because I think I have observed that a series of inflammatory symptoms in peritoneum, bladder, and rectum are thus avoided.

In the first five or six days, the urine is drawn off by the catheter, and I endeavour to check action of the bowels; but for some minutes every day I have the patient placed on her side, and an enema-pipe placed in the anus. Thus great relief is obtained by escape of flatus.

Milk, soft eggs, ice, wine, good beer, soup, Cognac with sugar and iced-water, I give as nourishment. When vomiting has been very lengthened, an ice-bladder over the region of the stomach, or rubbing over it an ounce of collodion, has been very useful.

Against hiccup, I have mostly used with good effect morphia injections or laudanum. In the days when I always inserted drainage-tubes or pushed them in beside the wire *écraseur*, I have removed them and the *écraseur* on the eighth or ninth day, carefully washed the tubes, shortened, and again inserted them. Every day after that, I took out the tubes and washed them. Five tubes were gradually reduced to one, and this one was daily shortened. Although I fastened every tube by a piece of silk thread, yet it did once happen that the tube slipped into the abdominal cavity, became encapsuled, and only appeared after exertion weeks afterwards. The nurse believed that the tube was lost in the bed, as the patient was very restless at night. The wound healed, the patient was discharged, and five weeks afterwards, on the sixty-second day after ovariectomy, gave a ball in honour of her recovery. All the gentlemen danced with the queen of the ball, and she became greatly excited. At four in the morning, during a very lively cotillon, she was attacked with violent pain deep in the abdomen; she could waltz no longer, and, after a quarter of an hour, could not stand. She went into another room, looked at her body, and saw a movable black spot in the middle of the painful cicatrix. She fixed the black spot with the nails of the thumb and forefinger, and drew up a drainage-tube seven centimètres long (three inches), wiped away the pus which followed the tube, and danced away merrily again, for she had lost all pain.

The recovery of women after ovariectomy is generally surprisingly rapid, even if before the operation they could eat nothing and vomited everything. A withered skeleton, with dried-up skin, in a few weeks becomes plump, and her appearance rewards our thousand cares with unusual pleasure and astonishment.

REMARKS ON METALLIC AND MAGNETIC THERAPEUTICS.

By HORATIO DONKIN, M.B. OXON.,

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THE late reappearance of the doctrines—long since regarded as flimsy and played-out superstitions—of the influence of magnets and coins on certain states of the human body, is certainly somewhat startling in these days of increased respect for scientific methods of research—a respect evinced nowhere more strongly than in the field of medicine. The accounts that reach us from the other side of the Channel of sensibility abolished and resuscitated by the application of coins and the approximation of magnets to the body, would seem, in their apparent absurdity and effete dress, to be quite unworthy of serious consideration, were they not endorsed by well-known and honoured names among physicians and others of scientific reputation. It can scarcely be denied that M. Charcot's rehabilitation of Dr. Burq's methods, under the name of metallotherapy, no less than his doctrine of the influence of magnets and solenoids on the human organism, are at first sight so improbable, so liable to fallacy in their data, not to mention their suspicious kinship to exploded fancies, that an attitude of the strictest scepticism is needed before their claim to be regarded as scientifically approved facts can be justified. In the investigation of well-known and undoubted phenomena on the healthy and diseased organisms, we are frequently calling into requisition new aids to our senses, and checks on too hasty conclusions drawn from our impressions; and endeavour, by constant repetition of observation and experiment, to verify all newly alleged facts, and *à fortiori* every generalisation or theory deduced therefrom. Surely, then, the study of the causal relation between any given agent, whatever it may be, and the confessedly shifting and subtle material of altered sensations, and the protean phenomena of hysteria, involving, moreover, results to some extent dependent on the patient's statement of subjective impressions, requires the severest scientific jealousy in its prosecution.

The following remarks are prompted chiefly by a perusal of Professor Gamgee's account of his visit to the wards of Professor Charcot, in the Salpêtrière Hospital, published, among other interesting papers on the same subject, in the BRITISH MEDICAL JOURNAL of October 12th. This endorsement of the conclusions arrived at by the French observers, being the most noteworthy English utterance on the subject, seems to call for some critical observations.

The phenomena of hysteria, hystero-epilepsy, and even the so-called "mesmeric" exhibitions, occurring in several of the cases alluded to in the above-mentioned paper, may be regarded as established facts, similar cases having been more or less known and described long since. Parenthetically, however, it may be observed that such mesmeric phenomena, although indubitably often genuine, are frequently consciously imitated and produced at will; and that, before accepting them, all possibility of collusion between operator and patient, or delusion of the former and deception by the latter, must be eliminated.

Admitting, then, the facts of rapidly disappearing and returning sensibility, of transferred sensation and the like, as genuine and indisputable, the question at issue is: whether there is, as Professor Gamgee appears to believe, a real causal connection between the special agents used and the effect produced; or whether there be not another factor in the process, well-known and competent to produce results of the nature of those alleged, quite independently of the particular instruments called into requisition, such as coins, magnets, or solenoids.

The influence of attention in lessening or increasing the sensation of pain, even in healthy normal organisms, is too well known to need fresh demonstration; and that, in what are called the hysterical and neurotic temperaments, more marked and curious modifications of sensation are apt to occur, clinical experience amply shows. Moreover, the manifold phenomena of morbid sensation and action, produced in states when the higher control of the cerebrum is probably in abeyance, such as somnambulism and so-called electro-biology, are familiar to most, and are perhaps nowhere more clearly set forth than by Dr. Carpenter, in his strictly philosophical writings on the subject. Kindred phenomena, in short, to those now recurring at the Salpêtrière exhibition have long been regarded as sufficiently covered by theories which do not require the special causative agency of metals or magnets. It would seem, therefore, that we have already a *vera causa* to start with at the very threshold of the inquiry.

Coming, further, to the question of so-called metallo-therapy (illustrated by M. Charcot's collection of "Sensitives"), which, however, as Mr. Ernest Hart remarks in his "Note on the Metaphysics of Metalloscopy" in the JOURNAL of October 12th, is now apparently held at a distance even at the Salpêtrière, and is giving way to the "electric current" and "magnetism" of past years, it may be noted that, in the same number of the JOURNAL, there is published, by Dr. A. Hughes Bennett, an experimental demonstration of that which, to many English readers of M. Charcot's metalloscopic researches, had already appeared to be the true view, viz.: that special "sensitiveness" to certain metals has nothing to do with the results of altered sensation, etc., obtained after the application of metallic *plaques*; for, in Dr. Bennett's case, the regulation series of phenomena, now well known from the French accounts, were found to follow the application of wooden buttons. A full account of this case, which has the immense value of a crucial experiment, with an extended criticism on this subject by the above-named author, appears in the current number of *Brain*.

Moreover, that we have no reason to seek further than some such theory as "expectant attention" to account for these vagaries of sensation would appear to be clearly indicated by a case which came under the notice of Dr. Bennett and myself at the Westminster Hospital, where the application, at Dr. Bennett's suggestion, of *wooden discs* on the left thigh of a girl subject to occasional and spontaneous attacks of anæsthesia in the right leg (the left thigh, however, never having been anæsthetic) was followed, in a short time, by complete insensibility of the part to pinching and pricking.

Having thus very shortly cleared the ground for the consideration of the latest development of the subject—namely, the "influence of magnets and solenoids at a distance on the nervous system of certain patients" (I quote Professor Gamgee's statement of a "*fact*" which he himself calls startling)—it is necessary to ask whether this conclusion, so fearlessly announced, was arrived at, judging from the Professor's own account, by a process of scientific induction worthy of the investigation of any newly alleged facts, however unimportant.

In the first place, it must be noted that the results observed by the writer of the paper in question were apparently all repetitions of previous performances. It seems clear that all experiments involving the possibility of the action of a mental, and therefore somewhat elusive, factor, or the slightest risk of the patient being "led", however un-

consciously, by operator or bystander, may lose rather than gain in force by repetition. This fact is in itself a weakness in the evidence.

Next, it nowhere appears, from Dr. Gamgee's account of the experiments which so fully convinced him of the action of magnets on sensation, that due precautions were taken to prevent the possibility of the patient's attention being in any way influenced by those around her, or even of a more obvious source of knowledge of the "magnetic" application. In no case, apparently, was the patient blindfolded; nor is it stated that perfect silence was observed by operator and spectators—two seemingly obvious and initial precautions towards excluding the influence of expectant attention, or even possible fraud. Even in the case of the blind patient (Case III) related in Dr. Gamgee's paper, her long residence of over thirty years in the hospital, coupled with the omission from the account given of all precautions against "leading" the patient being taken, renders the "experiment" far from conclusive, in spite of the statement of no effect following the application of the convex end of the magnet.

Again, while admitting freely the genuine return or disappearance of sensation on the application of a solenoid or the approximation of a magnet, it does not appear that exactly similar instruments, unendowed with magnetic or electrical power, were tried, as one attempt out of many similar ones to alter the conditions of and so to verify the experiment. Whether or not this precaution was taken by M. Charcot himself at the outset of his observations, it has clearly not contributed to Dr. Gamgee's convictions, as they appear stated in the paper under review.

Case v, quoted by Dr. Gamgee, describing the placing of a magnet at the distance of one-third of an inch from the patient's left temple, and the sequent phenomenon of vanishing achromatopsia, is a remarkable instance of inconsequent argument; no causal connection between the magnet and the result being even a plausible hypothesis in the absence of a comparative trial of other agents, and of the absolute certainty of the patient's ignorance as to what means, if any, were being employed to produce the result in question.

As a further instance of a rash arrival at important conclusions, I would quote Dr. Gamgee's statement regarding "ovarian hyperæsthesia". That a necessarily somewhat diffused pressure on an ill-defined anatomical part, conveniently called the "region of the ovary", proves, when tenderness is evinced, that the *ovary* is hyperæsthetic, is apparently, in Dr. Gamgee's opinion, a sufficiently accurate datum for the further conclusion of a necessary connection between ovarian disturbance and the phenomena of hystero-epilepsy. Such reasoning may possibly be called in question by the anatomist or the unprejudiced and scientific gynaecologist. I leave it to others to express opinions on the practical advisability of the *hasty* introduction of prolonged abdominal palpation into the therapeutics of feminine nervous diseases.

Lastly, as a piece of internal evidence taken from Dr. Gamgee's own paper against the truth of the conclusions therein contained, I would call attention to the statement that no results were obtained by either M. Charcot or himself from the application of electro-magnets to frogs. On the hypothesis of a human mental factor being essential to the production of the phenomena in question, the insensibility of the batrachian nervous system to such influences becomes a valuable "negative instance" in the inductive process.

Doubtless, as Dr. Gamgee says, an intimate connection between the study of physiology and clinical medicine is desirable in the interests of both sciences. Perhaps in England more than elsewhere this connection is kept up; and thus the physiologist, being somewhat conversant with the infinite permutations and combinations of nervous and so-called "functional" phenomena in the human organism, is, as a rule, aware of the complexity of the matter in hand, when these phenomena become the subject of a scientific investigation, and recognises, as physicians strive to do, the necessity of applying the most rigorous methods of research before arriving at any conclusion in so difficult a field of observation. The reciprocal advantage to the practical physician of the knowledge gained from the researches of the physiologist requires no advocacy.

It would appear from the above considerations, bearing in mind the *à priori* unlikelihood of the alleged causation of the phenomena under discussion, and the bygone explosion of similar theories, no less than [the fact of such explanations being gratuitous and unnecessary, that as yet Dr. Gamgee's opinion on "animal magnetism" is a conclusion drawn from very scanty data, and by no intelligible logical method. The importance, however, somewhat factitious though it may be, now given to this class of phenomena, renders some notice of the kind of arguments adduced in their support more than excusable.

THE TREATMENT OF GENU VALGUM.

By H. A. REEVES, F.R.C.S. Ed.,

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MR. VINCENT JACKSON'S note in the JOURNAL of September 28th calls for a reply. But for his remarks, private and literary engagements—the latter, unfortunately, long unkept—would have stood in the way of my reverting to the subjects of knock- and bow-knees for some weeks. I will first reply to Mr. Jackson, and then give my latest experience in the treatment of these deformities.

Mr. Jackson by implication raises the question of priority; but, if he will again read my lecture, he will see, first, that I said that the novelty of this my second method, which is suitable only for moderate cases of genu valgum, consists in the *combination* of the division of the biceps tendon, the ilio-tibial band, and the external lateral ligament, with *immediate* forcible straightening.

Secondly, Mr. Jackson says that in March of this year he divided the left biceps tendon and the adjacent tendinous structures completely. Mr. Jackson has wisely inserted the adverb, because any incomplete division would have been useless, and also because tendons, etc., are sometimes only transixed and partially divided. "A few days afterwards," continues Mr. Jackson, "the knee was forcibly straightened . . . , and the patient remained in the hospital for three or four months."

Now, in point of time, I preceded Mr. Jackson by more than three years; for, when the East London Children's Hospital was at Ratcliff Cross, I divided these three structures, but did not then attempt *immediate* forcible straightening, and only after three days had elapsed. The *recent* operation to which I referred in my lecture (which was sent to the JOURNAL about four months ago) was done a week after my first subcutaneous osteotomy of the internal femoral condyle in March of this year, and differs entirely not only from that which Mr. Jackson did, but is in direct opposition to the usual surgical teaching, which instructs us to let a tendon unite before stretching it. Mr. Jackson will, I am sure, pardon me if I say, and in no unfriendly spirit, that his remarks are somewhat vague. Let any one try to divide "adjacent tendinous structures", and he will find that, unless he seek for the external lateral ligament separately and independently, he will either not divide it at all or cut the peroneal nerve. The ilio-tibial band may be divided through the same puncture as that used for the biceps tendon—of course cutting *outwards*; but the seat of selection for tenotomy of the biceps tendon is high up in the outer boundary of the popliteal space, whereas, to *really divide* the external lateral ligament or ligaments, one must go lower down, passing the tenotome between the nerve and tendon, and cutting *inwards*; for to prick the joint-capsule subcutaneously is nothing to dividing the nerve. I have never seen the least mischief from thus dividing the ligament, although it would not be logical to conclude that the joint was never pricked. There is no necessity, however, if the operation be cautiously done, to interfere with the joint.

Mr. Jackson will excuse me for saying that, anatomically or histologically, I know of no "tendinous structures" adjacent to the biceps tendon; but, waiving this structural point, it would be well to know if he did as I did—*i.e.*, point out to the bystanders what he meant to do, and naming and dividing each structure as he went on. Many of us have surgical after-thoughts, in which we think we also must have done so and so; but there is considerable difference between this and operating with "malice aforethought" and intent made plain. I do not say Mr. Jackson did not do all this; but, to make his oblique claim to priority clear, he must not only show this, but also that his first operation not only preceded mine, but was identical in its method. Of "modifications" we have no end.

The three or four months' stay in hospital after this operation, of which Mr. Jackson speaks, is—other things being equal, or nearly so—a great drawback to these tenotomy methods; and my experience during the last four months goes clearly to prove that osteotomy of the condyle combines the great desiderata of simplicity, safety, rapidity in execution and result, and efficacy.

I note that Mr. Jackson gave an anæsthetic on two occasions to his patient. This is another drawback. At the Royal Orthopædic Hospital, we do a large number of tenotomies weekly, and I have frequently operated on from seven to ten patients on a Monday, and never do we give an anæsthetic.

I would remind Mr. Jackson and other surgeons interested, that the external lateral ligament was divided by Langenbeck for this deformity over twenty years ago; but he did not attempt immediate forcible straightening, nor did he divide the ilio-tibial band. The recently