had been separated, and closed the wound by silk sutures around the clamp, which lay at the lower angle of the closed wound.

Dr. Woodham Webb examined the tumour, and reported as follows: "Weight of solid, two pounds and a quarter; fluid contents, fourteen pints. The tumour was an outgrowth from the upper and back part of the uterus, about seven inches long, four broad at its widest part, and at one point two inches thick. It was of a flattened lozenge-shape, and consisted of uterine tissue very slightly changed in appearance. It was surrounded by three large cysts, which had developed on its surface, two of about equal size and one not more than half that of the others—the three having contained fourteen pints of a red serous fluid. The walls of the three cysts were thin, with a fine layer of muscular tissue, spread out in irregular bundles between the two serous membranes—the peritoneum and the cyst lining. Inside the cysts, on the solid mass, were several ecchymosed spots, the lining membrane being detached and giving rise to small secondary cysts. There were a few nodules of fibrous tissue in various parts of the cyst-walls."

The progress after operation was one of uninterrupted recovery. The highest temperature was 100.2 deg.; the most rapid pulse, 108. The clamp came off on the eighth day. The wound above the clamp healed by first intention. Thymol gauze was the only dressing used.

Writing to me, December 5th, 1878, the patient says: "I am wonderfully well, and am getting back my walking powers. I have not felt so well nor in such spirits for years past."

I cannot conclude the narration of this case without expressing my deep regret at the great loss which our profession has sustained in the death of an American brother, whose good work, especially in the diagnosis and treatment of uterine and ovarian tumours, will immortalise the name of WASHINGTON ATLEE of Philadelphia.

When I visited America in 1867, Atlee received me with the most friendly cordiality. He honoured me by attending a clinical demonstration which I gave in Professor Gross's clinical theatre. I assisted him to perform ovariectomy on one patient in Philadelphia and another in New York; and he and I both assisted his elder brother, John Atlee, in an ovariectomy at Lancaster in Pennsylvania. We have had an occasional friendly discussion in print and by letter; have compared notes as we have followed up similar lines of inquiry and practice, in which he (my senior by ten years in age and twelve years as an ovariectomist), while not unwilling to criticise, was always ready to sympathise and encourage. He had performed ovariectomy on nearly four hundred women. His life and labours are well worthy of careful inquiry and historical record. This will doubtless be given us in due course; but I cannot allow the present time to pass without this public expression of heartfelt regret at the loss of a great surgeon and a kindly honourable man.

THE EFFECTS OF ATTENTION ON THE BODILY ORGANS.

By W. B. CARPENTER, C.B., M.D., LL.D., F.R.S.,
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ONE of the wisest of Sir Henry Holland's thoughtful *Chapters on Mental Physiology* is entitled "Effects of Attention on Bodily Organs", a subject far too little studied, of which it seems to me that a clear conception, based on a large historical experience, affords the *rationale* of an immense number of abnormal phenomena that at first sight appear to have but little in common; and I am induced to bring it forward at the present time, by finding that, alike in the recent clinical observations of Professor Charcot on the effects of Magnets, and in the report of them by Professor Gamgee, this very potent source of error is almost entirely ignored.

The best preparation for a right understanding of these phenomena I find in a careful study of the succession of epidemics—from the Dancing Mania of the middle ages, to the Mesmeric outburst in Paris, just before the French Revolution—which, whilst extremely diverse in their symptoms, are all traceable to that morbid excitability of the nervous system, which is the special characteristic of what we are accustomed to call the hysterical temperament, though by no means limited to the female sex. A great body of information respecting these is contained in Dr. Babington's excellent translation of Hecker's history of the Dancing Mania, with additional notes and an appendix of his own.*

These strange visitations are, however, only exaggerated forms of

what is continually presenting itself in individual cases to the ordinary medical practitioner. Nothing is more common than a morbid intensification of some local pain, under the influence of the *attention* given to it by the patient; especially when this is kept alive by the kindly sympathy of those around her, under the mistaken idea (not unfrequently shared in former times by the doctor himself) that the pain is indicative of serious local mischief. It happened to me in early life to have under my care a young lady affected with the most aggravated forms of "hysterical" convulsive disorder*—tetanic and epileptic—who had been, some time before, kept in the recumbent position for two years, with a couple of large issues in her back. Wishing to learn all that I could of the history of the case, I sought information from the distinguished provincial surgeon under whose care she had then been, and his reply was very significant: "Well, Dr. C., we treated Miss P. as a 'spine case'; but we don't see so many spine cases now as we used to." Of her state of exalted sensibility during that illness, it was extremely significant that she could so distinctly hear what was said in an ordinary tone in the room below, that her doctor's conversations there with her mother had to be carried on almost in a whisper.—I well remember a pet lady's maid in a family with which I was intimate, who was brought, by the sympathy habitually shown to her most trivial ailments, to such a condition that, as her medical attendant said, "If you only point your finger at any part of her body, she will feel a pain there".—So, again, a young lady who had received a slight "nudge" in her right side, from the elbow of a person who accidentally jostled against her in the street, became the subject of pains radiating from the spot, which seriously alarmed her relatives. These first extended upwards to the muscles of the chest and shoulder, making all movement of her arm so distressing to her, that she entirely ceased to use it; and after a little time, during which she was the object of the most sympathetic attentions, the pains extended to the other arm; her inability to use which made it necessary that she should be fed, dressed and undressed, etc., by others. Being then consulted about her, I felt a strong assurance that her complaint was simply a resultant of the sympathy which had fixed her attention on her own morbid sensations; and contented myself with prescribing a tonic regimen, at the same time impressing on those about her the importance of withholding the kindly aid they had prodigally afforded, which soon produced the desired effect. Now, my intimate knowledge of this patient made me feel sure that there was no simulation, the pains being to her as real as if they had proceeded from some serious disorder; and, for the physician who is consulted about such pains to pooh-pooh them as "fancies", or to stigmatise them as "shams", merely shows that he has not mastered their *rationale*, which, as Sir H. Holland points out, is exactly that of the itching which most of us can bring ourselves to feel in almost any part of the cutaneous surface, by the simple direction of our consciousness to it for a short time.

I may mention, as additional illustrations, a couple of cases which have more recently fallen under my notice. A highly educated and very sensible lady, well known to me, having for many years been a governess in a family in the midland counties on whose regard she had acquired a very strong hold, becoming the subject of severe occasional pains in the hip-joint, was coddled and petted in every possible way. The pains in time became so constant, that she passed her days almost entirely on the sofa, and never went out of doors except in a carriage. She and her friends were strongly under the impression that she was the subject of serious disease of the hip-joint; but she shrunk from any surgical examination of it. Month after month passed without any decided change, either for the better or worse; and at last she yielded to my urgent representation that she should come up to town, on the promise that I would get for her a first-rate surgical opinion, in which she might place the most implicit confidence. That opinion was exactly what I expected. She was assured that there was nothing serious the matter; that she was not to mind the pain, but take moderate exercise, and do everything she could for the re-invigoration of her general health; and that in no very long time she would be "all right" again—which soon came to pass.—Again, one of my earliest friends came to town a few years ago for the removal of a scirrhous breast by Sir James Paget. She was domiciled in her brother's house; and her sister-in-law, a lady of most excellent sense, but of a delicate, sensitive temperament, nursed her with the most sympathising care. On going to inquire after my old friend, I was consulted by her sister-in-law about herself. "I have been lately suffering", she said, "from pains in the same part, which made me so seriously uneasy, that I spoke to Paget; and he,

* In my article "Sleep," in the *Cyclopædia of Anatomy and Physiology*, I gave some curious particulars of the somnambulistic phenomena presented by this patient.

* Hecker's *Epidemics of the Middle Ages*, published by the Sydenham Society.

finding that you were intimate with us, referred me to you". At once divining from this reference what Sir James meant, I explained to her that her pains simply proceeded—according to a well-known law of our constitution—from the very natural direction of her attention to the part; and that if she would dismiss from her mind all anxiety about them, they would soon cease to trouble her. This, also, speedily came to pass. Now if, instead of speaking out her trouble, my friend had gone on brooding over it in secret, I have very little doubt that some local disorder would have shown itself; for the truth of Sir Henry Holland's remark, that the strong and continued direction of the attention to a part, in all probability affects either its innervation or its circulation, or both, has been abundantly proved by the results of Mr. Braid's experiments. Of that influence, the vasomotor system of nerves appears to me the most probable channel.

Now, the effects of this direction of the attention are much more marked when there is a *definite expectation* of some particular result. This came out very strongly in the inquiries of the Commission appointed to report on the performances of Mesmer. The "crisis", as it was then termed—we should now call it a hystero-epileptic convulsion—was found to come on when the patients believed themselves to be in magnetic communication with the *baquet*, whether they were so or not; and so, when they were walked blindfolded about a garden in which some of the trees had been magnetised, the crisis came on when they believed themselves to be near one of these trees, although really distant from it; whilst it did not show itself when they were brought unawares into close proximity with a magnetised tree.

Not long since, a precisely parallel set of phenomena was related to me by my friend General Strachey, who was a member of a committee appointed by Lord Dalhousie, when Governor-General of India, to report on the mesmeric performances of Dr. Esdaile; who (as many of my readers will remember) performed a large number of great surgical operations—notably the removal of enormous scrotal tumours—on patients in a condition of profound coma, induced, as he maintained, by "mesmeric" influence. Of the reality of this state, General Strachey and his fellow committee-men had no doubt whatever; but Dr. Esdaile's asserted proof of a special mesmeric force was altogether disposed of by the experiments conducted under their direction. Thus, a patient who had been several times thrown into the sleep in the usual way, became (according to Dr. Esdaile) so susceptible of mesmeric agency, that he would become comatose on drinking a glass of mesmerised water. "But", said the Committee, "give him a glass of water not mesmerised, and let him be under the belief that it has been mesmerised", the patient went off into the sleep just the same. So Dr. Esdaile, having asserted that he could induce the sleep by passes made in the next room, on the other side of the partition, was requested by the Committee to go into the next room as if about to make the passes, but, instead of making them, to go over to the other side of the room and do nothing: the patient went to sleep as before.

To these obvious cases of *expectancy*, I may add one which has lately fallen within my own knowledge. A young friend who had been amusing himself with mesmerism, had brought a young girl, in a family in which he was intimate, into such a state of susceptibility, that she would go off into a comatose sleep on his simply pulling her hair. In this condition, she exhibited a number of its curious phenomena, which he was often called upon to demonstrate. On going one evening to a small party at the house of this family, he was introduced to a lady whom he had not previously seen; and he had not been in the room many minutes when, to his own surprise and that of every one else, this lady fell down in a dead sleep, from which she was with difficulty aroused. He was himself at the other side of the room at the time, and was engaged in conversation, without the least thought of the stranger. Having become possessed with my own views of the potent influence of "expectancy" in the production of these phenomena, he determined to trace out its *modus operandi* in this particular case; and learned that this lady had been specially invited to meet him, and had been assured by one of the young ladies of the family, while laying aside her bonnet and cloak in another room, that "Mr. S. is such an extraordinarily powerful mesmeriser, that he can send you to sleep by merely looking at you!"

Now, as a great deal of hysteric and hypochondriac disorder undoubtedly depends upon the patient's habitual direction of the attention to the part or organ which is the subject of them, so does a great deal of the success of remedial measures, in certain classes of cases, depend upon the patient's expectation of their favourable result. That the "charming away" of warts, for example, is a reality, I have not the slightest doubt; and the different "charms" have this in common, that they tend to fix the patient's attention on the part with the expectation of a cure; the success being generally proportional to the patient's assurance of it. That in persons of emotional temperament

the "strong assurance of faith", which is producible by religious influences, has a peculiar potency, is the concurrent result of all experience in present and past times. But the like occurs with impressible subjects, under the influence of a belief in the occult powers of some new physical agency. Thus there can be no doubt that the discoveries of Galvani and Volta, not long previously, prepared the public mind of Paris for Mesmer's doctrine of the magnetic fluid and its professedly curative results.

Now, with the guidance afforded with this knowledge, I cannot but wonder that Professor Charcot and his reporters should have so readily accepted the reality of "magnetic" and solenoid agency upon the peculiarly susceptible class of patients experimented on, without taking those precautions which experience has shown to be necessary for the elimination of the influence of "expectancy". That Professor Charcot believed himself to be sufficiently on his guard against it, only shows how little experience he has had of the fallacies of such results. When once a system of experimentation has been established, even if there be no intentional deception (such as is extremely common among hysterical subjects), the sense of "expectancy" becomes so keen, that indications afforded by tone and manner, as well as inferences drawn from words and acts, acquire an extraordinary significance, which makes it extremely difficult to exclude such influence; and the only way to test the matter fairly is to have a number of metallic bars prepared, all externally similar to each other, of which some shall be magnetised and others not, neither experimenter nor patient knowing which are the real magnets and which the sham; care being taken that the bars shall be drawn by lot from among a considerable number. The results of a succession of experiments made with each bar should be separately recorded. Again, another set of experiments should be tried with electro-magnets, "made" and "unmade" by an operator at a distance, without any knowledge on the part either of the experimenter or the patient as to when the circuit has been completed, and when broken—which times should be carefully recorded for comparison with the results.

I venture to predict with confidence, that a carefully conducted series of such experiments will as completely disprove the inferences drawn by M. Charcot and accepted by Professor Gamgee, as the results of Dr. Ashburner's experiments on the same subject, more than thirty years ago, were shown by Mr. Braid (in the following case, among many others) to be due to the influence of "expectancy"; and I would particularly suggest to Professor Gamgee, that in any further experimental inquiries he may carry out at Manchester, he should take into his counsels my friend Dr. Noble, than whom no one in this country has had more personal experience of the fallacies due to "expectancy", or of the remarkable results which Mr. Braid, under the guidance of this principle (for the knowledge of which he was indebted to Dr. Noble and myself), was able to evoke.

"When in London lately," says Mr. Braid, "I had the pleasure of calling upon an eminent and excellent physician, who is in the habit of using mesmerism in his practice, in suitable cases, just as he uses any other remedy. He spoke of the extraordinary effects which he had experienced from the use of magnets applied during the mesmeric state, and kindly offered to illustrate the fact on a patient who had been asleep all the time I was in the room, and in that stage, during which I felt assured she could overhear every word of our conversation. He told me that when he put the magnet into her hands, it would produce catalepsy of the hands and arms, and such was the result. He wafted the hands and the catalepsy ceased. He said that a mere touch of the magnet on a limb would stiffen it, and such he proved to be the fact."

"I now told him that I had got a little instrument in my pocket, which, although far less than his, I felt assured would prove quite as powerful; and I offered to prove this by operating on the same patient, whom I had never seen before, and who was in the mesmeric state when I entered the room. My instrument was about three inches long, the thickness of a quill, with a ring attached to the end of it. I told him that when put into her hands, he would find it cataleptise both hands and arms as his had done, and such was the result. Having reduced this by wafting, I took my instrument from her, and again returned it, in another position, and told him it would now have the very reverse effect—that she would not be able to hold it, and that, although I closed her hands on it, they would open, and that it would drop out of them; and such was the case—to the great surprise of my worthy friend, who now desired to be informed what I had done to the instrument to invest it with this new and opposite power. This I declined doing for the present; but I promised to do so when he had seen some further proofs of its remarkable powers. I now told him that a touch with it on either extremity would cause the extremity to

rise and become cataleptic, and such was the result; that a second touch on the same point would reduce the rigidity, and cause it to fall, and such again was proved to be the fact. After a variety of other experiments, every one of which proved precisely as I had predicted, she was aroused. I now applied the ring of my instrument on the third finger of the right hand, from which it was suspended, and told the doctor that when it was so suspended it would send her to sleep. To this he replied, 'It never will', but I again told him that I felt confident that it would send her to sleep. We then were silent, and very speedily she was once more asleep. Having aroused her, I put the instrument on the second finger of her left hand, and told the doctor that it would be found she could not go to sleep when it was placed there. He said he thought she would, and he sat steadily gazing at her, but I said firmly and confidently that she would not. After a considerable time, the doctor asked her if she did not feel sleepy, to which she replied, 'Not at all'; 'Could you rise and walk?' when she told him she could. I then requested her to look at the point of the forefinger of her right hand, which I told the doctor would send her to sleep, and such was the result; and after being aroused, I desired her to keep a steady gaze at the nail of the thumb of the left hand, which would send her to sleep in like manner, and such proved to be the fact.

"Having repaired to another room, I explained to the doctor the real nature and powers of my little and apparently magical instrument—that it was nothing more than my portmanteau-key and ring, and that what had imparted to it such apparently varied powers, was merely the predictions which the patient had overheard me make to him, acting upon her in the peculiar state of the nervous sleep, as irresistible impulses to be affected, according to the results she had heard me predict. Had I predicted that she would see any flame, or colour, or form, or substance, animate or inanimate, I know from experience that such would have been realised, and responded to by her; and that, not from any desire on her part to impose upon others, but because she was self-deceived, the vividness of her imagination in that state inducing her to believe as real what were only the figments of fancy, suggested to her mind by the remarks of others. The power of suggestions of this sort, also, in paralysing or energising muscular power, is truly astounding; and may all arise in perfect good faith with almost all patients who have passed into the second conscious state, and with some, during the first conscious stage; and with some weak-minded, or highly imaginative, or credulous and concentrative people, even in the waking condition."

ON THE EXTRACTION OF FOREIGN BODIES FROM THE EAR.*

By J. MARION SIMS, M.D.,

Honorary Fellow of the Obstetrical Societies of London, Dublin, and Berlin;
Ex-President of the American Medical Association; etc.

I AM induced to make a few remarks on the removal of foreign bodies from the ear, in consequence of a discussion on this subject a few months ago in the pages of the BRITISH MEDICAL JOURNAL.

In the summer of 1843, Dr. B. R. Jones of Montgomery, Alabama, asked me to see with him a little negro, five or six years old, who had some foreign body in the ear. The doctor had already made fruitless efforts to remove it with a bent probe and forceps. I repeated the operation with the same instruments, and with the same unprofitable result. The ear bled a little, and became so tender that we were obliged to desist, and I went home to get other instruments. I returned to Dr. Jones the next day, with a small syringe and a delicate pair of forceps from my eye-case. We found the child's ear very tender to the touch, and the canal was tumefied and filled with coagulated blood. On the previous day, the foreign body was visible; but now it was obscured by the blood. I at first used the syringe for the sole purpose of washing out the blood, expecting then to remove the foreign body with the forceps. The syringe easily dislodged the blood near the outlet of the canal; but it failed to remove that in immediate contact with the foreign body. For the purpose of dislodging the blood, I caught hold of the lower lobe of the ear and pulled it outwards, so as to straighten the canal, and then injected water forcibly into the ear. Greatly to my surprise, the first jet of water ejected the foreign body from the ear, and it fell on the ground at a distance of fifteen or twenty inches from the patient. I had no thought whatever of removing the foreign body with the syringe. I expected only to clear away the coagula, so as to see it, and then remove it with the forceps. The foreign body was a bit of cut-glass that had formed the ornament of a cheap finger-ring.

* Read in the Section of Surgery at the Annual Meeting of the British Medical Association in Bath, August 1878.

About a year after this, a negro man came to me, saying he had been deaf in one ear ever since childhood, and that with an ear-pick he had just discovered something hard in it. On examination, I found the ear completely blocked up with some dark-coloured substance that, to the probe, seemed to be of bony hardness. It was soon removed by syringing, and proved to be a cherry-stone, which the man now remembered to have pushed into his ear twenty years before, when he was a small boy.

This was the strongest test possible of the efficiency of this means of removing foreign bodies from the ear. For, here was a perfectly spherical body, that completely blocked up the whole tract of the canal; and yet the small jet of water, thrown forcibly in, passed by and dislodged it, so that it was removed with ease. Ever since that, I have never failed to remove with facility any foreign body found in the ear, whether wax, insects, seeds, pebbles, or bits of wood.

The first article* I ever wrote for a medical journal was on this subject, and, as appropriate now, I quote from it the following.

"Whoever follows the loose direction of 'syringing the ear forcibly', will often be disappointed. A sufficient explanation of this may be found in the anatomical peculiarities of the organ. The course of the meatus is curved, running at first forwards and upwards; then downwards and backwards; its concavity looking obliquely downwards and forwards. It is an inch and a quarter or more in length; about a quarter of an inch in its transverse diameter, a little more in the perpendicular direction, its section being oval. Its dimensions vary, being a little contracted at the orifice, still more so in the centre, which is the narrowest part, and again at the tympanum. The anterior semilunar edge of the concha, leading into the meatus, projects considerably below a line forming a tangent with the convexity of the lower wall of the canal. With the obliquity of this canal and its irregular dimensions, it will be found difficult, if not quite impossible, to throw a stream of water with any degree of force against the tympanum, even when the organ is unobstructed; for, if the nozzle of the syringe be directed parallel with the tragus, the force of the injection will be spent against the anterior inferior wall, or convexity of the floor of the canal; if turned more towards the semilunar ridge of the meatus, its power will be lost on the posterior superior wall, or convexity of this tube. But, let us change the direction of the canal by laying hold of the lower portion of the antihelix, pressing the dorsum of the concha forwards while we put the whole organ forcibly on the stretch, pulling it outwards, upwards, and backwards, thus making the meatus a straight channel of diameter, and then it will be found comparatively easy to throw a uniform stream of water to its very bottom. If the foreign body be small, the water will dislodge it as it rebounds from the tympanum; if it be large, plugging up the passage perfectly, two or three injections properly directed (the ear held as described) will be certain to change its relative position sufficiently to allow the next to pass by, impinge on the membrane tympani, and wash it out in its retrograde course, or so far dislodge it as to bring it within reach of the forceps. The syringe that I use holds about two drachms of water. The nozzle is of small diameter, and the piston is perfectly air-tight. Water, thrown from it suddenly and forcibly, does not pass in a long slow continuous stream, but seems to strike, as it were, all at once and with great power. A syringe of small dimensions is preferable to a large one; because, with a small one having a small tube, the stream may often pass undivided by the foreign substance, so that its reflex action is almost as strong as its direct force. On the contrary, in a large syringe with a large tube, throwing a larger volume of water, a small portion only of the stream may pass behind the foreign body, while a larger portion strikes against it in a direct line with greater momentum, thus counteracting the retrograde force."

The principles here laid down are as true to-day as they were a third of a century ago, when they were first penned. So universally successful have my efforts been in removing foreign bodies from the ear, that I was surprised to find that the same unflinching success did not attend the efforts of all other surgeons. And so I felt anxious to ascertain the reason of this. To this end, I went to three of the most renowned instrument-makers in London, and asked to see the best ear-syringes they had; and they showed me large clumsy metal syringes, with large nozzles, throwing a stream of water large enough for a rectum syringe. I then went to some of the best instrument-makers in Paris, and I found just the same sort of ear-syringes as I found in London. It was then plain to my mind that the fault was not in the principle, but in the clumsy and unsuitable instruments used.

The ear-syringe that we use in my country is made of hard India-rubber, holds only about four drachms, is light, and worked with one hand, while the other is used to straighten the canal by pulling the ear

* On the Extraction of Foreign Bodies from the Meatus Auditorius Externus. By J. Marion Sims, M.D. (*American Journal of Medical Sciences*, April 1845, page 336.)