pressed herself most grateful for the total absence of pain during the operation, and said she only felt the prick of the needle when the sponge was removed.—Half-past six P.M.: Has been very calm and free from pain, and has had some sleep since the operation. Feels as if the bowels would now act. Asked for a cup of cocoa, which was allowed.—Half-past ten P.M.: Bowels have comfortably moved; feels much better, and has not coughed much since the inhalation of the ether.

Thursday, twelve P.M.—Dr. Smith saw her early this morning, and ordered a dose of castor oil, which has acted three times freely. She passed a comfortable night; slept soundly; complains of a state of stiffness in the inguinal region, but no pain; abdomen distended with flatus, but not tender on pressure. To take some chicken broth *tous les mois*.

Friday, twelve P.M.—Is much better this morning; bowels were moved in the night; feels quite easy; the wound has united by the first intention, and her state is most satisfactory.

Sunday, 28th.—Much better to-day; has no tenderness in the wound, and is cheerful and free from pain. Dr. Smith ordered her some chicken for dinner.

Remarks.—This case illustrates the value of ether under a combination of circumstances apparently the most unfavourable for its success:—

1. The age of the lady, her extreme sensibility to pain, and previous debility, rendered the operation for hernia one of extreme danger under any circumstances: the etherial vapour presented that shock to the nervous system which, without this valuable agent, would in all probability have proved fatal to our patient.

2. The presence of bronchitis, with spasmodic cough, might appear, to those unacquainted with the power of ether vapour as a general sedative, to have excluded it from the class of cases in which its inhalation was admissible. The result, however, shows the fallacy of *à priori* reasoning in a science of observation and experiment like practical medicine.

3. The tedious nature of the operation, and the length of time which the sentient and excito-motory nervous systems were under the influence of this agent, without producing any unpleasant effect when the sponge was removed, leads to the conclusion, that the brain is not always in that state of congestion which some have supposed—that the primary influence of the vapour of ether is upon the expansion of the pneumogastric nerves in the lungs, and by them conveyed to the medulla spinalis. In some of the cases observed by Dr. Smith and myself, the cerebral functions were more or less active. Some patients, as Mr. H—, state that they were perfectly conscious. The nerves of the higher senses, the optic and auditory, were affected by light and sound at the time that common sensation and voluntary motion were suspended. The ganglionic system of nerves is not influenced by the etherial vapour, as proved by the regular action of the heart after the first shock has passed off, and by the vermicular contraction of the muscular coat of the intestine as it lay before me during the dissection, the fibres of which were aroused to increased contraction every time the director or back of the bistoury touched the peritonæal covering. I cannot conclude the history of this instructive case without bearing my humble testimony to the great value of Dr. Smith's method of administering the vapour of ether: its simplicity, efficacy, and superiority over the apparatus recommended, will be admitted after a trial; the patient and the agent are so completely under the command of the operator, that the effect is produced as by a charm. The porous canals of the sponge present an extensive surface for the evaporation of the ether; they, at the same time, allow of the ingress of atmospheric air for the support of the respiratory function, and the escape of the carbonic acid gas given off from the lungs, so that danger of asphyxia from that cause is effectually prevented.

Exeter-place, Cheltenham, March, 1847.

A CASE OF

DIFFICULT LABOUR FROM CONTRACTED PELVIS, COMPLICATED WITH HÆMORRHAGE, AND SHORTENED FUNIS.

By FRANCIS McEVOY, Esq., Surgeon, Balbriggan.

In the capacity of dispensary surgeon I was required to see a young woman, twenty-eight years of age, who had been taken in labour of her third child sixty-five hours previous to my seeing her.

During the first thirty hours, labour was reported as natural; the waters were discharged, the head had advanced

into the pelvis, and rested in the hollow of the sacrum. For the last thirty-five hours the pains gradually decreased, and finally disappeared altogether. She was much weakened and exhausted by her previous sufferings; she had some sleep for the last six or eight hours prior to my visit, which greatly refreshed her.

She gave me to understand that her two former accouchements were tedious, the first was of four, the latter of three days' duration. Complained much of a dragging pain, referrible to the right side of abdomen. Has come to her full time on this occasion, but did not with the two former children; with the first she wanted two months, the other six weeks, of her full time.

On examination, I found the pelvic outlet narrowed, the presentation natural; head well down in the pelvis, but small, and had not made the turn; the ear could not be felt; parts well prepared. Had not passed water for twenty-four hours; the catheter was with difficulty introduced into the bladder, and about two quarts of high-coloured urine drawn off. Could not detect the pulsation of the foetal heart with the stethoscope. It occurred to me, that prior to proceeding to any more decided steps, and as she had previously, by her unassisted efforts, borne two children, although they were premature, that some attempt should be made to reproduce the uterine action; I therefore determined to administer ergot of rye, in ten-grain doses, every fifteen minutes. In about an hour the uterine action was brought on, which, though strong, produced no impression whatever on the head. Passing my hand over the abdomen, the uterus appeared to contract sufficiently, with much pain.

On a more careful examination, there appeared a greater degree of narrowing than I at first imagined. I found the os coccygis ossified, and firmly adherent to the sacrum, and projecting anteriorly. The outlet of pelvis narrowed transversely, by reason of the right ischium being inclined towards the opposite side of the pelvic cavity. When questioned, stated she heard her mother say that she had had rickets when a child, but no other evidence of this affection was perceptible. Complained much of the dragging sensation before mentioned. Suddenly, (within six hours after the re-commencement of the uterine efforts,) whilst speaking to me, she was seized with a discharge of blood from vagina, accompanying each pain; became rapidly faint and exhausted, throwing her arms about, exclaiming she was dying. Face pale and anxious, covered with a clammy sweat; lips bloodless; surface and extremities cold; pulse scarcely perceptible at the wrist; pains diminishing.

I now saw that further delay on my part would be criminal, therefore I had recourse to the perforator, and delivered her, with the crochet, of a very small child, and simultaneously with the child the placenta came away, with a great gush of blood, the funis not being more than twelve inches and a half, which was twisted once round the neck; the uterus contracted well.

Notwithstanding her weakened and debilitated state, for the two or three following days, resulting from the hæmorrhage and the forced delivery, this woman's convalescence was rapid in the extreme, without even one unfavourable symptom, and was engaged in her domestic pursuits within a fortnight.

Remarks.—The foregoing complicated case exhibited some peculiar symptoms, in a practical point of view, worthy of remark.

- 1st. The narrowing of the pelvis, which, being altered in capacity and direction, the distance from the inside of one tuberosity to the other, appeared to have been about two inches and three quarters or three inches, exclusive of the soft parts.

- 2ndly. The ossification of the coccyx at right angles with the sacrum, the distance of apex of the coccyx from the angle at symphysis pubis, about three inches or three inches and a quarter, so that the tuberosity of the ischium, and the coccyx formed a complete ridge or ledge, which opposed the further progress of the head, and prevented the completion of parturition. Of course, the measurements here stated cannot be affirmed to be perfectly accurate, as I had no pelvimeter available, and they merely give my impressions at the moment of examination.

- 3rdly. The length of funis, twelve inches and a half, and being twisted once round the neck; could this be the cause of the dragging sensation she complained of?

- 4thly. The separation of the placenta from the uterus. Query—Was this caused by the renewed action of the womb, or by the funis being shortened, in consequence of taking one turn round the neck, and so acting mechanically? But perhaps this is of very little importance to the accoucheur.

This patient was evidently of scrofulous diathesis, and was externally marked in the neck; her habits of life, which were sedentary, tended to produce and confirm the deformity of pelvis, as she was, in her childhood, a bobbin-winder; in her girlhood, a weaver; and since her marriage, had been occupied in domestic duties; she however stated, that she enjoyed excellent health.

She left this neighbourhood a short time after her confinement, became again pregnant at the expiration of nine months, and died in labour. I regret much being unable to place the circumstances attending her last accouchement before the profession.

Balbriggan, March, 1847.

Reviews.

Rapport à l'Académie Royale de Médecine sur la Peste et les Quarantaines, fait, au nom d'une Commission, par M. le Dr. PRUS. Accompagné de pièces et documents, et suivi de la discussion dans le sein de l'Académie. pp. 1056. A Paris: chez J. B. Baillière, &c. 1846.

Report to the Royal Academy of Medicine on the Plague and Quarantines, edited, on behalf of the Commission, by Dr. PRUS. Accompanied by documents and papers on the subject, and followed by the discussion in the Academy, &c.

(Continued from p. 152.)

THE third portion of the Report of the Commission is devoted to the practical and important question of the infectious character of the plague. In the first place, what grounds have we for believing that individual contagion assists its fatal progress in its epidemic foci? And, secondly, what are the laws of its transmission to other localities? We will first briefly examine the circumstances which mark its progress in its indigenous seats, premising that all the facts must be received with due consideration to the existence of a common cause of disease, so overwhelmingly powerful, that cases which might justly be held conclusive arguments for the doctrine of contagion, become exceedingly doubtful.

1. The first mode of communication examined by the Commission is inoculation with the blood, serum, or pus of the infected. Passing over the various apocryphal narrations which form the staple of many historians of this disease, we proceed to relate the more accurate experiments of the French physicians, on criminals placed in their hands for that purpose. We make no remarks on the assumption of the office of the executioner by our French brethren. The heathen physician, Celsus, condemned those medical men, who, in his time, sanctioned by their practice the vivisections of notorious criminals, for the sake of scientific investigation. Not less powerfully would the censure apply to those modern investigators who introduced the blood and pus of plague patients into the miserable Egyptians. The experiments are as follows:—

"The 15th April, 1835," says M. Lachéze, "five criminals, condemned to death were taken from the citadel of Cairo to the hospital of Esbekii, for the purpose of resolving the question of the transmissibility of the plague by inoculation.

"The 18th of April," says our honourable colleague, "I took some blood on a lancet, as it escaped from the arm of a plague patient, and introduced it in two places under the epidermis of the inside of the arm of a criminal.

"The 21st of April he had a confirmed attack of plague, which Clot Bey incorrectly maintained to be doubtful in its character.

"The 26th the patient was recovering.

"The 22nd of April, 1835, M. Lachéze again inoculated with blood. No result followed.

"The 30th of April, the same experiment was renewed on another patient, with similar immunity from subsequent symptoms. Lastly, a young criminal (aged eighteen, who had slight symptoms of the disease after sleeping, on the preceding 15th of April, in a bed recently occupied by one suffering from a severe attack of plague) was inoculated on the 13th of May with some blood from an infected party. No result ensued.

"The same individual had, eight days previously, undergone, with the same impunity, an inoculation with serum,

taken from the vesicle arising on the carbuncle of a plague patient.

"Another criminal (aged sixteen years) was inoculated with the same fluid, and with a similar negative result.

"On the 30th of April, the same individual was inoculated in the right axilla and groin, with pus from a recently opened pestilential bubo. No ill effects resulted.

"Thus, out of four inoculated with blood from infected parties, only one suffered from plague, and that in its mildest form. The two inoculated with serum from a pestilential carbuncle, and one with recent pus, had no symptoms whatever."

In addition, it must be observed that the criminal, who, after inoculation, had the attack of plague, (the single affirmative result of the whole series of experiments,) was exposed, like all the other inhabitants of Cairo, to a common epidemic influence. He had been located for three days in an hospital which contained a great number of patients suffering from the disease—an hospital, in fact, which eventually became a focus of the pestilence. We may therefore reasonably doubt the cause of the symptoms which he exhibited, as he was surrounded by many recognised causes of infection.

After detailing some other experiments with an equally negative result, the following is the conclusion of the Commission:—

"Inoculation with the blood from a plague patient, or with pus from a pestilential bubo, have only furnished equivocal results; and inoculation with the fluid from the vesicle of a pestilential carbuncle has in no case been followed by any symptoms of plague. It is not, therefore, proved that the plague can be transmitted by inoculation, even when an epidemic of the disease is prevailing."

2. The second question propounded by the Commission is—"Can the plague be transmitted in the epidemic foci by the mere contact of the infected?" Those who have perused the narrations describing the monstrous power assigned by fear and imagination to the very touch of the infected, will hope, for the sake of its practical results, for a negative reply to this query. In obedience to the terror-stricken fancy of the physician and attendants, the unhappy patient was confined in a species of cage, and frequently died from absolute starvation. The buboes, when opened at all, were operated on with an instrument fixed at the end of a long pincers. Sometimes they were inefficiently opened; at other times, the trembling operator pushed the weapon into dangerous proximity with the great vessels of the axilla or groin. The absurdities of the speculator, considered *per se*, may only excite a smile; but very far from harmless is the practical application of his unauthorized conclusions. The physician arms ignorance with a firm belief, inspiring terrors of the most fearful description; and therefore he is to blame, if, under his auspices, the miserable victim of the plague perishes with all the aggravation of his evils which starvation and neglect can effect. We have been principally led into this digression by a conviction, which becomes more certain by every day's experience, that hypothetical errors in medicine, whether suggested by fear, as in the present instance, or, as is more commonly the case, arising from personal vanity, are sure to bear an evil harvest, abounding in obstacles to the progress of truth, and impeding the career of human amelioration.

To return to our subject. In the middle of the sixteenth century, Frascator, a celebrated physician of Verona, taught, "that the plague might be communicated in three ways:—1st. The most frequent cause of infection is distinct contact with the infected. 2nd. Next in frequency is infection by wearing apparel, furniture, &c. 3rd. The atmosphere is sometimes, though rarely, a medium of infection." These opinions, opposed as they were to the doctrines of the Greek and Arabian physicians, were nevertheless generally held by medical men till the year 1835, when a great number of European physicians had an opportunity of observing the destructive pestilence which reigned during that year in Egypt. The result of their experience was diametrically opposed to the prevailing opinions. Clot Bey observes,