Metalloscopy and Expectant Attention. By D. Hack Tuke, M.D., F.R.C.P.

Having availed myself, when recently in Paris, of the opportunity afforded of visiting the Salpêtrière,* I have made a few notes of what I witnessed in the service of M. Charcot, although, from the shortness of my visit, I was not in a position to do justice to the subject which has recently attracted so much attention—the influence of metals in anæsthesia, hystero-epilepsy, &c. I can do little more than record facts, and suspend my judgment as to their nature and the explanation which further investigation will give of them.

I first saw a young woman (Wandeline), aged about 24, the subject of "Hystero-epilepsy." On enquiry I found she had suffered from it more than three years, and had been in the hospital more than one year. Both sides of the body were affected with anæsthesia. When either arm was pricked she showed no signs of pain. Sometimes only one side—the left—is insensible. The first experiment consisted in fastening a small zinc plate upon the outside of the right arm, below the elbow. After a short time sensibility returned at this part, and in the neighbourhood. When pricked, the patient at once called out. In about twenty minutes the sensibility extended to the fingers.

In another case of hystero-epilepsy (Gleize), a young woman, aged 18, there was hemianæsthesia—right side. She also had attacks about once a week. The catamenia were irregular. Ovarian pain and tenderness were stated to be among the symptoms. Of this patient there was a spirited drawing hung up on the wall, in a hystero-epileptic fit. In this case a tin plate was applied, and fastened to the right arm, in a similar way to the other, and the same result followed, a return of sensibility at the spot. Previously M. Charcot had introduced a large pin in this part of the arm, as also through the folds between the fingers, and the right side of the neck, without the patient evincing any sign of pain. Sensibility having returned on the site and in the vicinity of the tin plate, M. Charcot pricked the corresponding part of the left arm, and the girl evinced no sign of pain. This is the *transference of anæsthesia*, to which M. Charcot attaches

* This visit was made in April, and this paper written shortly after. Unavoidable circumstances have delayed its publication until the present time.

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so much scientific importance. That it occurs, there can be no doubt whatever; the only question is the mode in which it is induced. The patient was not allowed to see the arm which was being pricked. Above and below the spot, the sensibility appeared to be perfect.

Another fact was pointed out by M. Charcot, that the thimble finger on the right side feels, and that only. The thimble worn is of iron, covered with tin.

In this case an experiment was made in regard to the power to distinguish colours. The right eye* fails to distinguish any except red, but after a tin plate had been for some time attached to the forehead on that side, the patient replied correctly when asked what certain colours were.

In a third case (Whitman), of the same age as the last, hystero-epileptic fits occur about once a week. She has been two years subject to them, and has been one year under M. Charcot. There is anæsthesia of the right side of the body; pain on pressure in the region of both ovaries; most on the right side. There is anæsthesia of the right nostril, the right half of the tongue and pharynx. Anæsthesia more or less marked, of some part of the body, appears to be a constant feature of hystero-epilepsy. Pricking and introducing a pin of large dimensions into folds of skin in the neck, and between the fingers, did not cause the slightest apparent pain.

In this case a small coil of copper wire (solenoid), was placed on the right little finger, and connected with a galvanic battery. In a few minutes the finger was sensible to pricking. Here again occurred the phenomenon of transference of anæsthesia. The corresponding finger on the left hand was insensible to pain.

Another experiment was performed with this patient. A large horse-shoe magnet was applied to the right temple, a linen rag intervening. After the lapse of a short period, perhaps five minutes, she said she felt pain in the head. Soon afterwards M. Charcot applied his large pin to the right temple. There was no longer anæsthesia, but unquestionable indications of the application being painful. This return of feeling extended some distance, and would go on, I was informed, to about half way down the arm, if the magnet remained in apposition with the head. Contemporaneously

* Nothing has been observed by the ophthalmoscope in these cases beyond "slight anæmia of the retina."

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with the above local return of sensation, there was anæsthesia over the left temple—pricking there with the pin causing no flinching or exhibition of discomfort.

In this case, before the magnet was applied, the left eye was closed, and the patient was requested to distinguish certain colours which were placed before her—red, violet, yellow, green, blue, &c.

She named red correctly, but in regard to all the others, she responded that they were white. The trial was repeated after the magnet had been applied to the temple as described. She then gradually acquired the power of distinguishing them, and in the following order—blue, green, yellow, violet. M. Charcot stated that he had observed the same order of returning perception of colours in other cases, both in and out of Salpêtrière.

In another experiment on this girl, the right hand was subjected to the influence of galvanism, having been previously demonstrated to be insensible to the prick of the pin. After a little while the feeling returned in the hand, and it was stated by M. Vigouroux that, with a stronger current, the whole arm would become sensitive. Along with this return of sensibility there was a transference of anæsthesia to the left hand.

The last experiment upon this case was with the electrifying machine. The application of electricity induced sensibility on the right side. There was, however, in this experiment, no transference of anæsthesia. I understood that the sensibility caused by the repeated use of the electrifying machine lasted a considerable time. In this experiment the power to distinguish colours by the right eye returned.

I was informed by M. Vigouroux that the metal which acts the most frequently in removing anæsthesia or increasing the temperature and producing other phenomena of metal is iron; then comes zinc, then copper, gold, silver, &c. So that, out of 100 persons subject to anæsthesia, 70 or 80 would be certainly found sensitive to the action in one way or other of iron; whilst the remaining 20 or 30 will be so in regard to one of the other metals. Many people are sensitive to two metals at once (as gold and zinc) and even more. Briefly, it may be stated, that it is alleged that the application of a certain metal upon a part affected with anæsthesia (1st), restores sensibility; (2nd) causes sensibility to disappear on the other side; (3rd), after the removal of the metal the sensibility remains for some time; (4th), lastly, it dis-

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appears definitively. "En tous deux, les oscillations sont complètes."

Such were the experiments in regard to the alleged influence of metals, galvanism, the magnet, and electricity, in removing anæsthesia, followed by its transference to the other side of the body. I should add that two patients took chloroform subsequently, one of whom passed into a state of hystero-epilepsy, closely resembling the drawings M. Charcot has had made of patients in one of these fits. The other was excited and hysterical, but the epileptoid condition was not so marked. In both cases M. Charcot suddenly roused them to consciousness by pressure over the ovaries.

In my brief narration of the foregoing experiments, I have avoided, as much as possible, confounding the *post hoc* and the *propter hoc*. Whatever the terms employed may seem to convey, I wish simply to describe what I witnessed, and in so doiug, bear my testimony to the succession of certain phenomena in these cases, in accordance with what M. Charcot has described in his interesting lectures. This is due to him. That these results admit of different explanations; that the facts are one thing and their *rationale* another, Charcot would be the first to admit. He has, in his lectures, referred to my work on the "Influence of the Mind upon the Body," and pointed ont that, from the principles maintained in that book, such phenomena would be explained by the author as due to expectant attention. That which I should wish to maintain, in regard to these experiments at the Salpêtrière, is that, knowing how many phenomena are due to the influence of the imagination only, every possible test should be resorted to in order to ascertain how far a metal or the magnet exerts a real effect *per se*. I should think it presumptuous on my part positively to assert that the facts whose true nature is *sub judice*, are all certainly explicable by suggestion, attention, imagination, and so forth. M. Charcot assured me he had employed tests which precluded the possibility of such explanations, and that the uniformity of results in different subjects, unknown to each other, in and out of the Salpêtrière, and without any knowledge in the first instance of what was expected, puts such a solution of the problem out of court. Obviously before an independent observer and a stranger to the exact tests which have been employed, can form a positive opinion on this point, he must go through a series of carefully prepared experiments himself. My time in Paris did not allow of my doing this, though every opportunity was

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most courteously offered by M. Charcot for a fuller and more frequent examination of the cases under his charge. Especially would one wish to see patients operated upon *for the first time*, and certainly *ignorant of previous experiments*. One man, indeed, I saw with hemiplegia and hemianæsthesia, who happened to come to the Salpêtrière for advice when I was there, but he was stupid, and it was difficult to be certain from his replies what the real facts were when the experiments were tried. Electro-magnetism *à distance*, however, removed the anæsthesia.*

With regard to the internal administration of metals, I did not obtain any definite information; and M. Charcot spoke guardedly on the subject. Indeed, on all points, he most fully admitted the necessity for caution, no less than the folly of an unreasonable scepticism. It cannot but be a satisfaction that a man of his profound knowledge of nervous diseases should have turned his attention to one of its most obscure forms; one so calculated to mislead the unwary physician, and tempt him into hasty generalizations. If, after all, the facts are found, when a series of rigorously-conducted experiments are tried in entirely fresh cases, to admit of a mental and not a metal explanation, they will have added a series of illustrations, by no means to be despised, of the "Influence of the Mind upon the Body." Believing, as I do, that M. Charcot's patients are not impostors, I am anxious that we should not fall into the mistake which has so often been committed before, in parallel instances, of denying the facts because we differ about their real character, and thereby miss the lesson (whatever that may be) which they teach when correctly explained by science.

It would be a great advantage, if a series of test experiments were published by M. Charcot showing the precautions which have been taken to prevent the fallacies arising out of mental influences.

Some months after writing the foregoing, I witnessed, through the kindness of M. Charcot, the same experiments at the Salpêtrière. I left with the same opinion: the great interest of these phenomena, however large a share in their causation may be due to the psycho-somatic element. Prejudiced as I naturally am in this direction, I feel that it is the more incumbent to be open to conviction, should facts determine an opposite conclusion. I also felt, as before, how

* This case has been reported in the "Gazette des Hospitaux," Mai 21, 1878.

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desirable a series of test experiments would be. It is not easy with old subjects to devise such as are altogether satisfactory. Something one says, or does, or the manner in which one says or does a thing, may vitiate the experiment. Thus one experiment which I tried, and which I ought to mention, because so far as it goes it supports M. Charcot, was open to objection. In the case of one of the subjects, whose anæsthesia is removed by a metal plate being bound over the arm, I substituted a strong card as similar in size, &c., as possible, and when bound in the usual way on the arm, with various accompaniments calculated to work upon the expectant attention, I thought it possible that the ordinary result would follow—a transference of anæsthesia. This, however, was not the case. Nothing whatever happened. The same impression, however, may not have been produced upon the skin of the arm, and therefore the same expectation as that which the patient was accustomed to, may not have been excited.

So again in reversing the magnet, with the result that the patient no longer experiences certain sensations, it is difficult to be sure that she is not aware of some difference having been made in the conditions of the experiment. All this only points to the extreme liability of an investigator to unconsciously vitiate the value of any test he employs, and the proportionate care which is called for.

This necessity is confirmed by the effects which, in cases of hemianæsthesia, have followed the application of bone, &c., by Westphal, and of wood by Dr. H. Bennett. Justice, however, demands the admission that the influence of bone or of wood does not *necessarily* prove that metals do not exert an influence apart from the expectant attention aroused by their use; any more than the purgation or the sleep induced by a bread pill, proves that the jalap or the opium for which it was substituted possesses no virtue. It is possible to be misled into denying as well as admitting an alleged force.

On the occasion of this more recent visit to the Salpêtrière, I saw a case of hystero-epilepsy and hemianæsthesia (right side), in which an attack which came on after hypnotising her and pinching the skin on the side about the fifth intercestal space was immediately arrested by pressure on the right ovary, which was stated to be hyperæsthetic. In the attack, the patient, who was at that time in bed, became rigid, her head was thrown back, and soon after there were

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spasms of various muscles, followed by violent movements. Subsequently psychical phenomena were well marked, the emotions of contempt and disgust, anger, and seraphic bliss. At one time there seemed a phase of apprehension and a consequent clinging to one of the physicians (M. Regnard) as her friend. When first hypnotised, it was easy to make the hands rigidly flexed by tickling the flexor muscles, as Braid produced the same effect long ago. The analgesia was complete. This patient and another were thrown into a cataleptic condition by making them gaze at a brilliant light in the darkened laboratory. This mode of employing Braidism did not, I think, occur to M. Braid, but it is merely an intenser form of his operation. The results are the same as those with which those accustomed to hypnotic experiments are familiar. The limbs could be placed in any position ; there was analgæsia ; and changing facial expressions indicating various emotions, the more striking being that which indicates some beatific vision. From this state the subject is easily wakened and as easily sent again into the hypnotic sleep by the action of the bright light. I have often seen a slight current of air, as in blowing on the face, instantly restore consciousness. Certainly nothing can exceed the beauty of the experiments which can be made upon the muscles of expression by means of hypnotism.

One subject was hypnotised by M. Charcot by means of looking at her for two or three minutes. While in this unconscious condition she sewed several stitches, and wrote some words of which after she was roused she knew nothing. I have often insisted on the importance of similar experiments to these in their bearing on the unconscious and automatic acts of epileptics. I have seen a young man when hypnotised walk under the influence of a dominant suggestion through the street for a considerable distance, enter a chemist's shop, demand some smelling salts (as ordered to do), and return with the bottle to the spot from which he was sent, without being conscious of what he had done when he was aroused, and yet no one who met him in the street would have suspected his abnormal mental condition. The bearing of induced hallucinations and illusions upon certain forms of insanity is also of the greatest interest, as I have pointed out in this Journal (" Artificial Insanity, especially in relation to Mental Pathology," July, 1865).

I should add to the foregoing the case of a *religieuse* who had been admitted at the Salpêtrière on account of tonic

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spasm of the fingers and wrist of the left hand. No doubt the contraction was hysteric in character. The magnet was employed by M. Charcot in this case with the result that the contraction rapidly disappeared. But he was interested to observe that the enemy only changed his quarters. In short, there was a transference, for a time, of the rigidity from the right to the left hand, parallel to the transference of sensation with which other cases had made him so familiar. This artificial contraction was induced on the right side while I was at the Salpêtrière by placing the poles of a large horse-shoe magnet in proximity with the anterior surface of the forearm. It came on in about five minutes. The contraction would last all day, but the time was shortened by the application of galvanism by M. Vigouroux. In this case it is obvious that any application to a hysterical contraction of a joint might, however impotent in itself, be followed by relief. Further, it is equally easy to understand how the application of a magnet to the healthy arm should in this woman cause tonic spasm from expectant attention. The absence of any effect when the magnet is reversed, is apparently opposed to such explanation, but it is difficult to be satisfied that there is no moral influence at work in any one single instance, fair as it would be to admit the force of this experiment when applied with uniform results in a number of cases.

Westphal, who was at the Salpêtrière just after my first visit, on returning home in May, applied to the arm of a patient affected with hemianæsthesia a copper plate varnished on the surface in proximity to the skin. By evening there was no change. Next morning sensibility had returned on the whole anæsthesic side. At the end of May he applied a copper plate, covered in the same way, with sealing wax fastened so tightly round the arm that on the following morning it was very painful, and the arm œdematous. Sensibility had returned to most of the affected side. On the sound arm, a few hours after—the plate and bandage having been removed some time—it was found there was anæsthesia on the spot corresponding to the place on the other arm upon which the copper plate had been applied. To the same patient's anæsthesic arm were also applied counters of bone tightly fastened on. Four and a-half hours after, pain was felt under this spot, but the rest of the arm remained anæsthesic. This application being repeated for a whole night, sensibility returned over nearly the whole forearm. The hand was red

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and swollen*. In some hours there was, with slight exceptions, a return of sensibility throughout the affected side. Prof. Westphal's assistant, Dr. Adamkiewicz, had, it should be stated, applied, in April, a mustard plaster to the arm of a patient with complete hemianæsthesia. In about two hours complete sensibility had returned to this part. Subsequently, with the same patient, the mustard plaster was applied at the same time and in the same place to both arms, they being alike anæsthesic. Only one arm (the right) became sensitive, whilst the other, in spite of the mustard, remained still anæsthesic. Hence the influence of the mustard in producing feeling prevailed on the right side, and it would seem that this influence, by the law of transference, neutralized the action of the mustard plaster on the left side.

In another case return of sensibility was induced by the same means, and this was followed by the phenomenon of transference.

In the "British Medical Journal," October 12, 1878, is reported a similar case, which was under the care of Dr. Inglis, Senior Assistant Physician to the Royal Edinburgh Asylum, and the same number contains notes of a case of hemianæsthesia treated by Dr. Hughes Bennett, with benefit in the first instance by metals, and subsequently with the same result by discs of wood.†

Of six cases of hemianæsthesia seen in London, I have observed the application of metals fail in two instances to produce any effect; in one, it was followed by return of sensibility. In two cases galvanism had a marked effect. In none was the phenomena of transference distinctly established. Several cases are now under observation.

In reply to the objections which suggest themselves to the theory of the direct action of metals, M. Vigouroux has been good enough to write me the following remarks on the recognition in the experiments made at the Salpêtrière, of the *rôle* which may be played by expectant attention in such phenomena:—

"I am much astonished, I confess," he says, "at the importance which you in England appear to attach to

* The effect of so tight a ligature complicates this experiment.

† The same number contains a full and graphic description by Prof. Gamgee of the cases seen by him at the Salpêtrière. See also *Idem*, Nov. 23, and *Brain*, Oct., 1878 (Dr. Bennett); case under Dr. Wilks, reported by Mr. Horrocks, "Brit. Med. Jour.," July 12, 1878; "On Hemianæsthesia of special and general sensation," by Dr. Allen Sturge, 1878; and "Neuropathologische Studien" (Hemianästhesie), von Dr. Franz Müller, Graz, 1878.

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this objection. It would be necessary to speak at length on this subject, but as I do not wish to exceed the limits of a letter, and as I treat the question in detail in an article which will appear shortly in the 'Revue Mensuelle,' I will confine myself to a few observations.

"The fact is that since the commencement of the experiments at the Salpêtrière, those who have made them have been fully alive to the probable influence of the imagination upon the results which have been obtained, and you will doubtless admit that the necessity for this caution did not require insisting upon. For myself, I took the precaution to read your work on the 'Influence of the Mind on the Body.' The first experiments were consequently surrounded by all the precautions required to eliminate this influence; eyes bandaged, substitution of one metal for another without the patient's knowledge; the use of inert materials, &c., &c. But it was soon seen that the difficulty did not lie there. Doubtless the first reports which were published, among others, the report to the 'Société de Biologie,' and the lecture of M. Charcot only slightly touched upon this aspect of the subject. Subsequently, rather from the practice of making rigorous experiments than from casting doubt upon what could not reasonably be doubted, it was never omitted to adopt all necessary precautions. Thus, if the magnet was employed, the poles and the neutral line were alternately applied. If piles were employed, they were used, sometimes with the circuit shut, at other times with it opened, as can be easily done without the patient's knowledge. Sometimes an accidental circumstance itself constituted a test, as when, for example, a wrong metal was used by mistake, as tin for zinc; or when in making use of some machine it was forgotten to fasten a screw. In all these cases, without any exception, we have observed that certain phenomena followed positive experiments, and not others. So in matters of detail, the application of different metals has been observed to change the course of the phenomena.

"I do not speak of the tests which foreign physicians have been permitted, in all liberty, to use at the Salpêtrière. You yourself, if I am not mistaken, applied inert substances, in order to test the influence of the mind.

"It would be easy, you will understand, indefinitely to enlarge upon the scientific precautions which have been taken. I content myself for the present in proving that expectant attention has been sufficiently allowed for, and I

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pass on to an argument perhaps still stronger for those physicians who *à priori* deny that our conclusions are legitimate. The experiments were made, in the first instance, upon certain women at the Salpêtrière, who, since then, have continued to appear at the public demonstrations; but outside the Salpêtrière, and in non-hysterical cases, experiments have been daily repeated for more than two years on an enormous number of patients of both sexes in ordinary practice, and in the most opposite conditions. Nothing was more striking than to hear from persons absolutely ignorant of the new means with which one experimented upon them, that they, under the same conditions, experienced the same sensations as the patients at the Salpêtrière, and employed the same terms to characterise them. From all these observations has resulted a body of doctrine as regular and as well defined as is found in any other part of physiology. Nor can one conceive this universal consensus of the imagination which establishes an *ensemble* of laws and the maintenance of the most vigorous methods of investigation; for not only the results once established are invariable for the same patient, but the series of phenomena is the same for all. Are these the characteristics of the imagination?

"My limits do not allow of my speaking of the curative effects obtained in some cases of organic lesion with subjects in whom the element of emotion was at a minimum.

"I cannot better explain our attitude of mind on this subject than by repeating what M. Charcot said to me the other day—'It is truly astonishing that the imagination of our patients has given us so little cause for embarrassment.'

"In conclusion, this is the question. The effects attributed to the metallic applications and to similar agents, the magnet, electricity, &c., are they due to the fact that the attention of the patient is directed to a part of his body, as it would be by a particular operation, and to the previous knowledge of these effects? or rather, are they the direct results of the application of these agents, and are they united by certain laws to the physical conditions of the operation? I declare the former hypothesis to be absolutely untenable, and the latter abundantly proved by facts."

One thing is certainly clearly established, no less by the experiments of M. Charcot than by those of Westphal and others, that various applications besides metals remove anæsthesia and induce transference to the other side.

It is important not to allow any difference of opinion as

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to the *rationale* of these phenomena to divert our attention from their great interest quite apart from metalloscopy. M. Charcot has brought under the notice of the profession the clinical facts of hystero-epilepsy, hemianæsthesia, and allied phenomena, in a way which no one else has done, and his good work will remain, whatever becomes of metalloscopy. Further, the courageous manner in which he is now employing hypnotism at the Salpêtrière can hardly fail not only to confirm, as it has done already, the results arrived at by Mr. Braid, but greatly to extend them.

There are those who think that because these lines of enquiry offer a tempting field for imposture they should not be pursued, and who, as regards hysteria, adopt for their text "Touch not pitch, lest thou be defiled thereby." There are others who believe it is more essential here than anywhere else to apply methodical scientific research and legitimate medical enquiry. To the latter class belongs, fortunately for medical science, the distinguished physician of the Salpêtrière, M. Charcot.

CLINICAL NOTES AND CASES.

Case I.

Insanity in a Boy following Injury to the Head. By G. H. Savage, M.D., Lond.*

Edward, J., schoolboy, 14 years. Mother insane thrice after delivery, including the birth of this boy.

The boy had been healthy and bright, though small. He had had no severe illnesses. Four months before admission he received a severe blow on his head. He was only insensible for a very short time. Two days after the injury he became dull and moody, then slowly changed and became noisy, boisterous, and violent, throwing things about in a wild way. He kicked and bit those about him. This attack lasted nearly a month, and then he became dull again, and for a few days his friends thought he was getting well, when again he became maniacal. During the four months before admission he had three or four distinct attacks of mania with intervals of a few days of quiet. In the attack preceding admission he was much more violent, and could not be managed at home. He chattered incessantly, talking nonsense; he would not dress himself, tore up his clothes, struck his mother, and tried to escape from the house in a state of nudity. He was dirty in his habits, refused food, and was sleepless.

* Read at Quarterly Meeting. *See* Notes and News.

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