ing to caustics or to division of the sphincter. Nature seldom fails to bring about a cure, or so to contract the wound as to leave but trifling inconvenience.

Perforation of the bladder.—I have twice witnessed this accident, once by the elbow of an assistant striking the handle of the staff while the anklets were being locked, and so driving its point through the posterior wall; and a second time, by either the point of the staff or the knife being carried too far across a contracted bladder. The accident has been attributed to the rough use of the forceps; but this, if it ever happen, must surely indicate a degree of violence that could only be used by a wild and nervous operator.

INGLEBY LECTURES ON SOME FUNCTIONAL DISORDERS OF FEMALES.

Delivered at Queen's College, Birmingham.

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LECTURE II—(concluded).

BEFORE proceeding to consider some of the details of this subject, let me ask your assent to certain postulates of a general character. These will serve the useful purpose of helping in the distinguishing of hysterical cases one from another. Let it be granted, then:

1. That any nerve-centre (that is, any collection of grey nerve-matter) may have its excitability exalted, so that it becomes unusually responsive to its accustomed stimulus.
2. That any nerve-centre may have its excitability lessened, so that it becomes less than usually responsive to its accustomed stimulus.
3. That the permeability of any part of the nervous system may be increased.
4. That the permeability of any part of the nervous system may be diminished.
5. That increased permeability may cause ganglia to co-operate which do not usually do so. That is, that excitement of one ganglion may excite the action of other ganglia which, under other circumstances, it would not excite.
6. That diminished permeability may have the contrary effect, so that ganglia which usually act consentaneously, may cease to do so.
7. That any one or more of the cerebral ganglia which usually co-operate may be thrown out of gear, so that it, or they, cease to co-operate with the rest.
8. That the ganglia not thrown out of gear continue to co-operate as usual.

It may be that nerve-trunks have their sensitiveness or excitability increased or diminished. I do not propose either to deny or to affirm that this is so. The result would be the same as if the centre with which they are connected were affected.

Let us turn, now, to the consideration of some diseases commonly termed hysterical; and, first, to a common form of hysterical paralysis.

The patient has lost, almost completely, the power of moving one or more limbs—almost, but rarely quite completely; for I can recollect only one case in which no power of movement remained. The motions which do remain are, although extremely limited in area, yet often of considerable complexity. Such a paralysis, though remaining constant for weeks or months, or even years, may yet disappear in an instant, under the influence of sudden and violent mental emotion—such, for example, as an alarm of fire. Hence it has very properly been termed functional, as distinguished from organic. How are we to interpret this curious phenomenon? It was long ago suggested that we should look upon it as a paralysis of “the will;” and this view is still pretty generally current.

Without raising abstruse metaphysical questions, I fancy that, in speaking of the “will,” we commonly include two separate and distinct ideas—one, the idea of a wish to do something; and the other, of our power to give effect to that wish. If we consider “paralysis of the will” by the light of this analysis, we come to this result: firstly, that, in regard to the second idea, it is a mere clothing, in pseudo-scientific phraseology, of the fact that the patient has lost the power of voluntarily moving her arm or her leg. It contains no explanation of this fact; and, therefore, conveys none. It is, in short, a

meaningless phrase. Secondly, as regards the first idea, it is untrue in fact. These patients, so far as I have been able to understand them, have a very strong wish to be able, and are deeply distressed by their inability, to move the limb. They are also extremely grateful to us when we have cured them. To arrive at a more adequate interpretation of these cases, we must go some way round, and consider subjects which may, at first sight, appear remote. Nature is, in some cases, not content to leave the activity of a nerve to be regulated by the greater or less amount of force emitted by its ganglionic centre. A second nerve, arising from a different centre, is told off to diminish, or even arrest, its action. Such superior nerves are called, as you know, “inhibitory” nerves; and the control which they exercise is called “inhibition.”

It is not difficult to perceive that this method is employed in the highest departments of the nervous system, as well as in the lower ones. We find evidence of this operation in connection with the high cerebral ganglia, the organ of the mind, as we call it. In the passage, “Thou shalt renounce, thou shalt renounce, thou shalt renounce—this is what the whole world is hoarsely crying to thee,” “inhibit” might be substituted for “renounce,” without any alteration of the meaning. An inhibition of this kind, which at first requires a voluntary effort, acquires, by frequent repetition, an involuntary or automatic character. Most of us can see a desirable object in the possession of another, without any nearer approximation to an intention of annexing it, than is contained in “I wish I had one like it.” In the child, this inhibitory power is, at best, but latent, and requires, for its development, moral and social education. In some children it is difficult to develop it, even when the reasoning-powers are indirectly stimulated by irritating applications to the peripheral nerves distributed to the skin covering the gluteal muscles. In some few, it appears to be absolutely non-existent; we hear of individuals in whom the reflex act of taking is, all their lives through, the inevitable result of the act of seeing a desirable object. Some little mass of grey, or some slender thread of white, nerve-tissue, is wanting. The same thing is seen in some savage races, who are incurable pilferers.

We find, in another set of circumstances, something even more directly bearing upon our subject. We have most of us, at some, now probably remote, period of our lives, run up to a hedge with the intention of jumping over it, but, not “liking the look of it,” stopped short, without making a spring. This is a rational and intelligent act, though it may afterwards be evident that it was a mistaken one. We have, also, similarly run up and made a spring, but, at the same moment, allowed the principle inelegantly called “funk” to intervene, with the result of inhibiting our muscular action, and thereby “bringing us to grief;” while a second, more confident—that is, an uninhibited effort—has carried us safely over. The secret of what is called “Dutch courage,” lies partly, at all events, in the fact that one great effect of alcohol is to relax or paralyse inhibition. To this, mainly, is to be attributed the loquacity of intoxication; and so is the proclivity to the committal of crimes, by persons under the influence of alcohol. Examples of the higher forms of inhibitions are common among animals. Most of the tricks taught to dogs depend upon it. The exertions of a horse who has, on some previous occasion, been severely flogged, are, in some cases, inhibited by his hearing the sound of a whip raised to strike him.

Let us now turn to an entirely different class of phenomena. Under certain circumstances, those parts of the nervous system which usually, and, for useful purposes must, act in concert, cease to co-operate, one portion remaining active, while another portion is in a state of suspended animation. This is what happens spontaneously in sleep-walkers. A similar condition is producible artificially by means of mesmerism, hypnotism, and the like. In some instances, there has, no doubt, been fraud and imposition practised by unscrupulous performers; but, the most liberal abatement having been made on this score, there remains a series of facts of the most interesting description. If you or I were told that we could not lift from the table a pocket-handkerchief because it was so heavy, we should at once confidently proceed to show that we could. Why? Well, to put it shortly, because we should believe our experience, and disbelieve what we were told; but, if a hypnotised person were told the same thing, he would be unable to lift it. Why? Well, again, to put it shortly, because he would believe what he was told, and would be unable to correct the statement by reference to his experience, in consequence of some portion of his brain being out of gear; and something like this, I submit, happens in hysterical paralysis, in which, on this theory, the primary cause would be not deficient, but excessive, nerve-action. Muscular action is inhibited by the belief that it is impossible. A principal element of cure, then, is to con-

vince the patient that her belief is erroneous. It is not easy to do this directly, for it is notoriously difficult to remove any belief by the mere assertion that it is erroneous, or even by argument. Such a course indeed, is likely rather to strengthen than to weaken a belief. In these cases, a little benevolent trickery, or pious fraud, is often successful, as in the following case.

A young girl was brought, by several friends, to the General Hospital, when I was house-physician. The whole party was in a state of great lamentation and woe. It appeared that the girl had lost the use of her left arm an hour or two before; the same thing had happened on two previous occasions, and the medical man who had attended her had, so they stated, told her and her friends that a third attack would be certainly fatal. Feeling, after examination, pretty certain of the real nature of the case, I pacified her by saying that, although it was no doubt a serious matter, there was every probability that she would not die, and even good hopes that she might recover the use of her arm. She was taken in and sent to bed. It does not do to pooh-pooh these cases. You may succeed; but if not, you lose all control over the patient, and then good-bye to any prospect of usefulness. The next morning, she moved her arm a little, as she had done the night before. She was comforted by my telling her that it was a good sign that the paralysis had not increased. The following morning, she again raised it to the same extent as previously. I put my finger a couple or three inches higher, and said, "Oh, but you moved it up to here yesterday," she immediately moved it up to my finger. On the fourth day, the same manoeuvre was repeated; on the fifth, the same trick being repeated, she got the tips of her fingers to her lips. I said, "How much farther do you want to get them?" She laughed, and the case was at an end.

Another girl was brought to the hospital who had been lying in bed for eleven months with a paralysed leg. On admission, she could, by moving the foot, rotate the end of the great toe through a circle of about an inch in diameter. I need not say that even such limited motion require the action of a number of muscles, and so facilitated diagnosis. By a judicious admixture of trickery, coaxing, and firmness, she was cured, though not so quickly as the former case.

I saw, some years ago, a fasting girl. The amount of food or even water that she was able to take was incredibly small. She was never exhibited; her emaciation was amazing, and there was no ground for reasonable doubt as to her veracity. She dwindled, and died some time after I saw her.

It is difficult to resolve the problem presented to us by these cases; but I am inclined to associate them with the preceding class, and to think that they may depend upon a belief that eating is impossible. And this belief may have had originally a substantial basis. From the dangerous extreme above mentioned, we can, without any distinct break, see all degrees of inappetence up to slight anorexia, or indifferent appetite. The slighter cases are undoubtedly often due to gastric catarrh, not necessarily of great severity. In saying this, it is perhaps right also to say that, in my opinion, there is little difference in point of frequency of occurrence between gastric and bronchial catarrhs.

Gastric catarrh, though sometimes provocative of ravenousness, is more commonly associated with diminished appetite. The view here suggested seems to me to receive support from the success of the method of treatment originated by Dr. Weir Mitchell. Take such a girl from her home, place her in the midst of new surroundings, of fresh faces, inspire her by confident assurances of her recovery, tell her you are going to restore her appetite by a new treatment, insist upon her taking food, subject her to massage, shampooing, rubbing, or whatever you like to call it. The effect is speedy, the result surprising. The quantity of food consumed soon becomes as remarkable for its largeness, as it had before been for its exiguity.

Cases of hysteria may be usefully, if not scientifically, divided into three classes; first, the simple, genuine, or involuntary cases; second, those in which there is unconscious exaggeration; third, those in which there is conscious fraud or deception. These classes might, perhaps, be as properly described as stages, for there is no doubt the first may glide into the second, and afterwards into the third; or, in other words, there has probably been a time when a case of the third class has belonged to the first. We meet, especially in the third class, with some very curious cases. And oftentimes they are not only curious, but puzzling. For example, a middle-aged unmarried lady's maid, precise and respectful, and quiet in manner and demeanour, as became the long and highly valued domestic of two or three decorous and not over youthful gentlewomen, came to the General Hospital. She had been living in London, but had been obliged to leave her place in consequence of feeble health, caused by repeated attacks of "inflammation of the bowels, with sloughing of

the mucous coat." Oh no! not dysentery, but inflammation of the small bowels. So she had been told by several medical men in London, whom she named, who had been called in by her mistresses from time to time. Such were her statements; and, though I cannot vouch for their truth, I saw no reason to discredit them. Her reason for coming into the hospital was, that she feared that another attack was impending. After a few days, during which she insisted that she was suffering from gradually increasing pain in the abdomen, but no objective phenomena presented themselves, she said she had diarrhoea; and, a few days after that, she produced a liquid stool containing some smooth-edged, flat, whitish pieces of something. We found by experiment that these sloughs could not be distinguished, microscopically or otherwise, from pieces of chamois-leather or kid-glove, macerated in water for twenty-four hours. I told her that what she had passed was not mucous membrane, but something she had eaten; and, a few days afterwards, she found herself so much better, that she would like to go home, and she went.

It is not much use arguing or quarrelling with these persons. What you want to do is to take the romance out of their cases; to show that, so far from your thinking them remarkable, and therefore interesting, you know them to be commonplace and uninteresting. Thus I entirely checkmated a girl who pretended that her bowels had not been open for weeks. I told the mother the truth (which was, that I knew it to be untrue), and got her to tell the patient that we did not think anything of it; that some people had evacuations, and others had not; that there was no rule about such things. With this somewhat audacious statement, which she, of course, knew to be untrue, but could not well repudiate without spoiling her case, the patient's renewed attempts to excite interest in her case were consistently met, and she was ultimately beaten out of the field. Instead of denying her statement, we accepted it as gospel, but in a way that took all the interest out of it. She kept up the pretence for a very long time; and, curious to say, although frequently watched, it was never discovered where or when the bowels were moved. I have seen other like cases. In another very curious case, an abandoned thief imposed upon the gaol authorities, and obtained a free pardon on the ground of ill-health, and so escaped a long term of imprisonment. On her discharge, she was brought to the hospital, and the only thing that was real about her case was, that her periods were very profuse. In the gaol, she was supposed to have copious hæmatemesis, and had been seen by the matron in the act of vomiting blood, the possible source of which I refrain from even suggesting.

On hysterical retention of urine, I have only two words to say. It is very commonly the initial phenomenon, and is most useful, therefore, as furnishing the key to subsequent manifestations; and, secondly, it is not often repeated, when its treatment is left in the hands of the nurse, instead of being undertaken by the medical man.

Of hysterical vomiting I have seen several cases, and have not found much difficulty in dealing with them; but a particular course must be followed, which presents no difficulties in hospital practice, in which only have I met with these cases. In private practice, it might be less easy to pursue.

These cases begin with occasional vomiting two or three times a day only, or even less. As soon as you know what it is (which at first you may not), treat it with indifference. It soon becomes more frequent. In a few days, the nurse will tell you that everything taken is immediately rejected. You order a teaspoonful of water to be given at intervals, and nothing else, saying you are quite sure that will stay down. You will hear, the next day, that even that is immediately thrown up. Then you have got her. Place her flat on her back, with the head well thrown back on the pillow, the neck and upper part of the chest being fully exposed. Let her swallow a teaspoonful of water, and keep your eyes steadily fixed on the throat. In a short time, you will see a slight movement in the muscles of the throat. Say at once, "Don't do that again; anyone can make themselves sick in that way." The attempt may be repeated, and must instantly be checked. You watch her for a quarter of an hour, and then point out to her that she is wrong in supposing that nothing can be retained. You will not be troubled with any recurrence of this symptom.

We do not see a great deal of emotional hysteria in our practice; and what we do see is rather in the middle-aged than in the young; and is, in fact, if I may be allowed the Hibernicism, not hysteria, but drunkenness. Judging from my experience in Parisian hospitals, I should say that this phase of hysteria is more common in the Latin races than in the Anglo-Saxon. "Attacks of the nerves" seem to me, also, of more frequent occurrence in French than in Anglo-Saxon works of fiction.

In slight attacks, a simple expedient is often successful, probably

By diverting the patient's attention. Cause her to make a deep inspiration and a full expiration; a few repetitions are followed by complete calm.

Hysterical coma is a very curious condition, and is commonly, in some degree, connected with alcohol. It is most quickly relieved by an enema of assafetida.

Reverting, for a moment, to the cases in which we are wont to recognise the existence of fraud and imposition, it is questionable whether, in many of them, we are not using a term not strictly accurate; whether we should not rather regard them as examples of partial mental aberration, the result of a dominant idea, the desire for sympathy, than of intelligent, intentional, and conscious deception. It is, perhaps, a fine distinction, and, if sometimes a just one, we must admit that, in the words of the cynical Heine:

"In case of women,
One knows never where the angel
Ceases, and the deuce commences."

These words are, also, apposite to another question. To what extent is there commonly in hysteria an erotic element? The answer to this turns largely on the meaning which we attach to "erotic." Amongst men of the world, the interpretation would be of the grossest and most carnal character; and too many of us would, and do, accept this view, thoughtlessly, I think, and erroneously, in respect to the virginal sex in general, and no less erroneously as regards cases of hysteria.

But the word has another sense, not less classical, for it rests on the authority of Plato, and one which, in my judgment, is more in accord with the passive or receptive function of women in the processes of reproduction. In this sense, it refers not so much to the lower animal passions—though these are not altogether excluded—but to the higher sentiments, from which spring all the graces and most of the charms which adorn the sex, and which do, as there is a physiological reason—indeed, necessity—for their doing, attract our own. In this sense, there is, I believe, commonly, and, in the other, but rarely, an erotic element in hysteria.

The time at my disposal, and, possibly your patience, are alike exhausted. Yet, even within the narrow limits I have fixed for myself, there are left many interesting and important topics at which we have not even glanced. For indeed, to quote a brilliant writer, "*La femme, vois-tu, c'est un thème inépuisable: on a beau l'étudier, on rencontre toujours de nouveau.*"

I have desired on this, as on other analogous occasions, to remember that I am speaking, as one of themselves, to those whose duty and honour it is to apply, in practice, the art of medicine. But, you may object, such professions are but little consistent with the scheme and tenour of my lectures. You may think that there has been but a pitiful proportion of the bread of clinical facts, to an intolerable quantity of the sack of speculation and opinion. It is true that this is so. But this has arisen, not from my forgetting, but from my remembering, that we are all practitioners of medicine.

Our predecessors have had, our successors will have, general conceptions, views, opinions, speculations, hypotheses, theories, choose which term you will, which did, and will, exercise an influence upon their acts. We ourselves, all of us, have them, and they influence our acts. None amongst us, indeed, are greater slaves to them than many of those who, in good faith, lay special claim to the title of practical men.

Nor is this subjection to speculative views peculiar to the medical profession. The conceptions of mankind are compounded in varying proportions: of truth, that is to say knowledge; of error, that is to say ignorance; of prejudice, of imagination, fancy, faith, wishes, hopes, fears. This strange medley produces forces which, intense and far-reaching in their operations, do indeed govern the world. Take a quite recent example of practical results from such a cause. The Arab conception of Paradise, and of the means of getting there, has cost us many lives and much treasure, and may yet cost us more. But we have no need to go so far afield for an illustration.

There is a prevalent conception of disease, which is not explicitly stated in books, and of which, in its nakedness, there is perhaps no one who would not be ashamed. It embodies the unquestionable fact that there are relations between disease and that which we vaguely call debility. It is true also that these relations are often so intimate and direct, that it is of the utmost practical importance to recognise their existence.

But, when this conception obtains such a preponderance in the mind that it almost effaces the distinction between disease and debility, its evil consequences are most serious. It renders therapeutical efforts injudicious, confused, timid, and irresolute. It may, indeed, inhibit

them altogether. It certainly does appear to me that such an undue preponderance is, at the present day, but too widely spread, and that the practice of many is thereby unfavourably influenced. This may be so, or it may not. What I really want you to admit is, that a abstract conception, difficult to formulate, except perhaps, if that can be called formulation, in the shape of a truism, does actually and largely influence practice; for, to put the question in a concrete form given a case, in which we correctly recognise the existence of disease, and also the existence of debility,—In what measure is our treatment of the disease to be biased by our recognition of the debility; or our treatment of the debility to be biased by our recognition of the disease? That is a reasonable question, which it is incumbent upon us to put to ourselves, and to answer. Now, in my firm opinion, the answer given, too often even in the present day, results in treatment unduly recognising the debility, and unduly neglecting the disease. And there is very much in female disorders to bring them within the possibility of being affected by such views.

It is unsound to think that conceptions, which contain an element of truth, cannot be noxious. On the contrary, it is that very fact which renders them dangerous; for it is that which makes them seductive, which gives them currency, and secures to them continued existence. The element of truth is an antiseptic which protects them against the forces of decay. It has, then, appeared to me that current conceptions of the relations between their periodical functions and the general health of women have been inadequate; that some facts have been unduly exalted, whilst others have been unduly debased; and I have endeavoured to bring this before you, in order that, in your own minds, the balance might be readjusted. In like manner has it appeared to me, that there are inadequacies and imperfections in current conceptions of the female nervous system, and its aberrations. And I have endeavoured to lay before you some considerations on this subject, which seem to me to be of importance. At all events, what I have done, however halting the argument, or deficient in coherence, I have done in good faith. I ask you to take the trouble to think about it; and then, I should be the last to quarrel with any one who arrived at conclusions different from my own. For I have been, all my life, an unswerving advocate of free thought; accustomed to swear by the words of no master, unless they commended themselves to my own independent judgment.

ON THE NATURE OF THE SO-CALLED "HYPER-TROPHY OF THE PROSTATE."

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ONE of the facts most familiar to the medical practitioner is the occurrence, in elderly subjects, of an affection of the prostate gland, frequently occasioning more or less retention of urine. At and after about fifty-four years of age, and rarely quite so soon, this change is liable to take place. As I have long ago shown by numerical researches, a small minority alone are affected by it; and it is, therefore, by no means a necessary concomitant of old age. On the contrary, it rarely commences at a period which can be thus designated; for, if it do not show manifest signs of its approach before sixty years of age, and become considerably developed before sixty-four or sixty-five, it is seldom manifested at a later period, or, at all events not to a considerable extent. It will, of course, be understood that from the above category all examples of malignant growth (carcinoma and sarcoma) are excluded, as well as all enlargements of a simple inflammatory character, not uncommon in youth, but rare in advanced years.

Hitherto it has been the custom to regard all non-malignant forms of enlargement met with among elderly men as "hypertrophy of the prostate," and invariably thus to denote them. Thirty years ago, when occupied with an extended anatomical examination of the prostate, I came to the conclusion that all enlargements which were neither malignant nor inflammatory could not be regarded as hypertrophy, in the true meaning of that term. Hence I avoided the use of it, and employed "enlargement" as a better appellation; adopting it also as the title of my first work on the subject. Subsequently, I contented myself with the old designation, "hypertrophy," finding that it was universally used, and that it sufficed, for the present, for all practical purposes. But the more exact knowledge of morbid structure and action which has been acquired since the date named supports the original view, that several varieties of enlargement have been confounded under that single term. It will be my object here very briefly to show that a more accurate apprecia-