

examination, and partly for the purpose of diagnosis, (as there was some supposition of exaggeration on her part,) it appeared to me desirable to remove consciousness and volition, and to accomplish this I suggested the use of chloroform. It was accordingly inhaled, and we were somewhat surprised to find that both limbs became perfectly flaccid, and could be moved at pleasure into any position that we chose. When the limbs were in this perfectly relaxed condition, reflex actions were readily obtained by the application of a heated plate to the soles of the feet. As consciousness returned, the spasm recommenced, and it did so before it could be asserted that distinct volition was exercised. The patient made inarticulate sounds upon being pinched; but could not, or at all events did not, make any attempt to answer questions, or to change the position of the limb. A second quantity of chloroform was then given, sufficient to relax the muscles, and the legs were placed in such a position that extension could not take place. Chloroform has not been inhaled again, as from some cause the muscular rigidity has not returned to anything like the same extent, and the patient now moves about with the aid of a crutch. There is some loss of power in both the legs, and loss of sensation in the right, but there is no spasm. To the present state of the patient and its diagnosis it is not my intention to refer. The only point to which I wish to direct attention is the effect of chloroform upon tonic spasm of such prolonged duration.

My friend Mr. Squire had under his care a most obstinate case of "hysterical knee." The leg had been kept at an acute angle for five years, and had resisted all attempts at extension. It was thought, from the firmness with which it retained this position, that at least ligamentous ankylosis must have taken place. The inhalation of chloroform proved, however, that such was not the case; for when consciousness and volition were in abeyance, the joint became perfectly flaccid, and the leg could be extended completely.

From these and some other cases, I am disposed to think that chloroform may be of much service in the examination, diagnosis, and relief of certain forms of spasm. It is a great assistance, sometimes, to remove volition, sensation, idea, and emotion from the field of investigation. Some spasms appear to be placed in curious relation with one or all of these centres of motor impulse, and to be, to a certain extent, dependent upon them. It is, perhaps, more probable that volition itself is in a morbid state, than that any intentional deception is practised. Ideas, as well as muscular movements, may be grouped abnormally. Sensations and emotions, in their exaggerated form, may be as real (so far as the patient, as a moral being, is concerned) as in any other case; for the absolute force of their external cause is but one of many concurring influences for the regulation of their intensity. There is a tendency to the grouping of muscular contractions; and by their frequent association, habits (of physiologic and pathologic character) are formed. It may be that, in the breaking of these habits, chloroform will prove of service.

The inhalation of an anæsthetic agent can do little more than relieve the symptoms, when there is, underlying them, some organic disease; but in other cases, where the concatenation of such phenomena constitutes the whole disease, (or the whole which we are able yet to recognise,) it is possible that much more may be accomplished. For whilst in the former class we can only prevent some of the effects of morbid processes which are themselves beyond our reach, in the latter class we may remove the whole. Pathology has not yet advanced to the perfect discrimination of these two classes of disease; but there are grounds for hope that by the discovery and careful application of some experimental processes, we may be able to place the phenomena of nervous disorders among the class of objective symptoms. Were this accomplished, we should have the same kind, and perhaps eventually the same degree of certainty, in their diagnosis, that we have now with regard to diseases of the chest.

Grosvenor-street, Oct. 1853.

A CASE OF LITHOTOMY.

By ARTHUR J. CUMMING, Esq., Surgeon, Exeter.

In July last I was requested to see a child, aged four years and a half, the son of Captain P—, residing at S— Street, Exeter. He was suffering very severely from all the usual symptoms of stone in the bladder, and I was informed by his mother that for the last eighteen months his sufferings had gradually increased in intensity, so as almost entirely to confine him to the house. It appeared that on more than one occasion a small calculus, the size of a split-pea, had been

voided, and that he had at various times been under medical treatment. Having communicated to his parents my strong suspicion of the nature of his case, I proceeded in the early part of September to examine the bladder, and immediately detected the presence of a stone, apparently a small one.

On September 21st, having for a few days previously restricted him to a milk diet, I proceeded to perform the usual lateral operation, in which I was assisted by my friend, Mr. F. H. Warren. The boy was, however, in the first place, before any other step was taken, and whilst yet unsuspectingly lying in his mother's arms, brought under the influence of chloroform, administered on a handkerchief, and in this state was placed on a table and secured in the usual manner.

The staff having been introduced, I succeeded in cutting into its groove at the first incision, and subsequently employed a narrow-beaked knife to complete the first part of the operation. A calculus, weighing nearly three drachms, and composed principally of lithic acid, was readily extracted, and with my finger I ascertained that another, a small one, still remained, which I soon removed.

No untoward symptom of any kind occurred during the recovery of my little patient, and after the lapse of seventeen days the wound had entirely healed, and the child had begun to acquire a buoyancy of spirits and health to which he had been long a stranger.

I do not report this case as having any extraordinary or peculiar points, but rather to afford an additional evidence of the extreme value—the great blessing—of an agent which, whilst it renders the patient insensible to the pain otherwise inevitably attendant on every surgical operation, at the same time in great measure prevents that shock to the nervous system, frequently of itself so fatal, especially to young subjects. There is yet one other point, and not an unimportant one, that the feelings of the operator are not only spared, but in the absence of struggle or loud expression of agony he is happily enabled the better and more easily to fulfil his object.

Exeter, Oct. 1853.

ON AN INSTANCE OF TRIPLETS.

By J. LARDNER GREEN, M.R.C.S.E. & L.A.C.

ON the 13th of March, 1851, I was requested by her husband to visit Mrs. D—, who was ill, as he stated, with bowel complaint, remarking incidentally that as his wife was advanced in pregnancy, the otherwise trivial complaint required professional attendance. At two P.M. I found my patient reclining upon the bed, and, in answer to my inquiries, stated that she had experienced griping pains in the bowels and looseness since the preceding evening; that she was six months advanced in pregnancy, but was sure the pains she felt were in no way connected with that condition, and that her complaint was that of the bowels only. He acknowledged, however, she had not had frequent alvine evacuations during the night. On closely watching her, I found the pains came on in distinct exacerbations every few minutes, with intervals of ease. These, with other circumstances, led me to suspect that labour might at least be approaching; on therefore making an examination I found the soft parts relaxed; the os uteri low down, and dilated to the size of a half-crown piece, the membranes entire; a feet presentation, but unusually *masked* and indistinct. The membrane soon gave way, with escape of liquor amnii, when another bag of waters protruded from the os, and the presentation became more distinct; this, having given way, was immediately followed by the birth of the first (male) infant, feet foremost, at half-past three P.M. I next detected a bag of waters with the head presentation; as the pains were energetic, the second child, a female, was born in about twenty minutes after. Before another pain came on, I found a head presentation, but no bag of waters; the child, a male, was born in about ten minutes. The uterus, at first rather lax, by firmly grasping the hand over the abdominal parietes and a tight binder, contracted well. The placenta, a large triple one united at the edges, was readily removed at the usual time. The mother was much exhausted. I gave an opiate draught, and repeated it in four hours. The labour terminated at about half-past four P.M. The patient quickly rallied, and ultimately recovered perfectly. The three children, two males and a female, presented the characteristics of the sixth month of intra-uterine life. All died within an hour of their birth.

There are two curious particulars connected with this case: the first, that the bag of membranes of the infant last born should have over- (or, rather, under-) lapped the bag of waters of the child born first,—as the presentation of the third child

was not observed to be preceded by any bag of liquor amnii, it is presumed that a portion of it first gave way, and before the birth of the first-born child; the second, that a patient who had been twice confined should be in labour of triplets for several hours, all the while considering herself to be suffering from bowel complaint only, for there could be no doubt whatever of the patient's veracity. This incident may be of some value in a medico-legal point of view, and as such I consider it worth recording.

Bristol, Oct. 1853.

Medical Societies.

MEDICAL SOCIETY OF LONDON.

MONDAY, OCT. 24, 1853.—FORBES WINSLOW, M.D., D.C.L., President.

Dr. ROUTH read to the Society a memoir from Dr. Bury, D.M.P.,

ON THE PRESERVATIVE POWER OF COPPER IN CHOLERA.

Dr. Bury is the author of a work on "Metallo-Therapica," and it was in carrying out experiments on this subject that his attention was first drawn to this influence of copper. He first related a case of cholera, which occurred in the Hôpital Cochin, in which the external application of a copper band arrested the cramps, and that unmistakably, as on the removal of the copper plates the cramps re-occurred. He found subsequently, on inquiry, that workers in copper and brass were remarkably exempt from cholera. This was the result of a careful and personal investigation in nearly all the metallic workshops in Paris in which from 100 to 600 (and over) workmen were employed. The individual workshops were given by name. In all these the mortality never exceeded 5 in 1000, and in many it was actually null; and this in the midst of a neighbourhood in which the population was decimated. Indeed, in many cases the wives suffered, while the workmen escaped. Dr. Bury was not satisfied with this result, but investigated the matter in other countries—Sweden, Vienna, Russia, Turkey, and England. In all the same exemption was observed. In many cases also, it was remarked, removal to a copper mine, or even district, arrested choleraic symptoms. The exemption of Birmingham, Sheffield, &c., he explained in this manner. The individual cases so tested amounted, in round numbers, to about 300,000. A review of all these facts led him to conclude,—1st. That nearly all metals with strong electric affinities were in different degrees preservative; but of all these copper and steel were most so, the mortality being almost null among workmen engaged in working these two metals. 2ndly. With a view of resisting cholera, he recommended the presence of copper and steel ornaments in rooms, as also plates worn next the skin on the body. 3rdly. In the treatment of cholera, their internal administration, especially copper, in powder in the metallic state, as also application externally of plates.* These plates he calls armatures; these he prefers to the salts of metals. The memoir concluded with an attempt to explain these phenomena, which he did not believe depended on any electric or galvanic influences; indeed, these he considered rather noxious than otherwise. He suggested—1, that, as in the production of ozone by phosphorus, an atmosphere affected by copper might be so modified that the cholera poison could not exist in it; 2, it might be due to minute absorption of oxide of copper by the skin and lungs, acting in a similar manner upon the system.

Mr. RICHARDSON related the particulars of a case of cancer of the uterus, which appeared to have been produced by inoculation, the husband having died five years previously from cancer of the penis.

Dr. SIBSON read a paper

ON PERICARDITIS.

A well-marked friction-sound, whether resulting from rubbing or creaking, once heard, can be recognised again with precision and certainty; but in the early stage, and in some peculiar cases, the best observers cannot say whether the noise be exocardial or endocardial. Of the forty cases observed and recorded by Dr. Taylor with care and good faith, in eight only was distinct friction-sound recognised at the beginning of the attack. In seventeen it was doubtful whether a bellows- or a friction-sound was heard, although subsequently the *frottement* in most was unequivocal; in four a bellows-sound was replaced

by a *frottement*. (One case was not analyzed.) Dr. Stokes has observed that after the application of leeches the friction has given place to a bellows-sound. Dr. Mayne, Dr. Law, Dr. Graves, Dr. Williams, Dr. Hope, and Dr. Walshe, all agree that while the exocardial murmur is in its very character usually distinct from the endocardial, yet that in some cases, and especially in the early stages, either of the sounds may be mistaken the one for the other. Dr. Watson and Dr. Latham claim the power to discriminate between the two classes of murmurs, yet even they admit that in some cases the two sounds are alike. Skoda, as rendered by Dr. Markham, says "that there is no kind of endocardial murmur, with the exception of the whistling which may not be imitated by a friction-sound of the pericardium, and no pericardial murmur which may not resemble an endocardial murmur." Here Skoda perhaps goes too far; but it is evident that other tests, besides the difference of the two murmurs, are required to distinguish between pericarditis and endocarditis in the early stages, when the discrimination of the disease is so important. The principal test proposed by Dr. Sibson is the employment of pressure. If a friction-sound exists, that sound invariably becomes louder and harsher by making moderate pressure with the end of the stethoscope, so as to bring the surfaces closer together. In the earliest stage pressure will often excite a friction-sound when none was previously present. In the last case of pericarditis under Dr. Sibson's care in St. Mary's Hospital, pressure brought out a sound like a bellows murmur. A few leeches were applied. Next day there was a loud to-and-fro *frottement*. In the advanced stages, when the effusion of fluid by separating the two surfaces, extinguishes the sound, pressure, by bringing the surfaces again in contact, will restore the lost friction-sound. The rationale of the effect of pressure in exciting and augmenting *frottement* is self-evident. An interesting illustration of it presented itself in a patient of Dr. Sibson's in St. Mary's Hospital the other day. A female affected with phthisis had ascites. On displacing the fluid, and pressing on the liver, a to-and-fro thrill was felt under the finger; as was anticipated, a loud, harsh, double, creaking noise during inspiration and expiration was heard on making pressure with the end of the stethoscope. On withdrawing the pressure the sound ceased. After death the surface of the liver was found to be roughened. The first patient in whom Dr. Sibson observed the effect of pressure in reinforcing *frottement* was a female affected with pericarditis, in the Nottingham Hospital, on the 25th of November, 1838. Since that period he has recognised this important sign in every case of pericarditis he has examined. He gave an account of it in his paper on the "Position of the Internal Organs," in the *Provincial Medical Transactions* for 1844. This effect of pressure is rarely excitable in men after the age of forty-five, the cartilages being then firm and unyielding. The only point to guard against is the possibility of exciting anæmic murmur in very young persons. The anatomical position of the *frottement* ought to be noted. Dr. Stokes found that friction-sound is audible generally only over the region of the heart, while endocardial murmurs are transmitted beyond that region. This rule holds good in the early stages, and while effusion is present; but when the fluid disappears, and the surfaces come in close contact, the noise may sometimes be heard extensively over the front of the chest. The *frottement* differs in many cases over different parts of the heart. To the right of the lower sternum, over the right auricle, the friction-sound is usually double, soft, and equally loud during systole and diastole; to the left of the sternum, over the right ventricle, it is also double, but harsher, especially during systole; over the apex it is generally only systolic. The form of the region of pericardial dulness is important, being conical, the point upwards reaching to the top of the sternum, as Corvisart pointed out. When the effusion is great, the heart is often raised, so that the impulse is felt, and the *frottement* and heart in the second and third spaces instead of the fifth. This Dr. Sibson pointed out in the paper referred to. Dr. Latham observes that at this stage not only is the impulse raised, but the thrill felt by the hand is raised also. Dr. Walshe in his valuable work has noticed the tilting upwards, during effusion, of the impulse of the apex from the fifth to the fourth intercostal space. Adhesions usually begin to form over the outer edge of the right and left ventricles. If so, the *frottement* ceases there, while it remains over the base; the region of *frottement* with the advancing adhesion gradually narrows to the middle of the sternum, and at length, when adhesions are complete, the friction-sound suddenly becomes inaudible. If adhesions do not take place, the friction-sound may remain long after the pericarditis has ceased. If a patient is seen at this stage for the first time, care must be taken not to treat him as if he

* These plates may be had of Mr. Weiss, Strand.