from a protracted first stage: a thing which in itself is very seldom a source of danger. There is too much reason to fear that, by some practitioners who have a large amount of union practice, ergot of rye is sometimes given more with a view to save the attendant's time than to promote the welfare of the patient, and there is no doubt that a similar abuse of the forceps would produce far greater mischief.

With regard, however, to the more frequent use of the forceps in a protracted second stage, my own experience is greatly in its favour. I find that, in my first two hundred cases (after excluding those to which I was called in by others), the forceps was used but twice, and with a favourable result as regards both mother and child; whilst craniotomy was had recourse to three times, the child in one case being previously dead. All the mothers made a good recovery. In my last two hundred cases, the forceps was used seventeen times, with a favourable result to all the mothers and also all the children except one, whilst craniotomy was performed twice, the child in each case being dead before the operation. The result in both cases was favourable to the mothers.

My notes of all the forceps-cases I have had since I commenced practice conclusively prove the safety of the operation. The total number is 138; of these, 54 were patients I was originally engaged to attend, and 84 patients to whom I was called in in consultation. None of the 54 died, and the deaths of but two infants are recorded. Out of the 84 consultation cases, 4 mothers, or 1 in 21, died. Two of these deaths were caused by septicæmia, one from scarlatina, and one from bronchitis. The deaths of 17 infants are recorded. Most of these consultation cases were difficult and dangerous, and, therefore, a maternal mortality of 1 in 21 is not to be considered excessive. On adding the two groups of cases together, the maternal mortality will amount to 4 in 138, or scarcely 3 per cent.

In my own 54 cases, three-fifths of the patients were primiparæ, and the operation was performed on account of delay in the second stage of labour, with scarcely an exception; whilst, in the 84 consultations, the operation was performed in several on account of complications dangerous to both mother and child, such as convulsions, prolapse of the cord, etc., and, therefore, the extra mortality is not to be wondered at, especially if we take into consideration lost time and other unfavourable circumstances. Most of these also were primiparæ. We still require additional statistics to show the relative utility and safety of the long forceps and of version in slight narrowing of the brim of the pelvis. That turning will often succeed in these cases when the forceps will not, is proved by three cases reported in the *Lancet* of March 25th, 1871, by Dr. Phillips of Guy's Hospital. In each of these cases, the long forceps was first tried and failed, and version was then had recourse to with complete success, both as regards the mothers and the children. I myself lately succeeded in delivering two living children by version, in a case in which I induced premature labour at the end of eight months. I had ascertained by measurement that the conjugate diameter of the pelvis was not more than two inches and three-quarters, and in consequence it had been previously deemed necessary to induce labour at seven months on four occasions; but the children were so feeble at this early period, that only one of them had survived. The twins above mentioned are still alive and well.

With respect to the comparative safety of the two operations, I do not think we have yet sufficient data wherewith to form a conclusion, and further statistics are still needed for this purpose.

The rules for the treatment of puerperal convulsions, after having been for a long time extremely definite and settled, have of late years become indefinite and unsettled, chiefly, I think, on account of the altered views which now generally prevail on the subject of bleeding. This, then, is another point which requires to be cleared up by satisfactory statistical information. If there were any disease in which the good effect of bleeding was recognised almost beyond dispute, it was puerperal eclampsia. Now, however, this mode of treatment is very generally discountenanced, for no better reason that I can find than that in the treatment of all diseases bleeding is gone quite out of fashion. So much is this the case, that it requires some moral courage to recommend bleeding for any complaint whatever; and my esteemed colleague Mr. Michell Clarke, in his recent presidential address to our Bath and Bristol Branch of this Association, regards me as a remarkable phenomenon because I so strongly advocate it in puerperal convulsions. I certainly cannot find any statistical basis for the condemnation of bleeding in this disease. It is true we have of late years acquired two most valuable agents in the treatment of eclampsia, viz., chloroform and chloral; and these have, perhaps, to some extent, caused us to lose sight of the value of bleeding. But yet, it is not the propriety of bleeding only that is now being questioned, but that of another remedy, the efficacy of which has hitherto been universally acknowledged in this country—I mean delivery. For instance, I find in Schroeder's *Manual*

of Midwifery the following remarks respecting the treatment of this disease: "The condition of the woman contraindicates obstetrical manipulations, and every irritation of the uterus, such as introduction of the hand for the purpose of turning, application of the forceps, or even mere friction of the fundus, is apt to cause a fresh attack by reflex action;" but the author adds this qualification: "In the interest of the child, however, active procedure may become necessary."

My own experience of puerperal convulsions being rather large in proportion to my experience of other complications of labour, it may be worth while to examine a little in detail the notes which I have made of these cases. Since beginning practice, I have attended twenty-six cases of puerperal convulsions. With the exception of three, all these were cases to which I was called in by others. Eighteen of the twenty-six cases were primiparæ, or more than two-thirds. This agrees with the experience of most other practitioners. For instance, Drs. Johnston and Sinclair give the particulars of sixty-three cases of puerperal convulsions, out of which forty-nine, or more than three-fourths, were primiparæ.

My own cases of convulsions may be divided into three groups, viz.: 1. Convulsions coming on during pregnancy, before the full term, and accompanied with no symptoms of labour; 2. Convulsions coming on during labour, and in some cases continuing afterwards; 3. Convulsions not commencing until after labour. There are seven cases of the first group; fifteen of the second; and four of the third. Convulsions accompanying labour are, therefore, the most common.

Convulsions of the first kind are, according to my experience, the most formidable and fatal of all. Out of the seven cases, five mothers died and all the infants.

In the second class, or those which accompany labour, there were two maternal deaths in the fifteen cases, and eight deaths out of seventeen children (there being two cases of twins). In the third class of *post partum* convulsions, out of the four cases recorded, no mother and no infant died. Convulsions belonging to this class are, therefore, far less dangerous than the others. It will thus be seen that the total mortality amounted to seven maternal deaths in twenty-six cases, or more than one-fourth; and an infant mortality of fifteen in twenty-eight, or rather more than half. These figures are quite sufficient to show that eclampsia is one of the most dangerous complications of labour.

In the treatment of puerperal convulsions, the three most important remedies are bleeding, anaesthetics, and delivery. The secondary remedies, which were used in nearly all my cases, are purges of calomel and jalap, enemata of turpentine, castor-oil, and assafoetida, cold applications to the head, and counterirritation by blisters, sinapisms, etc. I need not stop to discuss the efficiency of these last, but will merely state briefly my experience of the first three.

Bleeding (the quantity of blood abstracted varying from ten ounces to thirty ounces) was resorted to in twenty out of the twenty-six cases. In fourteen, it was decidedly beneficial, and appeared to be the most efficacious remedy employed. After the bleeding, the fits either ceased or were suspended for a time, or were mitigated both in frequency and in severity. In some few cases, convulsions, which had continued after the use of anaesthetics and delivery, ceased immediately after the abstraction of blood. In case 14 (a case of twins) the fits which continued after delivery and after opiates, at once ceased after bleeding, at the same time the albumen in the urine rapidly disappeared. The relief afforded by bleeding is most marked and striking in convulsions which commence after delivery. In nearly every case in which it was employed, venesection rendered the breathing less stertorous, and the pulse more tranquil and steady. The benefit derived from bleeding arises, I believe, from the diminished stress upon the vessels of the brain, and perhaps of the kidney. At all events, it seems to remove that peculiar condition of the kidney which causes albuminuria.

By anaesthetics, I mean either the inhalation of chloroform or the administration of chloral, either by the mouth or the rectum. These two remedies, which are similar in their action, have now quite superseded opium. They are both very valuable, especially chloral. In four cases, the symptoms were much relieved, or altogether cut short, by chloroform, and in three by chloral. In one case, No. 26, bleeding gave relief for a time, but, the fits returning, the patient was kept under the influence of chloroform for seven hours, and had no more convulsions. In another case, No. 22, chloral in large doses was equally efficacious.

Delivery gave decided relief in seven cases. In five, the forceps was used; and in one, craniotomy; and in one, turning. These two last were desperate cases, but, in all seven, delivery either abolished the convulsions, when other means had failed, or else appeared to complete the cure which had been begun by bleeding and anaesthetics. I cannot, therefore, look upon this remedy as valuable only for the child's sake.

The seven fatal cases of eclampsia, which I have recorded, were thus treated :

1. Date 1854. Convulsions in seventh month. No sign of labour. Bleeding to twenty ounces, six leeches, cold to head, purgatives, sinapisms, etc. Died undelivered.

2. 1863. Convulsions in seventh month. Bleeding to fourteen ounces, purgatives, cold to head, chloroform. Fits ceased, but she died soon after giving birth to a dead child.

3. 1868. Convulsions at commencement of labour. Seven leeches, chloroform, cold to head, purgatives, etc. Delivery terminated speedily after dilatation with Barnes's bags. No more fits; but she died on the third day, from total suppression of urine.

4. 1868. Convulsions at full term. Bleeding to fifteen ounces, purgatives, cold to head, etc. Gave birth naturally to a dead child; no more fits; but she died comatose the following night.

5. 1869. Convulsions in eighth month; no sign of labour. Bleeding to fourteen ounces, purgatives, cold to head and blisters. Twenty-six fits before bleeding, and none after. However, she gradually sank, and died undelivered on the next day.

6. 1872. Convulsions in eighth month; no sign of labour. Bleeding to eighteen ounces, purgatives, cold to head, delivery by turning, chloroform. Fits still continued, abating gradually; she died suddenly at the end of three weeks.

7. 1874. Convulsions in ninth month; no sign of labour. Cold to head, purgatives, chloral in two doses of thirty grains. No fit for an hour after last dose; os too rigid to turn; delivery by craniotomy; convulsions returned two hours afterwards, and she died.

In this last, which was the worst case of all, bleeding was not resorted to, because the pulse was feeble from the commencement. From my own experience, I am inclined to consider bleeding and anaesthesia as the two important remedies, I can scarcely say which is the most important; whilst delivery is very little inferior to them, its inferiority being chiefly due to the circumstance that it is not so universally applicable. I have, on previous occasions, given cases which undoubtedly show that bleeding is very speedily followed by a great diminution in the amount of albumen contained in the urine. To this cause I believe that it owes its great power as a remedy.

The last subject I shall indicate to which statistical investigation may be most advantageously applied, is the etiology of what is commonly called puerperal fever. This is very extensive, and has been so fully discussed lately, that I need not enter upon it at any length. It has been abundantly proved that the same train of symptoms may be produced in a pregnant woman by the absorption of a variety of animal poisons; these symptoms, in general, are rigors on the second or third day after delivery, followed by headache, fever, great rapidity of pulse, and elevation of temperature, suppression of the milk and lochia, and generally local tenderness and pain in the uterine region, spreading over the whole of the abdomen, and accompanied with much tympanitic distension. In order to demonstrate what are the peculiar poisons which most frequently give rise to this train of symptoms, so that we may more effectually guard against them, we need a great number of independent observations, especially from men engaged in country practice who are able to isolate their cases. The poison which contaminates the blood of a woman lately delivered may be generated within herself, or, as no doubt more frequently happens, be introduced from without in various ways. Scarlatina, as Dr. Braxton Hicks has shown, is the poison which is most frequently introduced from without, and next to this perhaps erysipelas. When scarlatinal poison has been absorbed, I have observed, in several instances, the rash and sore throat characteristic of this disease. But more frequently, I think, these are absent, and only the ordinary symptoms of puerperal fever are recognisable. The same may be said of erysipelas. As an example of the first, I may mention a case which came to my own knowledge lately. A medical man, whose children were ill from scarlatina, was sent for on a Saturday to attend a lady in her confinement. On the Tuesday following, she developed symptoms of puerperal fever, such as I have mentioned above, and on the next Friday the husband, who fetched the medical man, was attacked with scarlet fever in its usual form. Now, there can be no doubt, I think, that though these two diseases were different in symptoms, they were derived from the same source. The poison of erysipelas may thus be modified in a similar manner. My friend Mr. Waugh of Midsomer Norton, Somersetshire, gave me the particulars of some cases which lately came under his notice, and which I hope he will shortly publish. A midwife, who had been nursing a case of erysipelas, attended a poor woman in her confinement. This woman was attacked with puerperal fever, which ended fatally in a few days. A woman, who had been washing some linen soiled with the discharges of this patient, was attacked with severe phlegmonous erysipelas of the arm and shoulder. Thus it appears that a poison of

a specific kind may give rise, in a woman lately confined, to a peculiar train of symptoms which go by the name of puerperal fever, and yet this woman may communicate the original disease, whether it be erysipelas or scarlatina, to another person. We need a number of very careful observations to show what differences are occasioned in the symptoms produced by scarlatinal poisoning in women who have had scarlatina previously and in those who have not. There is every reason to believe that women who have had scarlatina before, may, if recently confined, be reinfected and have the symptoms of puerperal fever, in the same way that other persons, who have had scarlet fever before, sometimes get a troublesome sore throat, after nursing scarlatina patients. The name puerperal fever is generally applied to severe cases; but there is no doubt that when this disease is very prevalent slight cases of septicæmia often occur, just as cases of diarrhoea are frequent when cholera is prevailing; but these are not considered to be cases of puerperal fever. I believe that many cases of puerperal mania are, in reality, the result of blood-poisoning; for I have frequently noticed, before the attack of mania, the occurrence of rigors, some fever, hysterical excitement, sallowness of skin, and especially loss of sleep. Phlegmasia dolens is generally ushered in by symptoms of this kind, and I have often observed them before the outset of mammary inflammation and abscess. I regret very much that, although I have seen at different times a great many cases of puerperal fever, my notes of these cases are so incomplete that I can bring forward no statistics of any value. Nearly all the cases I have seen have been very severe ones, to which I was called in a day or two before the fatal termination; and I have no complete notes of them in the earlier stages. I trust, however, that before long such deficiencies will be supplied by others, who have had opportunities of watching these cases from the commencement, and who have been so unfortunate as to have two or three consecutively. Such information is very difficult to obtain; two or three cases of this kind make such a stir in a quiet country village, and are so injurious to the medical man in attendance on them, that he is glad to have the matter hushed up in every possible way; and to request him to publish the cases is very like asking a witness to criminate himself. All the more honour, then, is due to those practitioners, who for the benefit of medical science and for the public good, nobly come forward, undeterred by any fear of risking their reputation, and publish a candid statement of their experience.

HYDATID DISEASE:

AS ILLUSTRATED BY SPECIMENS CONTAINED IN THE PATHOLOGICAL MUSEUMS OF THE METROPOLIS AND IN OTHERS ELSEWHERE.

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III.

QUITE recently, I had an opportunity of inspecting the collection at St. Thomas's Hospital, which I found to be particularly rich in entozoa of various kinds, especially tapeworms and hydatids. I encountered 76 preparations of internal parasites; and, of these, 42 were of the hydatid kind, representing at least 33 different cases. I say "at least," because it is often impossible to decide in instances where no history of the specimens can be obtained. Thus, there are three similar preparations of hydatids passed by the urethra; and, from their appearance, I judge them to have come from one and the same patient; yet there is no statement in the catalogue to that effect.

Of the 33 cases of hydatids represented in the museum, I reckoned 18 as referable to the liver, 2 to the brain, 2 to the bones, 2 to the urinary organs, and 1 to the lung, spleen, uterus, and soft parts of the thigh respectively. There are also 3 that may be classed as peritoneal. There is another choice example in which the disease cannot be referred to any particular organ. I allude to Dr. Peacock's case already published (*Pathological Transactions*, vol. xv), where the lungs, liver, heart, spleen, and some other organs were all occupied by hydatid formations. As an instance of extensive visceral infection by echinococci in the human subject, I believe this case to be unique. The brain-hydatids are particularly fine. In the specimen presented by Mr. Boot of Lincoln, the hydatid, two inches in diameter, is lodged in the anterior horn of the left ventricle. One of the peritoneal cases is remarkable for the amount of forward displacement of the pelvic viscera, caused by four or more hydatids, each of them nearly as large as a cricket-ball. Amongst these abdominal cases, I have also included a recent preparation, to which Mr. Stewart called my attention. The hydatid in question, of the size of a large lemon, existed near the fundus of the