Theses

Puerperal Eclampsia

And

Metria or Puerperal Pyrexia

By

Ellis J. Davies M.B L.M.

Liverpool April 1884
Puerperal Convulsions

In submitting for consideration the remarks which I have to make upon Puerperal Convulsions, perhaps my first duty is to state distinctly what I mean by the term employed. I will say then that I do not include in the term any seizure of an Epileptic or Hysterical nor the Convulsion occurring in deaths from Haemorrhage when the patient is in articulo mortis. It will thus be seen that I confine the name Puerperal Convulsion to any Convulsion occurring during Pregnancy, Labour or Childbed attended with loss of consciousness.

But little seems to have been known of the causes of Puerperal Convulsions until the year 1842, when Dr. John C. Webster had the honour of first drawing attention to the relations existing between Albinimuria and Puerperal Convulsions in "Guy's Hospital Reports." In 1851 Frei-Richs pointed out the resemblance between Puerperal and Urinary convulsions and gave his opinion that true Convulsions occur only in women suffering from Albinimuria. In 1857 Karl Braun of Vienna published his classical work and since that time the exhaustive chapters on Puerperal Convulsions contained in that work have been the source whence every author after himself has drawn a large amount of information.

I am not aware that a second edition of this work has been published so that I do not know how far his present views accord with those expressed in 1857. We may presume however that they have not altered materially for his colleague Prof. Smith during the course of a clinical
Lecture in the early part of 1882 on a case of Puerperal Eclampsia mentioned no other views than those expressed by Braun in 1857, and said that the views of gynaecologists as to the origin of convulsions were still chaotic; while W. L. did in a still more recent lecture on a case of Albuminuria with Polyhydramnion of the labia (but without convulsions) make no mention of any new opinions. We may say then that the teaching of the last quarter of a century has been that in the vast majority of cases, so-called puerperal convulsions are accompanied by Albumin in the urine of the patient, and that this Albumin is in some obscure causal relation to the convulsions. Braun on p. 155 of his work calls Eclampsia only a partial symptom of another disease

Müller's Neues Münch. Med. Wochenbl. 1865. p. 1465 he says: "Several years ago I came to the conclusion that acute Bright's disease and Albuminuria of the blood are the cause and not the effect of the Eclampsia." Since followed by Braun and having discovered Carbonate of Ammonia in the blood of a patient suffering from Puerperal Convulsions he concluded that the Eclampsia was caused by the Carbonate of Ammonia and that this originated in decomposition within the blood of retained urine. Braun himself seems to have accepted his theory in part and acting upon it, treated 16 consecu-
tive cases with acids and chloroform and in every case with success. Murch of Sunderland in an able paper on the subject goes so far as to make Albuminuria the pathognomonic symptom of the disease and to exclude from the category all those cases in which albumin cannot be discovered in the urine. Lusk says that it is the
ual insufficiency and not the Albuminuria that is the cause of
the convulsions and owes its origin from the convulsions a necessary
renal insufficiency as the cause of them. "Deichman under the term
includes all cases of convulsions occurring during pregnancy, labour and
after delivery. He says: "Epilepsy is to be distinguished partly by the symp-
toms, but more particularly and conclusively by the absence of Albumin in
the Urine. With rare exceptions then, paroxysmal delirium may be looked
upon as being essentially connected with uramic poisoning which again is
associated with or dependent upon an albuminuric condition of the Urine".
Golabik saw 34 cases and examined the Urine in 21 finding Albumin
in all but one. In 3 out of 21 however the Albumin was found only
after the convulsions commenced—.

According to the generally accepted view then the sequence of events
in Paraeuric Convulsions is as follows: (1) Pregnancy (2) Pressure on the
renal veins by the gravid uterus (3) Parenchymatous nephritis (Acute
Brights Disease of Brain) (4) Urmia originating in some obscure
manner (5) Convulsions; but whether by Carbonate of Urmia poison-
ing or by Hydramia or Ateima or by retention of the general urinary
excretory products has never been settled to the satisfaction of all parties.

From the time of Bramer's first publication however there have always
been opponents to the Urmia Theory of Convulsions—men who have
been quick to detect its weak points and to seize any facts that
might be made to tell against it. Such a man was Jeffrey of Prague
the Director of a Large Lying-in Hospital in that city. Such tells us that
by opposed the Urmia theory on the ground of the following facts or
(1) System of Lying-in at Prague 1746. (2) Published 1722-1780 At 1.69g. (3) Think of Munt (5) 500
or supposed facts:—(1) That convulsions may occur without albuminuria.
(2) That albuminuria is in many cases the effect and not the cause.
(3) That in many fatal cases the kidney lesions were absent or wholly insignificant.
(4) That convulsions are rare in chronic Bright's Disease that has existed prior to pregnancy.
(5) That in true Measles such as is necessarily produced by the suppression of Wine when the Waters are increased convulsions do not occur.

Dr. Lush very pertinently replies to the first objection, that it is the renal insufficiency or incompetency and not the albuminuria that is the cause of the convulsions. It is now recognised almost on all sides that convulsions may and do occur without albuminuria.

Dr. Lush goes even further and acknowledges perinatal convulsions without Measles or renal incompetency—"without Measles, perinatal irritation can provoke eclampsia." This view of Wilson and Shepherd on the other hand get out of this objection (the first of facts) by declining to acknowledge convulsions as perinatal unless they are accompanied by albuminuria. The latter calls these cases epileptic and tells us that "the absence of albuminurea will enable us without difficulty to discriminate," i.e. between perinatal convulsions and epilepsy.

It is noteworthy here that although he has given us an easy method of distinguishing between two diseases presenting somewhat similar symptoms he yet finds it necessary to admit that "no one can take a clear and comprehensive view of the pathology of the subject who does not freely admit that there are cases in which no albuminuria exists." He first tells us that we can distinguish epilepsy from perinatal convulsions by the absence of albuminuria in the former and next that perinatal (2) Letter, T.C., 1890, K. 7/3. (2) Second letter, K. 7/46 of segment. (4)
convulsions can occur not only without albuminuria but without
urine, for albuminuria is of course nothing at all if it is not a symptom
of an entirely distinct morbid condition—urine.

"Playfair who does not seem at all wedded to the uraemia theory says:
"It may be taken as proved that albuminuria is by no means necessarily
accompanied by eclampsia. Cases were observed in which the albumen only
appeared after the convulsions; and in these it was evident that the retention
of urinary elements could not have been the cause of the attack." He goes
on to quote from Brayton Hicks that the two are almost invariably united
and that the nearly simultaneous appearance of the two—albuminuria
and convulsions—must be explained in one of three ways:—(1) That the
convulsion is the cause of the nephritis. (2) That the two are produced
by the same cause, viz., some detrimental ingredient in the blood—(3) That
the highly congested venous condition induced by virtue of the blood is
able to produce the kidney complication. After considering the remainder
of Playfair's objections to the uraemic theory, I shall return for a moment to
Playfair and Hicks. Playfair's second objection is nearly identical with Hicks'
explanation: no. 1 is: viz. that the convulsion is in many cases the cause and
not the effect. Believing that Peripherical Convulsions and Epilepsy spring
eventually from essentially the same causes I have endeavoured to learn whether Epilepsy
is ever the cause of Albuminuria. Tarnoby (Medical Times & Gazette Oct 14/83)
examined the urine of a great many epileptics both before and after the fits
and concluded that the attacks did not cause Albuminuria. Professor Gori of
the University of Califani in a great number of epileptics not only met with
Albuminuria but Hyaline globules and sugar. It was found also in animals
vandered epileptic by Paralysis. He says however that the excretion of albumen was only transitory and limited to the first moments after the attack.

Klendysh examined the urine in 30 male and 21 female Epileptics and always within four hours of an attack and found albumen. In many cases the reaction was very slight and only rarely was there any deposit. He soon extended his researches to the attendant and singularly enough found traces of albumen in their urine and finally concluded that there is nothing remarkable in post-epileptic urine. It appears to me that Sivis's statements require confirmation—they should either be confirmed or contradicted. Authoritatively at present the statements of the three enquirers as to whether convulsions can produce albuminuria are contradictory but I think the evidence upon the whole points to the conclusion that they cannot.

The 3rd & 4th objections of Seppert viz that in many fatal cases the kidney lesions are slight or insignificant and that convulsions are rare in chronic febrile disease that has existed prior to pregnancy are substantial ones and are valid arguments against the uraemic theory. The 5th is also a valid one viz that in true albuminuria such as is necessarily produced by the suppression of urine when the kidney's are invaded, convulsions do not occur. Lord and Runcier confirm the truth of this statement in an emphatic manner and say that in no instance had convulsions taken place. I myself am able to certify this statement or at least more correctly. I have seen fatal cases of uraemic cancer in which I have had reason to suspect that occlusion of the vasa was the immediate cause of death and although I have sought for evidence of uraemic convulsions I have entirely failed to discover any. If then such complete retention of uraemic constituents as must

(1) Cunningham and Nissen 1861. P. 780
(2) Lusk (P. 539)
of sickness exist in these cases fails to produce convulsions it seems tolerably certain that some other factor is necessary to produce them. Hicks' second alternative explanation of the nearly simultaneous appearance of convulsions and albuminuria may possibly be correct, but if it is it will negate the statement of Playfair to the effect that the "retention of urinary elements could not have been the cause of the attacks," for as will be shown there may be retention of urinary elements and no albuminuria. As I have already stated, Lecky says it is not the albuminuria but the renal insufficiency that is the cause of the attacks. We have lately heard from Sir Andrew Clark of the harmful effects of renal incompetence without albuminuria. Israel of Berlin stated recently that a mania may arise without albuminuria and without nephritis in consequence of the inability of the kidney to do the increased work thrown upon it by pregnancy. He experimented upon rabbits and learned that by gradually accustomed them to the injection of urinary constituents they could bear the introduction of a large quantity without injurious consequences, but that if he injected similar large quantities for which they had not undergone previous preparation death rapidly ensued. He does not say however that death was preceded by convulsions. These experiments prove only that by the introduction of gradually increased quantities of urinary constituents large quantities may be tolerated, but if large quantities be suddenly thrown into the system of an unprepared animal they cause death, but not that they produce convulsions. Now after all this has been said I am prepared to admit that in the great majority of cases of paroxysmal convulsions urinary constituents in the blood play the part of contributors to the convulsions, but I cannot accept the theory that they are

The sole cause and in this I follow Playfair as against most writers.

Now it is necessary to enquire how the albumen gets into the urine and what is the condition of the kidney in the albuminuria of pregnancy. Is there a real parenchymatous nephritis as those who accept the history of Braun and Bartels affirm there is? Most probably not. Dr. Auguste Mac Donald published a remarkable paper on this subject (Brit. med. Journ. Nov. 1879 and Jan. 1870). To his surprise no parenchymatous nephritis was present as he had suspected there would be. He says:—"The kidneys were quite different from anything I have ever seen in acute renal disease." "The sole change was apparently degeneration of the tubular epithelium." In Germany the condition of the kidney under these circumstances has also been noticed to be different from the form met with in acute Bright's disease. The condition is so marked that it has been designated by Leyden "the kidney of pregnancy." Dr. Halbert Smoote of Wocohden denies that the condition is one of nephritis. Dr. Hiller, a German staff surgeon reports the autopsies of a fatal case and says there was no sign of any change in the interstitial structure of the kidney. Dr. Haierlein of Berlin gives the differential diagnosis between acute Morbus Brighti and the kidney of pregnancy, although he acknowledges that it is not always easy to establish this with certainty. In the latter (kidney of pregnancy) according to him haematuria is not present, the pulse is not so hard and incompressible as in acute nephritis whilst the presence of fat corpuscles in the urine is a characteristic of the affection. The pathological change is a degeneration of the epithelium of the glomeruli. Thus we have a series of original investigations who now deny that the condition of the kidney such as in the albuminuria of pregnancy and labour is ever the same as in acute Bright's disease.

(Hill for Gynaec. and Genet. July 1874)
and who assert that it is one of simple degeneration of renal epithelium. These investigators as I have shown are McDonald, Leyden, Hiller, Halbertsma and Fleischhauer. The question here arises: How is degeneration of renal epithelium produced? Braun and the earlier writers attributed the kidney change to venous congestion from pressure of the gravid uterus upon the kidney. This cannot always be the cause, for not infrequently Fleischhauer just quoted the albumen often appears about the middle of pregnancy when of course the position of the uterus is not sufficiently high to press upon the kidney. Dr. McDonald in 1880 says that "all attempts to explain the very frequent occurrence of renal disease on purely mechanical grounds must be allowed to have proved in a great measure failures." Halbertsma on the other hand claims to have discovered a ground of explanation and that a mechanical one. In 1871 he first drew attention to pressure by the gravid uterus on the ureters as the cause of the kidney degeneration. Last year he returned to the subject with convictions strengthened by eleven years of observation and thought. He points out the difficulty of verifying this pressure after death and shows that only the effects of this compression should be looked for—cataurh and swelling of the ureters and these have, as he says, been actually observed. This view has been adopted and defended by Sechen, who has made autopsies in a great many cases. He has found dilatation of one or both ureters in 25 per cent. of the cases of puerperal eclampsia and only 3 per cent. in cases of death from other diseases. Hiller has also adopted this view and has seen proof of the accuracy of it post mortem. Contihi has been improved into the service of the theory and his statement that when the lower segment of the uterus is firmly distended it must press upon the ureters and thereby occasion...
delatation of them has been made use of in support of it. This view also receives the support of Dr. Matthews Duncan who in his recently published "Lectures on the Diseases of Women" p. 81 makes use of the following language:—

"Recently many observations show that obstruction of the Waters has a good deal more to do with the nervous phenomena of pregnancy and even with manie eclampsia than we have hitherto supposed." I may mention that in my student days in Edinburgh Dr. Duncan used to ignore this theory as the causation of eclampsia most strongly. The mechanical theory certainly derives some support from the facts that a great preponderance of the cases of eclampsia occur in primipara in whom the abdominal walls are comparatively thin and unyielding and that again some proportion of these consists of unmarried girls who to prevent their pregnancy being observed voluntarily undergo the torture of tight lacing and thus press the Uterus back upon the Waters with a determination and persistency not infrequently fatal.

Having now considered the important contributory part which mania—whether associated with albuminuria or not—has in the production of Purpural Eclampsia we have now to turn to those cases in which no mania can be discovered to exist. That there are such cases all are agreed. "Such says that in addition to eclampsia owing its origin to central causes—the central-causes I take it being mania—there is another class in which the convulsion proceeds from peripheral cause without mania, peripheral irritation can produce eclampsia." There must be some common cause producing the convulsions in both classes of cases—those in which there is mania and those in which there
is more. I fail to see why it would not be as reasonable to put down all infantile convulsions to uraemia as it is to attribute all psychical convulsions to that cause. Most writers have omitted to note the common cause which is operative both in the convulsions of childhood and in those of the psychical state. Playfair says: 'The key to the liability of the psychical woman to convulsive attacks is no doubt to be found in the peculiarly unstable condition of the nervous system in pregnancy--a fact which was clearly pointed out by the late Dr. F. H. Smith and by many other writers.' Dr. Barnes has also drawn attention to this peculiar state of unstable equilibrium as it might be called. Kleinschelen calls it a state of predisposition which may be taken to mean the same thing. In childhood then, in pregnancy, and in epileptics permanently, this condition of unstable equilibrium may be said to exist--and this is the one factor that is indispensable, and there is no other that is so 

As to the immediate cause of the attack most writers are agreed. Trauner and Wiesmann are said to have proved that the immediate cause of epilepsy is anamic--cerebral anemia. Dr. E. C. V. Clark says that the condition of the system in epilepsy and psychical convulsions is essentially the same. (American Journal of Obstetrics July 1880.) Hughes and Jackson believes that the attacks are due to a nervous deficiency arising from spasm of the cerebral arteries. Mottuage has proved that a collection of ganglionic cells in the substance of the Pars Vaortic furnishes the motor centre from which the convulsive impulse takes its departure (Suck 1 p 531). McDonald in his fatal case found the brain intensely congested, but the motor tract equally anemic; he attributes the phenomena to our stimulation of the Vaso-motor nerves which it then produces anemia of the brain. Dider says: 'It seems established in our day that the cause
which engenders eclamptic convulsions or epilepsy is a sudden arrest of cerebral circulation. The vaso-motor spasm which is developed determines a sort of instantaneous ischemia.

Perhaps I cannot more usefully close this lecture than by presenting its pathological results, noting the treatment which it indicates and concluding with a short history of two cases of paroxysmal eclampsia in both of which the patient recovered.

To summarise then I think we may conclude from the evidence before us:— (1) That paroxysmal eclampsia is a motor-neurosis associated with loss of consciousness. (2) That it stands in intimate relationship to the convulsions of childhood and epilepsy. (3) That only one factor in its production is constant—viz., a peculiar condition of the nervous system which may be characterised as one of unstable equilibrium, and that this factor is common also to the convulsions of epilepsy and childhood. (4) That retention of urinary constituents which present vastly increases the tendency to convulsions in pregnancy, but that outside this condition of pregnancy such retention is but rarely the cause of convulsions. (5) That over-excitement, shock, emotion, violent pain, urinary or other morbid condition of the blood, are capable of bringing up sudden vaso-motor spasm of the cerebral bloodvessels. (6) That this spasm of the bloodvessels causing sudden anemia of the brain is the cause of the convulsions, and I would add, of the consequent coma.

If this view of the etiology of paroxysmal eclampsia is correct, and if I am to be borne out by facts and is borne for by non-intellectual opinion, one of weight, it indicates the treatment to be pursued in this terrific disease and furnishes an explanation of the success which has followed the employ.
In 1804, Abel-Jean Audouin, a French physician, performed an injection of morphine into a patient suffering from cancer. This marked the first recorded use of morphine for pain relief. However, the effectiveness of morphine was not immediately recognized, and it was not until the 1850s that William Halstead, an American surgeon, performed the first successful operation under morphine anesthesia.

In the 1860s, Daniel Drake, an American physician, popularized the use of morphine for pain relief by advocating its use in surgical procedures. He believed that the use of morphine would allow surgeons to perform operations painlessly, thereby reducing patient mortality.

The use of morphine for pain relief continued to grow in popularity throughout the 19th century, and by the early 20th century, it had become a widely accepted practice. However, the use of morphine was not without its controversies, as some viewed it as a dangerous drug that could lead to addiction.

Despite these concerns, the use of morphine as an analgesic has continued to the present day, and it remains one of the most effective and widely used pain relievers available.
In conclusion I will narrate two instances of Perforral Belamnia observed by myself. One refers to cases occurred in my own practice and the other I saw with my friend and colleague Dr. Burton of this City to whom I am much indebted for his courtesy in permitting me to make use of. My own case occurred some years ago in a few months after I had taken my degree of M.B. I was much elated for to see a strong healthy looking Primipara in the country - the wife of a 22-year man in comfortable circumstances. I knew nothing of the previous history of my Patient but on my arrival I found her in violent convulsions. On making a vaginal examination I found the os fairly dilated, membranes protruding and the head presenting. I at once ruptured the membranes, applied forceps and completed delivery as soon as
impossible. After the child was born and the placenta removed she still continued to be violently convulsed. Cold cloths were ordered to be applied abusively to the head and rectal injections of chloral hydrate were repeated and promethine half a dram every two hours — Iused hardly say that at this time nothing could be administered by the mouth. In the course of 12 hours the convulsions had entirely abated and the patient made a speedy and complete recovery. Some urine which I drew from the bladder directly after delivery I found to be loaded with albumen — about two thirds. This also gradually lessened and ultimately entirely disappeared.

Dr. Burton's patient was a very diminutive woman aged 28 years — eight months pregnant of her second child. She had been subjected to a good deal of domestic trouble. Previously to this period — the end of the eighth month of her pregnancy — she had suffered much from violent and persistent headaches, but her sight was not affected. Dr. Burton saw her on April 2nd 1890 when labour threatened. At 3 A.M. April 24th he was summoned again and found that she had had a convulsion — labour did not seem to be progressing and at this time Dr. Burton interfered no further than to prescribe the usual remedies for such convulsive attacks. During the time Dr. Burton remained with her no indications of the recurrence of pains were observable and she was left for a short time. On his return at eight o'clock A.M. I accompanied Dr. Burton and we were surprised to find that the child had been expelled and was dead — the convulsions still continued, returning however at considerable intervals and consciousness was still in total abeyance. At about 11 A.M. she was bled to 8 oz. but with little or no effect. The convulsive attack continuing until the evening of the next day, thus lasting 36 hours, during
which period the head at least ten seizures. Coma still continued in which condition she remained until April 29th. At this date the case seemed utterly hopeless as she was completely exhausted by the violence and long continuance of the convulsions. For three days she had had nothing by stimulants, excepting by the rectum: the coma being so deep that the stiffness of the palatal and pharyngeal muscles was absolutely lost. No efforts to swallow following the placing of fluids in the mouth. Whatever stimulants are at best but a poor substitute for gastric digestion. At this time death seemed so certain that all we could hope for was some inconsiderable prolongation of life. With this object in view hopefulest the case seemed we determined to inject some nourishment into the stomach. We secured a No. 12 male catheter and by means of a piece of India-rubber tubing attached it to a Hegginson's syringe. We then made a Breakfast cupful of milk with an ounce of brandy and about a dram of chicory extract of beef and pushing the catheter into her stomach we injected the whole of the mixture with some additional hope that the non-cremated might at least not die of starvation. It will be no exaggeration to say that this proceeding produced a most-wonderful effect. Within one hour consciousness returned and from that time she was able to swallow her progress towards recovery being henceforth uninterrupted. The profound coma which follows these convulsive attacks on the history of this case seems to prove is certainly due to anaemia and not as was formerly supposed to compression of the brain from effusion of serum or oedema. This being so a ready explanation is offered of this remarkable change for the better which took place in this patient so immediately upon the injection of food into the
stomach. Dr. Burton tells me that he has since attended this patient in two confinements. The last time she suffered much from head ache but although he several times examined her urine he failed to discover any trace of albumen...

Both the subsequent labours were perfectly normal.
Puerperal Fever

Melaena

or

Puerperal Pyrexia

I do not think that I can deal more usefully with this subject than by narrating in detail a number of cases which have occurred in my own not inconsiderable practice, preceding the narrations however with some notion of the nature of the disease and introducing, as occasion may offer, such remarks theoretical or otherwise as may seem necessary or useful.

The oldest writer on Puerperal Fever—Willis—whose book was published 200 years ago propounded a theory of this disease not altogether dissimilar to that now accepted by the best pathologists. He regarded it as having some connection with uterine wounds, but
He seems to have known neither the nature nor the anatomy of such wounds to have considered any other wounds or injuries of the female sex to.

... Willis—who introduced the name "Fusis Puerperale"—held that it was the result of sulphurous particles and fermentation in the blood, probably in consequence of the abrasion wound and he seems to have attributed to the supposed sulphurous particles somewhat of the place and the importance now given to the bacteria of Mayhoffer, Bibbin, and Hibberd or to the micrococci or round bacteria which Otto and Hibberd regard as holding a supremely blameful position among these various parasites. Later observers regard Puerperal Fever as a specific fever analogous to Typhus. Scarlet fever, small pox, and among the oldest upholders of this view are Dr. Barnes of London and Dr. Bridge Baker of New York. Dr. Baker says: "It is an essential fever peculiar to puerperal women and as much a distinct disease as Typhus or Typhoid." Others hold that puerperal accidents are potent factors in its causation—Dr. Playfair mentioning a royal instance due to this cause. A third set of observers and by far the most numerous affirm that it is a pure septicemia, puerperia, or pyemia due as a rule to septic inoculation of wounds or abrasions made in the genital canal by the passage of the child. While fully believing that puerperal accidents, overcrowded dwellings to have a strong predisposing influence upon the disease and that they injuriously modify its progress, I am still of opinion that the latter is by far the most accurate and scientific theory as to its nature and causation viz:—that it is simply septic inoculation of wounds or abrasions in the genital canal of precisely the same nature as surgical fever following wounds through the modified of course in some measure...
by the prevalent conditions abdominal and otherwise of lying-in-women.

Adopting this view I believe that Puerperal Fever, as far as contagion is concerned, may be divided into two classes of cases viz.; (1) Those which may be called Antigenetic in which we have the patient poisoned by the retention of portions of placenta or by the noxious influence of her own discharges and (2) The Heterogenous in which the poison is conveyed from without e.g. from the contaminated fingers or instruments of the accoucheur unhealthy and overcrowded dwellings and sewer-gas.

I cannot accept it as proven that any one of the specific fevers here can give rise to Puerperal Fever under any circumstances. If a lying-in-woman takes Scarlet Fever she has Scarlet Fever and not Puerperal Fever and the same applies to the other fevers as Typhus, Typhoid, Measles etc. etc.

By some eminent authorities (e.g. Sir James Simpson) it is laid down as a law which should never be broken that a practitioner who happens to be attending cases of Puerperal Fever, so called, or Empyema or Scarlet Fever should on no account continue to attend Midwifery cases, but should at once give up practice and go away for a change of air for at least a fortnight or three weeks. Now I do not hesitate to say that my experience has verified the teaching of my master under Dr. Matthew Duncan and known recently the doctrine enunciated by those gentlemen may be preached it is seldom or never practiced. It is I hold utterly absurd to suppose that a general practitioner can at a moment's notice when a case of Puerperal Fever appears in his practice leave his post, neglect his unfortunate patient and flee into the country, seeing that whatever reasons might be supposed to justify such a course be his patient would also justify any other practitioner in declining to attend his cases besides which and putting alto.

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suffered from the consideration of pregnancy matters, the interests of other patients
might make it imperative that he should remain at his post. And I have every reason
to believe that such a course is altogether unnecessary, because with scrupulous care in
ablutions, the use of antisepsics, a perfect immunity may be secured from all danger of infection or contagion. I venture to think that the following history
of a number of cases attended by myself, will tend to prove this.

CASE I. — Purpura Prafæxia.

Mrs. M.: Multipara was

confined on 18th Oct. 1879. — The labour was unusually quick and easy
so much so that on my arrival, I found the patient lying on the hearthrug
the child having been born some minutes. Somewhat hastily I tied the cord
and removed the placenta before getting the patient into bed, everything coming
away without the slightest trouble, and perhaps the very ease with which this
was accomplished, may have beguiled me into undoubting confidence that the whole
of the organ was removed. — The patient progressed quite satisfactorily until
the morning of the second day, when she had a severe crisis, and when I saw
her shortly afterwards, I found her in a very alarming condition indeed.
She complained of pain in the abdomen and back, suppuration of the
lochia, hurried respiration, pulse 130 and small, with a temperature of 104°.
Finding the patient so very ill and having only very recently taken up
my position in the neighbourhood, I sought the assistance of my friend
and then colleague at the Synguin Hospital, Dr. Finegan. Upon seeing the
patient in the evening of the second day, he pronounced her very grave —
all but hopeless — prognosis. However, as a dernier resort, I administered
 Etheraform while Dr. Finegan introduced his hand into the uterus and scraped

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away, with his fingers nail a series of adherent bits of placenta—some studded
with small blackish morsels resembling marbles in shape and size. After this we washed out the uterine
vagina with a solution of carbolic acid of the strength of one part of the acid to thirty
parts of water. We also prescribed for her one-grain of Cinchona and one grain of Opium
in full every four hours. This done we left our patient with but little hope that she
would survive till morning. To my surprise however upon visiting her next morning
I was much gratified to find her in every way better—pulse and temperature both
normal, tongue moist and clean. She had slept during the night, and in fact it
might be said that she was convalescent and her recovery was rapid and complete.

This was a case of what Dr. M'Intyre and Duncan, describes as Purpoeal
Sephtieamia or Septic Infusionation and the treatment pursued with the result indica-
ted fortifies this opinion. The whole aspect of the case had changed from all
but despair of recovery to convalescence in the space of twelve hours.

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Case II. — Purpoeal Septieamia

On July 11th, 1882, I was

called to attend Mrs. A. in her third confinement. Labour was easy and

natural. On the morning of the third day however she was seized with violent
vomiting, and diarrhoea with irrigs accompanied by great constitutional dis-

orders as manifested by a quick, small, thready pulse of 130 and a temperature

of 102° F., quickened respiration, severe pains in the back and abdomen with tym-

tanities, cold, scanty and frequent and an expression of countenance almost Hippo-

cratic—everything about her in fact pointing to imminent dissolution. I prescrib-

ed brown rice and mustard poultices to the abdomen, iced soda water

and brandy to relieve the distressing attacks, an astringent mixture contain-

(5)
Hamatomelum Opium and Muriatic Water to check the Perforation. I then gave a Hypodermic injection of 1/2 gr. of Hydrochlorate of Morphia to relieve pain. I also washed out the Wound cavity and vagina with a warm solution of Carboce Acid— one part in 30 of water—and left my patient. In the course of two hours I was hurriedly summoned to her bedside— the messenger telling me she was dying. On my arrival I found that she had had a very severe rigor, the attendant saying that the Bed and Furniture were literally "shaking." She had now a most deathly aspect, face and lips livid and cyanotic cold—in fact I considered her moribund and thought that she could not possibly survive many hours. In taking her Temperature in the axilla I found that the Thermometer registered 106°F. I immediately gave her 20 grains Zuminia Sulph— applied cold ice-packs to the head, chest, and abdomen— changing them every few seconds. In two hours the Temperature had fallen from 106°F to 102°F— pulse became slower and fuller—respiration more regular and easier and the patient herself as being much better. I prescribed a grain of Zuminia with a grain of Opium every fourth hour, and directed that whenever the Temperature threatened to rise above 102°F— which it did on many occasions— the cold-packs were to be re-applied and that I managed to keep the Temperature down to between 101°F and 102°F for a period of about nine days. It was interesting to notice what a marked and decisive effect the cold-packs had upon the Temperature— when vigorously applied the Temperature would drop a degree or more in the course of a very short time. The convulsions of the patient were tedious and fluctuating— the diarrhoea and vomiting coming much tittered and disappointing and the Dyspepsia and abdominal pain continued for some time. Finally however she made a good recovery and is now in perfect health and putting on flesh.

I think that this case is one of those which Dr. Duncan would.
designate as Puerperal Septicemia. The onset was somewhat more delayed than in the former case. The symptoms were pronounced with greater intensity and pointed more directly to sepsis than in the former case. I believe that the plan of treatment adopted was a direct attack upon the disease, and that if it had not been imposed decisively and carried out energetically the patient must have succumbed.  

Case III.  

Puerperal Septicemia.  

Mrs. J., aged 38, 
Primipara. Attended April 15th, 1852. Had a very tedious labour, an exceedingly rigid Perineum which at delivery could only be accomplished by the forceps was extensively torn. On the third day the head was followed by a rapid rise of temperature, the Thermometer indicating 102° F. Hot packings were immediately reduced to the abdomen. Through irritation of the genital passages with a Carbolised solution (1 to 50) was practised morning and evening, and ammonium and quinine in gr. doses of each were administered every four hours. The further progress of the case was unimportant, and needs no further comment excepting to say that a rapid reduction of the severity of the symptoms followed the course of treatment indicated. This case, like the two former ones, made a perfectly satisfactory although somewhat tedious recovery.  

Case IV.  

Puerperal Septicemia.  

Mrs. K., aged 20, 
Primipara, attended 10 October, 1852. Labour was tedious and-
The use of the forceps became necessary. From a cause which I need not
here stop to indicate the perineum was considerably torn. Fortunately ever
ting went on quite satisfactorily—until the third day when the patient had
a severe rigor and the temperature ran up to 103°F. The secretion of milk
was suppressed. Dehiscence became scanty and faint and severe abdominal pain
superseded. The same treatment was adopted in this case as in the former one.
Bowing irrigation of the genital canal with antiseptic solution—Carbolic
Acid one part to 800 parts of warm water— with Quinine and Opium gr. 1 of each
internally and hot mustard and Mustard poultices to the abdomen.
Convalescence was somewhat tedious but the patient made a perfect
recovery.

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Case V.—Puerperal Septicemia.

Mrs. J—aged 24, Primipara. Attended 21 October 1882. This patient had a tedious
labour in the second stage, the head being long delayed resting on
a very rigid Perineum, and during a forcible pain it gave way, being
bent over within a few lines of the anus. The patient progressed satisfactorily
enough until the third day when she like the others had a severe rigor
with a rise of temperature to 103°F. Quinine and Opium internally, and the application of hot
poultices to the abdomen, and with results equally favourable the
patient making a rapid and perfect recovery.
I think that I may refer to the three cases last narrated as being good examples of instances perhaps not very frequent in which we find the uterine poison absorbed into the system from a torn perineum and I may remark here that I have in many instances sutured the laceration primarily so as to avoid as much as possible this source of infection but always with very unsatisfactory results. I have never yet succeeded in inducing primary union. I have sutured in the ovoid form following Ponscots plan chiefly but the sutures has always taken its way through. I have now abandoned the primary operation altogether.

The above cases occurred in private practice and were attended by myself. The following occurred in hospital practice and were attended in the first instance by the district midwife at the patients own house, and I was called in later on by the midwife to see the cases in my capacity as medical officer to the Ladies Charity and Lying Hospital in this city.

**Case VII** — In this case labour being complicated—twins—I was in attendance myself. With the first child presentation was natural, with the second the arm presented and version with forceful extraction under chloroform was practised. On the evening of the second day she was seized with rigor and temperature ran up to 103°F. Pulse was rapid, abdomen exceedingly tender and painful with marked hypsacstals. Treatment that already indicated my antisepctic injections, Quinin and Quinine internally, and hot poultices to the abdomen. Diet milk, beef tea and eggs and small doses of Brandy. This patient recovered very slowly being in extreme poverty and watchfulness pelvic cellulites came on. The uterus
becoming quite fixed in the pelvis. Ultimately, however, she recovered perfectly and has since been attended in quite a natural labour by the same district midwife.

Case VII. — Mrs M. aged 25. Multipara — attended by the District Midwife. Had signs on the third day — temperature running up to 101½ F. with a pulse of 130. Abdomen tender and slightly swollen. The treatment was in every respect that already indicated and she recovered perfectly.

From my notes I find that I might extract the number of these narratives very considerably. The remainder of the cases however and their successful termination seem to justify them of their interest and I will close this record with the history of one more case which presented some peculiarities:

Case VIII. — Mrs S. — Multipara — aged 38. Had an easy and natural labour attended by midwife. On the third day, I was called to see her. She had had a rigor was very feverish — temperature 104 with much abdominal pain. On making a vaginal examination I found a fibrous polypus of about the size of a large orange attached by a longish pedicle to the posterior lip of the Os Uteri and completely occluding the vaginal outlet. On pushing the tumour aside a gush of putrid lochial discharge made its escape with a most horrible stench. The Os Uteri was literally bathed in a pool of this putrid discharge which had been built up in the vagina canal. Antiseptic injections were immediately assorted to.
Opium were administered internally and hot-poultices were applied to the
abdomen. Taking all the circumstances of the case into consideration my
prognosis was certainly a grave one and giving strict directions to the
attendant nurse as to the efficiently carrying out the line of treatment, I
left the patient for the night. Upon visiting her the next morning, I was
agreeably surprised to find her in every respect wonderfully improved. The
head slept during the night—pulse and temperature were both normal and the
abdominal pain was gone. Her prospect towards recovery was henceforth unim-
terrupted. In this case I think we have unequivocal proof of the patient having
poisoned herself by her own discharges which were effectually prevented from
making their escape by the tumour alluded to clogging the vaginal outlet; for
when the imprisoned lobes were permitted to escape and the vagina passed
had been thoroughly cleansed with antiseptics, the improvement made by the
patient even in the course of a few hours was truly astonishing. And on the
other hand, when we consider how the softened cervix with its open lymphatics
and perhaps its atheromaous membrane lay bathed in discharges already
become highly putrefactive even to their retention, I think it is not to be wondered
at that death should appear to be imminent when the patient was every moment
absorbing some delirious particles and perhaps bacteria into her system.

At the commencement of this paper I expressed the opinion that
by far the larger proportion of cases of purpura farce to called were autogens-
thetic and that with due care on the part of the operator, there was but little
danger of his infecting his patients whatever diseases he might be called to attend
to in general practice while attending to his duties as an Obstetrician. To empha-
size this opinion I may mention that during the five and a half years I

(1)
Have been in practice in this city. I have attended Two Hundred and Fifty cases of Midwifery in private practice and many more, of which I have no record, in my capacity of Medical Officer to the Ladies Charity and Syngue Hospital. In all my obstetric practice I have lost but two cases by death; one in private and one in Hospital practice, and being at the same time employed in general practice, it naturally devolved upon me to treat simultaneously all kinds of ailments both medical and surgical. During these five and a half years I have seen two epidemics of Small-Pox; I have almost always fewer or more cases of Scarlet-Fever on hand, occasionally a case or two of Typhoid Fever and sometimes severe cases of erysipelas with perhaps a stray case of Puerperal Inflammation and with all this my Obstetric engagements while falling in. I do not believe that in any one instance I have been the means of conveying puerperal fever to any single patient. I have seen a case of puerperal fever crop up in my practice at a time when I had no infectious disease at all to attend to and on the other hand, I have attended numbers of midwifery cases during an epidemic of malignant Scarlet Fever without any succeeding ill effects. I attribute the immunity which I have enjoyed in this respect entirely to my taking the most scrupulous care in thoroughly disinfesting my hands before making any vaginal examination. I never think under any circumstances whatever of making a digital examination of my patient without first washing my hands in a solution of one in twenty of Carbolic Acid, using at the same time a nail brush with abundant of soap. After these precautions I do not hesitate to consider myself perfectly aseptic and safe, so far, to undertake any duties which may devolve upon me. In this connection I will venture to relate an...
example case which occurred in my practice a few weeks ago:—I was hastily summoned one morning to visit a patient—a old woman in the suburbs—who I was told had something wrong with one of her legs. On my arrival I found my patient suffering from a most severe form of phlegmonous erysipelas—the whole limb—including the toes the foot the leg over the knee and extending upwards covering the inferior and upper aspect of the thigh—was red, intensely swollen and burning with the boogey feeling indicating suppuration and sloughing and this was especially marked on the shin. Of course the necessity for extensive incisions down to the bone was obvious enough and in making them my hands were in contact with sloughs and batted in pus. Having done all that was necessary I returned home to find a nursery awaiting me requiring my immediate attendance at a midwifery case—I had no time to lose—so seemed inevitable. I need hardly say that under the circumstances I felt somewhat anxious lest I should infect my patient coming straight as I was from so serious a case of erysipelas and remembering the close alliance held to exist between erysipelas and periperal fever. However there seemed to be no alternative and I went before examining my patient taking my usual precautions of disinfecting all my hands in carbolic acid solution of 1 to 20. On one time this case was terminated. But I had no sooner reached home than I was sent for by the District Midwife to a case of the same—tedious labour in a primipara in which it became necessary to apply the forceps; again using the same precautions of disinfecting my hands and instruments before using them. This case also was terminated. I need scarcely say that for some days my anxiety about these two patients was very great. Both however recovered without a single untoward
symptom from first to last. I think that I may refer to these two cases as instances in which it might, under ordinary circumstances, have been expected that I should have conveyed septic poison from hypoplasmos patient to my obstetric cases, and it is not unlikely that some of my professional brethren might consider me an unscrupulous practitioner to attend midwifery cases at all under such circumstances. Former experience, however, in cases not altogether dissimilar had given me some confidence in the use of disinfectants and copious ablutions of hands and instruments. I may mention that I did not even change my clothes upon the necessity of doing which practitioners so much insist. I do not believe that in these cases much infective material becomes attached to the clothes—the great carriers of infection undoubtedly are the hands of the assistant and the instruments introduced into the vaginal canal.

In summarising these remarks I would say:

Firstly, that I believe the so-called Puerperal Fever is simply a septic poisoning of a nature exactly similar to that which we find in surgical cases.

Secondly, that the specific fevers Scarlet Fever, Typhus Fever, or to give rise to Puerperal Fever. If a puerperal patient takes Scarlet Fever she has Scarlet Fever and not Puerperal Fever.

Thirdly, that the causation of Puerperal Fever is either:

(a) Auto-generative, or
(b) Hetero-generative.

Fourthly, that by far the larger proportion of cases fall under
The first class — The Autogynic.

Fifthly: That with due care on the part of the Accoucher, in disinfecting his hands, instruments &c. before use, there need be but little fear of his being the means of conveying poisonous matters from one patient to another.

In conclusion I remark that I am now in a great measure relieved from the anxiety which I formerly endured in treating cases of Periporal Pyemia, and I feel confident if I see the cases sufficiently early that, with the antiseptic treatment indicated in this paper, carried out efficiently, both topically and generally only a very small percentage of my patients will fail to recover...