two cases of recovery from undoubted tubercular meningitis; and Trousseau declared he had seen but two instances in his extensive experience, in one of which the diagnosis was subsequently verified by *post mortem* examination, the child having died of some other disease a few months afterwards.

Now, the writers who take so desponding a view are speaking of the tubercular disease only, while the more sanguine are those who have been less precise in their definition, and have probably included under the term hydrocephalus various forms of cerebral disturbance which closely resemble tubercular meningitis in symptoms. And then most of the recoveries have been in the first stage; and, as Guersant remarks, the nature of such cases must always be to a certain extent doubtful. Sir Thomas Watson shrewdly guesses that, in many of the so-called recoveries from acute hydrocephalus, the disease has been what he terms simple encephalitis—not dependent on tubercle; and I think we might go further, and say that in many there has been no inflammation of the brain or its membranes at all. Trousseau, indeed, accounts for the apparent success of certain practitioners in dealing with this disease, by the supposition that they have mistaken for it typhoid with cerebral symptoms. The more strict observers are unquestionably right according to my experience: true tubercular meningitis is almost always fatal. I have seen one case only in which the disease had advanced so far that the diagnosis could not be mistaken, in which apparent recovery took place for a time; but the patient eventually fell a victim to the same disease, which shortly returned with fatal severity. And yet it seems absolutely certain that complete recovery has resulted in a few rare and exceptional cases. Whether the one before us must be ranked as one of true tubercular meningitis is an interesting problem, but one which we shall not find it easy to solve with certainty. That there was meningitis there can, I think, be no reasonable doubt; but, then, was this set up or accompanied by tuberculosis?

Let me explain to you first why I believe the case to have been one of acute hydrocephalus—not some form of disorder simulating it—and then we may attempt to determine with which form of that disease it may be classed.

There is such a vast difference in the danger involved in the several diseases simulating acute hydrocephalus, and that disease itself, that it becomes a matter of extreme importance to distinguish one from the other during life. The diagnosis is often difficult in the earlier stages; yet I think it can generally be made. With regard, however, to the recognition during life of the different forms of acute hydrocephalus itself, the difficulties are extreme; but they have been separated in the *post mortem* room, and the task will become easier now. The diseases which simulate acute hydrocephalus are three—viz., the simple convulsions of childhood, typhoid fever (the infantile remittent of the old authors), and what is usually called false hydrocephalus—that condition of insensibility accompanied by convulsion which occasionally supervenes upon protracted vomiting, diarrhoea, or starvation.

Now, if we compare the case which I have made the text of this lecture with each of these, and examine the evidence carefully, we shall be able to see at once that it presents essential points of difference from all of them. First, then, the case was not one of simple infantile convulsions, due to reflex irritation, without any primary lesion of the encephalon. The age of the patient, when the irritation of teething or intestinal disorder readily stirs up excited action of the cerebro-spinal centres, alone favours such a view: but there were no signs of teething, and, indeed, the child did not cut a tooth until five months afterwards. Then, again, in infantile convulsions, the attack comes on suddenly, and is not preceded by a long train of previous symptoms. The convulsion is the first, or one of the first, symptoms. This is followed by a period of insensibility of short duration only; unless, as sometimes happens, the convulsive seizures succeed each other at short intervals, when consciousness may not return for a considerable time. Yet this insensibility is not in itself of a persistent kind; its continuance is due to the constant renewal of the comatose state by successive attacks of convulsion. The stupor is due to the convulsion; it follows it and passes off shortly afterwards, unless reinduced by a recurrence of the exciting cause—viz., by a fresh convulsion. Since, then, in the simple convulsions of childhood the cerebral symptoms are not indicative of any primary lesion of the encephalon, but depend solely upon the condition of the brain, produced by the epileptiform attack—and in the case before us, as you will remember, no convulsion occurred until three weeks after the child was first observed in a semi-comatose condition—it is clear that the cerebral symptoms could not be due to this cause, but to some primary affection of the brain or its membranes. As additional proof, if any were needed, I may point out to you that persistent, rigid retraction of the neck, incessant vomiting, and intolerance of movement, are not met with in simple convulsions. It is equally clear that the disease was not

typhoid fever. The symptoms of this disease are often greatly modified in children, and at the outset closely simulate the invasive stage of acute hydrocephalus. There is often little or no purging at first, or indeed throughout; and the characteristic spots are not uncommonly entirely absent. The disease is usually less severe, and runs a shorter course. The absence of diarrhoea and of rose spots would not, then, be sufficient in themselves to prove this case was not one of abortive typhoid; but the dark offensive scanty stools, the moist, thickly coated tongue, the continued vomiting, the retraction of the neck, the blindness, and the duration of the attack, are in striking contrast to the light watery motions, the dried tongue, the transitory sickness, the muscular relaxation, and the definite shorter course of the enteric fever: and then typhoid is, as far as I know, never met with in the first year of infancy.

The remaining disorder, which might be supposed to have deceived us, is that state of convulsion and semi-consciousness which sometimes follows diarrhoea, or other exhaustive drain upon the system. It is due, no doubt, to anæmia of the brain and cord, and is closely allied to the convulsions and paralysis which result from excessive bleeding or extreme starvation. That such was not the condition in this case, is at once shown by the well-nourished state of the patient, and by the history. Neither anæmia nor wasting existed, nor had there been any antecedent flux or malnutrition to produce them, for the vomiting was not sufficiently constant and was of too short duration to have given rise to any serious results of this kind.

[To be continued.]

REMARKS ON THE MECHANICAL DILATATION OF THE CERVIX UTERI.

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THE practice of dilating the cervix uteri, in the treatment of certain affections of that organ, is now so common, that too great importance can hardly be attached to the consideration of the best method of accomplishing it. Dr. Matthews Duncan has, therefore, done good service in publishing his scientific researches thereon; but from the conclusions which he deduces from them I must most emphatically dissent on two grounds—namely, the results of his own experiments and of experience. I think I may assume that, the more nearly we can imitate the action of Nature in the production of any artificial process for the cure of disease, the more safe and satisfactory will be the result.

Dr. Matthews Duncan, in the able article which has just appeared from his pen on the subject under consideration, says that the uterus during pregnancy expands, by what he in a few lines before terms “a vital action,” “so as to remove the smallest active pressure on its contents”. To this “vital action” he attributes the effect produced by the sea-tangle tents. To quote his own words: “In ordinary cases the tangle does not expand by exerting force, but, a length of time—at least some hours—being allowed, through some vital change in the tissues touched. The superiority, then, as an expander in ordinary cases of a tangle over a metallic tent lies chiefly in this—that, while the metallic tent cannot enlarge, the tangle-tent does enlarge, and follows the expanding tissues and produces a further expansion in the same, but now also continuous, manner as the limited amount of expansion produced by a metallic tent is effected.” To allow the metallic tent to arouse this vital action, “hours at least” must, by Dr. Duncan's own showing, be allowed. But Dr. Duncan in a preceding paragraph says, in speaking of the bougie, “here the shortness of time used excludes vital relaxation or yielding, or nearly so”. Therefore Dr. Duncan, in recommending the use of the bougie in gynaecological practice, deliberately recommends that process which, by his own showing, directly violates the laws which Nature follows in dilating the cervix.

In this city, the practice of dilating the cervix uteri by means of tangle-tents has been adopted almost universally for several years past. In my own practice no unpleasant symptom has followed their use, nor have I yet met with a case which resisted their action. I do not mean to say that serious consequences may not follow their use, because, where predisposition to disease exists, the most trifling interference may be followed by such; but in a tolerably extensive practice such have not occurred to me, and I think I may safely infer that such cases are rare. In America, however, where dilatation of the cervix by graduated metallic bougies is extensively practised, serious and even fatal consequences are not uncommon; nor can that be a matter of wonder when it is borne in mind that, “the vital action” being disregarded, a rigid unyielding muscle is forcibly expanded in a few moments. That rupture of some of the muscular fibres takes place, we may assume as most probable; that the peritoneal covering of the uterus is injured in some cases,

seems certain. Thus Dr. Thomas of New York some time since published the particulars of a case in which, after the introduction of three of Peaslee's dilators, fatal peritonitis followed.

One other point in Dr. Duncan's paper demands notice. I have ever taught that no actual force should ever be used in attempting to introduce the uterine sound; and I believe the most serious consequences will ensue if, following the practice of Dr. Matthews Duncan, practitioners unskilled in the use of the instrument exert a force capable of bending it. I introduce the uterine sound daily, and have done so for years, but never yet perceived the most trifling bending. I would repeat here what I have already said in my published *Lectures* (p. 11): "If any obstruction occur, it should be overcome by gentle continuous pressure; force must not on any account be used, lest injury be done to the uterine walls. Better far that you should never introduce it, than that you should run the risk of injuring the uterus, and perhaps cause a fatal result by doing by force what should only be accomplished by tact."

ILLUSTRATIONS OF THE SURGICAL USES OF THE PNEUMATIC ASPIRATOR.*

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AMONGST the many valuable additions which have been made within recent years to the means whereby the physician or surgeon is enabled to diagnose and treat disease—some of which, indeed, such as the stethoscope, the ophthalmoscope, the laryngoscope, and the thermometer, have become indispensable—I know of none which, in my own hands, has proved more useful—the loss of which I should feel a greater deprivation—than the instrument which I have selected as the subject of my communication; viz., Dr. Georges Dieulafoy's Pneumatic Aspirator. Invaluable, however, as it has proved in the hands of many, and widely as it is now known, at least amongst hospital surgeons, it is my daily experience to find that medical practitioners in general have not as yet been made fully alive to the great disadvantages which they labour under from the want of a thorough appreciation of the many and varied purposes which the aspirator has been found to fulfil.

It is with the twofold object of exciting the interest of such of my brethren of this Branch as have not yet had an opportunity of seeing for themselves, or of hearing from others, the very satisfactory results achieved by means of the aspirator, which are daily to be witnessed in the wards of our hospitals, and of eliciting a discussion on the part of those experienced in the use of the instrument, that I have determined to recount a few examples showing various conditions in which aspiration has been resorted to with more or less success. But before proceeding with the relation of my cases, I must occupy a few moments, first, upon the general subject of aspiration; and, secondly, upon a brief description of the instrument which we now employ.

Every medical man well knows how extremely difficult it is, under some circumstances, to decide upon the presence or absence of fluid; and, again, upon the nature of a fluid which is clearly proved to exist. How many anxious hours I myself have spent in vainly trying to solve these two questions! We have been in the habit of relying, in such cases, partly upon probabilities, partly upon that *tactus eruditus* which every surgeon strives to acquire, but which can only be acquired through long and patient practice; and occasionally upon evidence obtained from the introduction of small or, as they are termed, exploring trocars—instruments which are, to use Dr. Dieulafoy's words, at once too large and too small: too small to permit the passage through them of viscid or flaky fluids, and too large to allow their safe introduction into vascular or sensitive organs.

Having repeatedly observed how unimportant a lesion is produced by the insertion of the fine nozzle of the hypodermic syringe, and also the impunity attending the introduction of acupuncture needles, even deeply, into the substance of important and highly organised structures, Dr. Dieulafoy conceived the idea of creating a vacuum in connection with an extremely fine tubular needle, so as to exercise a powerful suction upon any fluid into which the needle should be introduced. This principle he embodied in his instrument, the pneumatic aspirator. And it became clear that not only could an amount of fluid be removed by this means sufficient for diagnostic purposes, but that, by a continuance of the process, the cavity might be entirely emptied of its contents, and so, in cases where the presence of the fluid constituted the sum of disease, a permanent and ready cure might be effected.

As is the case with all inventions, many improvements have been made

upon the original instrument. The one which I now show you is the latest issued by Messrs. Weiss and Son, of the Strand, London; and, in my judgment, it possesses decided advantages over all others which I have seen.* It consists of a brass syringe, capable of holding five ounces of fluid, fitted with an air-tight screw-piston, and having its nozzle guarded by a tap. Attaching the nozzle to the bottom of the syringe is a short tube of glass, which enables the surgeon to ascertain the quality of the fluid as it passes into the body of the instrument. To one side of the instrument, in its whole length, is applied a fine hollow glass tube, which communicates at its lower extremity with the interior of the syringe, and is shut off from the external atmosphere, at its upper end, by means of a tap. This glass tube is marked at regular intervals in half ounces, for the purpose of demonstrating the quantity of fluid contained in the syringe at any given time; and forms, besides, a means of exit, whereby the syringe can be emptied of its contents when, during an operation, it is desirable not to disengage the nozzle. Several tubular needles and trocars, some of which are of extremely fine calibre, made accurately to fit the nozzle of the syringe, either directly or through the medium of an elastic tube, complete the instrument.

In describing how to work the aspirator, I cannot do better than make use of Dr. Dieulafoy's own words, altered only so as to apply to the improved instrument. He says: "In order to produce a vacuum within the pump, it is necessary first to close the two taps, and then to screw up the piston. The vacuum is thus obtained as a preliminary measure, and the operator is in possession of a powerful aspirator, ready to be used when the proper time arrives." "Let it be supposed that we wish to examine an effusion into the cavity of the pleura. The tubular needle No. 1 or No. 2 must first be introduced into an intercostal space; and, when it has penetrated the tissues for about a third of an inch, it must be connected with the pump in which the vacuum has been established, either directly or through the medium of a caoutchouc tube. This done—and to this point I desire to call special attention—the tap must be opened between the needle and the vacuum, and the needle pushed gently forward. We may thus slowly traverse the tissues, so to speak, with the vacuum in hand, until we discover the effusion. The eye of the operator should be directed to the short glass tube between the needle and the pump; and, at the moment when the needle enters the liquid, the latter rushes forcibly into the instrument. The diagnosis is at once complete, the manœuvre is absolutely harmless, and the desired object is attained."

Such is the method of employing the aspirator which I have myself usually adopted; and the directions here given apply to all cases, medical and surgical, in which its use is indicated, equally with the example selected by Dr. Dieulafoy for his description. As a precaution against the introduction of septic materials, I always take care that the needle or trocar has been dipped overhead in carbolic oil. I have said that in the aspirator we possess a valuable aid both to diagnosis and to treatment. I will now go on to make a few selections from the numerous cases in which I have resorted to its employment.

And first, I will speak of its use for purposes of diagnosis. There are few groups of diseases which present greater difficulties, or which tax the skill of the surgeon in a higher degree, than *abdominal tumours*. Not many of us can look back upon our experiences without calling to mind numerous instances in which grave mistakes have been made, leading to pernicious treatment, and destroying the confidence of our patients both in ourselves and in our science. That in the aspirator we possess a powerful means of elucidating some at least of these cases, is sufficiently shown by the following examples.

CASE I.—On August 30th last, a woman, aged 46, was admitted under my care into the Leeds General Infirmary with an abdominal tumour of twelve months' growth, and equal in size to a seven months' pregnancy. The tumour was somewhat fixed towards the pelvis; several hard movable nodules could be distinguished through the abdominal walls; it was doubtful whether fluctuation existed or not. The pelvis was found, on vaginal examination, to be filled posteriorly by a firm mass, and the os uteri could not be felt, being apparently pushed up above the pubes, high out of reach of the finger. Menstruation had for some years been very irregular. My colleagues examined the case with me, and we all felt doubtful as to its nature. On September 21st, aspiration was made through the abdominal walls with the result of extracting four ounces of a thick glutinous brown-coloured fluid, such as is found only in ovarian cysts. The nature of the disease having been thus established beyond doubt, a fortnight later I performed upon her the operation of ovariectomy.

* I have received a note from Messrs. Weiss and Son within the last few days, informing me of a still further improvement which they have been able to effect in the mechanism of the instrument: by which they avoid all contact of the aspirated fluid with the syringe, and are at the same time better able to regulate the amount of vacuum created.