matter was discovered in any part of the body. The following was Dr. Ferrier's report of the part sent to him.

The base of the brain (including all the structures comprehended in the circle of Willis) was involved in a semipurulent mass enclosed in meshes formed by recent adhesions. The lips of the Sylvian and longitudinal fissures were glued together, and the cortical matter softened by similar tuberculous exudation. On dissecting down and tearing through the adhesions, the circle of Willis and its branches were found to be thickened with tubercular thickenings of the adventitia, and embedded in semipurulent matter. The exudation accompanied the anterior and middle cerebral arteries in almost their whole extent. The gyrus fornicatus, the island of Reil, and the convolutions overlapping it on each side, were coated and softened with tubercular matter. The posterior cerebral arteries were similarly involved, and the exudation extended round the crura cerebri on to the superior surface of the cerebellum. The membranes on this situation were studded with miliary tubercles, especially on the right side. The blood-vessels were pervious apparently in their whole extent. The vessels of the corpora striata were not occluded; but both corpora striata were considerably congested, especially the right. The torn-out vessels were filled with fluid blood. No embola could be detected. The spinal cord was not examined.

THE TRUE AND FALSE CROUP OF FRENCH PATHOLOGISTS.

By GEORGE JOHNSON, M.D., F.R.S.,
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IN the last number of the JOURNAL, Sir John Rose Cormack reiterates his statement that the "faux croup" of the French is synonymous with our English terms "laryngismus stridulus" and "spasmodic croup". "It is," he says, "a neurose, and not an inflammation." And, on the same page of the JOURNAL, Dr. Risdon Bennett maintains that Sir John Rose Cormack is right, and that I am wrong in denying his accuracy. Let us, then, look at the evidence in proof of this position. Sir J. R. Cormack quotes several French writers; and first he gives from the *Dictionnaire de Médecine*, etc., a passage, the essential part of which I venture to translate as follows.

"STRIDULOUS LARYNGITIS, OR SPASMODIC CROUP, OR FALSE CROUP.—Stridulous laryngitis, a disease apparently bordering on croup as regards the age of the patient and the attacks of suffocation which characterise it, is only a *neurosis of the larynx grafted upon an acute laryngitis*."

Now, upon this passage I remark, that the name of the disease, "stridulous laryngitis", implies that it is an *inflammation* of the larynx; and Sir J. R. Cormack's statement, that it is a neurosis, and not an inflammation, is in direct opposition to the passage which I have placed in italics.

The second authority quoted I translate thus:

"STRIDULOUS LARYNGITIS. Syn.: False croup (Guersant); stridulous angina (Bretonneau); spasmodic laryngitis (Rilliet and Barthez). The spasm of the larynx which constitutes, properly speaking, one of the elements of stridulous laryngitis, may exist alone, and differs from it then only *in the absence of the primary or concomitant laryngeal catarrh*."

Here, again, we have the disease named and described as an inflammation of the larynx complicated with spasm, as we know that acute laryngitis—in children especially—usually is. No one surely would maintain that the spasm without the laryngeal catarrh is the same disease as catarrh with the addition of spasm.

I propose now to refer briefly to those unquestionable French authorities on this subject upon whom I rely in support of my statement that their "faux croup" is *not* identical with our spasmodic croup or laryngismus stridulus, as Sir J. R. Cormack and Dr. Bennett maintain. I suppose it will be admitted that the "true croup" of the French pathologists is by them defined as a diphtheritic membranous laryngotracheitis. It will be conceded that, upon this subject, Trousseau is as high an authority as can be quoted; and he describes "true croup" as a product of diphtheria. What, then, according to Trousseau, is "false croup"? His lecture on this subject will be found in the third volume of his *Clinical Medicine* (p. 72, New Sydenham Society's translation). The title of the lecture is, "Stridulous Laryngitis, or False Croup". Will Sir J. R. Cormack and Dr. Bennett maintain that the subject of that lecture is what we in England call spasmodic croup or laryngismus stridulus? Will they deny that the disease there described is an acute catarrhal laryngitis—what we in this country should call simple inflam-

matory croup, as distinguished from diphtheritic croup? The disease which we English call spasmodic croup, and sometimes false croup, is described by Trousseau as a neurosis in connexion with infantile convulsions, under the names of "laryngismus stridulus" or "thymic asthma" (vol. i, pp. 354-5, New Sydenham Society); and all French authorities of eminence are entirely in accord with Trousseau as to the pathology and the nomenclature of these diseases. On a disputed question of this kind, it is not usual to quote, as Sir J. R. Cormack has done, a grinder's cram-book.

Dr. Risdon Bennett quotes a passage from Guersant on "stridulous laryngitis" or "false croup", and assumes that the disease there described is laryngismus stridulus or spasmodic croup; whereas, to any one who will take the trouble to read the context, it must be obvious that Guersant is referring to simple inflammation of the larynx, which he contrasts with the specific diphtheritic membranous inflammation. Is it conceivable, that any French pathologist would describe a pure neurosis of the larynx under the name of laryngitis? Dr. Bennett's misinterpretation of the term "false croup", which Guersant states that he first proposed as a substitute for that of "the acute asthma of Millar", has probably been favoured by his erroneous belief that "Millar's asthma" is synonymous with "thymic asthma" or laryngismus stridulus". With reference to this mistake, I quote the following from Trousseau (New Sydenham Society's translation, vol. i, p. 355). "I need not remind you of the distinction between *thymic asthma* and the *acute asthma* of Millar; the latter is stridulous laryngitis, in which the spasm of the larynx which characterises it is due to an inflammation of the respiratory tract."

If, then, I am wrong in my statement that the stridulous laryngitis or false croup of French pathologists is an inflammatory disease, with or without spasmodic complication, and not a purely spasmodic laryngismus stridulus, I err in the very respectable company of Trousseau and Guersant, and I may add, of Bretonneau, whose doctrine is identical with theirs. I would ask Sir J. R. Cormack this question: If the disease which the French writers designate stridulous laryngitis or false croup be not identical with our catarrhal laryngitis or inflammatory croup, under what name do they describe our English inflammatory croup? Do they omit it entirely, as he omitted it from his proposed nomenclature of diseases allied to croup?

With regard to the relation between croup and diphtheria, Dr. Bennett says, "The absence of all reference to paralytic symptoms in the older writers is remarkable". Still more remarkable is the absence of all reference to these symptoms in the elaborate and for the most part accurate and exhaustive memoirs of Bretonneau on *Diphtheria*.

Before I conclude, I desire to point out a mistranslation which, to English readers of Trousseau's *Clinical Medicine*, may be very perplexing. In the second paragraph from the bottom of page 477, vol. ii, reference is made to "the cough of laryngismus stridulus or false croup". Here the term *laryngismus stridulus* is the translator's erroneous substitution for *laryngite striduleuse*; so that, by the mistranslation of this passage, it is made to appear that Trousseau employs the term laryngismus stridulus as a synonym for false croup. No argument is needed to show that the conversion of the French *laryngite* into the Latin *laryngismus* is unauthorised and misleading. The sound is somewhat similar; but the sense is as different as that implied in the words "inflammation" and "spasm". The passage in the original French occurs in Trousseau's lecture (xix) on Diphtheria, vol. i, at the top of page 342, 2nd edition.

A NOVEL TREATMENT OF OBSTINATE VOMITING IN PREGNANCY.

By EDWARD COPEMAN, M.D., F.R.C.P.,

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IN furtherance of the views I expressed at our last annual meeting of the Association, that *practical knowledge should be freely communicated to the profession through our JOURNAL*, and which I still urge upon the attention of our members as a valuable means of effecting improvement in the treatment of disease, I forward three cases which have occurred recently in my own personal experience, and which, if further corroborated, may open a wide field of usefulness in the treatment of a very obstinate, rebellious, and sometimes fatal form of sympathetic disorder. The disease to which I refer is urgent sickness in connection with pregnancy, which is now and then so severe as to render premature delivery necessary to save the life of the mother. During a long professional life, I have had much experience of this troublesome affection, and amongst medicines have found calumba and oxalate of cerium the most beneficial; but these and all other medicines often fail,

and the treatment suggested by the following cases, discovered by accident as it were, and never, as far as I know (although nothing is new under the sun), employed before, promises some chance of our being able with more certainty to overcome this very threatening concomitant of pregnancy.

On June 9th, 1874, I was summoned to a lady, thirty-five years of age or thereabouts, to consult with two other practitioners already in attendance. She was about six months gone in pregnancy, and was so reduced by almost incessant vomiting that great fears were entertained as to her safety. I noticed there was slight uterine action accompanying the sickness, and, on examination, I found the os uteri partially dilated so as readily to admit the finger. I thought it right under the emergency to advise bringing on labour without delay; the gentlemen present, however, expressed no little apprehension as to whether or not she would have strength to undergo the effort of parturition on account of the very depressed and exhausted state of her system. They nevertheless concurred in the advisability of the course I recommended, and asked me to perform the operation. I at once dilated the os uteri as much as I could with the finger, and could feel the membranes and the head of the child. I tried to rupture the membranes with a telescopic female catheter (the only instrument at hand), but they were so flaccid and the head offered so little resistance, the catheter shortening itself also on my making pressure, that I could not succeed; and, thinking it wise to wait awhile before resorting to any other expedient, we retired to another room for further consultation. In about an hour, we saw the patient again, and were surprised to find that a longer period had elapsed without sickness than before; and we again waited, in the hope that she might be able to take a little nourishment, and so be better prepared to undergo any further proceeding. We waited another hour, and another, but there was no return of vomiting; and we spent the rest of the night in watching, during the whole of which time she was improving, and we determined to let well alone. I left her early in the morning, and had a favourable account of her a few days afterwards. There was no return of sickness; she went on to the full period of pregnancy, was then delivered of a healthy child, and made a good recovery.

This case made a strong impression on my mind; and I wondered whether the relief to the vomiting, so urgent and threatening to her life, could have been effected by my having dilated the os uteri, and thus removed any undue tension that might be producing sympathetic irritation. It was not very long before I was called some distance into the country to consult about another case of vomiting during pregnancy of great urgency, occurring about the second month. The surgeon in attendance had adopted the best acknowledged medical treatment, and had arrived at the conclusion that artificial delivery would be necessary to save her life. With the full recollection of the former case, I examined the uterus, and found some degree of anteversion and the os patent enough to admit the end of my finger. I forthwith dilated it as much as I could, passing my finger all round, removing all puckering and making a smooth edge. She vomited only once slightly after this proceeding, and we left her with the understanding that, if the sickness continued, I should be summoned again in a few days to bring on abortion. This summons never came; but in about a fortnight I had a letter from the husband, stating that his wife began to get better an hour or two after I left, and that the sickness had entirely ceased. I have heard several times since that the patient is going on remarkably well, and I believe she expects to be confined some time this month.

A third opportunity has since offered itself for a trial of this novel (as far as I know) treatment. On the 6th of this month (April 1875), I saw, in consultation with a very intelligent country practitioner, a lady in delicate health just entering the eighth month of pregnancy. She was the mother of nine or ten children, and her life was valuable. Generally during early pregnancy, and sometimes for several months together, she had been troubled with vomiting; but during the last three weeks, the sickness had been almost incessant; she could keep nothing down, and was in a very feeble and emaciated condition. She had moreover a considerable amount of albumen and some pus in the urine, a few casts also; and fears were entertained of there being extensive kidney-disease. There was, however, no dropsy, and our opinion was somewhat modified by the knowledge that the urine does often, during pregnancy in the latter months, contain a good deal of albumen. The patient was so ill, that she would willingly have consented to artificial delivery, if really necessary. I examined the uterus, and, as in the other cases, found it patent, puckered, and dilatable, and I dilated it as much as I could with my finger in the hope that the sickness might cease after such a proceeding. I should say that the usual remedies had been carefully employed without producing the desired effect. A few days after my visit, her husband called upon me to say that his wife had no return of sickness after I left, and was now able to take food without

inconvenience, although he still thought her very weak and ill, and feared she would not recover.

On the 23rd, I received a very satisfactory letter from the surgeon in attendance, to the following effect. "I am exceedingly glad to tell you that Mrs. — was confined yesterday. I should think it is not more than an eight months' child, still it looks healthy and strong. You may most certainly add her case to the others you related to me. *There was never any urgent sickness after you dilated the os uteri*, and the last week Mrs. — has frequently taken with relish and no inconvenience solid food, such as boiled mutton, with asparagus, and drank home-brewed beer. This morning she was going on quite well; she was not even faint or at all exhausted after her labour. I am very glad I called you in, for I now know how to proceed with cases of sickness in pregnancy. Should I meet with any more patients, I will either ask you kindly to meet me again, or report them to you."

This subject appears to me of so much importance, that I send my cases for publication without waiting for further experience or attempting to explain the *modus operandi* of the treatment suggested. It is my intention to communicate further on the matter when I have more thoroughly digested it myself; and I will not fail to report any future success or failure that may come under my observation. I trust, moreover, that others who may be induced to pursue the same plan of treatment will report their experience for the guidance and instruction of the profession.

ON PUERPERAL FEVER.

By PERCY BOULTON, M.D.,
Physician to the Samaritan Hospital.

THE discussion on Puerperal Fever, which is now taking place at the meetings of the Obstetrical Society, is likely to spread over at least two more months, and even then it will be impossible for many to speak. Thus far the debate has been, I think, disappointing. The ground does not seem to have been at all cleared, and remains in much the same state of confusion as before; no sparks of fresh light have been struck, nor has any definite scheme been suggested which would be likely to lead to a result hereafter. My colleague Mr. Spencer Wells wished, I think, to elicit either definite opinion or conjecture on certain heads; but the speakers have not specially addressed themselves to these, or adhered to any plan, and the consequence is that, unless there be a genius lurking somewhere, who will come to the fore and illuminate our darkness, the debate seems likely to end where it began. For the sake of lying-in women, and also for the honour of the Obstetrical Society, it is to be hoped that some advance may be made before the subject is closed.

My own experience is so limited, that I should not have stated it, had there not been such a want of definite opinion; but perhaps I may shortly express the little I have to say. All the cases which I have seen have been in consultation, and all have been sporadic.

They have all presented the following sequence:—1. Inflamed or torn uterine sinuses or veins, followed by clot, pyæmia, and secondary congestion (phlebitis); 2. Lymphangitis, either septic, producing septicæmia, or suppurous, followed by peritonitis, as described by Dr. Tilt (*Obstetrical Transactions*, vol. xvi, page 163, etc.); 3. A combination of the two.

I have never seen puerperal fever in its epidemic form. I am convinced, however, that, whatever the cause, the result is inflammation of some of the parts employed in parturition. Is the fever or the inflammation the first step? This is a question which should be answered definitely, and I may say that my own view is that, if autogenetic, that is, caused by something local, as retained clot, or placenta, or phlebitis, or lymphangitis, the inflammation is primary and the fever secondary. In the epidemic form, probably several factors come into operation, amongst which are the following:—A state of blood peculiar to the puerperal state, *plus* all that poverty, dirt, scarcity, overcrowding, ill-ventilating, and general want of sanitary arrangement, can produce; a condition prolific of fever and of erysipelas after operation, as of puerperal fever after confinement. Hence it is most common amongst the poor and in hospitals, where several parturient women are collected, and in cold damp weather, when vitality is low. All this, in short, may be summed up in a state of blood, a state of temperature, and a state of atmosphere. The first may produce a predisposition; but either exposure to cold air, or an atmosphere charged with poison, is still necessary.

The following poisons, frequently afloat at times of puerperal epidemics, are said to produce this fever; viz., scarlet fever, measles, typhus, typhoid, erysipelas, small-pox, as also *post mortem* effluvia