to bear upon them. If this preparation be incomplete—i.e., if the os be still dense and indisposed to yield, the exhibition of ergot will be dangerous. Time alone, under ordinary circumstances, time and opium where there is much irritability, are the indications where, on the other hand, the os uteri has been so far relaxed as to require for its expansion the lightest touch only of the fingers or the head. The first stage may, in fact, be regarded as complete, and a dose of ergot sometimes be admissible, with a view to accelerating the commencement of the second.

The doctor then proceeds to recommend its employment in the three following cases, where we have retained placenta, from inaction, irregular contraction, or morbid adhesions. Simple inaction is so readily overcome, after a few minutes' rest, by a slight traction of the cord and titillation of the os, with one hand; and a simultaneous but gentle compression of the uterus, with the other; that no possible necessity appears to exist for the introduction of a different practice. If the inaction depend upon sluggishness, we supply in this way, what alone the ergot can give—a stimulus—if, upon exhaustion, we afford likewise assistance and support. The employment of the hand, too, has another decided advantage over that of the drug, inasmuch as the amount of interference may be, and is, adjusted to the requirements of the case—from the simple ascertaining that " all is right" to the active removal and firm compression required in hæmorrhage. The ergot will most likely do nothing; but it may inflict many hours' unnecessary pain. I do not say that in these cases, hæmorrhagic or otherwise, it should not be made use of as an adjunct; on the contrary, I very frequently employ it myself; but I do submit that the man is foolish who relies upon it much, and culpable who trusts it alone. Dr. M'Gregor appears to overrate, and that considerably, the pain and ineligibility of the manual process, a portion of which must be gone through, even if we employ successfully the ergot of rye. The custom of awaiting for three hours the natural expulsion of the secundines is, too, so singularly foreign to the ideas and practice of myself and medical neighbours, that we really seek in amazement some proofs of its propriety or advantage—a quest assuredly not satisfied by the cases of Dr. M'Gregor, wherein it appears that his time and the woman's blood were alike sacrificed, simply for the purpose of employing, after the lapse of some hours, means equally available in the first instance.

The question of hour-glass and irregular contraction is one of much importance, from its probable connexion with many cases of severe hæmorrhage, and unfortunately requires for its discussion a larger amount of experience and observation than I can bring to bear upon the subject.

Any amount of spasm occurring so as to retain and incarcerate the whole placenta, or even the greater portion of it, appears to be always located in the os and cervix; and I have observed it repeatedly in connexion with a certain series of antecedents, in this way. There will be, in the first instance—i. e., in the first stage, an unusual amount of substance, tonicity, and resistance about the os and cervix; the liquor amnii is discharged early, from the natural activity of the uterus; the employment of ergot; the interference of the accoucheur; pushing up the edges of the os over the head, and so forth; or from some similar cause the head at last enters the aperture suddenly, and the os becomes impalpable; —here ensues a little delay. The pains are active, the passage is free; but the advance of the head is not proportionate, because it is gripped from the chin round to the occiput by the lower fibres of the uterus, which in this way counteract the pressure of its superior portion. Presently the head does sweep the pelvis, and is again delayed at the os externum, not from any actual impediment in this situation, but from a transference of the stricture to the body and shoulders. Shortly, however, with slight assistance, the child is expelled *per saltum,* the cord appearing to be very short. In a few minutes, unless prepared by these consecutive phenomena, the accoucheur, on attempting to glide his fingers up the cord, is astonished to find all access to the placenta cut off by firm annular contraction. How, then, is he to proceed? If he can insinuate two or three fingers, let him do it, (in any case, I think, at once—assuredly so if there be hæmorrhage,) keep up a dilating effort, disregard the root of placenta, and lodge some portion of the edge upon the palmar surface of his fingers; with a little coaxing of this description you can extrude the placenta by pressure from without as you would the contents of an atheromatous tumour. This is always a proceeding of some pain and difficulty, but in my little experience—half-a-dozen cases—it has never proved so to any remarkable extent.

Many other points might deserve mention, but I merely allude to, without pretending to describe, the execution of this particular office. Once in my hands, when used experimentally, the ergot proved a signal failure. After a sequence of preliminaries as above described, occurring in a tradesman's wife, I found myself confronted by a very close, convulsive—i. e., irritable—stricture, pretty high up, without hæmorrhage, and no portion of the placenta to be touched. The pains being unusually regular and active, I determined to see what half an hour would bring forth; this proved to be nothing. I then administered a full dose of secale cornutum, which induced, during the next half-hour, severe and almost unceasing uterine efforts; finding, still, placenta, stricture, &c., precisely *in statu quo*, I gave nearly a drachm of laudanum, (this was just prior to the appearance of chloroform upon the stage,) and with extra caution, the fibres being irritable, soon employed successfully the ordinary manual method. What effect a repetition of the ergot might have had in a few hours, or the next day, I am not prepared to determine; if you employ patience as a vehicle for your medicine, opium doubtless deserves the preference.

There is another form of partial contraction incident to the uterus not so readily recognised by vaginal examination, but manifesting itself as a lobulated, uneven surface, to be felt above the pubes, instead of the ordinary globose figure of the uterus. To this condition, or a parallel one, I presume Dr. M'Gregor alludes. Gentle moulding and steady compression appear to be *the* remedies. There seems some ground for the apprehension that an early exhibition of ergot in such a case would be quite as likely to irritate and keep up the already existing local spasms as it would be to make them "merge into a general contraction of the entire substance." First model and reduce the uterus, then promote its firmness and further diminution by ergot if you like.

Upon the subject of morbid adhesions, again, much already said might be repeated, when really nothing but the skilful and deliberate employment of the fingers, or, failing this, the solvent action of incipient putridity, can avail us. Irritation, compression, and ergot, may assist, and ought to be employed; on the whole, adhesion of placenta is a condition much more frequently spoken of than met with. I have never seen it except to a very limited extent, involving, perhaps, one cotyledon; and Dr. M'Gregor must pardon my suggesting a doubt as to the nature of his case No. 1, which was probably one of partial separation without adhesion at all.

As an epitome, then, of the claims of ergot, I think we may venture to assert, that in every six instances of its employment it will be in two needless, and in two useless; in one more, perhaps efficient only as an adjuvant, or by performing for us what we might equally well have performed without it. Its capability of acting appears to diminish in a direct ratio with the increase of necessity for its doing so; when the uterus is acting, or disposed to act, this drug will frequently augment its efforts; when the organ is thoroughly inert, the drug appears to be so likewise. Let the womb be lax, the functions failing, blood flowing, and woman fainting, and ergot will almost never stand you in stead. After other, and far more energetic, treatment has fairly arrested the chariot of death, it is a satisfaction, and may be an advantage, to maintain contraction by a full dose, to induce the uterus, in this way, by progressive efforts, to withstand the impulse of the rising circulation.

These very thoughts have often occurred to me with reference to another expedient, eloquently advocated by Dr. Rigby, —application of the baby to the breast; once get an urgent case, in which the resource would be really valuable, and you are sure to find the infant dead or refusing to suck; the breasts inacessible for stays and envelopes; the woman insensible to mental emotions, or the uterus not susceptible of its wonted sympathy. In fine, both plans are striking and feasible enough to enunciate, but, in too many instances, like Mokanna's miracles, they lack to be true.

Brenchley, Kent, Sept. 1849.

COMMUNICATIONS ON THE TREATMENT OF CHOLERA.

On the Treatment by Repeated Doses of Calomel.

By J. H. Nankivell, Esq., M.R.C.S., St. Columb, Cornwall.

When cholera first appeared in this county in 1832, I had an opportunity o seeing about forty cases in the practice of Mr. Cornish, at Falmouth, and he very soon found that calo-

mel, in small and frequently-repeated doses, was the only medicine which appeared to have any influence in controlling and arresting the disease; indeed, so manifest was the advantage derived from the persistent administration of this drug, that he directed his assistants to remain with the patients as much as possible, and to give, every quarter of an hour, two or three grains of calomel; and thus relieving guard night and day, they frequently had the high gratification of witnessing, that as soon as the gums became a little sore, the patient was safe to make a rapid recovery. It is true that many died in four, five, or six hours, and others in a longer period; but certainly not one in whom the mercurial had produced its characteristic effect. Now my object in sending these few hastily-written lines is by no means to detract from the credit justly due to Dr. Ayre, of Hull, for the efficient and successful manner in which he has, from 1832 to the present time, carried out a similar practice; on the contrary, I believe him to be entitled to the utmost respect for the manly and unaffected recital which he has given in your pages of the results of his experience; all I would maintain is, that there is really no novelty or exclusive originality in this practice, and that seventeen years ago, my respected teacher, Mr. Cornish, did, from independent thought and reflection, become convinced that calomel was the medicine, *par excellence*, in cholera, and that all other drugs, in point of efficiency, were separated from it, *longo intervallo*. In conclusion, I may be allowed, as a humble unit in the profession, to express my hearty assent to this dogma.

(*Letter from Dr. Badeley, Chelmsford.*)

Sir,—I have read most of the communications made by various medical practitioners to your valuable journal, respecting the treatment of cholera; and that which has been suggested by Dr. Ayre, of Hull, consisting of small and frequently-repeated doses of calomel, with or without a drop or two of tincture of opium, appears, in the aggregate, to have been the most efficient. Considering it right, in the conflict of medical opinions on the subject, that every one should contribute whatever his experience may have taught him to be calculated to oppose the fatal issue of this formidable disease, I venture to state, in corroboration of the efficacy of calomel, that, in a very severe case of it, in which my assistance was invoked, and in which the patient was exhausted, cold, nearly pulseless, with the Stygian dew standing thick upon him, and all the characteristics of collapse manifesting themselves, I ordered a scruple of calomel to be given every hour, till I could visit him again. Three hours had elapsed, and a drachm had been swallowed, soon after which, although every remedy which had been tried by the able medical attendant had ended hitherto in disappointment, and all hope was nearly or quite abandoned, we were gratified to find a subsidence of all the symptoms; our patient rallied, and recovered so rapidly, that in a fortnight he was again at work in his garden. No salivation ensued, the calomel appearing to be solely occupied in combating with the disease, and no untoward consequence resulted. I am, Sir, your obedient servant,

J. C. Badeley, M.D. Cantab.

Treatment of Cholera by Saline Injection.
To the Editor of The Lancet.

Sir,—I was sent for on Tuesday, the 21st of August last, at twelve P.M., to visit Benjamin J——, aged fifteen years, of spare habit. I found him suffering from true Asiatic cholera. The symptoms had run a rapid course. He was quite pulseless; the purging, vomiting, and cramps, had been of a most severe character; the rice-coloured evacuations extremely profuse; thirst intense; the corrugated and blue skin strongly marked; and when I arrived he was in a state of collapse.

Having, on the Sunday previous, witnessed the marvellous effects produced by saline injection into the veins, I determined, without a moment's hesitation, to employ this remedy; but not having been aware, till I arrived, that it was a case of cholera, some valuable time was lost in procuring assistance and the necessary apparatus. I called up my friend, Mr. Girdwood, who immediately accompanied me, and on reaching the patient, found the icy coldness of the body more decided, and the collapse more complete, and in my opinion he was decidedly moribund. We immediately opened a vein in the arm, and injected a quart of water at 98° Fahr., in which one drachm of common salt, and half a drachm of sulphate of potash were dissolved. In less than ten minutes our patient showed symptoms of resuscitation: the pulse returned, the brow became warm, the blue and corrugated skin resumed its natural appearance, and as soon as the warmth became more general, and the pulse was fully re-established, we enveloped him in a sheet wrung out in cold water, and covered with several layers of blankets; hot bottles were applied to the pit of the stomach and calves of the legs and feet; a profuse perspiration was the speedy result. Allowing this to continue for upwards of an hour, we removed the sheet, and placed our patient in bed, enveloped in hot blankets. His pulse was then 120, quite firm, his skin warm and moist, his reason completely restored, and on his expressing a wish to sleep, we left him.

On my visit in the morning, I was delighted to find him in every respect better. Some sickness and fever existed, but were speedily arrested by small doses of calomel-and-opium. Each day since he has gone on improving, and he is now convalescent.

This case but confirms me in the opinion I have long entertained, that saline injection into the veins, if adopted before the patient reaches too collapsed a state, is the most rational treatment, and one which, I feel assured, if a fair trial were given it, would be crowned with abundant success. It narrows itself, in my opinion, into a very small compass—demand and supply. The blood is generally deprived with intense rapidity of its serum, so as to leave it in such an inspissated state as to be, in a case I saw lately, thicker than treacle. What, then, so rational as to supply it with its fluid properties again, and that immediately? Not by the slow process of absorption, which must of course be the more tardy the greater the collapse, but by introducing it at once into the circulation. I would urge my medical brethren to try the usual modes of treatment, particularly that of Dr. Ayre, in the first stages of the disease; should they fail, however, in arresting its progress, and collapse set in, not to lose a moment in employing venous injection, and the result, I am fully persuaded, will be the rapid diminution of deaths from that most fearful scourge, the cholera. I am, Sir, your obedient servant,

Henry Howlett.

Treatment by Alcoholic and Saline Fluids.
To the Editor of The Lancet.

Sir,—Your readers may be interested in the fact, that the cholera patient at the London Hospital, reported in your last number to have had 250 ounces of alcoholic and saline fluid injected into the veins, is walking about the wards, entirely recovered. Five others more or less completely moribund, injected with amounts of fluid varying from forty ounces to 100 ounces, were not permanently relieved by the process. A sixth patient, injected on the 28th inst., is going on favourably.

These results of an experiment, instituted, not for "the cure of cholera," but intended as an additional mode of temporarily maintaining the functions of the heart and tissues, whilst the powers of the economy throw off the mysterious poison of cholera, may encourage further investigation, but not the indiscriminate use of venous injections resorted to in 1832. I am, Sir, your obedient servant,

Aug. 30, 1849. W. J. Little, M.D.

*** We regret that this letter arrived too late for insertion last week.—Ed. L.

On the Pathology and Treatment of Cholera. Disturbance of the Spinal Centre remedied by Counter-irritation over the Spine.

By William Reeves, Esq., Surgeon, Carlisle.

Many considerations induced me, some years back, (*vide* The Lancet, vol. i. 1845,) to entertain the idea that all the symptoms might arise from spinal mischief. At first, inflammation of the cord impressed itself on my mind, and from two examinations of the cord in fatal cases, I found great congestion of the membranes, and softening of the cord itself. Yet as it would not be logical to deduce a general conclusion from two cases, I pass this over with merely a record. One gentleman wrote to me after I had stated my views to him, saying we may have cramp and dyspnœa, and suppression of urine, &c., without original mischief in the spine, therefore your view is open to objection. My answer is—Yes, we may have many such symptoms, as in English cholera, that do not directly spring from original mischief in the spine; but I view it in this way. In English cholera there is offensive matter in the bowels, which gives rise to great irritation of the mucous membrane, and through the connexion of the ganglionic, the irritation is conveyed to the spinal nerves, and thus excites cramp, &c. Diarrhœa is a necessary symptom of this disease; the irritation in the bowels begins the disease; but not so in Asiatic cholera. Diarrhœa and vomiting, though common, are not necessary symptoms in this latter disease; vital de-