

cecum, which was more healthy than the other, he discovered two foreign bodies, which he now exhibited. One was a plum stone, and the other was a black substance, of a fibrous nature, resembling woody tissue, and seemed to be the fruit-stalk of a plum. The mass was extremely hard, but did not present any of the characters of colloid or other form of malignant disease. It was composed, microscopically, of a great number of undeveloped cells and fibres. All the other organs were healthy.—*April 25, 1863.*

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.*

TWENTY-FIFTH ANNUAL SESSION, 1862-63.

DR. BEATTY, President.

Report of Cases of Delivery by Forceps, in the Rotundo Lying-in Hospital, for Ten Months, from June 1st, 1862 to April 1st, 1863. By JOHN CRONYN, L.K.&Q.C.P., L.R.C.S.I., Assistant Physician Rotundo Lying-in Hospital.

I. June 3.—Johanna Doyle—primipara—aged 34, delivered at 7 30, p.m.; 16 hours in labour; $7\frac{1}{2}$ hours in second stage; 20 minutes third stage; first cranial position; fetal heart audible; male; inertia; 2 doses of ergot given without effect. Died, June 8, of uterine phlebitis.

II. June 15.—Norah Geoghegan—primipara—aged 30, delivered at 5 45, p.m.; 19 hours in labour; came in in second stage; third stage, 8 minutes; male; still-born; fetal heart inaudible; inertia. Died, June 21st, of metritis and peritonitis.

III. June 29.—Margaret Clare—primipara—aged 28, delivered at 7, a.m.; 31 hours in labour; came in in second stage; third stage, 10 minutes; male; first cranial position; fetal heart audible; inertia; head locked in pelvic brim; great difficulty in extraction; post-placental hemorrhage; dose of ergot, cold, &c. Discharged July 9.

IV. July 27.—Mary Cleary—primipara—aged 28, delivered 6, p.m.; 19 hours in labour; came in in second stage; third stage, 5 minutes; male; still-born; fetal heart inaudible; first cranial position; inertia; head in perineum for 7 hours. Discharged August 5.

V. August 9.—Catherine Mathews—third child—aged 30, delivered at 12, night; 19 hours in labour; 8 hours in second stage; 10 minutes third stage; first cranial position; fetal heart audible; male; inertia. Discharged August 17.

* These reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

VI. August 12.—Margaret Barry—primipara—aged 26, delivered at 7, a.m.; 25 hours in labour; second stage, 7 hours; third stage, 10 minutes; first cranial position; fetal heart audible; male; inertia. Discharged August 19.

VII. August 13.—Eliza Bourke—second child—aged 36, delivered at 4 50, p.m.; 15 hours in labour; $2\frac{1}{3}$ hours in second stage; third stage, 5 minutes; face to pubes and hand to head; fetal heart audible. Discharged August 21.

VIII. August 15.—Bridget M'Dermott—primipara—aged 38, delivered at 7 50, p.m.; 30 hours in labour; twins; first footling; second head; membranes ruptured at the end of an hour, and dose of ergot given; in $2\frac{1}{2}$ hours forceps applied; child still-born; inertia. Discharged August 24.

IX. August 16.—Susan Barnett—second child—aged 24, delivered at 11, a.m.; 17 hours in labour; second stage, 8 hours; third stage, 20 minutes; male; first cranial position; fetal heart audible; inertia; post placental hemorrhage; dose of ergot. Discharged August 24.

X. September 19.—Teresa Adams—second child—aged 24, delivered at 10 30, a.m.; 13 hours in labour; second stage, 3 hours; third stage, 15 minutes; first cranial position; fetal heart audible; male; inertia; head on perineum $2\frac{1}{2}$ hours. Discharged September 26.

XI. September 27.—Mary Glinty—primipara—aged 28, delivered at 11 30, p.m.; 26 hours in labour; second stage, 6 hours; third stage, 25 minutes; first cranial position; fetal heart audible; male; inertia; hand to head; head on perineum 4 hours; post placental hemorrhage; dose of ergot. Died, October 3, of puerperal scarlatina.

XII. October 21.—Ann Reilly—primipara—aged 22, delivered at 3, a.m.; 12 hours in labour; second stage, 5 hours; third stage, 30 minutes; fetal heart audible; female; had one fit of convulsions. Discharged October 29.

XIII. October 30.—Margaret Magee—second child—aged 18, delivered 3 40, p.m.; $29\frac{1}{2}$ hours in labour; $7\frac{1}{4}$ hours second stage; third stage, 20 minutes; face to pubes; fetal heart audible; male; still-born; difficulty in extraction of head; shoulders hitched in pelvic brim; narrow pelvis; child large, weighing 10 lbs. 7 ozs. Discharged November 11.

XIV. December 11.—Mary Farnell—primipara—aged 19, delivered at 6, p.m.; 32 hours in labour; second stage, 7 hours; third stage, 5 minutes; third cranial position; fetal heart audible; inertia. Discharged December 22.

XV. January 12.—Jane Byrne—primipara—aged 25, delivered at 11 15, p.m.; 16 hours in labour; second stage, 8 hours; third stage, 25 minutes; first cranial position; fetal heart audible; female; chloroform; narrowing of pelvic brim and outlet from deformity of pubic arch. Died January 18.

XVI. January 23.—Ellen Daniel—primipara—aged 23, delivered at 7 15, a.m.; 22 hours in labour; second stage, $8\frac{1}{2}$ hours; third stage, 5 minutes; 3×2 cranial position; fetal heart audible; female; head on perineum for some hours; inertia. Discharged February 3.

XVII. January 23.—Mary Haddock—primipara—aged 20, delivered at 5 45, a.m.; 14 hours in labour; second stage, 5 hours; third stage, 40 minutes; first cranial position; fetal heart audible; female; chloroform; inertia; vomiting; great excitement; albuminuria; convulsions threatened. Discharged February 15.

XVIII. January 31.—Catherine Holdbrook—primipara—aged 19, delivered at 11 15, p.m.; $16\frac{1}{4}$ hours in labour; second stage, $8\frac{1}{4}$ hours; third stage, 45 minutes; first cranial position; fetal heart audible; female; chloroform; retained placenta from morbid adhesion; introduction of hand for removal. Discharged February 9.

XIX. February 8.—Eliza Murray—sixth child—aged 30, delivered at 6, p.m.; 11 hours in labour; second stage, 7 hours; third stage, 10 minutes; first cranial position; fetal heart audible; male; accidental hemorrhage; disproportion; head very much compressed; child born with suspended animation; restored by Marshall Hall's method. Discharged February 16.

XX. February 8.—Margaret O'Callaghan—sixth child—aged 32, delivered at 10 20, p.m.; 17 hours in labour; second stage, 3 hours; third stage, 15 minutes; first cranial position; fetal heart audible; female; disproportion; great excitement and hysteria; chloroform; delivered in hospital by forceps 9 and 5 years previously. Discharged February 18.

XXI. February 9.—Anne Byrne—primipara—aged 38, delivered at 12 15, a.m.; $33\frac{1}{4}$ hours in labour; $8\frac{1}{2}$ hours second stage; third stage, 30 minutes; first cranial position; fetal heart audible; female; inertia; great exhaustion and debility; hemorrhage, post placental; ergot, wine, brandy; very quick pulse. Died, February 14th, of uterine phlebitis.

XXII. February 21.—Eliza Corbett—second child—aged 30, delivered at 10 10, a.m.; 15 hours in labour; second stage, $10\frac{1}{4}$ hours; third stage, 15 minutes; first cranial position; fetal heart audible; male; chloroform; ergot 38 minutes before delivery; inertia; former delivery, in hospital, by vectis. Discharged March 5.

XXIII. February 22.—Catherine O'Neill—primipara—aged 16, delivered at 8 50, p.m.; $21\frac{1}{2}$ hours in labour; second stage, $6\frac{5}{8}$ hours; third stage, 15 minutes; first cranial position; fetal heart audible; female; convulsions; one very bad fit; delivered during fit; urine albuminous. Died February 24, of "peritonitis and pneumonia."

XXIV. February 26.—Sarah Farrell—primipara—aged 25, delivered at 11 50, p.m.; 18 hours in labour; second stage, $5\frac{5}{8}$ hours; third stage, 20 minutes; first cranial position; fetal heart audible; male; inertia. Discharged March 13.

XXV. March 10.—Martha Pierson—primipara—aged 21; twins; first born 3 15, p.m., second at 4, p.m.; 56 hours in labour; second stage, 6 hours; third stage, 30 minutes; males; inertia; ergot given without effect; first delivery by craniotomy, forceps being ineffectually applied; second by forceps; face to pubes. Discharged March 22.

XXVI. March 12.—Mary Fitzpatrick—seventh child—aged 36; delivered at 3 15, p.m.; 24 hours in labour; second stage, 9 hours; third stage, 10 minutes; fetal heart audible; male; very difficult extraction; very large child. Discharged March 20.

During the above period of ten months, the forceps were applied and deliveries effected in 26 cases out of 856 patients delivered in hospital, being rather over 3 per cent.

Of the mothers and children, in 17 cases both were saved, and in one both were lost; in 5 cases the mothers died, and the children were preserved; and in 3 cases the mothers recovered, and the children were lost. There were 17 primipara; 5 in labour of the second child; 1 of the third; 2 of the 6th; and 1 of the seventh. Of the 26 cases, 18 were for inertia, 6 for disproportion, and 2 for convulsions. There were 6 deaths of mothers—2 from uterine phlebitis, 2 from metro-peritonitis, 1 peritonitis and pneumonia, and 1 puerperal scarlatina. Of the children, 6 were still-born; in 4 cases the fetal heart was inaudible. In addition to the 26 cases detailed, the forceps were ineffectually applied in 3 other instances, and craniotomy afterwards resorted to.—11th April, 1863.

Dr. KIDD read the following paper on *The Mechanism of the Expulsion of the Head in Face to Pubes Presentations*.

The cases in which the head passes through the pelvis, with the face presenting to the pubes, are not numerous; thus, in Sinclair and Johnston's report, there are but thirty-three cases recorded, in 13,748 deliveries, in which this occurred. This circumstance will probably account for the fact, that in the majority of systematic treatises there is no account given of the mode in which the head escapes from the outlet of the pelvis in such cases. In one book, however, which has recently come under my observation, the subject is incidentally alluded to in connexion with the directions given as to the use of the forceps, when it is requisite to afford assistance in such cases. The directions are so peculiar, and so much at variance, not only with what I consider to be the true mechanism of such labours, but with my own experience and mode of operating, that I think it well to bring the subject under the notice of the society. I shall read the passage I refer to, as it occurs in *Bedford's Principles and Practice of Obstetrics*, first premising that Dr. Bedford uses a forceps with a second, or lateral, curve, and, when operating, places the patient on her back, the hips brought to the edge of the bed, and the feet supported by an assistant at each side:—

"Second Position—the Occiput, regarding the concavity of the Sacrum, the Face to the Pubes.—It will at once be seen that the head here is completely reversed; and, moreover, in this position the forceps will, in the majority of instances, be indicated for the reason of the protraction of the labour; for you are not to forget that the occiput, being posterior, must have traversed the entire length of the posterior wall of the pelvic cavity—consisting of the sacrum and coccyx—before it can make its exit; and, as a general rule, the increased duration of the labour will have so far perilled both mother and child as to render it necessary to resort to the forceps. But, in addition, any of the accidents already mentioned would constitute another motive for the use of the instrument. The rules for the introduction of the forceps are precisely the same as in the first position. It is well, however, to remember that there will be more difficulty in the extraction of the head in this second position, and the force employed should be more guarded, for the face cannot be brought under the pubes with the same facility that the occiput was in the preceding case, because of the greater irregularity of its surface. Again, the distension of the perineum will be much greater because of the rounded and more prominent configuration of the occiput. It must also be recollected that in this position the forceps, as soon as the head begins slightly to protrude, instead of being elevated, must be depressed, for the purpose of bringing the chin from the sternum, so that when the head is delivered the instrument will be at a right angle with the spinal column."

I do not stop to criticise the statement in this passage, as to the forceps being required in the majority of such cases, but pass at once to the direction that "the forceps, as soon as the head begins slightly to protrude, instead of being elevated must be depressed, for the purpose of bringing the chin from the sternum, so that when the head is delivered the instrument will be at a right angle with the spinal column," as it is, I believe, founded on a mistaken view of the mechanism of such labours; and it is to this point I wish to direct the attention of the society.

With a view to the illustration of what I conceive to be the true mechanism of such presentations, I will narrate two cases:—

On 21st September, I was called to see Mrs. —, an out patient of the Coombe Hospital, residing in Ardee-row, in labour of her first child. The membranes had ruptured early. I saw her at noon. The head was at the brim of the pelvis; the anterior lip of the os uteri was rigid; she had vomiting and restlessness; her pulse was quiet; the head in the fourth position of Nægelé. I ordered an opiate, and saw her again at 3, p.m., when no advance had been made, the anterior lip still retarding the progress. I pushed up the lip, and applied the forceps, but could not move the head without using more force than I thought advisable, and therefore withdrew the instrument.

At 4 30, Dr. Churchill, in the absence of my colleagues, Drs. Sawyer and Ringland, saw her with me. No advance of the head had been made; the anterior lip had again descended; the pains were troublesome, but not effective. We ordered a dose of ergot, and that the anterior lip, which was still down, should be supported.

At 8, p.m., I again saw her, when I had the benefit of the assistance of both Drs. Ringland and Sawyer. The ergot had produced ergotic pains, the lip had gone up, and the head was slightly lower in the pelvis. I again applied the forceps, and with great difficulty effected delivery. I made some effort to turn the head into the first position, but could only move it very slightly, and with the next pain it resumed its former place, and I extracted it with the face to the pubes, allowing the head, as it came to the outlet, to determine, in a great degree, its own course. As soon as the forehead came under the symphysis pubis it rested there, and the occiput began to make the sweep of the perineum, when it was necessary to carry the handle of the forceps forwards, between the mother's thighs, and even more up on the abdomen than when the head is in the first or second position.

The rectum did not at any time open out, nor did the perineum become as much distended as in delivery in the first and second position.

The child was still-born, in consequence, I believe, of the ergot. The mother recovered. The sphincter ani was not injured.

The next case was one of labour at the eighth month, with a roomy pelvis, and a putrid child, with the bones of the head separated. On visiting her I found a portion of the scalp protruding through the vulva, the face to the pubes, and the pains inefficient. I made an assistant keep his hand on the uterus and make pressure, and, grasping the scalp, made such traction as I could. The pressure over the uterus seemed to help its contractions to expel the child. The head came down till the chin cleared the arch, and then the occiput swept over the perineum.

These cases appear to me to illustrate the mechanism of expulsion in such labours under two conditions:—1st. When the head is large, and fits the pelvis closely, the head descends till the forehead, where it joins the nose, rests on the arch of the pubes; here it rests, as the occiput does in the first and second position, and the occiput makes a sweep of the perineum; but in the second case, where the head was small and softened, it descended lower, and the chin emerged before the rotation began or the occiput swept the perineum; and I would lay it down as a rule that, when it is necessary to use the forceps in face to pubes presentations, the handles should be carried well forward as soon as the forehead emerges from under the arch of the pubis.—11th April, 1863.

Forceps.—At the opening of the meeting of the 8th of May, the PRESIDENT made the following statement:—

Before we proceed to the regular business of the society, I wish to ask your attention for a few minutes to a circumstance arising out of a passage in the address which I had the honour to deliver at our first meeting this session. In alluding to Dr. Graily Hewitt's highly practical and very valuable paper, in the 3rd volume of the *Transactions of the Obstetrical Society of London*, I said the forceps described and recommended by him, corresponded in every particular with my forceps, described and represented twenty years ago in the *Dublin Medical Journal* for 1842; so much so, that one would be disposed, at first, to think him guilty of piracy—but that I entirely acquitted Dr. G. Hewitt of any such intention, as it was very likely he never read my paper or saw my engraving. I was led into what I now find was a mistake by the manner in which Dr. G. Hewitt described his instrument. He says:—"The length of the blades is eight inches instead of six and a-half or seven, and the curve is an arc of a circle of fourteen inches in diameter, instead of ten or eleven." The plate accompanying Dr. Graily Hewitt's paper, represented an instrument so like mine that, taking his account of the length of his blade, I was at once struck with the resemblance, and noticed it as a coincidence, but never for a moment considered it a copy. In the description of my forceps, I mention that the length of the blade to the lock is eight inches, just what Dr. Graily Hewitt states his to be; and thus the error arose. In a correspondence which I have had with Dr. G. Hewitt, he states:—"You are not quite correct in saying my forceps is identical with yours; I have carefully examined your plate, and I find that my forceps has a longer blade than yours; the two are in the proportion of eleven to ten. When I say my forceps is eight inches long, I do not mean that it is eight inches from the point to the lock, but that it is eight inches from the point to the crossing of the blades when locked. From this it results that my instrument has a working length about seven-tenths of an inch more than yours." Dr. Graily Hewitt has kindly furnished me with a diagram, representing the two instruments in juxtaposition, which I have now the pleasure to submit to your inspection, and from it you will perceive that Dr. G. Hewitt is quite correct in what he has stated. I forwarded one of my instruments to Dr. Hewitt, with a request that he would kindly send me one of his, and I am happy to say, he at once and most promptly complied with my wish. It affords me very great gratification to be able to exhibit the instrument to you this evening, and thus to give you an opportunity of seeing that there is a difference between the two instruments. I very much regret the error into which I was unintentionally led, but it entirely arose from Dr. Hewitt's not mentioning the points from which his measurement was taken in his original paper. If he had explained, as he has since done, that he did not adopt the usual mode of measurement, this mistake could never have occurred.—8th May, 1863.

Ruptured Uterus.—Dr. J. R. KIRKPATRICK exhibited a ruptured uterus taken from a patient who had been delivered in the Lying-in Hospital. The rupture was at the right side of the uterus, extending through the cervix, and upwards for about three inches. A large quantity of blood was effused in the cavity of the abdomen, round the seat of injury. The structure of the uterus did not appear to be diseased. The patient was a delicate sickly-looking woman, 30 years of age. It was her fifth pregnancy; her former labours had been easy. She had been two and a-half hours in the second stage, when rupture took place. The head was the presenting part, and was hydrocephalic, containing 32 ounces of fluid. The labour pains had not been strong, or bearing down at any period of the labour. Craniotomy was immediately performed; but she sank and died in six hours after delivery—*8th May*.

Cursory Remarks on Scarlatina; relating chiefly to its Natural History and Treatment. By HENRY KENNEDY, M.B., one of the Physicians of Sir Patrick Dun's Hospital.

At a late meeting of this society, the chairman, Dr. M'Clintock, suggested that the debate then going on, in reference to Dr. Halahan's paper on "Puerperal Scarlatina," might be adjourned with advantage. Acting on this hint, I venture to bring the disease, as it is usually met amongst the young, again under notice. When, many years since, some account of scarlatina, which then prevailed for a lengthened period in Dublin, was published by myself, I had not enjoyed those opportunities for treating it which have occurred since; for what I have now to offer has reference mainly to treatment. Two points only I would premise here:—First, that the remarks will be strictly cursory; and so I will only notice such parts of the subject as I believe to be of importance; and, secondly, that the remarks will be taken in a general way—that is, as being usually applicable. General principles, I believe, are all that should be attempted in speaking of the treatment of any disease, for every case must be dealt with according to the symptoms it presents; and, what is of more consequence, every one must learn this for himself. I would not make the former remark, but that some recent writers, amongst whom may specially be mentioned Professor Bennett, of Edinburgh, have endeavoured to render the treatment of some acute diseases, as pneumonia, quite a routine. On this point I shall only say here that the cases detailed by the Professor himself afford a flat contradiction to his theory.

And now, as to the more immediate subject of my remarks; and, first, of the sore throat attending scarlatina. Is the treatment by caustic too indiscriminate? I rather think so; and I believe I have seen mischief, in the shape of the external swellings, come on as a consequence of caustic being applied to the whole of the inner throat. And, if we consider

the extent of surface inflamed, and the constant secretion going on from it, there appears to me sufficient reasons for, at any rate, exercising caution on the point. Of late years, I have used caustic to the ulcerated parts alone, and this only when the surface was very painful; for all whom I address must be aware of the very marked contrasts which exist amongst patients—some suffering a great deal from the act of swallowing, &c., and others literally nothing. When, then, the former state exists, the destruction of the surface of the ulcer by lunar caustic is usually followed by marked relief, and is, of course, judicious.

Cleanliness, and the removal, as far as may be, of all the secretions from the throat, I believe to be of very much greater consequence than the use of caustic; and, where the patients are of such an age as to be unable to assist themselves, by gargling or otherwise, too much attention cannot be given to the point; and, with the assistance of such an elastic bag as is used for syringing the ears, a great deal may by this means be done. I am the more anxious to call attention to this, because it has, I have found, much more to say to the welfare of our patients than might at first sight appear. I mean the removal, as far as lies in our power, of the morbid secretions poured out, which may do injury in more ways than one. They may keep up the vomiting, which, as all know, so often ushers in the attack in the severe cases of the disease, but usually subsides within the first thirty-six hours. It may, however, last very much longer, and become most serious; nor could I attribute it in those cases I have seen to other cause than the secretions being swallowed, and, acting on this idea, have arrested it by the use of an emetic, which is an old and popular remedy at the beginning of the attack, but which, it is not so generally known, may be used with good effect much later. The following was a case in point:—

CASE I.—Miss —, a young lady of fifteen, was seized with a sharp attack of scarlatina, ushered in by severe vomiting and purging; after thirty-six hours the latter ceased, but the vomiting continued, in spite of treatment specially ordered to control it; and so went on for a whole week. During this period she got a certain amount of broken sleep, and I observed that it was after it she always had the vomiting, which was preceded by nausea, of which she complained much. In this state she was given a mustard emetic, which acted strongly; but it had the effect of at once putting a stop to the vomiting and nausea.

The principle of using emetics at this late stage of the disease I believe to be worth keeping in mind; and Copland, I find, recommends the same. It may be mentioned that I have often used them in common fever, and with advantage, at the same stage.

In place, however, of the stomach being the organ which suffers, or appears to suffer, from the morbid secretions, it may be the bowels, showing itself by diarrhea—not that which so often ushers in the attack,

but a recurrence of it, or even a tendency to it, at the end of a week or longer. This state always demands a special attention, for I know of none so apt to be attended or followed by serious complications. No single symptom, I believe, ought to guide our prognosis more than this one. A confined state of the bowels is safety in comparison; and something of the same obtains, I rather think, in some cases of puerperal fever.

Of the complications which may arise at this stage of scarlatina—that is, after the patient has been seven or eight days ill—I shall here only call attention to the secondary fever. All know that in small-pox secondary fever very commonly, though not always, comes on, and is frequently the cause of death. But it is not so generally known that exactly the same thing occurs in scarlatina; and as regular typhus as it is possible to be, with the exception of the spots, may arise. It is best marked when the disease attacks adults; but it is also to be seen amongst children, and, both in adults and children, may be the direct cause of death. Hence it is of importance to be aware of such a point in the natural history of scarlatina, for to be forewarned is to be forearmed. The occurrence seems to be due to the absorption of the morbid secretions, just as takes place in small-pox, and is an additional reason for paying every attention to the point already spoken of this evening—that is, their removal, as far as lies in our power. On the treatment of this kind of fever I have nothing specific to offer. It resolves itself into the general principles which are applicable to all acute diseases of a low type, which must, of course, be modified according to the demands of each case.

Of the medicines which answer best in scarlatina the acids appear to me to merit the first rank. I have found them far superior to either the chlorate of potash or the much-vaunted carbonate of ammonia. Cases do occur where the fever presents more of the sthenic type, and in which the chlorate may be given with good effect. It acts very generally on the skin, and so relieves the patient. But it must always be watched; for cases are not uncommon where the alkaline treatment, as I may call it, so far from doing good, does positive harm, by causing rapid waste of the system, and having a strong tendency to run off by the bowels; and this seems to me still more marked with the carbonate of ammonia, about which salt a special work has been written—I forget by whom—giving it the most unqualified praise in the treatment of scarlatina. Against such a use of it I must hold out a caution. We must never overlook now the effects of alkalis—and these, it will be recollected, are established facts—on the frame, even in health. But when, in addition to this, we recollect the type which most acute diseases have assumed of late years, there appears to me to be the strongest grounds for exercising a great caution on the matter; and the rule I would venture to lay down would be, that if the fever presents the sthenic type, with a hot dry skin and full

bounding pulse, alkalies may then be given with advantage; but the moment these subside our medicines should be changed to those of the opposite class. Cases of this type are, no doubt, exceptional; still they do occur. It may be added that this principle, which I hold to be most important, does not apply to scarlatina alone, but to almost all the acute diseases of the day.

It has been already stated that the treatment of scarlatina by acids is that which answers best. Of these the muriatic, in the proportion of one or two drachms of the dilute acid to the pint of fluid, seems to suit well. This medicine was a great favourite of the late Sir Henry Marsh, who used it in acute diseases of the low type, including common fevers; and, whether it be due to the chlorine or not, it certainly seems to possess marked antiseptic powers. It was very much used by the older writers, of the last century, and in the same type and class of diseases; so that it has a large experience in its favour, and has, besides, the advantage of being readily taken by children.

But there is another form in which this acid may be used; and, from what I have seen of it, one very likely to be more largely given than it is at present—I mean the tincture of muriate of iron. Within this two years I began to give it in scarlatina, having been led to use it, in the first instance, in a case where erysipelas attacked the face, just after the rash had declined, and fever had risen a second time; and here its effects seemed very decided. It is curious, of nearly all the cases where erysipelas appeared with, or just after, scarlatina, the former came out of the nostril, if I may so speak, and spread then over the face. The exceptional cases occurred in puerperal scarlatina, and then the erysipelas came from the vulva.

Did time permit I could give cases of the two diseases occurring together, and treated, with success, by the preparation of which I am speaking. But I must hasten on; and shall only say that its use in such cases led me to give it in the simple disease, and I think I may say with advantage. In the cases in which it was tried it seemed to act even more favourably than the simple muriatic acid; that is, by apparently shortening the stages of the disease; but it is not of such general application as the simple acid. It was given as follows:—Tincture of muriate of iron, two drachms; syrup, half an ounce; distilled water to four ounces; a tea-spoonful in water each three hours. I am not aware whether anything on this preparation has yet appeared in print as being used in bad scarlatina; but I have learned that others in this city have so used it. Professor M'Dowel is one of these.

To the use of anodynes during the progress of the disease I would call the attention of the meeting, as I believe them to be little, if at all, used as they deserve. Many years since, I satisfied myself, by *post mortem* examination, that whether we had raving, convulsions, or coma to deal

with, these were not dependent on inflammatory action ; and, besides, the late Dr. Graves, who wrote so ably on scarlatina, had given a full trial to the antiphlogistic treatment, but without a corresponding success. These considerations led me to the conclusion that, as it was some irritation we had to contend with, so there was the greater probability that anodynes might be of use, and under this idea they were given ; and, as it was mainly children with whom I had to deal, the tincture of hyosciamus was, as a matter of prudence, the one chosen. That it acts, and powerfully too, on children does not admit of doubt, no matter how we may explain Mr. Donovan's experiments ; and enough of experience has, I think, now been afforded to enable me to speak with some confidence on the point. When the raving or great restlessness exists, the tincture is given, either as a draught at night, or in small but continuous doses, till rest or quiet is obtained ; and in this way the dose has varied from half a scruple to three drachms at once. Opiates, I should say, have also been tried ; but they require at least as much caution as in common fever ; and with children to deal with, it is best, as a whole, not to use them. The idea of giving anodynes at all may, very probably, have arisen in my mind from Dr. Graves having so strongly recommended tartar emetic and opium in certain stages of fever ; for the class of cases of scarlatina of which I am now speaking affords the closest analogy to the cases in which that physician gave the tartar emetic and opium. At any rate the principle is one which seems to me well worthy of being followed, and as such is mentioned here.

In connexion with, at least, some of the cases of raving, and of others where none exists, there is a point in their natural history worthy of note. I mean the occurrence of crisis, just as marked as ever occurs in common fever, and most generally by sweat. I am not aware of having read this anywhere ; but I have now seen the pulse fall from 120 to 80 within the twenty-four hours, at the same time that a general perspiration was present, too frequently to have any doubt on the matter. The same thing, too, may be met with in measles ; and the possibility of its occurrence should not be forgotten, as our treatment will be modified by it.^a

In the last place, I would make a few remarks on the use of wine, and which, it may be recollected, was one of the chief topics brought forward in Dr. Halahan's paper on Puerperal Scarlatina. Whilst agreeing with him in the general principle of the great benefit to be derived from its free use, I wish I could consider it as an agent on which we might at all

^a In this present month (May, 1863,) a boy, of sixteen years of age, was admitted into Dun's Hospital, labouring under a very severe attack of scarlatina. He had a rapid pulse, livid extremities, a dark eruption, and raving. Whilst being treated with wine, tincture of muriate of iron, and an anodyne, at night, with the rash still out, a marked crisis by sweat appeared, and within thirty-six hours this case could be pronounced out of danger. My colleagues, Drs. Smith and Moore, saw the case.

rely as much as he seems to think we may. I have seen it fail too often, in puerperal cases especially—and even given very freely—to have such confidence in it. The truth is, wine, though having some other valuable qualities, must be looked on as mainly a stimulant. On the other hand, puerperal scarlatina, at least as I have seen it, and even in numerous cases of the simple disease, presents characters which show a profound alteration in the blood itself. Patches of redness, spots of purpura, sometimes erysipelas, an extraordinary tendency to stripping, &c., all betoken this, and force us, as it were, to employ some agent or agents which may counteract such tendencies. Whilst, then, I would not for one moment think of discarding wine, I would not trust it alone, but conjoin with it such medicines as the experience of the day warrants; and amongst those I believe none holds out a better prospect than the muriatic acid and the tincture of muriate of iron, of which I have already spoken. To these it might be well to add the internal use of barm, which, in cases of common fever, I have often seen used with marked benefit; I mean fever presenting all those bad signs which puerperal scarlatina, in its worst form, exhibits. I may mention, that one of the few cases of the latter disease which I have seen recover, did so under the use of barm and wine; and it appeared to me improvement directly followed the administration of the barm, for she had been getting wine previously.

In speaking of the use of wine in these cases it may be well to know that, just as occurs in simple fever, cases are common where it is not borne well. The patient either refuses it, or the symptoms do not better under its use. In these circumstances it is always best to take the hint, and act accordingly by not pressing it.

In those cases of scarlatina where wine is required, it is usual to combine with it fluid food, such as beef tea or chicken broth. Now, I do not deny that in many instances this is quite right; but I would venture to suggest that a proper discrimination be made. It seems to be a general impression that if wine be suitable beef tea must be so likewise; and we are told to keep up the patient's strength by all means. Now this seems to me a serious mistake, and in no acute disease more so than scarlatina; but, having spoken generally on this point in another place, I shall only add here, that if it be not attended to, and a proper judgment formed in each case, secondary inflammations will be sure to arise, and so render most serious what would otherwise have gone most favourably through its several stages.

In concluding these cursory remarks, which may well be entitled "*disjecta membra*," the meeting will probably allow me to recapitulate the heads of the several points which have appeared to me worthy of their notice:—In the natural history of the disease there were the occurrence of vomiting at a late stage of the attack—the supervention of secondary fever, quite analagous to what occurs in small-pox—and the

occurrence of crisis, as marked as is seen in common fevers; and on the treatment, the more restricted use of the nitrate of silver to the inner throat—the use of emetics at a late stage of the attack—the very questionable benefit arising from the use of alkalies, including the carbonate of ammonia—the use of anodynes, especially the tincture of hyosciamus, as best suited to children—the administration of the dilute muriatic acid, and its combination with iron—and, lastly, the importance of separating wine in its effects from those of beef tea, the effects of the two being in very marked contrast.—8th May, 1863,

DR. DE RICCI read the following paper *On the Use of Sulphites and Hyposulphites in the Treatment of Zymotic Diseases* :—

Professor Polli, of Milan, has, for several years past, devoted much of his time to the investigation of zymotic diseases in general, but more especially of those which depend upon purulent absorption, with a view, if possible, to discovering an agent which would be capable of destroying those fermenting principles which are supposed to be the cause of these diseases, while it would be, at the same time, innocuous to the constitution. In May, 1862, *The Dublin Quarterly Journal of Medical Science* published a short review on Dr. Polli's work *On the Use of Alkaline Sulphites in the Treatment of Diseases depending on Morbific Ferments*, and that valuable periodical fully recognised the importance of his researches. Since that time I have been in direct correspondence with the Italian professor, and, having received from himself much additional information, together with the request that I would prominently bring the subject of his investigations before the medical profession in this country, I have complied with his request, and do so at present by laying a brief sketch of his labours and results before the Obstetrical Society of Dublin.

I should have been happy if I could have brought additional personal evidence of my own to corroborate Dr. Polli's accounts, but, unfortunately, my opportunities have been very limited. I have, however, proved to my complete satisfaction that the sulphites of soda, potassa, and magnesia, which are the remedies he relies on in cases of purulent infection, can be freely administered to human beings, even in large doses and for a considerable time, without producing any disagreeable consequences; and this fact will, I trust, induce many to give a trial to these substances in every case they may appear to be indicated, in order to test their real value.

I do not stop now to argue whether these so-called zymotic diseases really depend upon the presence in the circulation of a special ferment, as their name would imply; it is generally thought to be so, and for the present we must take it for granted, as Professor Polli bases all his theory and practice upon that assumption. The great physiologist Bernard held this view, and believed "that fermentation may arise in the blood, and

give origin to poisonous principles, which may, in their turn, produce certain grave accidents in the living frame;" but adds:—"We cannot neutralize these ferments in the living organism; *it is impossible*; because, to effect such a purpose, it would be necessary to interfere with the characters of the blood to such a degree that it would no longer be capable of maintaining life." The Italian professor, however, thought differently. He had long been studying the antiseptic effects of sulphurous acid upon animal substances; he had established, by repeated experiments, that animals recently killed, if suspended in a well-closed vessel, containing but a small quantity of a solution of sulphurous acid in water, just sufficient to supply, by evaporation, a slight atmosphere of sulphurous acid gas, would keep perfectly fresh for months. He also established, by direct experiment, that sulphurous acid, in very small quantities, had the power of preventing not only the ordinary vinous fermentations—a fact well known in all wine countries—but also those other fermentations, such as the diastatic, by which starch is converted into glucose, that of the pancreatic juice upon fatty substances, and that of emulsine upon amygdaline; and he came to the conclusion that in sulphurous acid we possess a substance capable of arresting every form of catalytic action. But sulphurous acid could not, with impunity, be introduced into the animal economy; so the professor turned his attention to the compounds of sulphurous and hyposulphurous acid, which, by analogy, he hoped to find equally capable, though probably in a minor degree, of arresting the action of ferments. He was not disappointed. He found that the action of the sulphites of soda, potassa, and magnesia was quite as decided as that of the acid itself in preventing fermentation; while, at the same time, he fully established the fact, that an animal, such as a dog, can take as much as fifteen grammes (225 grains) of an alkaline sulphite per diem, during a whole fortnight, without any inconvenience. I can fully corroborate the assertion of Dr. Polli so far; and am happy to be able to add my testimony to the fact of the perfect tolerance, by even the human stomach, of the sulphite of soda in full doses. I have often prescribed it, during the last two years, in doses of as much as one drachm three times a day, without producing the slightest gastric derangement; and, although I have not, as yet, had an opportunity of testing its value in a case of either pyemia, puerperal peritonitis, or glanders, still the results I obtained in the cases I employed it have led me to hope much, from its extended use in all diseases arising from purulent infection or septicemia.

Before relating, however, the results of my small experience in this matter, I wish to lay before the society some further account of Professor Polli's investigations. Having satisfactorily proved to himself that animals can bear with impunity large doses of sulphites, he took three dogs of about the same size; two of these he fed upon food containing

sulphite of soda, the third he fed exactly alike, only minus the sulphite. After twenty-four hours the three animals were killed, and, by analysis, he discovered the presence of the sulphite in the blood, the liver, and the urine of the first two dogs, while, as might have been anticipated, he found none in the third. This was a very valuable experiment. It proved that the sulphites, *as such*, were carried into the circulation; and, if they were able to prevent and even arrest the action of an animal ferment outside the body, Professor Polli argued that they might equally prevent or arrest the action of a ferment within the body; and, as the dogs which had been largely dosed with sulphites had shown no bad effects from it during life, nor any organic lesion from the use of them when examined after death, they having been purposely killed while still under the full influence of the sulphites, he concluded that in all probability we possessed in the compounds of sulphurous acid, with earths and alkalies, a means of arresting catalytic action in the animal economy without in any way interfering with the vitality of the blood—contrary to Claude Bernard's assertion, that any substance capable of destroying the action of a ferment in the living body would exert so destructive an action on the blood itself, as to imperil its vitality. Professor Polli now varied his experiments. He administered two grammes of sulphite of soda daily to a dog, for the space of five days; at the end of that time he drew off two ounces of blood, and exposed it to the air in an open vessel, beside another vessel containing blood drawn from a dog to whom no sulphites had been given. In a very few days the blood of the latter dog was quite putrid, while that of the dog which had been dosed with the sulphites was perfectly fresh even three weeks later. Two great facts had now been arrived at:—1st. That sulphites, when administered to a living animal, are carried, *as such*, into the circulation, and diffused all over the organism without the slightest inconvenience to the animal; and, 2nd. That the presence of these salts in the liquids and solids of the body retards the putrefactive fermentation for a very considerable period. Dr. Polli, having obtained such encouraging results, proceeded to more crucial experiments. He took two dogs of about the same size, and equally in good health; he fed them exactly alike for five days, with the exception of administering to one of them two grammes of sulphite of soda daily—the other dog getting exactly the same food, minus the sulphite. At the end of five days he injected into the femoral veins of both animals one gramme of pus, taken from a fetid abscess occurring in a broken-down constitution. The operation, in both cases, was carefully performed, and the animals suffered but little. Immediately after the injection both dogs appeared stupefied; they lay down and refused all food, remaining quite prostrate for twenty-four hours. On the following day, however, they both seemed a little better, and took some food. A second injection of pus was now practised on both animals to the same amount, but the first dog had, the

meanwhile, been getting two grammes of sulphites daily, while the other was only getting plain food. The effect of the second injection was most interesting; both dogs were affected instantly alike; both were seized with stupor; in both the pulse was rapid, but feeble, while the respiration was greatly accelerated. Both dogs refused to eat; both lay down in a state of stupor; and, when made to rise and walk, they tottered and reeled across the room. The first dog, however, continued to receive, daily, a dose of two grammes of sulphite of soda, and in four days was so far recovered as to be able to eat his food with relish, while the wound in the femoral vein was rapidly healing. The other dog fared differently; he got no sulphites, either before or after the operation, and the result was that he daily became worse; the wound in the thigh became gangrenous, the limb swelled up, and ten days after the second injection the dog died with all the symptoms of typhus, the first dog being already about and well. The result of this experiment was highly satisfactory; but the professor did not rest satisfied with it, so he again proceeded as follows:—On the 9th of March, 1861, he injected into the femoral veins of three dogs three grammes of putrid blood to each. (This blood was defibrinated bullock's blood which had been kept for four months; it was quite putrid, and exhaled a highly fetid ammoniacal odour.) The three dogs were about the same size, and were fed exactly alike, with the exception of dog No 1, who, for two days previous to the operation, had taken sixteen grammes of sulphite of soda in one gramme doses. A few minutes after the injection the three dogs were affected very much alike. No. 1 vomited almost immediately, looked ill, lay down at once, and for several hours refused all food; the following day it was still heavy and stupid. Two grammes of sulphite of soda were administered to it, and repeated the following days; on the third after the operation the dog was already much improved, and by the fifth it was perfectly well. Of the other two dogs, one, after having vomited, remained standing, with its legs wide apart and its head hanging down; it shook and shivered all over, then fell on its side, panting violently; gradually it became worse and worse, and finally died in five hours. The other suffered much in the same way; it lived five days, during which it was too weak to stand; it did not eat; the wound in the thigh became gangrenous, and it expired comatose, with all the symptoms of typhus fever. It was examined after death, when the lungs were found of a deep red colour, dotted all over with ecchymotic patches, some of which had suppurated in the centre, the right cavity of the heart was filled with black grumous blood, while a pale yellow fibrinous clot filled not only the left ventricle, but extended also into the aorta. The whole gastro-intestinal tract was injected, and smeared in places by purulent matter.

This same experiment was now repeated in a modified form. Two dogs of nearly same weight, in good health, were submitted to an

injection of one gramme of putrid blood each ; but in one case the blood was diluted first with three grammes of a saturated solution of sulphite of soda. Both dogs were very ill at first, and both dogs recovered eventually ; but while the dog which had received the diluted injection was only sick for two days, the other was more than a fortnight before it struggled into convalescence.

This was a very valuable experiment. It showed that these sulphites can with safety be introduced directly into the circulation, without in any way endangering the vitality of the blood. I should have premised that Dr. Polli had previously tested the safety of injecting a solution of sulphite of soda into the circulation by trying it in healthy dogs, and had done so repeatedly without any bad results.

Encouraged by the evident success of his experiments Dr. Polli determined to test the efficacy of the sulphites by bringing them into direct antagonism with one of the most virulent of animal poisons, that of glanders. He took a strong, healthy dog, and, having made a cutaneous incision between its shoulders, where the animal could not reach with its mouth, he introduced through it into the subcutaneous cellular tissue, some discharge obtained from the nares of a glandered horse. In a few days the wound became gaping and ill-conditioned ; the dog looked ill, heavy, and stupid ; it refused its food ; and, by the fourteenth day, the animal had a number of unhealthy pustules over his body, which spread out into ill-looking, sanious ulcers. The dog became daily worse ; and finally died twenty-six days after the inoculation. An examination after death revealed a highly injected condition of the mucous membrane of the stomach, with many ecchymotic patches. The intestines were highly vascular, and of a dark livid colour ; while the lungs were thickly studded with apoplectic clots. Professor Polli now took two large dogs, as similar as possible both in size and health, and having administered to one of them eight grammes daily of sulphite of soda, he injected into the femoral veins of both dogs three grammes each of the muco-purulent discharge obtained from the nares of the same glandered horse which had served for the previous experiment. The first dog which had received the sulphites seemed at first to suffer the most from the injection. It at once fell to the ground as stunned, and its breathing was rapid and panting ; but in a few hours it began to recover, and the following day it was able to eat. The second dog bore the operation better, and did not appear to sustain so severe a shock ; but on the following day it began to mope ; towards evening it was very drowsy, and with difficulty it could be got to stand ; by the third day the animal's extremities had become œdematous and painful ; by the fourth, a purulent discharge was running from its nose and eyes—the wound in the thigh was now almost gangrenous ; on the sixth day the animal died, worn out by pain, fetid suppuration, and diarrhea. The first dog was by this time completely recovered.

I might relate many other instances of equal interest, as Professor Polli has repeated these experiments nearly a hundred times, *almost always with a successful, always invariably with a satisfactory result*. He seems to have clearly established that, contrary to C. Bernard's assertion, those ferments which, in the animal frame, are capable of originating zymotic diseases, *can be neutralized* by substances which do not in any way prove injurious to the animal economy. I cannot bear full testimony to the correctness of Professor Polli's experiments, not having as yet had as ample opportunities of testing their accuracy as I could wish; but I can bear testimony to the harmlessness of the sulphites of soda, potassa, and magnesia, when administered internally, even in large doses; and I think that in three cases in which I employed these remedies I observed a decided improvement after their administration. One was a case of phthisis, with excessive purulent expectoration. The patient took for several months one scruple of sulphite of soda three times a-day, with very manifest advantage. In this case, although the cure was hopeless, I gave the sulphite with a view to diminishing the purulent secretion, and I was not disappointed. Another case was that of a gentleman who consulted me about an unpleasant eruption by which he was tormented, and which looked extremely like rupia. He assured me, however, that he had not had syphilis for eighteen years previous; and I have no reason to doubt his assertion. I gave him half drachm doses of sulphite of soda three times a-day, in a bitter infusion, and in a short time he became perfectly well. I gave him no other medicine. The third case was one of constantly recurring boils, some of which were sufficiently large to require incisions. The patient was a young gentleman in easy circumstances, well nourished; and although a student in an English university, not in any way broken in health by over study. He had been suffering from these troublesome boils for upwards of six months. After trying divers remedies, I placed him under a course of sulphite of soda, giving him one drachm of the salt three times a-day; and in less than a month not a boil could be seen on his body, though his face, neck, and shoulders still exhibited plentiful traces of his former tormentors.

It is not in private practice, however, that the real value of a remedy can be fairly tested. I have proved to my satisfaction that the sulphites are, at any rate, uninjurious to the animal economy. It now becomes the duty of those physicians and surgeons who enjoy the incomparable advantage of hospital practice to put these remedies to the test. It is to them that my friend, Dr. Polli, appeals. "To the test of clinical investigation and clinical result I leave the issue of my discovery," are his words to me.

If Professor Polli has not deceived himself—if these sulphites really have the power of neutralizing animal poisons, even after their absorption into the circulation—his discovery would be as great, if not greater, than

that of Jenner. Nothing can be more candid than his conduct has been throughout. He has made no secret of his discovery; and nothing can be more modest than his constant expression:—"I wait for the verdict of the clinical students of Europe."

In conclusion, I earnestly beg to call the attention of all practitioners, and especially of those who enjoy the privilege of hospital practice, to the subject of this paper. Let these remedies be tried in every case in which they offer a chance of success, both in the treatment of disease and as prophylactics also. Whenever they meet a case of scarlatina, let them treat not only the patient, but let every individual in the family take a certain quantity daily of one of these sulphites; and let the same plan be adopted in every case apparently depending on some zymotic poison, whether fever, pyemia, septicemia, or puerperal peritonitis.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.*

SESSION 1862-63.

DR. W. C. TOWNSEND. President.

DR. POPHAM *on Carbonate of Ammonia in the Urine.*

Dr. Popham exhibited a specimen of urine, passed by a patient in confluent small-pox, containing carbonate of ammonia in such abundance as to effervesce briskly upon the addition of the mineral acids. The patient was a woman of 45 years of age, almost idiotic. The urine presented nothing unusual until the seventh day from the appearance of the pustules, which had just commenced to sink in. On that day, perceiving the urine to be darker than ordinary, he tested it with nitric acid for albumen, and was surprised to find that the addition of a few drops was followed by a rapid effervescence. The specimen produced was brown, rather smoky-looking, turbid, with a strong ammoniacal odour; it was examined shortly after being voided. Dr. Popham observed that he tested the urine of the same patient daily, until convalescence was fully established, but without being able to detect a recurrence of the carbonate of ammonia, and was led to suppose it was critical, occurring just at the turn of the disease. Copious deposits of the phosphates also existed. In cases wherein carbonate of ammonia is present in the urine there is found an absence of urea. Some interesting observations upon this subject are found in *Graves' Clinical Lectures*. However, this author apparently held that the carbonate of ammonia was directly secreted by the kidney; whereas

* These Reports are supplied by Dr. T. W. Belcher, Secretary to the Society.