Notes

ON THE PATHOLOGY AND TREATMENT OF CHOLERA.

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[Continued from page 608 of vol. i for 1865.]

In my last communication, I endeavoured to show that the effect of alcoholic stimulants and of venesection on the symptoms of collapse is such as to be quite irreconcilable with the hypothesis that the symptoms of collapse are due to a drain of liquid from the blood through the intestinal canal. I proceed now to examine the influence of other modes of treatment.

The Influence of Purgatives. If the symptoms of collapse were due to the drain of liquid from the blood, and its escape by the intestinal canal, it would seem to be impossible that the symptoms of collapse should pass away while the drain of liquid by vomiting and purging is continually going on. It would seem, too, that the action of purgatives during the stage of collapse must greatly increase the mortality. I do not here propose to consider the merits of the purgative plan of treatment. I wish only to refer to the indisputable fact that there are on record numerous well authenticated instances of recovery from extreme collapse, while the intestinal discharges were encouraged by repeated doses of emetic and purgative medicine. And, further, I challenge the advocates of the theory which I am endeavouring to refute to refer to a single case of recovery from collapse in which the intestinal discharges have not continued, in a greater or less degree, while the symptoms of collapse were passing off. If the theory in question were a true theory, the cessation of the intestinal discharges must always, and of necessity, precede recovery from collapse.

During the early part of the cholera epidemic of 1849, all the cases of cholera admitted into King's College Hospital were treated by liberal doses of brandy and opium. Under this mode of treatment, the mortality was very great. The treatment was then entirely changed; brandy and opium were discontinued; and large quantities of salt and water were administered. The effect of this treatment was to excite frequent vomiting, and certainly not to check, but rather to increase, the purging; and the result was a much larger proportion of recoveries than under the previous mode of treatment. I had no share in conducting the treatment on that occasion; but I was greatly struck by the different effects of the two opposite modes of treatment. I was also deeply impressed by observing that, during that epidemic, the arrest of the purging by opiates was in several instances followed by the worst symptoms of collapse; and a painful question arose in my mind, whether the collapse in such cases was not a direct result of the arrest of the purging.

At the commencement of the last epidemic—that of 1854—I had arrived at the conclusion that the

commonly received theory of choleraic collapse is erroneous. I had the chief charge of the hospital during the whole period of the epidemic; and I gave emetics and purgatives to all the patients who came under my care. I have since published full particulars of all my cases.* I am convinced that in many instances I gave an excessive quantity of castor-oil; yet the result was a mortality, to say the least, below the average mortality in cases of equal severity. During that epidemic, many cases of choleraic diarrhoea came under my observation—cases in which there were vomiting, bilious purging, and cramps. These were all treated by castor-oil, without opiates. They all recovered; and not one case so treated passed into collapse. Several of the medical officers, pupils, and nurses, and a considerable number of patients who were in the hospital for other diseases, had the premonitory symptoms of cholera. All were treated in the same way, and all recovered. In contrast with this most satisfactory result, stands the fact that, during the previous epidemic of 1849, several inmates of the hospital, nurses and patients, having been seized with choleraic symptoms, and being promptly treated by opiates, passed into a state of collapse and died.

I see no way in which the facts here stated can be reconciled with the hypothesis that the worst symptoms of cholera result from the loss of liquid, and that the main object of treatment is to check the vomiting and purging.

The Effect of Injecting Hot Saline Solutions into the Veins. It is well known that the injection of a hot saline fluid into the veins during the collapse of cholera has often been followed by great temporary relief. The pulse improves; the temperature rises; the countenance becomes natural; the voice recovers its strength; and, in short, all the worst symptoms speedily disappear—usually, however, to return with all their former severity within a very short time. The late Dr. Mackintosh of Edinburgh, during the summer of 1832, injected the veins of 156 patients, of whom only twenty-five recovered. There are probably but few practitioners who now expect any practical benefit from this mode of treatment—few who would consider it right to repeat this experiment. But there are many pathologists who maintain that, since all the symptoms of collapse speedily disappear after the injection of a certain quantity of liquid into the veins, this experiment proves conclusively that the symptoms which previously existed must have resulted from the loss of a liquid similar in character to that which the operation restores to the blood. I believe, however, that the true explanation of the manner in which the hot saline injections afford the surprising temporary relief which they are acknowledged to have done, has been missed; and that, rightly interpreted, the results of this experiment afford as little support to the hypothesis that collapse depends on loss of fluid, as do the effects of other modes of treatment to which reference has already been made.

In a future communication, I will give what I believe to be the true pathology of collapse; and then I hope to explain the *modus operandi* of the saline injection into the veins.

Before attempting to give what I believe to be the true interpretation of the symptoms of cholera, I have thought it desirable to direct attention to some of the facts and arguments which are opposed to the commonly received theory. There is so much of apparent probability in the theory which explains the symptoms of collapse by the loss of the watery

* On Epidemic Diarrhoea and Cholera: their Pathology and Treatment. With a Record of Cases. Longman and Co.

portion of the blood, that the practice of giving opium and other astringents to arrest the intestinal discharges will continue more or less, in spite of failure and disappointment, until it can be clearly shown that the state of collapse has an entirely different origin and cause from that which the theory in question assumes.

[To be continued.]

Original Communications.

CASE OF PUERPERAL TETANUS, FOLLOWING ABORTION AND PLUGGING OF THE VAGINA.

By JOSEPH BLACKSHAW, Esq., Stockport.

ON Saturday, November 8th, 1864, I was called to see Mrs. H., aged 48 years, the mother of a numerous family, of a highly nervous temperament, and whose general health had previously suffered from some domestic anxieties.

She was in bed, very faint from profuse hæmorrhage from the uterus. I made an examination, and detected an ovum of about ten weeks' growth within the os uteri. In consequence of the amount of the hæmorrhage, I plugged the vagina; ordered cold applications, stimulants, and the usual astringent remedies, including the ergot of rye; and she rallied in the course of the following day. The plug remained in the vagina twelve or fifteen hours, and, when removed, was not again resorted to, as the hæmorrhage had almost ceased, and the ovum was expelled a few hours afterwards. She progressed satisfactorily for about nine days, at the end of which time she was able to sit up, and about to leave her room. Thinking it unnecessary for me to continue my visits daily, I left her with directions to report to me her progress.

On the day following (Tuesday), just ten days from my first visit, I was unexpectedly requested to see her. She thought she had taken cold, and was then complaining of great stiffness of the deep-seated muscles of the neck and throat, with difficulty of swallowing, and was unable to open her mouth perfectly. As there was some febrile excitement, she was ordered to remain in bed, and to take a saline mixture with an aperient; to use hot fomentations; afterwards hot moist bran; to steam the fauces; and, if possible, to use a gargle.

On the two following days, Wednesday and Thursday, the painful tension of the masseters, as well as the muscles of the neck and throat, had so greatly increased as to prevent deglutition and bring on a state of perfect trismus. On Thursday afternoon and evening, the tetanic seizures became increasingly frequent, producing great muscular rigidity, contorsion of features, and slight opisthotonos. During the paroxysm, the pulse was small and feeble; but the consciousness was entire throughout. She continued in this state until Saturday evening, the tetanic spasms and opisthotonos gradually becoming more severe, when she died from exhaustion, five days from the first setting in of the tetanic symptoms.

Owing to the clenched state of the jaws, little was done in the way of treatment, beyond a little counter-irritation to the spine; the administering of a turpentine enema; and the removal of fecal accumulations, which might prove a possible source of spinal irritation.

Puerperal tetanus is a very rare species of that disease in this climate, and this must be my apology

for this communication. I have been in practice in this town for more than thirty years, the greater part of which I have been medical officer to a large district of a Poor-law union including the union workhouse, and must have attended three or four thousands of women in labour at different periods of utero-gestation; and this is the first case that has occurred in my public or private practice, or, so far as I can learn, in that of the oldest obstetrician either here or in Manchester, with the exception of one case mentioned by Dr. Whitehead. Dr. Radford considers it a rare disease, but has known it to occur. Mr. Robertson, of the same city, also says that obstetric tetanus is a novelty to him, and that he has never seen it during a long and laborious practice. Nor is the disease one usually recognised in treatises on midwifery, and the diseases of lying-in women. The extreme rarity and infrequency of this disease, notwithstanding the various lesions from the application of instruments, manual interference, turning, and other violence that the uterus must often sustain in protracted and difficult labours, points to a very different state of the nervous system from that which gives rise to traumatic tetanus in the various external injuries to which the body is subjected. Physiologists attribute this to the uterus receiving its nerves from the great sympathetic. It may be so. But of what that peculiarity of the nervous system consists in these cases, where the irritation from the uterus, as in the case of Mrs. H., is propagated to the cerebro-spinal system, is still shrouded in mystery. We can only say that, in the case of my patient, cold applied to the body, previously lowered by mental anxiety and loss of blood, may have given rise to a state of reflex spinal irritation, followed by tetanus; though probably, in ten thousand other instances, the same exciting causes would produce no such effects.

I regret that chloroform was not tried, as, besides the good effects it is said to have in convulsive diseases, it presents facilities of application where no internal remedy can be given.

My object in this communication is rather to seek than give information; and I trust that some of our more learned associates will ere long throw some fresh light upon this class of diseases, which are yet among the *opprobria medicorum*.

CASE OF RUPTURE OF THE UTERUS: ABDOMINAL SECTION: SUBSEQUENT PREGNANCIES.

By SAMUEL S. DYER, M.D., Ringwood.

JULY 29th, 1862, 8 P.M., I received a note from Mr. Clifton of Fordingbridge, stating that he was in attendance at a case of midwifery at Mockbeggar, midway between our respective residences; that the uterus had ruptured, and he believed the woman would die. He requested that I would meet him there at once. I went immediately, and found the patient, Mary Ann Woods, aged 37, in a state of great prostration, and evincing signs of severe shock to the nervous system. She was quite sensible; the pulse was quick and fluttering; the breathing short and hurried; the skin cold, and covered with perspiration, which stood in large drops on her face and forehead. On placing the hand on the abdomen, which was very tender, the outline of a fetus could be distinctly felt lying beneath the parietes; the uterus, contracted to the size of the foetal head, was evident; and, on examination *per vaginam*, it was clear that the uterus was emptied of what it had contained. This was the fourth labour of the woman;