PROCEEDINGS OF THE GENERAL MEETING ON

December 30, 1884.

The eleventh General Meeting of the Society was held at the Rooms of the Society of British Artists, Suffolk-street, Pall Mall, on Tuesday, December 30, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

II.

The President opened with the statement of a proposed change in the relations between the Council of the Society and the investigating Committees. The Council, he said, had come to the conclusion that it was desirable that, for the future, the responsibility for both the facts and the reasonings in papers communicated to the Society, and published in the *Proceedings*, should rest entirely with their authors, and that the Council, as a body, should refrain from expressing or implying any opinion on the subjects thus brought forward. The reason for this change was to be found in the position which the Society had now reached. On the one hand, by the work which had been accomplished it had obtained the adhesion of a certain number of men of scientific reputation, whose services on the Council would be of the highest value, but who, though generally sympathising with the Society's aims and methods, would be reluctant to pledge themselves to the conclusions of any investigation in which they had not personally been concerned. On the other hand, a necessity was now strongly felt of concentrating effort on the most difficult and obscure part of the task originally undertaken—namely, the phenomena of so-called Spiritualism. In this region it could hardly be hoped to maintain, even in the Council itself, the amount of agreement which had been arrived at in the investigation of Thought-transference and Mesmerism.

Professor Barrett read a paper on certain narratives which had been received from various correspondents, containing some remarkable instances of success in the "willing-game," and also a few cases of apparent Thought-transference without contact. In this paper, and in the discussion that followed it, special attention was called to the conditions which must be fulfilled in order to give the records of such experiments an evidential value. A careful account of the experiments should be written down *at the time* that they are made; and this account should in every case include the *whole set* of trials made at that time, failures as well as successes. The importance of excluding contact, or any other opportunities of unconscious indications, was again emphasised.

Mr. Edmund Gurney then gave a brief abstract of the following paper :—

III.

M. RICHET'S RECENT RESEARCHES IN THOUGHT-TRANSFERENCE.

The number of the *Revue Philosophique* for December, 1884 (Félix Alcan, éditeur, 108, Boulevard St. Germain, Paris), contains an account, by the well-known *savant*, M. Ch. Richet, of some most interesting and original experiments in Thought-transference, conducted by himself and a group of his friends. The title of his paper is "La Suggestion Mentale et le Calcul des Probabilités." By *mental suggestion* he means exactly what we term Thought-transference—the communication of ideas from one mind to another otherwise than through the recognised sensory channels.

Beginning with some general remarks on the limitations of our knowledge, M. Richet shows the unwisdom of attempting to set limits to the possibilities of Nature on *à priori* grounds, and the importance of keeping the mind open to evidence for novel facts. He further points out that experiments in delicate psychical phenomena necessarily differ from experiments on the matter and forces of the inorganic world, in that the results can never be predicted with certainty. When it has once been discovered that iodide of sodium is decomposed by chlorine, this result can for ever after be produced with absolute certainty under certain simple conditions. But in the case of Thought-transference, the conditions of success are inscrutable and unstable; out of 20 trials, the faculty, even if it exists, may only once take appreciable effect.

After this brief preface, M. Richet comes to the details of his own investigation. And here his originality does not immediately show itself in the *form* of the experiments. They are divided into three main groups; and in the first of these the plan was one which has been repeatedly adopted by our own Committee on Thought-transference. A card being drawn at random out of a pack, the "agent" fixed his attention on it, and the "percipient" endeavoured to name it. The novelty of M. Richet's method was this; that though the success, as judged by the results of any particular series of trials, seemed slight (showing that he was not experimenting with what we should consider "good subjects"), he made the trials on a sufficiently extended scale to bring out the fact that the right guesses were *on the whole*, though not *strikingly*, above the number that pure accident would account for, and that their total was considerably above that number.

This observation involves a new and striking application of the calculus of probabilities. M. Richet takes advantage of the fact that, the larger the number of trials made under conditions where success is purely accidental, the more nearly will the total number of successes attained conform to the figure which the formula of probabilities gives. For instance, if some one draws a card at random out of a full pack, and before it has been looked at by any one present, I make a guess at its suit, my chance of being right is of course 1 in 4. Similarly if the process is repeated 52 times, the "probable" number of successes, according to the strict calculus of probabilities, is 13; in 520 trials the probable number of successes is 130. Now if we consider only a short series of 52 guesses, I may be accidentally right many more times than 13, or many less times. But if the series be prolonged —if 520 guesses be allowed instead of 52—the actual number of successes will vary from the probable number within much smaller limits; and if we suppose an infinite prolongation, the proportional divergence between the actual and the probable number will become infinitely small.* This being so, it is clear that if, in a very short series of trials, we find a considerable difference between the actual number of successes and the probable number, there is no reason for regarding this difference as anything but purely accidental; but if we find a similar difference in a very long series, we are justified in surmising that some condition beyond mere accident has been at work. If cards be drawn in succession from a pack, and I guess the suit rightly in 3 out of 4 trials, I shall be foolish to be surprised; but if I guess the suit rightly in 3,000 out of 4,000 trials, I shall be equally foolish *not* to be surprised.

* M. Richet mentions one condition as calculated to interfere with the rule of pure accident, in a series of guesses made in the way described. He remarks, truly enough, that the guesser, if *told*, after each guess, whether he has been right or wrong, may have an unreasonable instinct that the next card or suit will be different from the last; and he adds: "Mais cet instinct trompe plus souvent qu'il ne sert, de sorte qu'en règle générale, on dit moins bien quand on devine, que quand c'est le hasard qui parle pour nous." But whatever goes on in the guesser's mind—even if he laboriously follows some complex system, as is so frequently done by gamblers, still, if the cards are drawn absolutely at random, the rightness or wrongness of the guess must remain a matter of pure accident. For the rightness or wrongness of the guess involves a relation between *two* things, of which the guesser can only control *one*. The drawer supplies a card; the guesser supplies a card's name; correspondence between the two events means a success. Now, *ex hypothesi*, nothing but pure accident governs the first event—the drawing of the card; therefore, any guess that is made has exactly the same chance of being right as any other, whatever be the principle or no-principle on which the guesser acts. And if his mental processes are irrelevant to his chance of success in each particular case, they are, of course, equally so to the percentage of his successes in the aggregate.

Now M. Richet continued his trials until he had obtained a very large total; and the results were such as at any rate to suggest that accident had not ruled undisturbed—that a guiding condition had been introduced which affected in the right direction a certain small percentage of the guesses made. That condition, if it existed, could be nothing else than the fact that, prior to the guess being made, a person in the neighbourhood of the guesser had concentrated his attention on the card drawn. Hence the results, so far as they go, make for the reality of the faculty of "mental suggestion." The faculty, if present, was clearly only slightly developed, whence the necessity of experimenting on a very large scale before its genuine influence on the numbers could be even surmised.

Out of 2,927 trials at guessing the suit of a card, drawn at random, and steadily looked at by another person, the *actual* number of successes was 789; the *probable* number, had pure accident prevailed, was 732. The total was made up of 39 series of different lengths, in which 11 persons took part, M. Richet himself being in some cases the guesser, and in others the person who looked at the card. He observed that when a large number of trials were made at one sitting, the aptitude of both persons concerned seemed to be affected; it became harder for the "agent" to visualise, and the proportion of successes on the guesser's part decreased. If we agree to reject from the above total all the series in which over 100 trials were consecutively made, the numbers become more striking. Out of 1,833 trials, we then get 510 successes, the probable number being only 458; that is to say, the actual number exceeds the probable number by about $\frac{1}{10}$. Some further experiments, where the particular card, and not merely the suit, was to be guessed, gave a number of successes slightly, but only slightly, above the probable number. Out of 782 trials the actual number of successes was 17, while the probable number was 15; and there was a similarly slight excess over the probable number when only the *colour* of the card was to be guessed; whence M. Richet surmises that there may be a field of choice too wide, and a field of choice too narrow, for the influence of "mental suggestion" to have effective play.

Clearly, however, no definite conclusion could be based on such figures as the above; they at most contain a hint for more extended trials, and would be valueless if they stood alone. M. Richet himself is careful to state this in the most explicit way; we shall find, indeed, as we proceed, that he sometimes carries caution of tone to a point which may not be excessive, but which is certainly extreme. At the same time he appears to me to press the calculus of probabilities to a point where it ceases to be instructive, when he deduces from these results a provisional estimate of the probability of "mental suggestion" as a fact in Nature

Taking the number of successes on the whole as about $\frac{1}{10}$ (apparently misprinted $\frac{1}{18}$) above the probable number, he concludes that "mental suggestion is, in a certain degree, probable, but with a degree of probability of only about $\frac{1}{10}$." Such a statement could only have a practical meaning, if we could view the results in oblivion of all the legitimate objections to which the hypothesis of so novel a faculty is exposed. Nor, indeed, is any immediate estimate, of the sort that M. Richet seems to intend, possible; for when a result is due to either A or B (*i.e.*, here to either chance alone or some further cause) the calculation of the probability that B has produced it cannot be made without assuming some value (as small as we please) for the *a priori* probability that B exists. It seems better, therefore, to regard a result like M. Richet's simply as indicating the most probable measure of the influence of the faculty (for these cases) *if* it exists, rather than as measuring the probability of its existence.*

The persons who took part in the above experiments, as has been said, were not specially distinguished by any natural or acquired aptitude for "mental suggestion."† M. Richet regrets that he has not been able to experiment in the same way with any really sensitive "subject;" and at this point he falls back on the reports of our own Committee on Thought-transference. He dwells specially on the case reported in the Proceedings, Part I., pp. 22, 23, where cards, drawn at random from a pack, and seen only by the members of the investigating Committee, were correctly named by the percipients in 9 out of 14 trials. But while admitting that our results, if genuine, are more conclusive than his own, he rightly prefers to take his stand on trials which he has personally supervised.

In his next batch of experiments, cards were replaced by photo-

* In n trials, with a probability p of success in each, the most probable number of accidental successes is np, which would give $n-np$ failures. If, then, the number of successes in excess of np be represented by a, the fraction which expresses the most probable measure of efficiency of an unknown cause, supposed to exist, is $\frac{a}{n-np}$ $\left(\text{not } \frac{a}{np}\text{, which would represent M. Richet's } \frac{1}{10}\right)$.

† We are often asked by acquaintances what they can do to aid the progress of psychical research. These experiments of M. Richet's suggest, at any rate, one answer; for they can be repeated, and a valuable contribution made to the great aggregate, by any two persons who have a pack of cards and a little perseverance. One person should draw a card at random from a pack, and regard it steadily; the other should try to guess it, and his success or failure should be silently recorded. The pack should be shuffled after each trial, and the number of trials made at any one time should be limited to 50. The total number of trials contemplated (1,000, 5,000, 10,000, or whatever it may be) should be specified beforehand; and in order that the guesser's mind may be in as blank and receptive a state as possible, he should not be allowed to see the record of results until the whole series is completed.

graphs of landscapes, pictures, statues, &c., all mounted in an absolutely similar manner—the object of the change being to occupy the eye and mind of the "agent" with something more vivid and interesting than a mere playing-card. Different numbers of these photographs were used on different occasions; and as M. Richet gives the probability of a right guess as $\frac{1}{4}$, $\frac{1}{6}$, &c., according to the number employed, it is to be inferred that the guesser was always accurately informed, before the experiment began, what his field of choice was. Out of 218 trials of this class, in which the probable number of successes was 42, the actual number was 67. This total consisted of 28 series; and in only 6 of these did the actual number of successes fall below the probable number.*

We pass now to the second main division of M. Richet's work. Here he boldly confronts the possibility that the impression made by "mental suggestion" may act, not on the conscious but on the unconscious part of the percipient's mind—"sur les facultés inconscientes de l'intelligence." That this is what often happens in the case of spontaneous telepathic impulses has been more than once suggested in the

* M. Richet has calculated the probability of the number of right guesses actually given in each of these 28 series. In one particularly successful case, it was as small as $\frac{1}{2600}$; in another it was $\frac{1}{130}$; in another $\frac{1}{100}$, and in others some larger fraction. In strictness, what we want, for each case, is a fraction expressing the probability, not of that *one particular result*, but of *at least that amount of success;* which fraction may then be compared with the fraction of ideal probability, $\frac{1}{2}$. M. Richet proceeds to illustrate his results in the following way. He supposes a row of urns containing balls, the number in each corresponding to the denominator of one of the fractions. Thus there will be one urn with 2,600 balls; a second with 130; a third with 100; others with various lower numbers; and several with only one, which is to express the wholly unsuccessful series where not a single right guess was given. Each of the urns contains one white ball; and in those which contain more than one ball, the rest are all black; except in the sixth urn where three are white and one black, to correspond with a series where the actual number of right guesses fell below the probable number. M. Richet then asserts that the probability of his results may be represented as the probability that a person who draws a single ball at random out of each urn will in every case draw a white one. Now even if the number of balls in each bag were made to correspond with the correction given above, this method of representing the *totality* of the success seems clearly illegitimate. The error may be most easily seen if we consider the case of the sixth urn. In that urn there are several white balls; but still the chance of drawing a white ball from each of the first six urns must clearly be less than that of drawing a white ball from each of the first five. That is to say, the urn representing a series where the amount of success was *below* the probable amount that accident would give, is made to *heighten* the probability that something beyond accident was at work. This illustration, however, is in no way vital to M. Richet's argument.

reports of our Literary Committee.* Experimental analogies are, therefore, of extreme interest to us. Now, if we are to have experimental evidence of an *unconscious* mental impression, received from another mind, it must show itself in some bodily affection, either sensory or motor. In our *Proceedings*, Part VI., pp. 203-4, a *sensory* case is given. The fingers of a "subject" having been concealed from him by a paper screen, anæsthesia and rigidity were repeatedly produced in one or another of them, by a process in which the concentrated attention of the "agent" on the particular finger proved to be an indispensable element. A psychical account of this result seems possible, if Thought-transference can work, so to speak, underground. And as specimens of the *motor* affection, we have in our collection several cases where a mental question on the part of some one present has been answered in writing, with a *planchette* or a simple pencil, without any consciousness of either the question or the answer on the part of the person whose hand was automatically acting.† It is to the *motor* form of experiment that M. Richet's contributions belong. But he has developed it in a quite novel way; and here again, as in the case of the card-guessing, he has brought the calculus of probabilities to bear effectively on various sets of results, which, if looked at in separation, would have had no significance. The "subjects" of the experiment, as has been said, were persons possessing no special aptitude for "mental suggestion"; and this being so, it was clearly desirable that the bodily action required should be of the very simplest sort. The formation of words by a *planchette*-writer requires, of course, a very complex set of muscular co-ordinations: all that M. Richet sought to obtain was a single movement or twitch. He accordingly applied the principle of the divining rod, in the following ingenious way. The two ends of a wand which had a certain appreciable flexibility, were held by the would-be "percipient" in his two hands. An article having been hidden by the "agent" in one of a certain number of specified places (underneath the box of one of a row of orange trees in a garden near Paris), the hiding place was revealed to the percipient as he passed in front of it, by a move-

* *Proceedings*, Part II., p. 140, where it is remarked how the effect of such an impulse seems sometimes to remain latent for several hours, until a season of stillness and passivity allows it to emerge into consciousness. See also Part VI., pp. 121 and 171. For cases where the emotional or volitional condition of one person seems to have produced unusual actions in another person at a distance, see Part VI., pp. 124-126.

† Some of these cases have been cited above by Mr. Myers, and others will be given in the sequel to his paper. Mr. Myers and the present writer have both witnessed the phenomenon, under conditions which left no possible doubt of its genuineness.

ment of the wand due to an extremely slight muscular tremor which brought his two hands nearer together. In 25 trials, where the number of successes, according to the calculus of probabilities, should have been 4, the actual number was 12. One of the diviners, apparently the most sensitive of the group, obtained 4 successes in 4 trials, the probability of each success being $\frac{1}{6}$; M. Richet himself, as diviner, under similar conditions, obtained 6 successes in 13 trials. He made some more trials of the same sort, using the shelves of his bookcase as the hiding-places; and again, hiding the object on the person of some one present. Taking the total results of these three sets, we find that out of 67 trials, where the probable number of successes was 16, the actual number was 34. The improbability of obtaining such a result by accident is enormous; and though M. Richet is again careful to point out that an immensely larger number of trials would be necessary before the influence of "mental suggestion" could be held to be proved by such a method, the importance of the method is incontestable.* It is, however, one which needs very special precautions, owing to a danger to which M. Richet is fully alive—the possibility of unconscious indications. Hints from the expression or attitude of the "agent" may be prevented by blindfolding the "percipient" and in other ways; but if the two are in close proximity, it is harder to exclude such signs as may be given by involuntary movements, or by changes of breathing.

The form of the experiment was again varied by placing a number of toy-pictures— of medals, sabres, scales, animals, &c.—in a row on a table; the diviner then, passing along the row, had to discover from the movement of his wand on which of these objects the agent had fixed his thoughts. The actual success here was only slightly above what accident might probably have given. But there were special points of interest in several of the failures. The set of objects contained only two pictures of medals; when one of these was the selected object, the other one was "divined." One trial was made with 78 cards, two of which represented a man on horseback, and one of these two was the selected object; the diviner narrowed down his field of choice to these two, but

* It may be worth while to remark that either the selection of the particular hiding-place ought to be settled each time by lot, or the percipient ought to be prevented from knowing whether or not his divination has been correct. Otherwise the chances of success may be really affected in the way which M. Richet imagined in the case of the card-guessing. If we allow the *mind* of the agent to govern the selection, then a process in his mind may find its counterpart in the mind of the percipient; for instance, the agent may decide to be a little crafty, and to hide the object twice running in the same place; and the percipient's mind, following a similar track, may reckon that this amount of craft has been exercised, or has not been exercised. Here then (as in the game of *mora*) we should have the operation of something which is neither mental suggestion nor accident.

finally guessed the wrong one. In 6 other cases, out of a total of 31, there was a similar close approach to correctness; and in 3 more, M. Richet, who was closely observing the process, believes that the error was due to the wrong interpretation of the movements of the wand—the object guessed being next in the row to the right one. If these facts were allowed for, the actual number of successes would be 10, while the probable number would be about 2. But M. Richet, with his usual candour and caution, admits the danger of applying the calculus to results which have been rectified in this manner. His reason for drawing attention to the close resemblance between the object guessed and the right object is the intrinsic interest of the fact. The explanation of this fact by the hypothesis of "mental suggestion" involves, however, certain difficulties which must not be ignored.

M. Richet does not inform us whether the agent was *watching* the diviner as he passed in front of the various objects. If he was, we should find the readiest telepathic explanation of the successes in a movement of his *will*, whereby the diviner's organism was affected. But then in that case the diviner's frequent selection of an object *closely resembling* the right object loses all significance; for the agent would not be *willing* the selection of the resembling object any more than of any other of the wrong objects. If, on the other hand, the agent was *not* watching the process, then we might perhaps conceive a connection between the idea of the *object* and the idea of *movement* to be firmly established in the agent's mind, and to be a preparation for the particular connection in the percipient's mind. We should, however, have to extend our view of the operations of Thought-transference in a decidedly novel manner. It is not merely that a composite idea, consisting of an image and an impulse, is transferred to the percipient's "unconscious intelligence"; but the one element, the impulse, is to reveal itself when—and only when—the other element, the image, finds its fellow in a conscious percept, the actual picture of the object.

But the above, I imagine, is not the way in which M. Richet himself conceives the event; for he says nothing about an impulse of will or an idea of movement. I infer from his language that in such cases he would consider the chance of success through "mental suggestion" to be just the same, if the agent were unaware of the precise form of experiment adopted, and exercised his will, not to produce a movement, but merely in the sense that every agent in experimental Thought-transference exercises it—that is, in a concentrated desire that the idea of the object in his mind should reappear in the percipient's mind. But then the percipient's sub-conscious or unconscious idea of a medal, or a man on horseback, or whatever it may be, could apparently have no more tendency to produce a movement than any other of the ideas, conscious, sub-conscious, or unconscious, of which, at the time, his mind is the theatre ; and it is hard to see what difference in this respect could be made by the fact that the idea had been transferred from the mind of the agent, instead of occurring spontaneously. But even if we grant—and it is very possible that in time we shall have to grant—that the mere fact of a telepathic impression may entail peculiar physiological effects, why should the effect of the unconscious idea manifest itself only at the moment when the percipient's mind becomes consciously occupied with a similar idea—that is, when his advance along the row brings him opposite to the actual picture of a man on horseback? To put the case briefly, if the idea of movement is not in the agent's mind, one can only attach psychical significance to the frequent selection of a wrong object which resembles the right one, by making two suppositions : (1) that the telepathic fact, which produces a physiological effect in the percipient apart from his consciousness, may be pictorial, not volitional—statical, not dynamical—in character ; and (2) that the effect is produced only after a sort of parley has taken place between the conscious and unconscious parts of the percipient's mind. That these suppositions are not mere speculative curiosities will be plain when we proceed, as it is now time to do, to the last and most important division of M. Richet's work.

In the final group of experiments the place of the flexible wand was taken by a table : and M. Richet prefaces his account by a succinct statement of the scientific view as to "table turning." Rejecting altogether the three theories which attribute the phenomena to wholesale fraud, to spirits, and to an unknown force, he regards the gyrations and oscillations of séance-tables as due wholly to the unconscious muscular contractions of the sitters. It thus occurred to him to employ a table as an indicator of the movements that might be produced, by "mental suggestion." The plan of the experiments was admirably conceived. Three persons (C, D, and E) took their seats in the semi-circle at a little table on which their hands rested. One of these three was always a "medium"—that is to say, a person liable to exhibit intelligent movements in which consciousness and will took no part. Attached to the table was a simple electrical apparatus, the effect of which was to ring a bell whenever the current was broken by the tilting of the table. Behind the backs of the sitters at the table was another table, on which was a large alphabet, completely screened from the view of C, D, and E, even had they turned round and endeavoured to see it. In front of this alphabet sat A, whose duty was to follow the letters slowly and steadily with a pen, returning at once to the beginning as soon as he arrived at the end. At A's side sat B, with a note-book ; his duty was to write down the letter at which A's pen happened to be pointing whenever the bell rang. Things being arranged thus, the three sitters at the first table engage in conversation, sing, or tell stories; but at intervals the table tilts, the bell rings, and B writes down the letter which A's pen is opposite to at that moment. Now, to the astonishment of all concerned, these letters, when arranged in a series, turn out to produce rational words and phrases. M. Richet has not given us any examples of these words and phrases in his present paper; but he promises a future one, in which the omission will probably be made good. Meanwhile, how is such a phenomonen to be regarded? To whom or to what is it due? A and B are mere automata. C, D, and E are little more, being unconscious of tilting the table, which appears to them to tilt itself; but even if they tilted it consciously, and with a conscious desire to dictate words, they could not possibly succeed. For they have no means of ascertaining at what letter the pen is pointing at any particular moment, and they might thus tilt for ever without producing more than an endless series of incoherent letters, or at best meaningless syllables. It is surely hard to imagine a more convincing display of the *unconscious intelligence* on which M. Richet insists.

This phrase is no doubt a somewhat equivocal one, and it is necessary to know in every case exactly what is meant by it. It may be used in a purely *physical* sense—to describe the unconscious cerebral processes whereby actions are produced which as a rule are held to imply conscious intelligence; as, for instance, when complicated movements, once performed with thought and effort, gradually become mechanical. But it may be used also—as M. Richet seems to use it—to describe *psychical* processes which are severed from the main conscious current of an individual's life. Unconsciousness in any further sense it would be rash to assert; for intelligent psychic process without consciousness of *some* sort, if not a contradiction in terms, is at any rate something as impossible to imagine as a fourth dimension in space. The events in question are outside the individual's consciousness, as the events in another person's consciousness are; but they differ from these last, in that there is no continuous stream of conscious life to which they belong; and no one, therefore, can give an account of them as belonging to a self. They are essentially fragmentary, unappropriated, and inarticulate; and they can only be inferred to exist from certain sensible effects to which they lead.

But it may be asked what right we have to make any such inference; since, *à la rigueur*, the effects, being sensible and physical, do not require us to suppose that they had any other than *physical* antecedents. It is true that it is impossible to *demonstrate* that the physical antecedents, which undoubtedly exist, have any psychical correlative: but the dogmatic assertion that they have *not* loses much of its force from two considerations: (1) Analogy fails. The results in question are often wholly dissimilar to the effects of practice, and the various other automatic actions which may be fairly attributed to "unconscious cerebration": they are *new* results, of a sort which has in all our experience been preceded by intention and reflection. I advance this view with some hesitation; but we shall find considerable justification for it in some of M. Richet's experiments. (2) The undoubted phenomena of what has been called "double consciousness" have familiarised us with the idea of a *double* psychical life connected with a *single* organism. In those cases the two lives which know nothing of each other are *successive*; but no new difficulty is introduced by conceiving fragments of such mutually exclusive existences as *simultaneous.**

However, it is not with the proof of "unconscious intelligence," but of "mental suggestion," that we are here more immediately concerned. Subject to the above explanation, we may accept M. Richet's view on the former question, and confine ourselves to the bearing of his experiments on the latter. And in the case already described, we have already a pretty plain indication that something of the nature of "mental suggestion" was at work. For to whatever source we ascribe the rational words and phrases which B.'s note-book records, it is impossible not to admit that the idea of the letters necessary for their formation had an existence, though an unconscious one, in the mind or brain of one or more of the three sitters at the tilting table. The letters had at any rate the same existence for them as those written by the *planchette*-writer have for him, when he produces a string of words and sentences without knowing what they are. But the peculiarity of the present case is that the letters could only obtain this existence in the mind or brain of the "medium," (C, D, or E) *through* a "mental suggestion" from A. No one but A (or possibly B) knew the moment at which alone the next letter required could be caught and fixed; and the "medium's" involuntary seizure of that critical moment to tilt the table cannot possibly be disconnected from this knowledge in A's mind. In M. Richet's words, "The *unconscious person* in the 'medium'—whose conscious Ego is thinking of something else—follows mentally, with rigorous precision, the movements of the other who points to the successive letters."

* These suggestions of "unconscious intelligence" are drawn from facts observed in connection with a single individual. But they may be reinforced by a consideration drawn from the phenomena of telepathy. It has been more than once pointed out in our *Proceedings* that, while a coherent account can be given of telepathic phenomena on purely *psychical* ground, there are peculiar difficulties in conceiving any complete *physical* account of them. Now this remains just as true when the percipient is unconscious of the "mental suggestion" as when he is conscious of it. This being so, it seems specially arbitrary and unnecessary to insist on confining such phenomena to the physical plane—the very place where the difficulties are thickest—and to deny their psychical existence, in respect of which our ideas of them can at any rate be made orderly and consistent.

But the experiment was carried to a much further point than this ; and in its further development the calculus of probabilities is again turned to most striking account. A sixth operator (whom we will call F) is introduced, who stands apart both from the tilting table and from the alphabet ; and *his* thought is now the source of the "mental suggestions" which lead to the same connected series of events as before—the tilting of the table, the ringing of the bell, the fixing of successive letters, and the gradual formation of rational words in B's note-book.

For the sake of comparing the results with those which pure accident would give, M. Richet first considers some cases of the latter sort. He writes the word NAPOLEON ; he then takes a box containing a number of letters, and makes 8 draws ; the 8 letters, in the order of drawing, turn out to be U P M T D E Y V. He then places this set below the other, thus :—

N A P O L E O N
U P M T D E Y V

Assuming the number of letters in the alphabet to be 24, the probability of the correspondence of any letter in the lower line with the letter immediately above it is, of course, $\frac{1}{24}$; and the most probable number ~~probability~~ that in the whole series there will most be one ~~such~~ success ~~is $\frac{8}{24}$ or $\frac{1}{3}$~~. If we reckon as a success any case where the letter in the lower line corresponds not only with the letter above it, but with either of the neighbours of that letter in the alphabet* (*e.g.*, where L has above it either K, L, or M), then the chance of a single success in the series is three times greater than before ; that is, it is 1. In the actual result, it will be seen, there is just 1 success, which happens to be a complete one—the letter E in the sixth place. It will not be necessary to quote other instances. Suffice it to say that the total result, of trials involving the use of 64 letters, gives 3 exact correspondences, while the probable number was 2.7 ; and 7 correspondences of the other type, while the probable number was 8. Thus even in this short set of trials, where accident had full scope, the experimental result very nearly coincides with the strict theoretic number.

We are now in a position to appreciate the results obtained when

* This procedure of counting neighbouring letters seems to require some justification. It might be justified by the difficulty, on the theory of mental suggestion, of obtaining an *exact* coincidence of time between the tilting and the pointing. But I think that M. Richet does justify it (p. 654) by references to the other more striking experiences of which he has not here given us any examples, where neighbouring letters also appeared, but where there seems to have been no room for doubt, in the reader's mind, as to what the letter should have been.

the factor of "mental suggestion" was introduced. In the first experiments made, M. Richet, standing apart both from the table and from the alphabet, selected from Littré's dictionary a line of poetry which was unknown to his friends, and asked the name of the author. The letters obtained by the process above described were J F A R D ; and there the tilting stopped. After M. Richet's friends had puzzled in vain over this answer, he informed them that the author of the lines was Racine; and juxtaposition of the letters thus—

J F A R D
J E A N R

shows that the actual number of successes (of the type where neighbouring letters are reckoned) was 3, the probable number being only $\frac{5}{8}$. One would be glad to know whether M. Richet was actually concentrating his thought on the author's Christian name. Even if he was not, it probably had a sub-conscious place in his mind, which might sufficiently account for its appearance. At the same time, accident has of course a wider scope when there is more than one result that would be allowed as successful.

It is, of course, better—with the view of making sure that F's mind, if any, is the operative one—not to ask a question of which the answer might possibly at some time have been within the knowledge of the sitters at the table ; and in the subsequent experiments the name was silently fixed on by F. Two more specimens may suffice.

Name thought of: D O R E M O N D
Letters produced: E P J Y E I O D.

Here the probable number of successes (of the type whose neighbouring letters are reckoned) was 1, and the actual number was 4.

Name thought of: C H E V A L O N.
Letters produced: C H E V A L.

Here the probable number of exact successes was $\frac{1}{4}$, and the actual number was 6.

Taking the sum of 8 trials, we find that the probable number of exact successes was a little over 2, and the actual number 14 ; and that the probable number of successes of the other type was 7, and the actual number 24. It was observed, moreover, that the correspondences were much more numerous in the *earlier* letters of each set than in the later ones. The first three letters of each set were as follows—

J F A—N E F—F O Q—H E N—C H E—E P J—C H E—A L L
J E A—L E G—E S T—H I G—D I E—D O R—C H E—Z K O

Here, out of 24 trials, the probable number of exact successes being 1, the actual number is 8 ; the probable number of successes of the other type being 3, the actual number is 17. The figures become still more striking if we regard certain consecutive series in the results. Thus the probability of the 3 consecutive correspondences in the first experiment here quoted was $\frac{1}{512}$; and that of the 6 consecutive correspondences in the last experiment quoted was about $\frac{1}{160,000,000}$.

And now follows a very interesting observation. In some cases, after the result was obtained, subsequent trials were made *with the same word*, which of course the agent did not reveal in the meantime; and the amount of success was sometimes markedly increased on these subsequent trials.

Thus, when the name thought of was				D O R E M O N D,
the letters produced on the first trial were				E P J Y E I O D.
,,	,,	second	,,	E P F E I.
,,	,,	third	,,	E P S E R.
,,	,,	fourth	,,	D O R E M I O D.

Summing up these four trials, the probable number of exact successes was just over 1, and the actual number was 9; the probable number of successes of the other type was just over 3, and the actual number was 18. The probability of the 5 consecutive successes in the last trial was about $\frac{1}{8,000,000}$. The complete accuracy of this last fraction may, however, be called in question; since M. Richet, who was the agent, tells us that he had imagined the name as spelt thus—d'Ormont. Even so, however, the amount of success is very large; and the admission introduces a new point of interest. It recalls to mind the numerous cases where automatic writers, or table-tilters, have obtained responses with mistakes or peculiarities of spelling, sometimes persistently repeated, to which their conscious selves were in no way prone. And if the analogy be a just one, then in M. Richet's experiments one might discover a hint of an "unconscious person" in the agent, as well as in the percipient.

The experiment was repeated 4 times in another form. A line of poetry was secretly and silently written down by the agent, with the omission of a single letter. He then asked what the omitted letter was; it was correctly produced in every one of the four trials. The probability of such a result was less than $\frac{1}{300,000}$.

A result of a different kind was the following, which is specially noteworthy as due to the agency of an idea that was itself on the verge of the unconscious. M. Richet chose a quotation at random from Littré's dictionary, and asked for the name of the author, which was Legouvé. The letters produced were J O S E P H C H D, which looked like a complete failure. But the quotation in the dictionary was adjacent to another from the works of Joseph Chénier; and M. Richet's eye, in running over the page, had certainly encountered the latter name, which had probably retained a certain low place in his consciousness. Another very interesting case of a result unintended by the agent, though due to something in his mind, was this. The name thought of was Victor: the letters produced on three trials were

D A L E N
D A M E S
D A N D S

seemingly complete failures. But it appeared that while the agent had been concentrating his thoughts on "Victor," the name of a friend, Danet, had spontaneously recurred to his memory. We should, of course, be greatly extending the chances of *accidental* success, if we reckoned collocations of letters as successful on the ground of their resemblance to any one of the names or words which may have momentarily found their way into the agent's mind while the experiment was in progress. Here, however, the name seems to have suggested itself with considerable persistence, and the resemblance is very close. And if the result may fairly be attributed to "mental suggestion," then, of the two names which had a certain lodgment in the agent's mind, the one intended to be effective was ineffective, and *vice versâ*.

These latter results have very great theoretic importance. It is to be remembered that we found a certain difficulty in conceiving the telepathic production of movements by what is at most an idea, and not a volition, on the agent's part. But it is now difficult to resist the evidence that this is what actually has taken place; and even that the very idea may be below the threshold of conscious attention. Nor is it only in these exceptional cases that this startling hypothesis has to be faced. For we must remember that in a sense A is throughout more immediately the agent than F; it is what A's mind contributes, not what F's mind contributes, that produces the tilts at the right moments. But this is of course through no *will* of A's; he is ignorant of the required word, and has absolutely no opportunity of bringing his volition into play. His "agency" is of a wholly passive sort; and his mind, as it follows the course of his pen, is a mere conduit-pipe, whereby knowledge of a certain kind obtains access to the "unconscious intelligence" of the "medium." If, then, the knowledge manifests itself as impulse, can we avoid the conclusion that in this particular mode of access—in "mental suggestion" as such —a certain *impulsive* quality is involved?

It may perhaps be objected that F might produce the movements, in a similar way to that proposed above *à propos* of the cases where an object *resembling* the right one was divined. It was there suggested that in the agent's mind the idea of the *object* had been combined with the idea of the *movement*, and that this complex idea was what was transferred and what ultimately took effect. This explanation, if sound, might possibly be extended to the far more complicated cases where—as in the word-experiments—there are as many objects as there are letters; and where, further, the actual perception of the resembling object, when the percipient comes opposite to it in a row, is replaced by an unconscious image of it received from a third mind. But it is surely hard to include under the notion cases where a word is produced which, though latent in F's mind, has no resemblance to the word whose production he is willing. The transference of the idea of the latent word, even to the exclusion of the right word, can be quite conceived; but can we suppose that, sub-consciously or unconsciously, an idea of *movement* was combined with the idea of its letters in the agent's mind, at the very moment when that on which his attention was fixed, and with which *ex hypothesi* the conscious idea of movement *was* connected, was a quite different set of letters? Can we suppose that the idea of movement overflowed into the unconscious region of his mind, and there on its own account formed an alliance with alien elements, the effect of which on the percipient would prevent the effect intended? It must be remembered that where a word which is not the one intended gets transferred from F to the "medium," there is no knowledge, conscious or unconscious, on F's part, as to what that word will be. A number of words are latent in his mind; one of these finds an echo in another mind. But how should the idea of movement find out which particular one, out of all the words, is destined thus to find an echo, so as to associate itself with *its* letters and no others? And if we suppose the association to be between the unconscious idea of movement and the unconscious idea of *letters in general*, this is no less dissimilar and opposed to anything that the conscious part of F's mind has conceived. For it is not in letters as such, but in the exclusive constituents of a particular word, that he is interested; if indeed he is interested in anything beyond the word as a whole. The difficulty here seems yet further to justify the suggestion—with which I imagine that M. Richet would agree—that the physiological impulse does not depend on any idea of *movement*, or any special direction of the agent's will to *that* result. This might be tested, if F were a person ignorant of the form of the experiment, and out of sight of the table.

But of course the relation between F and the "medium" does play a necessary part in the result; the impulse to tilt when a particular letter is reached only takes effect when it falls (so to speak) on ground prepared by "mental suggestion" from F—on a mind in which the word imagined by him has obtained an unconscious lodgment. And whereas we spoke above of "a parley between the conscious and unconscious parts of the percipient's mind," we shall now have to conceive the *unconscious* part, if not as dual, as at any rate the scene of confluence of two separate streams of influence, which proceed to combine there in an intelligent way—one proceeding from F's mind, which produces unconscious knowledge of the word, and the other proceeding from A's mind, which produces an unconscious image of the successive letters. Such a conception seems to support what I said above, as to results of the supposed "unconscious intelligence" which go beyond the received results of mere unconscious cerebration. Unconscious cerebration is amply competent to produce such seemingly intelligent actions as ordinary writing; but what is now done more resembles the formation of a word by picking letters from a heap, or type-writing by a person who is unused to his instrument. The process is not one in which every item is connected by long-standing association with the one before and after it; every item is independent, and implies the recognition, at an uncertain moment, of a particular relation—that between the next letter required for the word and the same letter in its place in a quite distinct series. There is, no doubt, an alternative to the hypothesis just suggested; we might suppose that F's thought affects, not the "medium," but A, or conversely, that A's thought affects, not the "medium," but F; that A obtains unconscious knowledge of the word, or that F obtains unconscious knowledge of the letter, and so is enabled to communicate an impulse to the "medium" at the right moment. But we should then have to suppose a secret understanding between two parts of A's or F's mind, the part which takes account of the letters of the alphabet, and the part which takes account of the letters of the word—the former being conscious and the latter unconscious, or *vice versâ*, according as A or F is the party affected.*

One hesitates to launch oneself on such conceptions; but the only alternative would be to question the facts from an evidential point of view. So regarded, they are of an extremely simple kind; and if their genuineness be granted, we are reft once and for all from our old psychological moorings. The whole question of the psychical constitution of man is opened to its furthest depths; and our central conception—telepathy—the interest of which, even in its simpler phases, seemed almost unsurpassable, takes on an interest of a wholly unlooked-for kind. For it now appears as an all-important method or instrument for testing the mind in its hidden parts, and for measuring its unconscious operations.

M. Richet holds out hopes that on a future occasion he may return to this aspect of the subject, and treat the problems on a more extended scale. We ask for nothing better. Meanwhile, I cannot withhold an opinion that he decidedly underrates the general effect of what he has

* There is yet one other conceivable view of the process which, in spite of its desperate improbability, should not be left out of account—namely, that the "medium" perceives the movements of the pointer by unconscious *clairvoyance*. It would be worth while to try whether A is indispensable, and whether similar results could be obtained when the pointer is moved by mechanism.

so far accomplished. He concludes his account by a synopsis of his numerical results, as measured in three ways;—by the *actual number* of successes in the several series; by various instances of several *consecutive* successes; and by the *probability* of the amount of success in the more striking cases, which is represented by very small fractions.* Summing up from these data, though purposely keeping below the mark, he suggests that "the probability in favour of the reality of mental suggestion may be represented by $\frac{2}{3}$"; that is to say, if he were compelled to decide one way or the other, under pain of death if he were wrong, he would allow chance to decide for him in this way—that he would put two white balls and one black ball into a bag, and then draw at random; if he drew a white ball, he would declare for the reality of "mental suggestion"; if he drew the black one, he would declare against it. One cannot but think that if it came to the pinch, he would make his reckoning differently. Exception was taken above to interpreting a result which indicates the most probable efficiency of mental suggestion, *if* it exists, as the measure of the probability of its existence†; and with an increased number of trials, the illegitimacy of such a proceeding becomes still more manifest. Whatever be the number of trials, the *efficiency* of the new cause, as calculated from a certain ratio of the actual to the probable number of successes, will be the same; but if this ratio remains undiminished with a continually increasing number of trials, the probability of the *existence* of the new cause continually increases. Otherwise, results might go on for ever deviating in the same direction from the probable results of accident, and yet without producing even approximate certainty that anything beyond accident was at work. Suppose that a pair of dice are thrown a billion times in succession; and that the total result shows that half the total number of throws have been sixes. We should, of course, conclude that the dice were unevenly weighted; but on M. Richet's mode of reckoning, such a conclusion would only amount to a considerable probability, on which no sane man would willingly stake anything he cared about. In so extreme a case, to estimate the probability as falling appreciably short of certainty, would argue, not an excess of caution, but ignorance of a fundamental principle. M. Richet's estimate of the probability of "mental suggestion" is not, of

* These fractions are not absolutely correct, for the reason pointed out on p. 243. But the correct fractions would also be very small. The improbability of the *runs* of success is also overstated. Allowance should be made for the fact that they mostly occurred as part of a longer series; *e.g.*, the improbability of obtaining 5 successes running is less in a series of 10 than in a series of 5.

† This is practically what M. Richet has done in the present case. For he gives the fraction $\frac{2}{3}$ as approximately expressing the excess of the actual number of successes over the probable number, divided by the total number of the trials.

course, open to such a charge as that; but still he certainly seems to have shown caution of statement in the wrong place. Granted the *genuineness* of the results—granted, that is, the *bona fides* of all concerned and the perfection of the conditions—the probability that "mental suggestion" was at work, or in other words, the improbability that the results were purely accidental, is, when we consider the multitude of trials, something enormous. If it falls appreciably short of certainty—as to which opinions might differ—it still cannot possibly be represented by such a proportion as 2 to 1. So much may be stated boldly. The place for caution is rather in estimating the chances of a flaw in the conditions; and though speaking on this point with considerable confidence, M. Richet puts in more than once a judicious note of warning. He insists that the experiments must be repeated; and the importance of this cannot be too strongly urged. Even if the experimenter's assurance as to the perfection of his conditions be complete, it is in the nature of things impossible that strangers, who only read and have not seen, should be infected with it. It is on this side, then, that the case for "mental suggestion" needs fortifying; and the fortification (as has been frequently pointed out in these *Proceedings*,) can only consist in spreading the responsibility—in multiplying the number of persons, reputed honest and intelligent, who must be either knaves or idiots if the alleged transference of thought took place through any hitherto recognised channels.

It is needless to remark in conclusion that, such being the means by which an overwhelming proof may in time be attained, M. Richet's investigations form a most important step towards its attainment. His paper can hardly fail to be a permanent landmark in the slowly widening domain of psychical discovery.

NOTE.

On the very eve of our going to press, I have received the following communication from Professor Lodge and Mr. Alfred Lodge:—

Liverpool, January 8th, 1885.

DEAR GURNEY,—In cogitating over your remarks anent the valuable suggestion of M. Richet, that feeble thought-reading powers or slight mental reverberation may be possibly detected in most persons by applying the laws of probability to a great number of guesses made by them, at a limited series of objects, while under the influence (real or imaginary) of another mind strongly cognisant of these objects in random succession; we have emancipated ourselves from the fallacy under which M. Richet appears to labour, and which you have already detected, and think that a statement of our present position may be not uninteresting.

The evidence in favour of an unknown law, which can be legitimately deduced from a definite departure from probability in a large number of cases, is an important matter in connection with such experiments, and it is by no means obvious what such evidence really is.

Thus M. Richet finds, as the result of a number of series of guesses, that the number of successes actually attained was in a large majority of cases above the theoretically probable number; and he sums up, as the cumulative evidence of the whole number, an extraordinarily great probability in favour of law, in an illegitimate manner. The inference to be drawn from a number of series of varying success will seldom be greater than that to be drawn from the most remarkable of those series;* but, indeed, no single unsupported series of a few trials can afford any legitimate inference at all.

The whole validity of the method depends on a great number of trials being made, and before any short series can be used as an argument it must be confirmed again and again by repetition. But the division of the whole number of trials into series of so many guesses each is unnecessary and deceptive.† All the series, or at any rate all

* This may no doubt be true, if "varying success" be taken to include results where the number of right guesses is *below* as well as *above* the theoretically probable number. But suppose that a die has been thrown 600 times, in 100 batches of 6, and that in each of these batches the ace has appeared either 2 or 3 times, except in the first batch, where it appeared 4 times. The presumption that the die is unevenly weighted is of course stronger at the end of the 600 throws than it was at the end of the first 6. And this is a sort of illustration which M. Richet might perhaps consider fairly justified by his results.

† Supposing the odds against each individual success to be constant throughout (which was not the case in M. Richet's sets of trials), the division into series is, of course, irrelevant to the calculation of the *à priori* probability that chance will produce a certain total of successes. But when we look at certain results obtained, with a view to discovering the probability that something beyond chance has acted, the mode of distribution may sometimes have a real importance. For, supposing a *vera causa* to exist, by which the actual number of right guesses is made to exceed the probable number, there is no *à priori* ground for assuming that it would act certainly or uniformly. Suppose that 1,000 guesses at the suit of a card are made on M. Richet's plan, in batches of 200 each, on five successive days; and suppose that all the 200 guesses are right on the first day, and that 50 more right guesses are dispersed over the other days,—the total number of right guesses, 250, will then not exceed the

those where the probability of success at each individual guess was the same, constitute one large series, which is only available for legitimate inference if it consist of a sufficient number of trials.

This expression "sufficient number" is, however, only susceptible of subjective and psychological definition; rigidly, an infinite number would be necessary for perfect inference, but practically a large number serves.

The result at which we have arrived may thus be stated for practical purposes.

Let a series of m guesses be made at a set of n things, and let r of these guesses come right. The inference to be drawn is, that the probability in favour of law or bias, as opposed to pure chance, is,

$$1-\left(\frac{2m-r-k+1}{(n-1)(k+r+1)}\right)^{r-k}$$

where k stands for the integer just smaller than $\frac{m+1}{n}$. Thus, to take an example: let the objects of guess be the nine digits, and let 100 guesses be made, of which 20 turn out right. The value of the above letters is $n=9$, $m=100$, $r=20$, $k=11$; and the probability of bias, or action of Thought-transference instead of mere guessing, is about $\frac{39}{40}$. The outlying possibility of pure chance being only $\frac{1}{40}$. This is still a long way from certainty, and more trials must be made.

The investigation is as follows:—

At every trial let there be n possible guesses, of which b are to be considered right and the rest wrong; what is the chance of guessing right r times in m trials?

The answer is the $(r+1)^{th}$ term of the expansion $\left(\frac{n-b}{n}+\frac{b}{n}\right)^m$, namely:—

$$\frac{\underline{|m}}{\underline{|r}\ \underline{|m-r}}\cdot\left(\frac{n-b}{n}\right)^{m-r}\cdot\left(\frac{b}{n}\right)^r.$$

But it is usually unnecessary to consider b as anything but unity,

probable number; but would it be unreasonable to surmise that on the first day something beyond chance had been at work? From this point of view it is clear that there may be cases where the division into series—a division which accords with actual facts, and is no arbitrary manipulation of figures—cannot be accounted wholly deceptive.

for only one of the possible guesses is generally correct. We can therefore re-write the formula conveniently thus

$$\frac{\underline{|m} \cdot \left(\frac{n-1}{n}\right)^m}{\underline{|r}\,\underline{|m-r}\,(n-1)^r},$$

where it will be observed that r occurs only in the denominator.

The chance of guessing wrong every time is $\left(\frac{n-1}{n}\right)^m$.

The chance of guessing right every time is $\frac{1}{n^m}$.

The event which is most probable, or that for which the probability is a maximum, depends upon how many trials are made—that is upon $m : n$.

If fewer than $n-1$ trials are made, the most probable event is that all the guesses will be wrong.

In a set of $n-1$ trials it is an even chance whether all be wrong or one right.

If more than $n-1$ but fewer than $2n-1$ trials are made, it is most likely that one of the guesses will be right.

If more than $2n-1$ but fewer than $3n-1$ guesses are made, two of them will be most probably correct.

And in general if $\frac{m+1}{n}$ lies between two consecutive integers, k and $k+1$, the most probable event is to succeed k times in the m guesses. The chance of this event actually happening is

$$\frac{\underline{|m} \cdot \left(\frac{n-1}{n}\right)^m}{\underline{|k}\quad\underline{|m-k}\quad(n-1)^k} = \frac{1}{p} \text{ say,}$$

The chance of an actually observed event, viz., r successes, is

$$\frac{\underline{|m}\ \left(\frac{n-1}{n}\right)^m}{\underline{|r}\quad\underline{|m-r}\quad(n-1)^r} = \frac{1}{q} \text{ say.}$$

Now, in estimating what is the legitimate inference that can be drawn from an enormous number of trials in favour of the action of some unknown law, as opposed to mere chance, we must compare these two probabilities; that is, we must find the *relative* probability of the observed event as compared with that of the event most likely to happen on the hypothesis of pure chance. For if mere chance rules, the most probable event is bound to happen in an enormous number of trials, whereas if there be a bias some other event will happen, whose relative probability is $\frac{p}{q}$. The probability in favour of mere chance is therefore $\frac{p}{q}$, and the probability of a disturbing cause is $1-\frac{p}{q}$; or the betting odds in favour of some bias or interference with chance are $\frac{q-p}{p}$.

The value of $\frac{p}{q}$ is

$$\frac{\underline{|k}\ \underline{|m-k}\ .\ (n-1)^k}{\underline{|r}\ \underline{|m-r}\ .\ (n-1)^r}\text{, which reduces to }\frac{(m-r+1)\,(m-r+2).....(m-k)}{(k+1)\,(k+2)....r\ .\ (n-1)^{r-k}}.$$

When m is very large, it is a long business to find the numerical value of this expression, and an approximation may be conveniently used instead. The following is deduced on the principle of using the arithmetic mean of a series of numbers instead of the geometric mean, and for all practical purposes it is sufficiently close to the true value:

$$\frac{p}{q}=\left(\frac{2m-r-k+1}{(n-1)\,(r+k+1)}\right)^{r-k}.$$

This is easily calculated, and it represents the outstanding probability in favour of pure chance very nearly.

We will recapitulate the meaning of the symbols.

m means the total number of guesses made, all under proper conditions;

n means the number of objects, or specified things, to be guessed at;

k means the most likely number of successes or right guesses on the hypothesis of mere chance (it is the integral part of $\frac{m+1}{n}$);

r means the number of successes or right guesses actually made.

It may be well to illustrate by a simple example. Let the things to be guessed be the four suits of cards, and let 32 guesses be made, 13 of them being right, the rest wrong. What probability of bias can be deduced from such a set as this?

The most likely number of successes in this case is 8 (since $\frac{m+1}{n}$ is $\frac{33}{4}$), and so we have to put in the above formula $k=8$,

$r = 13$, $n = 4$, $m = 32$; with the result that the probability of mere chance is rigidly $\frac{1288}{6477}$, or from the approximate formula $\frac{32}{243}$, or say $\frac{1}{8}$ roughly. The probability of some law is therefore $\frac{7}{8}$, and the odds in favour of it are 7 to 1.

It is unnecessary to know the separate value of p and q; they can be calculated from formulæ given above; but the arithmetic is in most cases rather long.

Another mode of writing the approximate formula is perhaps just worth recording. Let kn trials be made, and let s be the excess of the actual number of successes over the probable number, that is, let $s = r - k$; then the probability of bias is

$$1 - \left(\frac{2k - \frac{s-1}{n-1}}{2k + s + 1} \right)^{s}.$$

Calling the ratio of $s : k$, t, and taking k large, this becomes

$$1 - \left(\frac{2 - \frac{t}{n-1}}{2+t} \right)^{s},$$

which shows that if $\frac{r-k}{k}$ tends to diminish as the number of trials is increased, the result of an infinite number of trials would be completely in favour of mere chance; whereas, if $\frac{r-k}{k}$ remains constant, or shows a tendency to increase with the number of trials made, the fact is strongly indicative of law.—We are, yours sincerely,

OLIVER J. LODGE.
ALFRED LODGE.

The problem of discovering from the results of a number of series of trials, varying in length and kind as M. Richet's did, what is the probability that some cause beyond chance has acted, should surely admit of a theoretic solution based on this simple proposition :—

An event occurs which must be due to A or B.

The *à priori* probability of the existence of A is P; and the *à priori* probability that, if A exists, such a result will follow is p.

The *à priori* probability of the existence of B is Q; and the *à priori* probability that, if B exists, such a result will follow is q.

Then, on any occasion when the event has happened, the *à posteriori* probability that it was due to A is .... $\frac{Pp}{Pp+Qq}$,
and the *à posteriori* probability that it was due to B is $\frac{Qq}{Pp+Qq}$.

The following is an attempt at such a solution :—

The result of any particular series represents a certain degree of success; and the attainment of at least that degree of success must be due either to chance alone, or to chance *plus* some other cause, which we will call θ, the amount of whose efficiency is unknown.

The probability of obtaining at least that degree of success is

(1) if chance alone acts, (say) q,
(2) if chance $+\theta$ acts .... $\frac{1}{2}$,

because in our ignorance of the efficiency of θ, we must suppose it as likely to bring the degree of success up to that point as not to do so.

Now we cannot proceed further without assigning some value, as small as we please, to the *à priori* probability that θ acts. Let this probability $= x$: then the *à priori* probability that chance alone acts $=1-x$.

Taking now the result of a particular series, the *à posteriori* probability that at least that degree of success has been obtained by chance alone is $\frac{q(1-x)}{q(1-x)+\frac{1}{2}x}$, or $\frac{2q(1-x)}{2q(1-x)+x}$;

and the *à posteriori* probability that at least that degree of success has been obtained by chance $+\theta$ is $\frac{\frac{1}{2}x}{q(1-x)+\frac{1}{2}x}$, or $\frac{x}{2q(1-x)+x}$,

which is greater than x if q is less than $\frac{1}{2}$.

This latter fraction must be taken as a first approximation to the probability that θ has acted. The expression may now be used instead of x, to deduce in a similar way the probability that θ has acted in a *second* series; and the new expression so obtained may in its turn be used in application to a *third* series; and so on. The successive expressions will have a constantly increasing value as truly representative of the probability that θ has acted, in that they will depend on continually widening experimental results.

Now if θ_1, θ_2, θ_3, &c., represent the successive probabilities that θ has acted, obtained from a series of experiments in which q (as above defined) is represented by q_1, q_2, q_3, &c., successively, then

$$\theta_1 = \frac{x}{x+2q_1(1-x)}\ ,\quad \theta_2 = \frac{\dfrac{x}{x+2q_1(1-x)}}{\dfrac{x}{x+2q_1(1-x)}+2q_2\left((1-\dfrac{x}{x+2q_1(1-x)}\right)} = \frac{x}{x+4q_1\,q_2\,(1-x)}$$

θ_3 in the same way $=\dfrac{x}{x+8q_1\,q_2\,q_3\,(1-x)}$ and generally

$$\theta_n = \frac{x}{x+2^n q_1\,q_2 \cdots\cdots q_n(1-x)}\ .$$

The final value of θ_n is independent of the order in which the results of the experiments are used.

It will be seen that, if the representative of q in any series is $\frac{1}{2}$ exactly, the probability for the action of θ will be unaffected by that series; and that if the representative of q is $\frac{1}{2}$ throughout, the *à posteriori* probability for the action of θ will be the same as the *à priori* probability—*i.e.*, will be $= x$ throughout. The illustration of the urns (p. 243), on the other hand, would give an immense *à posteriori* probability for the action of θ, even where the representative of q was $\frac{1}{2}$ throughout; in other words, it would give an immense improbability for the exclusive action of chance, even where the result of every series represented no more than the degree of success which the exclusive action of chance was as likely as not to attain. To represent these conditions, each urn would have to contain one white ball and one black one, and the illustration would require all the white balls to be drawn; that is to say, a total amount of success which corresponded with ideal probability would be illustrated by a total amount which was the furthest possible departure from ideal probability. The probability of drawing all the white balls, $\frac{1}{2^n}$, expresses the probability of obtaining, in the sets of trials, not the total amount of success supposed, but that one particular distribution of it.

E. G.

IV.

THE PROBLEMS OF HYPNOTISM.*

By Edmund Gurney.

Of all physical analogies, the one which most constantly suggests itself outside the limits of the physical universe is that of the pendulum. Alike in our sensory experience where excitement leads on to fatigue and satiety begets aversion, and in the wider domains of religion and politics where movements and opinions so constantly tend to one extreme by a mere impetus of repulsion from the other, the rhythmic law of action and reaction is ever at work. But sensation and sentiment by no means exhaust the region to which these further applications of the law extend: we find it operating where it would least be sought, and invading the passionless paths of science herself. The characteristic instinct of the scientific spirit is, of course, to *simplify* and *unify*; as science advances, theories of a multitude of separate agents, whether personal deities or abstract faculties, gradually give way to the recognition of large general laws. But if in the main this tendency towards unity and simplification brings nothing but good, it is inevitable that an end in itself so eminently conducive to intellectual peace and satisfaction should also act as a temptation—that in yielding to the generalising instinct the mind should sometimes be swayed too fast and too far, and so be landed in premature hypotheses. And thus it is that, even as the old pre-scientific speculation sought a transcendental unity of things in such principles as water or fire, so even advanced science may occasionally do injustice to the immense variety of Nature, and, in the determination to formulate a law for some special department of facts, may seek and observe too exclusively the facts which can be made to square with the law. It would be hard to find a better instance of such over-simplification than is afforded by the modern science of Hypnotism. For so short a span of existence, few sciences can have been so prolific in theories, presented often concurrently, and with little attempt at mutual refutation; and the time has perhaps come when the experimental knowledge of the subject which is so rapidly advancing may be usefully supplemented by a brief critical review of its theoretic vicissitudes. If such a review reveals how divergent have been the various paths which speculation has taken, and how one after another

*In the endeavour to give some sort of completeness to the following sketch, it has been found necessary, here and there, to resurvey ground which has already been to some extent traversed in the Reports of the Committee on Mesmerism.

of them seems to leave this or that set of facts on one side, it may at least aid in defining the problems that actually remain.

The facts of hypnotism, it is needless to say, first became prominent in connection with theories which science has with one voice rejected, finding nothing therein but absurd personal pretensions, and an ignorant jargon about forces and fluids. The facts themselves, however, were too indisputable and extraordinary to be neglected; and the first and most comprehensive theory of them, advanced in opposition to those of "mesmeric" influence and "odic" emanations, was that of *suggestion* and *imagination.* The singular mental phenomena which followed "mesmeric" manipulations were ascribed to a temporary suspension of the "subject's" independent powers of will and judgment, whereby both his beliefs and his conduct were left at the mercy of external suggestions. This was the theory crudely set forth exactly a century ago by the Commission of the French Academy of Sciences which was appointed to examine Mesmer's claims. Though presumably regarded by them as an explanation, it clearly contains no explanatory power whatever; it is simply a description (and, as we shall see later, a very imperfect one) of the particular mental condition which the "subject's" actions suggest. The crucial question remains: If the doctrine of a specific influence from the operator be rejected as outside the domain of natural law, what are the natural laws to which this peculiar mental condition can be referred?

This question remained for a long time without an answer; but two answers were at last given by countrymen of our own—one which, as far as it went, was of a clear and definite character, by Braid; the other, of a hazy and unexplanatory character, by Dr. Carpenter. This description may at first sight seem unjust to the latter, inasmuch as he professes general agreement with Braid, and does not seem aware of having adopted a different basis. It will not be hard, however, to justify what has been said.

Dr. Carpenter's explanation ("Mental Physiology," c. xiv.) rests purely on *mental* ground: his argument is concerned with states which (though, of course, like other mental states, they have their physical correlate in the nervous system) he treats throughout in their purely mental aspect. There could be no objection to this treatment, were it successful as far as it goes—the conditions of success obviously being that the phenomena of the mental state for which we seek explanation should be brought into relation with phenomena of other and more familiar mental states; for scientific explanation consists in bringing out identities between new and old knowledge. Dr. Carpenter's failure to realise this condition seems to me to be complete. The region where he seeks the needed identities is the well-recognised one of *reverie* and *abstraction;* and his endeavour is to embrace the phenomena of these familiar states with those of hypnotism in the common category of "automatic mental action." As an instance of the automatism of reverie, he describes the loose play of fancy to which the poet may resign himself under the influence of some pleasing aspect of Nature. To illustrate the automatism of abstraction, he describes the "absence of mind" which has characterised many clear and profound thinkers,—showing itself in their eccentric conduct in the streets, or in random answers to persons who have addressed them when their whole attention was absorbed in following some complicated train of logical thought. The reader will observe, even before we begin to test the resemblance of these "explanatory" phenomena to the unexplained facts of hypnotism, how confused and confusing the idea of automatism has already become. It is more than doubtful, to begin with, whether "automatism" correctly describes the poet's condition at all. As long as the idea of *will* is absent, "automatic" is an excellent word to describe actions, the conditions of which are inside and not outside the subject of them: such, for instance, is its appropriate meaning in physiology. But the mind is not a cell or a tissue; and, in the present connection, to call the mind's actions automatic, simply because it is taking its own path unsolicited from moment to moment by new sensory impressions, seems very misleading. "Automatism," if it is to serve Dr. Carpenter's purpose and to embrace hypnotic facts, must mean something quite distinct from spontaneous and unsolicited origination of ideas; it must mean nothing less than temporary paralysis of the directive power of the will; and there is nothing to warrant the assumption of this paralysis in the fact that the mind's action for the moment is unimpeded and effortless.

But even if we waive this objection and extend the meaning of "automatism" to cover what is properly expressed by *spontaneity*, the automatism in the described condition of the poet and that displayed by the absent-minded mathematician are surely so far from identity that they present an absolute contrast. The spontaneity or "automatism" of the poetic day-dreamer, in the sense of a free and aimless play of mind, belongs to the essence of his activity; so far as it is a correct description at all, it is a description covering the whole ground of what his mind is doing. The mathematician's mental activity, on the other hand, is just exactly *not* free and aimless, and just exactly *not* automatic. It is the most conscious and strenuously-directed effort, concentrated on successive points in an argument which it may require all the strength of his will to stick to and grapple with; and any automatism that he may display is a mere *accident* of this state, showing itself if external demands happen to solicit an attention which is already irresistibly set in one particular channel. The condition described as "automatic" in the case of the poet is charged with consciousness, which may be of the most vital and delightful kind; it *is*, in fact, itself the stream of consciousness in a particular aspect—*i.e.*, winding hither and thither in a roving and easy way. The automatism of the mathematician who does or says odd things while solving a problem, is essentially remote from the stream of his consciousness ; *that* is engrossed with other things, and his automatic sayings and doings are distinctly reflex actions, the result of suggestions which may never reach even the threshold of conscious perception.

Having thus observed the total dissimilarity, or rather opposition, of the two mental states whose fundamental characteristics Dr. Carpenter treats as identical, we shall not be much surprised to find that the hypnotic state, which he goes on to identify with them —in order by that means to obtain an expression for the less known in terms of the better known—is essentially distinct from either. The looseness of thought which has already made "automatism" cover two quite distinct things very easily extends it to a third equally distinct thing, which, being thus referred to a class, is so far—and all by the magic of a word—explained ! The automatism of the hypnotised "subject," in his response to external suggestions, is often automatism in a true sense ; in that respect differing *toto cælo* from those spontaneous or internally-originated impulses of fancy to which, in moments of random reverie, the poet's mind may give the rein. But it differs no less distinctly from the automatic or reflex words or actions with which we saw the absorbed mathematician responding to external impulses. For of those responses, as we observed, the essence was that they were unattended to, the stream of consciousness being rapt away in another direction ; while in the hypnotic case, consciousness and attention,* so far from being abstracted from the things which are being done in response to the external suggestion, are directed with even abnormal concentration upon those very things. We might without incorrectness describe the higher hypnotic phenomena as *reflex* action, in respect of the certainty with which particular movements follow on particular stimuli ; but they are, and their peculiarity consists in their being, *conscious* reflex action. The central problem of hypnotism lies in the *combination* of those two adjectives ; and in the following pages each of them will have to be emphasised in turn. The hypnotised "subject" who carries out complicated orders is a conscious, and often even a reckoning and planning, automaton. Reflex response (if we wish to retain the phrase) is here raised from the merely physical to the mental plane ; the external suggestion evokes a particular idea in as certain and as isolated a way as an appropriate electrical stimulus evokes the isolated action of one particular muscle. This isolation of a single object in the

* Consciousness and attention, that is, so far as they are present. The very varying degrees, and in many cases the indisputably high degree, in which they may be resent will be discussed a little later.

mind naturally implies abeyance of the normal controlling and relating power. In the normal state, successive vivid points of consciousness are surrounded by a swarm of subordinate perceptions and ideas, by reference to which it is that conduct is instinctively or subconsciously kept rational, even though the attention may be strongly focussed on its immediate aim or object. In the hypnotic state the contact is broken between the predominant idea and this attendant swarm; and conduct thus ceases to have reference to anything except the predominant idea. And the difference between that isolation of the dominating idea which is the cause of automatic answers and actions in the case of the absorbed mathematician, and the isolation of the dominating idea in the hypnotic automaton—though to a superficial observer the states seem similar just because each produces irrational actions—clearly goes to the very root of the phenomenon, regarded as mental. The mathematician has no fraction of attention to spare for external solicitations; his mind is in a state peculiarly impregnable to them: the mind of the hypnotic "subject" is absolutely at their mercy. The one mind is working with unusual force and individuality in its self-elected channel, and what its owner says or does in response to external influence is as little attended to by him as the influence itself. The other mind is working with marked absence of individuality in a channel elected by others, and what its owner says or does in response to external influence is that on which his attention is concentrated to the complete exclusion of every other thought.

The attempted explanation of the phenomena on *mental* ground, by bringing the mental condition within the recognised domain of abstraction or automatism, thus falls to pieces. Braid's explanation was a very different one. He fearlessly took *physical* ground, and attributed the hypnotic effects to an exceptional and profound nervous change produced by a particular muscular strain. His experiments and conclusions, which were the foundations of the actual science of hypnotism, are too well known to need recapitulation. They dealt, it is true, chiefly with the lower phenomena—the obvious bodily effects, and Braid's grasp of the subject on the psychical side was certainly very imperfect; still his claims to have traced to their true source effects which had hitherto been ascribed to imagination and imitation are sufficiently explicit to pass as a suggestion, at any rate, of a physiological basis for the higher phenomena with which we are here chiefly concerned. Since his time, the principal gain to our knowledge has been the proof that it is not necessary that the eye should be the organ employed, or even that the strain should be of a muscular sort at all. With sensitive "subjects," the ticking of a watch held at the ear, and light monotonous passes acting on the nerves of touch, have been found as effective as the fixed gaze. But this, it will be observed, is a mere extension of Braid's doctrine; for the physiological condition of preparatory nervous adjustment to a regularly-recurring stimulus is really *fixation* in as true a sense as where the employment of muscular apparatus more immediately suggests the word. Concentration of attention is, no doubt, the natural mental concomitant of the physical fixation; it may even be that for the artificial production of the state in man it is a real condition, in the sense that physical fixation alone would not be effective if the attention were kept actively employed on external topics. But Braid never for a moment suggested that the peculiar muscular or nervous strain could in the first instance be dispensed with, or was anything less than the full and sufficient cause of the subsequent phenomena. He is throughout consistent and urgent in his view that the basis of hypnotism is a complete alteration or rebalancing of the nervous system, artificially producible by special means of an obviously physical sort.

Here, then, we seem to have at any rate the beginning of a satisfactory account of many of the facts popularly attributed to "mesmeric" influence. Braid clearly saw—what Dr. Carpenter has failed to see—that the hypnotic state is a unique one and is due to a quite special cause. If the fact is experimentally established that a particular sort of physical process is perpetually followed by an exceptional mental state having no apparent relation to it, the hypothesis of an exceptional nervous change—as a middle term, and as the proximate condition of the mental state—is one which, in the present stage of our knowledge as to the connection between mind and nerve-tissue, we not only may but must make. And so far as the mental change is profound and the mental state unique, to that extent, we are justified in saying, is the nervous change profound and the nervous state unique. Even to enunciate this doctrine may appear somewhat out of date, now that science is attempting to define, what Braid left uncertain, the exact nature of the nervous events—whether, for instance, they consist in "cortical inhibition" or in "local erethism." But there is a special reason for constantly insisting on the more general position. For Dr. Carpenter's is by no means the only attempt that has been made to frame an explanation of hypnotic phenomena out of psychical factors; and such factors have proved themselves peculiarly liable to illegitimate use. Above all, they have tended to confuse the important distinction between the production of the state and the state itself—a distinction which Braid's conception enables us to keep clear.

The psychic factor mainly relied on has of course been that of *attention*. And there are doubtless cases where such reliance might seem justified—cases where the physical means which are successful in producing hypnotism seem much less exceptional and violent than those described by Braid, and where, therefore, the accompanying psychical element of concentrated attention becomes relatively more conspicuous. Even these cases, however, include none where physical means have been altogether absent: we have no record of the production by attention, however concentrated, of the characteristic phenomena of suspension of directive power and loss of memory, unless accompanied by some amount of physical fixation.* And indeed the very nature of the concentration, when present, seems to involve some such fixation: it would probably be impossible where the bodily state was wholly pliant and natural. In its strained immobility it is itself exceptional—so much so that an educated mind may find the greatest difficulty in attaining to it—and is, in fact, the very opposite of the active sort of attention with which an object is normally contemplated or a train of thought followed. Nor can attention be represented even as an inseparable accompaniment in the production of the state, unless by resolutely ignoring a large part of the hypnotic field. As Mr. Romanes some time ago observed, and as Professor Stanley Hall again pointed out in his most interesting and suggestive paper in *Mind* XXX., it is easy to hypnotise animals, but not easy to credit such an animal as a frog or a crayfish with any true power of mental concentration. And the phenomena of natural somnambulism or "sleep-waking," which in respect of the absorption of the mind in one direction present the closest analogy to those of hypnotism, demand no previous concentration of attention at all. But even if we confine ourselves to cases where attention *is* actually present during the production of the state, what ground is there for describing it as the *cause* of that state, in the absence of any extraneous empirical proof of a tendency in the antecedent to produce the consequent? The general effects of a one-sided strain of mind or body are pretty well known; and "tonic cramp of the attention" (to adopt Professor Stanley Hall's phrase) may be a very satisfactory description of the one-sided absorption in a particular direction which characterises many isolated stages of the hypnotic trance. But what tendency should the cramp of an attention which is directed to a button held in the hand have to produce, or to facilitate, a fresh cramp or series of cramps when the attention is diverted to quite fresh objects? The cramp of a limb which has been kept too long in one position does not issue in a tendency to move it rapidly into new positions; yet it is just such an anomaly as

* This of course does not apply to the production of the state in sensitive "subjects" who have been hypnotised on previous occasions, and who fall into the trance by attention, not to a button, but to their own memories of past sensations. The power of representing and revivifying past states is one which manifests itself in many directions, and has no special relation to the hypnotic problems.

this that the hypnotic process and its sequel perpetually present. Professor Stanley Hall's excellent remarks—*à propos* of the idea of attention—on the danger of using terms in a manner which necessitates "radically reconstructing the notion of them familiar to common consciousness," would surely be equally in place here *à propos* of cramp. Even the case which he himself describes—where powerful excitement, both physical and psychical, was produced by the effort to change the current of the "subject's" ideas, and where it was necessary to wake and re-hypnotise him before impressions of a new *genus* could be given—presented a feature which seems an odd result of previous rigid attention to a button, namely, "great mobility of attention" within the single genus of ideas suggested. But this necessity for waking and re-hypnotising is so far from being constant that in my experience it is unexampled. I have again and again found the complete change to a new genus of ideas to be absolutely effortless and instantaneous—found, that is, that the attention, which had been as usual fixed during the *process* of hypnotisation, became quite abnormally mobile afterwards. Thus—to give one example out of many—a youth well-known to me, with whom I have made many experiments, was told by the operator, before the proceedings began, that when hypnotised he was to recognise and converse with me. He was then hypnotised by fixation in the usual way; after which he talked to me for a minute quite naturally. Then, with a single sentence, he was taken from my room to a churchyard, and was set to work at trimming a grave, where the grass had grown too long. He put great energy and humour into his task, and he now regarded me as a stranger who wanted to interfere with him and rob him of his job. Another word from the operator, and he was in a boat in a storm, running up an antimacassar for a sail, and lashing his companion to the mast for safety, his comments throughout being extremely vivid and amusing. Another word, and he was engrossed in watching a conjurer spinning plates in the Brighton Aquarium; he indulged in very free criticism, and, while greatly admiring, opined that the plates were loaded and the table made to slope inwards. I now got the operator to introduce me to him, and to place my hand in his, and by this means I obtained sufficient hold on him to make him half believe that he was in church; but he was puzzled by the continuation of the plate-spinning, and at last he compromised his beliefs by saying that, though he would consent to sit in church, he must insist on watching the conjurer. A word from the operator obliterated the latter impression, and brought him wholly to church, where he pointed out various objects and, without the slightest suggestion, began mimicking the manner of a local preacher. I now again addressed him, and he again disowned acquaintance with me, though curiously he regarded me as the same stranger who had interfered with him before. Another word, and he was at home helping his mother with accounts, and did a sum which I gave him on paper correctly and with rapidity.* Here then the order of mental events, in the whole experiment, was unusual rigidity followed by unusual mobility. But how can we pretend that we *account for* the latter by recognising the former? So long as we keep to physical ground, it will be observed, no such difficulty occurs. If Braid had been asked how it is that fixation of particular muscles or nerves reacts on the higher nervous centres in so remarkable a manner, he might have fairly replied that physiology abounds in puzzles no less special and insoluble. But if I am told that a particular mental attitude—that of fixed or one-sided attention—is the *cause* of certain mental phenomena which are new to me, I am surely justified in demanding that the order of events shall present some perceptible coherence—shall at least not run directly counter to what my general experience would have led me to expect. Such an objection might be pedantic—as against writers who of course have no thought of differing from Braid, or of denying the physical correlative of the attentive attitude—were it not that in their advocacy of attention they have curiously disregarded the *facts*, such as those just recorded, where this want of coherence is evident. The oracular simplicity of Bürger's formula, that the cause of hypnotic phenomena is essentially psychic, would hardly retain its impressiveness in face of hypnotic phenomena which in psychic character are at the precisely opposite pole from their antecedent.

But this objection has yet another side. Suppose we were told that the final result of cramping a limb or a psychic faculty was *paralysis*, where should we expect to find the paralysis? Surely in that limb, or in that faculty. And in the case of the *lower* hypnotic phenomena

* It may be asked what guarantee can be had, in such cases as the above, that the "subject" is not acting a part in a condition of normal wakefulness. The test of pain cannot well be immediately applied, as in the alert stage of hypnotism there is rarely a marked diminution of sensibility. But the sensibility test can nevertheless be effectively brought to bear; for, if left alone at the close of such manifestations as the above, the "subject" will fall, usually with great rapidity, into the deeper stage of trance, in which any amount of such minor torments as pin-sticking and pinching may be applied without arousing him, or his conjunctiva may be touched without evoking more than a feeble reflex response. This is a state into which it cannot be maintained that robust youths are wont to pass at will out of a condition of normal wakefulness. Another test, which I have repeatedly applied, is to inform the "subject," on his complete waking, that he has apparently been dreaming of taking part in various scenes, and to offer him £20 if he will say what the scenes were. It will still, perhaps, be objected that though truly in a hypnotic condition and unable subsequently to recollect what has passed, the "subject" may still at the time be only pretending to be a party to the scenes suggested. This supposition deserves careful attention, and there are cases to which it certainly seems applicable. (See the very interesting remarks in M. Richet's *L'Homme et l'Intelligence*, p. 166.) But even if universally true, it would still leave the fact of the mobility of the attention just where it was.

what happens to the faculty of attention may doubtless be regarded in this light. Thus if a favourable "subject" be allowed to stare at a button undisturbed, he will soon pass beyond the "alert stage," when his imagination and his body might be brought into activity by suggestions, and will simply become torpid and indifferent, though still often capable for some time of rational conversation; the cramped condition of his attention has not resulted in continued and absorbing attention to the button, but in gradual paralysis of the whole perceptive function. We have just observed how different is the case with the attention, if the "subject" be taken in hand and suitably treated for the higher phenomena before this deep state has supervened; but the further point to be now noted about such phenomena is this—that while in them the *attention* is so little paralysed that it is even found to be abnormally mobile after a period of fixation on the button, *other* functions—those namely of choice, and will, and reaction in the way of attraction and repulsion—*are* paralysed. The effect on these reactions admits (as we shall see) of various degrees, but there can be no doubt as to its reality. In psychical terms, then, cramp of the *perceptive* has led to paralysis of the *appetitive* faculty—a fact which it would surely need a very enthusiastic psychicist to regard as self-explanatory.

It may be worth while here to note what I think has been a main reason (though a very illogical one) for the tendency to regard previous fixity of attention as in itself a sufficient ground for the unhinged automatic mental condition of hypnotised "subjects." It is that certain *physical* phenomena, which may at first sight seem more startling (but are in reality far less unique) than the mental condition in question, have undoubtedly been known to follow or to accompany the state of fixed or expectant attention—that attention, however, being then always directed to the part of the body in which the phenomena were actually to appear; as in the familiar case where the steady contemplation of a particular finger leads to a sense of tingling in it. But even in this direction, where the mere attitude of attention and expectancy does actually seem of distinct efficiency, cases occur where the physical change cannot possibly be ascribed to that attitude, inasmuch as not even the vaguest realisation of the bodily part to be affected was in the patient's mind. Such a case is that of a woman who had been hypnotised by Braid for relief of violent pain in the arm and shoulder, and who found, much to his and her own surprise, that an opacity which had been left by rheumatic fever over more than half the cornea of her left eye was gradually clearing. This case has been most unaccountably quoted by Dr. Carpenter as an instance of the curative effects of mere attention. The result seems clearly attributable to that rebalancing or re-direction of nervous energy which Braid regards as characteristic of the hypnotic state—to those nervous events which are no mere correlate of an act of attention, but the result (as he explains) of a quite special physical cause.

But the objections to the attention-theory are not by any means exhausted by the difficulty of connecting the *process* with the *results* of hypnotisation; on the contrary, they become even more substantial if we confine ourselves to the latter. And they deserve note the more just because the theory here will actually cover so much ground—because so many of the hypnotic phenomena may be truly described as belonging to the "pathology of the attention," and admit of interesting treatment (*e.g.*, in G. H. Schneider's treatise, *Die psychologische Ursache der hypnotischen Erscheinungen*) in connection with other branches of that wider subject. But in the first place, even in the alert stage of hypnotism, where mono-ideism with its accompanying loss of balance and control is often most conspicuous, it must always be borne in mind that this is not the essential peculiarity of the state. The fundamental fact according to our formula is not that the psychic activities are abnormal, but that they are *reflex;* it is not the mere subjection of the mind to a single idea or set of ideas, but the certain production and alteration of that condition *ab extra.* In the second place, results are often loosely set down to the "dominance of an idea," where, if we inquire what the particular idea is, we fail to find it. A hypnotised boy is told that he may have a £5 note if he can pick it up. To suit the theory, the dominant idea must be that of the impossibility of the act; but even if we allow this idea a momentary dominance, in what sense can it be held to continue dominant during a struggle in which every word and gesture express the strongest determination and incredulity? It may, perhaps, be suggested that the words and gestures express no more than an ineffectual effort to resist a nevertheless dominant idea; but to this suggestion we may often oppose the "subject's" subsequent description of his experience. And lastly, there remains the large class of cases which do not belong to what I have called the "alert stage" of hypnotism at all; and where the attention-theory can only be applied by the desperate assumption that unusual deadness of sensibility in one direction necessarily involves unusual concentration in another. If a jet of gas is seen burning specially brightly, it is doubtless reasonable to connect this condition with the fact that the other jets in the chandelier are turned off;* but if no light at all can be perceived, the natural

* *Apropos* of this metaphor, which is often, of course, an entirely just one, the following instances may be worth recording. A hypnotised "subject" who strongly resented being even slightly pinched was impressed with the idea that a person to whom he was attached had died. He showed considerable emotion, and was now completely indifferent to the most savage pinching. Again, several "subjects" who were sensitive to pain in the alert state, were thrown into the deep state, and impressed with a command which was to be executed when they emerged again into the alert state: when the emergence came, they showed entire insensitiveness until the command was duly performed.

hypothesis surely is, not that some jet is burning brilliantly somewhere out of sight, but that *all* the jets are turned off. The energy of attention is not a fixed quantity, bound to be always in operation in one direction or another; nor does the human mind, any more truly than nature, abhor a vacuum. Even in the "alert stage," when the "subject" can be made by an occasional word to enact scene after scene with astonishing truth and vigour, the indications, if he be left alone, are of blankness, not of concentration. He knows where he is, and will answer if spoken to; but otherwise he sits inert and listless, if asked what he is thinking about will usually answer "nothing," and soon passes into the deeper stage with closed eyes, in which, though, still able for a brief period to respond to questions, he is insensible to any ordinary tactile stimuli. It would surely be irrational to refer that insensibility to the strong concentration in some unknown direction of an attention which, even in the previous alert state with open eyes, there was no ground for supposing to be active. I do not urge the cases of obvious reflex action (on which Professor Stanley Hall has made some good remarks in the paper already referred to), since on a theory like that of Herr Schneider—in which the lower centres, so far as they distinguish stimuli, are credited with an embryonic consciousness—the question might there be a mere question of words. The cases which I have in view are those where the results observed cannot, by any stretch of the meaning of attention, be reasonably connected either with the "positive field"—*i.e.*, with unusual absorption of the attention in the line of the result, whether as immediately producing it or as inhibiting its opposite*—or with the "negative field," where insensi-

* I gather from some expressions of Professor Stanley Hall on the subject of "active inhibition," that he holds that the condition of the attention in this positive field may be further subdivided—that the *actual direction* of concentration may be not only towards the production, but towards the inhibition of a particular mental phenomenon. This direct action of inhibition is hard to picture. The activity of active inhibition appears to me always to lie in a determined setting of the mind in some *new* direction: I mitigate a pain not by attending either to the pain or to an imagined absence of the pain, but by clenching my teeth and thinking of something else, *i.e.*, by opening quite new channels of nervous energy. So when Professor Stanley Hall inquires whether when a hand is made insensitive to pain, it is "due to abnormally intense inhibition of sensation or motion by consciousness, or is better conceived as an entire detachment and vagrancy of attention from consciousness, of which it is conceived only as a concentration," I find a difficulty in admitting the possibility of the first alternative, as also, I must confess, in catching the meaning of the second. In the proposed case, at any rate, I should not myself see the necessity of having recourse to either. If the hand is rendered insensible in the ordinary way by faint sensory stimuli, it is surely a case where the theory of direct physical inhibition of the lower sensory centre is exactly in place. The very different case where the manipulations employed do not produce any sensory stimuli at all, as where no contact is used and the arm is thickly enveloped in clothes, is one on which I shall have a word to say later.

bility and irrational conduct are the result of an unusual draining-off of attention from the ordinary sensory or ideational tracts into some other line. They are cases where, if we wish still to hypostatise attention, we must just say that it is paralysed or has fallen asleep. But such a mode of expression is not to be commended. For the sleep and paralysis may invade some faculties and not others, *e.g.*, colour-blindness may supervene while the hearing remains perfect; and even in the deeper state of trance, ideas, and especially commands, may be impressed on the "subject's" mind. What do we gain, then, by employing a general term to describe such special effects? When once the chandelier-metaphor is abandoned—when once it is recognised that in a multitude of cases the quantity of attention turned on in one direction is in no way connected with a withdrawal from any other—the idea of a common psychic factor seems out of place and misleading. The "subject's" ear wakes while his eye sleeps; so in ordinary paralysis the right side may feel while the left does not; and it does not then occur to us to talk about the patient's *attention* being asleep on the left side and concentrated on the right.

And now we must make a sudden transition, from the theories which have unduly magnified psychic functions in hypnotism, to those which have unduly ignored them, and have substituted the shibboleths of physical reflex action and automatic cerebration for that of attention. If we trace the natural logical route of the subject, we shall see that a time was almost bound to arrive when a purely physical account of the whole range of phenomena would be attempted. Up to the time of Braid's death, no serious question seems to have been raised as to the relation of *consciousness* to the hypnotic manifestations. No doubt, at any rate, was expressed as to the presence of consciousness in those higher phenomena which belong to the lighter stage of the trance, and which form by far the most interesting part of the whole subject. Braid himself speaks of "the extraordinary power of concentration of thought," "the rapt contemplation," "the glowing scenes and images" presented to "the fervid imaginations" of his patients. But the very fact of tracing the observed phenomenon, as he did, to a peculiar physical condition must lead on to the question how far the psychical factors of consciousness and volition are really involved in them at all, and how far the suggested idea has any true existence in the "subject's" mind. Granting that his attention has to be directed in the first instance to the monotonous process by which the state is produced, we have seen that the power of attention might naturally be expected to be paralysed by that very process, so as not to survive when the state is once reached; and if the "alert stage" is not caught and used, this is what actually happens, the gradual dulling of the faculties passing on into comatic unconsciousness.* But there is nothing *primâ facie* unreasonable in supposing attention to have passed away *before* this deep stage is reached—in supposing that it does not survive that profound nervous change which, following Braid, we infer to have supervened as soon as the alert stage is reached. The state of the "subject" is so obviously peculiar that there would seem to be no strong *à priori* obligation on us to interpret what would ordinarily be accounted signs of consciousness in the usual way. Dr. Carpenter's wavering utterances already indicate some suspicion on this point; and the gradual progress in our knowledge of physical reflex action, and of its special connection with the hypnotic state, has naturally given the question a new shape and significance. Hypnotism being, beyond doubt, the field on which such reflex action reaches its furthest limit, where is that limit to be drawn? The consideration of this point will further establish the distinction between the lower and the higher hypnotic phenomena, and will thus further define the fundamental peculiarity of the latter.

If we begin at the bottom of the series of phenomena, we certainly find no reason to suppose that they are accompanied by any distinct consciousness or concentration of attention. If we find it hard to credit the frog with attention during the process of hypnotisation, it is still harder when the process is complete; and the insensibility and immobility of the human "subject," if left to himself in the "deep stage," seem to indicate a mental condition not very different from the frog's. Higher in the scale, actual experiments in reflex action suggest a decided lowering of the psychical functions. The heightening of the reflex responsiveness of the *muscles,* which is often the first symptom of hypnotic influence, does not, it is true, serve as a sign of diminished mental activity, especially as the phenomenon itself—the twitching limbs and the inability to control them—is peculiarly calculated to stimulate the "subject's" attention. But Professor Stanley Hall's recent experiments avoid this difficulty, and give us just the indication that is needed. For, in establishing the diminution, during the alert stage of hypnotism, of the time necessary for *voluntarily* reacting on a stimulus, they suggest that the reaction has become to some extent reflex;† and since this implies that the brain-action associated with conscious attention to the work of reacting is

* A fuller account of the different stages of hypnotism will be found in a former paper, *Proceedings*, Part V., p. 61, &c.

† It is worth observing that this extension of reflex action in one direction may perfectly well co-exist with what might appear a contrary result in another. For instance, Professor Stanley Hall's "subject" (I presume while in the same hypnotic stage as he was in during the reaction-experiments) could gaze at a sunny window for 13 minutes without winking. But the ability to do this doubtless

diminished, we may fairly suppose that the amount of conscious attention is itself diminished. And this leads us on to the more general supposition, that actions which would normally involve very distinct consciousness may be performed by the hypnotic "subject" either with a lesser degree of it or entirely without it. There is thus considerable justification for Heidenhain's explanation of the singular exhibitions known as hypnotic *mimicry*. According to him, the movements or words of the operator, acting on the eyes or ears of the "subject," stimulate as usual the lower sensory centres; but in the hypnotic state, the functions of the higher cortical portion of the brain (to which nervous discharges are supposed normally to pass from the lower sensory centres) are *inhibited*, and consequently no effect is produced in the way of consciousness. At the same time, the disturbance in the lower sensory centres, though thus unaccompanied by consciousness, is sufficient to pass on the nervous discharge to the most nearly associated *motor* centres, which will naturally be those whose activity will produce the *same* words or movements; since clearly no association can well be closer or more constant than that between the sight and sound of a movement or word and the act of producing that movement or word. And since the same inhibition of the cortical functions, which precludes consciousness of the impression, precludes also the normal exercise of the power to direct and control movements,* the mimicry takes place mechanically and unfailingly, *i.e.*, as genuine reflex action. Heidenhain further extends this explanation to the phenomena of what he calls "automatism at command." He attributes the machine-like obedience of the "subject" to a similar inhibition of cortical function, and to the consequent opening of an unimpeded channel of discharge from the lower ideational to the motor centres—*i.e.*, from the place of the nervous discharges which, if allowed to pass on in the normal way, would result in the mental picture of an action, to the place of the nervous discharges immediately associated with the performance of that action.

But while it is important to note the facts to which this hypothesis

arose not from an inhibition of the normal reflex movement, but from a direct deadening of sensibility in a particular organ. So extreme a deadening in the "alert stage" of hypnotism is rare, though out of several hundred "subjects" I have found two whose eyes remain open even in the deep stage. Such exceptions are valuable as showing the variety that may exist even in the simplest facts of hypnotism.

* Heidenhain has introduced an *équivoque* into the terminology of the subject by calling the hypnotic action on the cortical functions *inhibition*, without pointing out explicitly that the *normal* action of those functions in respect of motion is to a large extent *inhibitory*, and that the complete description of the method by which the automatic reflex responses are brought is thus *inhibition of the inhibitory function.*

will apply, and in which ideation and volition apparently play no part, it is of still greater importance to avoid mistaking this limited portion of the field for the whole. It is no doubt convenient for the theory to conceive the inhibition of directive and volitional power as accompanied by inhibition of consciousness; but the positive grounds on which the sweeping assertion of the unconsciousness rests are so flimsy that, but for the high authority of those who are opposed to me, I should almost have thought it waste of time to discuss them.

The most thorough-going statement of the doctrine in question appears, I regret to say, in a book which for general acuteness and comprehensiveness of treatment is superior to any other on the subject with which I am acquainted—the *Étude Scientifique sur le Somnambulisme* of Dr. Despine, which in 1879 obtained for its author a medal from the Medico-Psychological Society of Paris. The acuteness, it is true, is not unfailing. When a man concludes that the highest psychic manifestations may take place without consciousness, from the fact that the complicated vital functions of the animal and vegetable creation, while seeming to demand a capacity at least equal to that of an intelligent man, nevertheless take place unconsciously, and that the highest human intellect could not construct a butterfly's wing, we may defer our answer till a stomach or a tree begin to reason, or a butterfly's wing to decide knotty points. Popular arguments, moreover, are sometimes caught at in a manner fatal to consistency. Thus an appeal is made to the well-known ability of somnambulists to keep their balance in dangerous places and at giddy heights; which may reasonably be connected with unconsciousness of danger, and so far might pass muster as an argument for the temporary abolition of *all* psychic function. But on Despine's own principles, how should the somnambulist be any the safer for his unconsciousness? If cerebration, even in its most subtle and complicated forms, can go on just as usual without any psychic correlative, why are we to except the particular cerebration that would normally be accompanied by fear, giddiness, and loss of balance? What is to prevent *that* in the given conditions from functioning in the normal way, and so producing a fall? So far from affording a proof of true automatism, this is emphatically a case where the theory of *attention*—of a mind exclusively occupied with the next step and not occupying itself with ideas of falling—seems of most assistance. Dr. Despine's next argument, however, has more force. He finds an indication that even the most complicated psychic phenomena of hypnotism may be purely automatic in the fact that in certain abnormal states the personality seems doubled; as when a person recovering from typhoid fever spoke and sang, while seeming to himself to be listening to another's performance, and without any idea what the next sound to be produced would be. Similarly religious ecstatics and "trance-mediums" have delivered impromptu discourses without conscious cerebration, and have been the devout and admiring auditors of eloquence whose sense they grasped only after it had issued from their own lips. In these cases Dr. Despine attempts to make the one part of the person—the watching and attentive part—the witness of the automatism of the other part; and since, viewed in this light, the presence of the witness is not necessary to the production of the result, an argument is obtained for the general possibility of similar manifestations without any participation of consciousness. This artifice of making two people, A and B, out of one, in order (in the absence of evidence) to obtain a sort of presumption that A's presence is not a condition of B's actions, is not very convincing; at the same time there need be no great difficulty in admitting the particular possibility claimed. Few will dispute that the talking and singing might appear as purely automatic phenomena; and even the impromptu discourse, with its far more complicated series of actions, may be conceived as producible either in the absence or with a minimum of consciousness—its contents being presumably a string of familiar ideas, closely connected by association, and clothed in a hackneyed phraseology. Despine's error is in sweepingly applying the same argument to hypnotism, without remarking how radically different are many of the phenomena there presented. The psychologist who claims for his study the dignity of a science is surely bound to follow the physicist's example, and to take some trouble to vary the conditions of his observations; and in this question of the presence of consciousness, the very simplest experiments suggest the sort of variation that is necessary. A "subject" is asked a question to which the obvious reply is "yes," and answers "yes": he is asked another to which the obvious reply is "no," and answers "no." "Clearly automatic reflex action," say Despine and Heidenhain, with a great show of reason. But now let us take a case where, though the answer is equally simple, the question itself does not suggest one answer rather than another. For instance, let some one, standing behind the "subject," give a very light clap of the hands at intervals, and let the "subject" immediately before each clap and also at other times between the claps, be asked the question "Do you hear this?" He will be found to answer "yes" when a clap follows, "no" when no clap follows. Now here even to suppose the answer "yes" to be automatically given involves some strain of the reflex theory; for granting that the physical attention might be fixed by the question—*i.e.*, that the nervous events corresponding to expectation of a faint sound might be thus produced—these events would in themselves have no tendency to produce the word "yes" in response to the clap. In the normal state, that answer would involve a sense that a doubtful point had to be decided by the person himself, and the result truly communicated—a mental operation of some complexity and great delicacy; and if the same result be produced without any psychic concurrence, the physical events must at any rate differ considerably from anything involved in the trance-medium's self-propagating stream of irresponsible verbiage. But when the answer given is "no," the indication of a true psychical event—*viz.*, the consciousness of not hearing—corresponding to it seems almost irresistible; for here the answers, besides involving just the same delicate operations as the former one, would have to be reflexly jogged out not by the stimulation of sound, but by the *non*-stimulation of silence. Similar and far more complicated instances could be easily multiplied *ad infinitum.*

But Despine's principal argument, and Heidenhain's only one, depends on the "subject's" subsequent *defect of memory* as to what has passed during his trance. It does not seem to have occurred to either of them that the requirement, as a test for present consciousness, that its content shall be afterwards remembered, requires itself any justification. Yet if the reality of that test be granted, the question whether a man was conscious when he read an article in the *Times* will depend on whether or not he receives a blow on the head when he has finished it. In his development of the argument, however, Despine shows considerable controversial ingenuity; and it must be admitted that those who have maintained the presence of consciousness in hypnotism and somnambulism have not always been happy in their way of accounting for the subsequent forgetfulness. This has been attributed by Dugald Stewart to the "subject's" defect of attention to the events that are passing; by M. Maury to a mental paralysis brought about by an exhausting concentration of attention on those events,—theories so weak and baseless that we certainly need not grudge Despine the satisfaction of setting them off one against the other. So again, he has no difficulty in disposing of M. A. Lemoine's explanation that memory cannot survive the shock of the sudden change from the somnambulic to the natural state. A single instance, however, will show that his own counter-positions are very little stronger. He adduces the extreme violence of the things which have been done or suffered in the trance-condition, and argues that since these things, though so impressive in their nature, are not remembered, they must have been done or suffered unconsciously. The reply is obvious that equally violent things are done and suffered in *dreams* (which Despine again and again distinguishes from the trance-condition by the presence to them of consciousness and the *ego*), and are forgotten within a minute of waking. Equally obvious is it to notice that Despine's argument quietly begs the whole question; for he is *assuming* for the abnormal state the same relation of consciousness to memory as exists in the normal state, forgetting that that identity of relation is precisely what he has to *prove.*

But there is a more radical objection to all these arguments for the unconsciousness of the "subject" from the fact that subsequent memory is absent—the fact, namely, that it is frequently *present.* The *primâ facie* indications of this subsequent memory, found in correct descriptions by the "subject" of what he has been doing, are too obvious to be long overlooked; and Heidenhain, when he observed them, brought them into harmony with his general theory by supposing that, when the abnormal inhibition of cortical function is removed, the excitation remaining in the lower centres transmits a stimulus to the liberated sensorial ganglion cells—to be psychically represented as memory of the original exciting cause, which, when it actually operated, had no place in consciousness. He holds, however, that some distinct hint or impulse is necessary to bring up this residuary excitation to the requisite strength. And for many of the simpler phenomena this seems a satisfactory hypothesis. Further, the outward indications of remembering, in a new hypnotic state, what occurred in a former one, and the apparent taking-up of an old track of ideas, or even of a connected discourse, at the point where it had been abandoned, have been brought by Despine within the scope of purely automatic brain-action—the renewal of the hypnosis bringing with it the former special excitatory conditions. But other phenomena seem quite beyond the legitimate scope of such a theory. If the "subject" is brought into the "alert state," made to go through the ordinary platform buffooneries, and then re-awakened, there is often not any breach of consciousness at all; and he gives a description, which there is not the slightest ground for calling in question, of his state of mind in performing the actions—as, *e.g.*, that he felt disinclined to do them but could not help it, or that he was aware of their absurdity but could see no reason for not doing them. Again, on re-hypnotisation the events of the previous hypnotic state are sometimes spoken of with fulness, in answer to perfectly neutral questions; nor do they merely recur in unrelated sequence, as by the release of particular springs, but are compared and estimated. There is here no mere rejoining of a temporarily-broken associative chain: the phrases used are to all appearances the normal results of a discursive and critical review of past experiences; and the *onus probandi* rests with those who deny that what gives every sign of being genuine memory *is* genuine memory. When once this hypnotic memory is duly recognised, the study of its varieties will be found of great interest; and in the paper above referred to I endeavoured to make this study the basis of a clear separation between two states—the "alert" and the "deep"—that have commonly been distinguished merely as steps in a single process, continuous changes on the path to hypnotic sleep.

The results there given have an important bearing on the central question as to the respective relations of consciousness and of reflex action to the hypnotic state, or rather states. Under appropriate conditions, we saw reason to recognise true subsequent memory, and therefore present consciousness, even in the lighter stages of what I called the "deep" state; and when we passed upwards to the lower phenomena of the "alert" state—*e.g.*, mimicry, and mechanical obedience—we could often appeal further to the absolute unbrokenness of the conscious stream. Where memory is absent, and where there is a distinct breach in the train of consciousness, arguments may still be drawn from experiments such as the one on hearing above recounted, where a point is left to the "subject's" decision, and the truth of this decision is independently ascertained. Again, the end being suggested, the "subject" will take his own means to accomplish it. He will use his reasoning powers—as in the case of Dr. Hack Tuke's "subject," who was asked if he could not walk forwards, and who "remembers arguing out in his mind, wearily, that it followed from this he was walking backwards." He will even form original theories as to what is told him. For instance, a young man who had been impressed with the idea that he was going to be hanged, was then told that his sweetheart had been blown off the pier and drowned, and that the announcement of the event was in the evening paper. He at once surmised that she had purposely thrown herself off, through grief at his approaching fate. He was now told that the second edition of the paper showed it was a mistake, and he suggested two explanations; the first that the name Newington had been wrongly printed for Newton; the second (in which he testified his belief by seizing a paper and pointing to what he imagined to be the actual passage), that the words in the second edition ran "*almost* blown off the pier, and *almost* drowned." Again the hypnotised person, like the somnambulist, will sometimes go through complex calculations, and bring out a correct result with greater ease and certainty than in his normal state; while the vividness and inventiveness of his imagination, as under the stimulation of questions he pursues aloud the course of his waking-dream, are a source of ever fresh astonishment.

These latter facts alone cannot, of course, be made conclusive against a thorough-going automatic theory; but they at any rate suggest strong probabilities, which ought to be met by something better than bare assertions. First we have Heidenhain's crude attribution to reflex mimicry of *all* the phenomena which others have attributed to the dominance of an idea—his statement, *e.g.*, that a "subject" will never eat a raw potato on the suggestion that it is a pear, unless the operator makes movements of mastication in his sight. This view produces a treatise. A few more experiments are made, and these produce a practical recantation with an admission of the sufficiency of suggested *ideas* to produce the appropriate bodily movements. Then comes Despine and maintains that even the "ideas" have only a physical existence. We de not dispute the admissibility of his conception in simple cases, but ask in vain why we are to extend it over the whole hypnotic field, and apply it to elaborate actions which have been accompanied by consciousness in the whole of our experience, and present every imaginable sign that the attention is completely engrossed by them? Are we to forego all discrimination, just because it saves trouble to have a simple and thorough-going theory? The situation may be put thus. One set of facts (notably the unbroken persistence of consciousness and memory in the passage into and back from the lightest stages of the trance) show that a hypnotic condition is not *ipso facto* an unconscious one. Another set of facts show that, in hypnotism, the line which separates mechanical and reflex from conscious and volitional actions is considerably shifted, and actions which would normally be above the threshold of consciousness sink below it. But what of that, if a multitude of actions, performed in that lighter stage of the trance to which the most interesting phenomena of hypnotism belong, do according to any natural interpretation imply a state distinctly above the threshold? We readily grant that we cannot draw the new line with certainty, even for a particular case; but all analogy is against supposing it shifted to the utmost limit at the very outset.

I cannot, then, for a moment believe that the automatic theory, in the extreme form which asserts unconsciousness for all hypnotic and somnambulic actions, will hold its ground. But in proportion as the theory becomes less sweeping, it gets into difficulties of detail. Heidenhain, as we have seen, has found himself obliged to recognise the psychic element in the higher hypnotic manifestations: but he seems oddly unaware of the effect of this admission on his exposition of the physical processes involved. The point of that exposition, it will be remembered, was the opening of a *direct* channel from impressional to motor nerve-centres, through inhibition of cortical function; and now we find a vast number of cases where consciousness, though conditioned by cortical activity, is *not* inhibited. It is of course easy to reply that here it is only the functions specially associated with spontaneous control and choice of movements that suffer inhibition. But that goes not a whit further as an explanation than Braid's general assertion of a profound nervous change; it is merely a necessary inference from the palpable fact that spontaneous control and choice have ceased. It is just this cessation—which, translated into physical terms, we should naturally call "inhibition"—that constitutes the novel feature of the case; and nothing that we have otherwise known about inhibition (as Mr. Romanes, in his preface to the translation of Heidenhain's *Animal*

Magnetism, has rightly admitted) could have led us to expect or conceive the results attributed to it. Heidenhain's *explanation,* in fact, like that of the French Commission, is no more than a *restatement* of the problem. As long as the *whole* of cortical function can be supposed to be eliminated, his theory has a certain symmetry and explanatory power. It gives a plausible account, involving neither consciousness nor volition, of the power of the simple sound "go," to produce an immediate corresponding movement of the "subject"—the merit of the explanation being the easily conceivable picture of the nervous events which it supposes, and which are quite on a par with the recognised facts of reflex action. But when, *e.g.*, a long series of orders is thoughtfully and painfully carried out, long after they were given—a phenomenon not uncommon in "mesmeric" exhibitions—it seems impossible to adapt the old (or any other) neat and symmetrical hypothesis of the nervous processes to the new phenomena; and the word "reflex" can be applied to the latter, if at all, only in the peculiar and carefully guarded sense which confines it to their *psychical* aspect. It seems hardly possible that Heidenhain should have missed seeing this, had he waited to formulate his theories until he had witnessed some of the higher phenomena in their more striking forms. As soon as the elements of consciousness and volition are clearly recognised as active in such phenomena, it surely must equally be recognised that the fundamental peculiarity of the condition is simply the absorption of those elements into the one suggested channel of attention or expectancy, and is thus quite removed from the lower plane of physical "reflex."

So far, then, our formula of *conscious* or *psychic reflex* action, as expressing the true peculiarity of the higher hypnotic manifestations, has been defended against two sorts of over simplification; that which ignores the part played by the mind in the phenomena; and that which, accepting the part played by the mind, fails to see that its differentiating feature is the liability to respond to suggestions with the same mechanical readiness as a stimulated muscle displays when the normal inhibitory influence is withdrawn. It is only stating the condition thus indicated in other words, to say that the heart of the problem lies not in *consciousness* but in *will.* And here another important distinction presents itself. The hypnotic automatism must not be conceived as necessarily implying any abrogation whatever of the will, taken as the *sense of desire or impulse.* That element admits of all degrees. It may be absent altogether, and the "subject" may perform his acts, consciously indeed, but with complete indifference—in which case nothing is commoner than for him to believe and to assert afterwards that he could have avoided doing those things if he had chosen; but it may also be present in full force, and may even be directly opposed to the course of action pursued. What *is* abrogated is not the sense of desire or the power of willing, but the sense of self-determination and the power of choosing. Even here questions of degree come in. In the very lightest stage—as exemplified by the boy who strove to pick up the bank-note—it cannot be said that choice, any more than desire, is abrogated; while even in a deeper stage, a "subject" will sometimes experience such a sense of repugnance as seems to involve some residual power of avoidance; and occasionally he will retain complete power of choice in some isolated particular.* At the same time, this suspension of choice must be accepted as the most marked and central characteristic of the higher form of the hypnotic trance. And for those who regard the intuition of free-will as a subjective illusion, it is a point worth notice that decided abnormalities of conduct should present themselves precisely when, and in proportion as, the sense of having a free-will and being a choosing *ego* disappears. The variations are at any rate concomitant; and if nothing else varies, such concomitance would, outside metaphysics, be held to imply some sort of casual connection. It may perhaps be objected that it would be incorrect to say that nothing else varies—that the essential variation is simply a change in the particular *motive* that assumes prominence; *e.g.*, that when a command is given to put the hand between the bars of the fire, and the determinant motive to a normal mind would be the dread of being burnt, the determinant motive to a hypnotised mind is the desire to obey the controller. But this is not at all in accordance with the evidence of many hypnotic "subjects" who have been able to recall and give an account of their state of mind. They are often conscious of the falseness of what is told them, and of the folly and harmfulness of the things they are bidden to do;† they are even sensible of a strong objection to doing them, and *not* sensible of any positive motive impelling them to act; but it simply does not occur to them that they have a choice in the matter.‡ Even if we abandon free-will and stick to psychology,

* I was recently experimenting with a youth who had formerly been a telegraph-boy, and who had taken a strong dislike to the *métier*. When hypnotised, he was at the mercy of any suggestion or command, except one; nothing would induce him to carry a telegram. In its strength of resistance to the hypnotic mono-ideism, this repugnance really itself reached the mono-ideistic intensity; for the refusal was unaffected by considerations that would certainly have reversed it in the youth's normal state,—*e.g.*, when he was told that the matter was one of life and death, and that he should have £20 for the job.

† See a case reported by Dr. Hack Tuke, in the *Journal of Mental Science*, for April, 1883, p. 70.

‡ The determinist may, no doubt, make a more general objection: he may say that the consciousness of free choice, however interpreted, is a normal antecedent of human voluntary action; and that therefore he would have expected

such facts as these seem decidedly awkward for the mechanical theory of the determination of conduct, at every point, by a motive that represents the greatest balance of foreseen pleasures or immunities from pain.

And we are thus led to the completing step in our description of the higher hypnotic state. That state may be regarded as the most complete exemplification of Professor Bain's fruitful formula—*the tendency of an idea to act itself out.* I cannot regard Professor Bain's own instances (*Mental and Moral Science*, p. 91) as the best examples of the law; they seem to me rather to exemplify the common impulse to produce a marked effect, to "make a scene" of some sort, even at one's own cost. At the same time, I think that his formula represents a reality, the scope of which even in ordinary life has hardly been sufficiently recognised. It seems to me the only possible ground for certain brief phases of sulkiness or perversity—the shade of meaning may be best conveyed by the slang "cussedness"—where a person finds himself persisting in an attitude or a line of conduct which causes him acute discomfort at the time, with a promise of nothing but discomfort as the consequence. But in the case of hypnotism, at any rate, the idea is a most helpful one. For it enables us to bring under rule the cases that seemed most exceptional—where, *e.g.*, a "subject" is told that he cannot do a particular thing, and struggles ineffectually to do it. We saw how absurd it was to represent his mind as throughout possessed by the idea of the impossibility, or his will as paralysed. But there is no great difficulty in supposing that the idea of impossibility obtains a momentary lodgment, and then tends to work itself out *physically*, even after the opposite idea—the idea that the action is possible and *shall* be accomplished—has dislodged it from consciousness. We might fairly compare the automatic continuance of the brain-movements which are evoked by the momentary stimulus of the first idea, to the long-continued and far-spreading muscular contractions which in a sensitive "subject" will follow on a brief sensory stimulation: both are signs of the characteristic hypnotic irritability. This view seems strongly confirmed by the fact that, if the boy's muscles be examined, they will be found in the state which corresponds to the first idea—that of impossibility—and not to the second. I have myself tested this many times. A boy's arm being flexed, he is offered a sovereign to extend it. He struggles till he is red in the face; but all the while his triceps

its absence to be accompanied by some other abnormality. I must, however, take leave to doubt whether, if taken unawares, he would have evolved this expectation—whether he would have regarded the (to him) purely illusive sense of having a free choice among several different courses as an indispensable element of directive force in the line of the one course that is actually taken. The hypnotic facts might therefore have an interest for him, if only because they clearly show the logical necessity of this very odd-looking admission.

is remaining quite flaccid, or, if some rigidity appears in it, the effect is at once counteracted by an equal rigidity in the biceps. The idea of the impossibility of extension, *i.e.*, the idea of continued flexion, is thus "acting itself out," even when wholly rejected from the mind.

It is perhaps well that my space is nearly exhausted; for it might be held unfitting, in a paper on Hypnotism, to do more than hint at results, however simple and precise, which break away from every form of hypnotic hypothesis. Community of sensation between "subject" and operator; the distinction by the "subject" of the operator's faintest whisper, either amid deafening uproar, or among a number of other faint whispers of similar sound; local anæsthesia produced in the absence of expectancy by a process which is itself unfelt; inhibition of speech or memory without a word or sign of any sort;—a writer who owns to having participated in experiments which establish these facts grievously imperils his chance of being listened to on the sober ground of hypnotism. At the same time the *naïveté* and suddenness with which the clamorous facts of hypnotism itself were welcomed within the portals of science, as soon as a *savant* of established reputation took the trouble to learn (very imperfectly) the A B C of them, and to proclaim that they actually were realities, that his own brother had been experimented on, and that it was *not* all cheating, as he had all his life supposed—may perhaps suggest a quiet surmise as to the scientific future of other events which, with all their absurdity and inadmissibility thick upon them, still go through the hollow form of taking place with surprising accuracy. But leaving these matters aside, no sketch of the "problems of hypnotism" could be in the least complete without mentioning certain objections which present themselves in the direct path of orthodox hypnotic experiment, and which concern the very processes on which the hypnotic explanations are made to rest.

At the very outset, there is the difficulty of the vast differences of degree that exist in the power to produce the results—a difficulty which has never been fairly faced, much less surmounted. It has been asserted, and in a sense it may be true, that any one can hypnotise any one: any one, that is, may be competent to make passes in the gentle and monotonous manner which acts on the organism of sensitive "subjects," and with immense perseverance may produce some amount of the hypnotic effect. But let a score of likely "subjects" be taken who have never before been hypnotised, and let a dozen persons who have been instructed in the right method of making the passes be set in turn to operate on them, and let this dozen include one recognised and successful "mesmerist." If the experiment be often repeated, always with fresh "subjects," it may be pretty confidently asserted that in the long run the successes of the "mesmerist" will outnumber those of all his rivals put together, and moreover will as a rule be far more marked in character and far more rapidly effected. And this is the more noticeable in that, supposing the usual process to be adopted, the condition which on the hypnotic hypothesis would appear to be the most important—the staring fixedly at an object in the hand—would be common to all the attempts, and little if any of the required monotonous stimulation would be derived from the actual passes—the operator's hands not being in contact with the "subject" till perhaps the very final stage of closing the eyelids. But if the truth of the above assertion be denied—and fully to carry out the experiment with a new set of "subjects" daily would involve great practical difficulties—the result of repeated attempts with the same "subject" will afford a still stronger argument. A recognised "mesmerist," after a very few successful trials on favourable "subjects," can send them into the trance in a very short time, sometimes even with a single pass; but except in response to him they will show no particular susceptibility; and no attempt of others, extending only over the few seconds that suffice the successful operator, will produce any effect whatever. To account for facts like these, as Heidenhain has done, by differences in the moisture or temperature of the operating hands, seems little better than childish—as if a somewhat warm and moist hand (even were it indispensable, which it is not) were a sort of *lusus naturæ*. Somewhat more plausible is the suggestion that the facts really exemplify the dominance of an idea—that the "subjects" *believe* that their mesmerist has special power, and as a consequence of that belief succumb to him. But it really seems absurd to suppose that this faith in a single individual's power is unfailingly complete and absorbing in every member of a set of careless boys who are new to the whole business, and whose obedience to the simple directions which they receive, and passive acceptance of what happens to them, certainly do not suggest any nice criticism of the nature and limits of their operator's faculties. There is no difficulty in impressing such "subjects" with the idea that some other person present, who may be of a more dominant and imposing aspect than their recognised controller, is also a powerful "mesmerist"; but this preparatory idea will not be found to invest that person with any of the controller's powers.

The hypothesis of suggestion and expectancy is still more obviously inappropriate where the end in view is not the *production*, but the *ermination*, of the trance-condition. It would be very strained to imagine that the mind of the "subject" is in every case dominated, or even that he *was* dominated at the time when the condition was being produced, by the idea that only the producer of it has the power to put a stop to it. Yet he will often remain completely uninfluenced by the efforts of others to awake him, and that, too, even when only a light phase of the trance has been induced. The upward passes, or the slap of the hands and sudden call, which are at once effective when used by the right operator, may be repeated in vain by others. This fact has occasionally led to very awkward results; as, for instance, in London some years ago, when one mesmerised "subject" was set to mesmerise some one else, and then, after he had succeeded in producing a state of profound coma, passed himself into a condition in which it was impossible to impress him with the necessity of undoing his own work.

This disagreeable incident suggests another well-known class of phenomena of which no explanation on any purely hypnotic hypothesis seems possible—the so-called "cross-mesmerism" or agitated bewilderment which is apt to result when a mesmerised person is subjected to new treatment from a second operator, before the effects of the former treatment have disappeared. The phenomena are of too alarming and distressing a kind to admit of deliberate experiment, but when once seen, are not easily mistaken; and a slight but sufficient indication of their nature is sometimes afforded in a momentary way by the violence which the "subject," who is perfectly docile in the hands of his mesmerist, will display when accidentally touched or interfered with by a bystander. If this be explained away as an instance of the "dominance of an idea," we ask, of what idea? Is it the idea of the operator, with which the "subject's" mind is so wholly engrossed as to react with violence on any attempt to divert it? But if he is in the alert stage, his mind is so little riveted on his operator that it is abnormally ready to be borne off by any and every suggestion; and if he is in the deep stage, it is an unwarrantable assumption that his mind is engrossed with anything at all. Nor can the view that *suggestion* is the cause of the phenomenon—though a natural enough one to start with and applicable to some cases—survive a prolonged and patient study of the facts. Instances will be found where it is practically certain that no idea, tending to make the "subject" dread interference from all persons save one, had been even remotely suggested to him; and where, if any such idea were really dominant in his mind, it could only itself be an instance of the specific *rapport* which hypnotism fails to account for.

Such considerations as these, though they lie across the threshold of the subject, are apt from their very generality to be disregarded; but it is easy enough to find in single definite phenomena—and these not among the outlying marvels above referred to, but among the experiments which are the stronghold of the hypnotic theories—a starting-point for similar objections. A boy is placed in a chair and is not hypnotised; but his arm is rendered stiff and insensible after a minute of downward stroking. "Reflex irritability," say some of our friends; "the monotonous sensory stimulation has produced the well-known tonic spasm." A thoroughly sound explanation; but let us try the effect of downward passes made without contact or any possibility of sensory stimulation. The same result ensues; the usual tests of torture and bribes may be applied, with complete impunity, to the "subject's" arm in the former case, and to the experimenter's pocket in the latter. "Expectant attention," say other of our friends; "an interesting example of the power of mind over body; the boy believes in the operator's power, and his mental energy, being absorbed into the single channel of the expected effect, brings that effect to pass." Very probably; but on experimental principles it is surely just worth while, before promulgating this very probable theory, to test it by a single variation of conditions. Let the experiment be repeated, then, with this difference—that the boy is made to read aloud a paragraph from a newspaper as long as the process continues, having been previously warned that he must carefully attend to what he is reading, as he will be examined in it afterwards. After this warning it is not surprising that he should stand the examination successfully; but a little surprising, on the proposed theory, that the stiffness and insensibility should again have supervened.* When such an experiment has succeeded with "subject" after "subject," and when their expressions of astonishment have suggested that in many cases the idea of the result was not even latently present in their minds, it is natural to devise measures for preventing the possibility even of the latent idea; as, *e.g.*, by extending the "subject's" ten fingers on a table in front of him, with a thick screen between them and his eyes, selecting a couple of them (the combination being of course varied each time), and then subjecting the selected pair to the same process as the arm. But I am approaching the region of marvels and the theory of specific influence which I have here forsworn. To relieve one's mind by observing how fairly the mesmeric hypothesis embraces and explains the facts which so violently break away from the hypnotic one, is perhaps not more unscientific than to neglect and ignore those intractable facts; but to those who do not share it, such relief will naturally seem to resemble the escape from subordinate perplexities which the devout Catholic makes by swallowing one huge assumption at the outset.

* With regard to the question how far the idea of his arm was present to the "subject" of this experiment, it was instructive to compare his vivacious reading and subsequent remembrance in this case with his mechanical reading and subsequent oblivion when (as described in *Proceedings*, Part V., p. 71) he had been thrown into the hypnotic state, and then had the idea of his arm prominently brought before his mind. In that case the idea remained truly dominant, and left no room for attention to the reading.