SUPPLEMENT.

I.

INTERNATIONAL CONGRESS OF EXPERIMENTAL PSYCHOLOGY.

By A. T. Myers, M.D.

The Congress of Physiological Psychology—whose name, however, was changed in the course of its debates to that printed above—held its first meeting in Paris, August 6-10th. Nearly 200 members had inscribed themselves (the fee being 10 francs), and more than half of these attended the meetings, although the rival attractions of other congresses tended to make attendance somewhat irregular. The principal meetings were held in the new Amphitheatre of the École de Médecine, and the sub-sections met in the class-rooms adjoining, the governing body having placed these very convenient quarters at the disposal of the Congress without payment.

Prof. Charcot, under whose presidency the Congress was convened, was unfortunately prevented from being present by indisposition, but Dr. Magnan and Prof. Ribot as Vice-Presidents, Prof. Richet as General Secretary, and MM. Gley and Marillier as Assistant Secretaries extended a courteous welcome to the foreigners present. Members from Austria, Belgium, Brazil, Chili, England, Germany, Holland, Italy, Mexico, Roumania, Russia (including Finland and Poland), Salvador, Sweden, Switzerland, and the United States, took part in the debates, and we believe that members from other countries were also present. The English Society for Psychical Research was represented by the President and Mrs. Sidgwick, Mr. Barkworth, Mr. Kleiber, Dr. Myers, and Mr. F. W. H. Myers. The American Society of the same name was represented by Prof. William James, Prof. Jastrow, and Mr. Riley (Delegate of the United States to the Exhibition). M. Marillier, one of the Secretaries of the Congress, is also Secretary for France to our Society. Many men well known in Medicine, Psychology, Physiology, and other branches of Science were present. Among them were MM. Ballet, Bernheim (Nancy), Binet, Bourru (Rochefort), Carus, Danilewsky (Kharkoff), Déjerine, Delbœuf (Liège), Drill (Moscow)

Espinas (Bordeaux), Ferrari, Fontan (Toulon), Forel (Zurich), Galton, Grote (Moscow), Pierre Janet (Havre), Jules Janet, Lapotine (Moscow), Liégeois (Nancy), Lombroso (Turin), Münsterberg, Neiglick (Helsingfors), von Schrenk-Notzing (Munich), Ploix, de Rochas (Tours), Séglas, Tokarsky (Moscow), and de Varigny.

The proceedings were opened on August 6th by an address from Prof. Ribot, who worthily filled the chair in Dr. Charcot's absence. He dwelt with justifiable exultation on the recent abundant and varied development of psychological studies—in the direction in which he has himself been a pioneer and a leader—and pointed out how this development, tending as it does to substitute a partially objective and physiological for a purely subjective and introspective method, has caused the need of mutual explanation among psychologists to be more strongly felt. He concluded by expressing a hope that this International Congress of Psychologists would be the first of a series of similar meetings.

The Secretary—Prof. Ch. Richet—then proceeded to sketch briefly the task marked out for the Congress. He explained that among the questions proposed for discussion there were three that especially demanded "collective" work. Among these he placed first the statistical inquiry into Hallucinations, mentioning the work that had been already done in this department by the Society for Psychical Research, and especially by Mr. Gurney, "dont la science déplore la mort prématurée." He dwelt on the importance of concentrated effort to establish, if it be possible to establish, by precise and trustworthy testimony, the fact of coincidental or "veridical" hallucinations, before proceeding to frame hypotheses to explain the fact. On this point he thought the Congress would be unanimous.

After speaking of the question of Heredity, as the second subject calling for collective effort, he went on to the third, "qui passionne aujourd'hui tous les psychologues," the question of Hypnotism. He expressed a hope that the rivalry between the schools of the Salpêtrière and of Nancy would soon be a thing of the past; dwelt on the need of introducing more precision into the terminology of Hypnotism; and pointed out that the proper business of the Congress was not to solve questions—which can only be done by the labour of individual experimenters—but, by free mutual communication of the results of such labour, both to obtain a clear view of the questions already solved, and to mark out lines of future study.

The Congress then divided for its morning meetings into four subsections, dealing respectively with Hallucinations, Heredity, Hypnotism, and the Muscular Sense. A fifth section dealing with "Coloured Audition," or the mental association between certain sounds and certain colours, was formed in the course of the proceedings.

The section on the study of Hallucinations met on August 7th, and discussed the question whether the sanction of the Congress should be invited for a further prosecution of the Census of Hallucinations, &c., as already set on foot by Prof. Sidgwick in England and France, and by the American Society for Psychical Research in the United States; or whether some modification of the scheme was desirable. Prof. Pierre Janet, Dr. Ballet (the well-known author of *Le Langage Intérieur*), and others urged that the hallucinations of the insane or hysterical should be studied and recorded along with the casual or unique hallucinations of sane and healthy persons. It was agreed that information of this kind also should be collected, but that the "census-paper" should be adopted practically as it stood, with one or two verbal modifications. A report to this effect was presented by M. Marillier to the Congress on the same afternoon, and some further discussion followed. Prof. Delbœuf, of Liège, recommended that special note should be taken as to the mental habit,—"visual, audile, or motile,"—of the subjects of hallucinations of each of these types, with which Mr. Galton and others have made the world of science familiar.

The resolution to continue the statistical inquiry on its present lines was then agreed to without any dissentients.

Prof. Grote, of Moscow, M. Marillier, Prof. James, and Prof. Sidgwick were afterwards designated by the Committee of Organisation to superintend the work in their respective countries.

A "questionnaire" for wide circulation was also adopted by the section on Heredity, which met under the presidency of Mr. Galton, —who subsequently presented to the Congress a very interesting report on the chief questions in this department that appear to admit of experimental solution through co-operative work. On the motion of Prof. Gruber, of Roumania, the section on Coloured Audition adopted a similar method for collecting information. The section on Hypnotism was, as had been expected, the most largely attended. We give a brief account of some of the speeches.

The Committee on Hypnotism met first on Wednesday morning, August 7th. Prof. Delbœuf (of Liège) was elected chairman for the day, and Prof. Ch. Richet introduced the discussion on the terms that had best be used in hypnotic description. Along with M. Brissaud he had drawn up definitions of many of the chief words, and he wished further suggestions and discussion, especially on "Hypnotism," "Animal Magnetism," and "Somnambulism." "Hypnotism" was a word introduced by Braid, and might be defined as an artificially produced somnambulism (*somnambulisme provoqué*). "Somnambulism" they proposed to define as a condition analogous to sleep, but differing from it in retaining more signs of external impressions (*persistance de*

quelques phénomènes de la vie de relation), and differing also from the normal waking state by showing an alteration of personality and a complete loss of memory. It might be natural or artificially induced. When natural it was a pathological condition, commonest in young subjects, and coming on, as a rule, during normal sleep. It might be artificially induced by some ill-defined manipulation which was called "magnetic," or by suggestion, or by some physical action such as gazing at a bright body, or more often by some combination of these causes. "Animal Magnetism" was a term which was not accurately defined in common use, but which could be used for all the agencies which bring on somnambulism; for example, the "passes" that were sometimes called "magnetic." Magnetism used to be considered an exact term in the 16th and 17th centuries when applied to both plants and animals, and it very nearly corresponded then to what was now called action at a distance.

In the discussion that followed on the definition of these terms, MM. Bernheim, Forel, Espinas, Liégeois, Ch. Richet, and Delbœuf took an active part, but it was found that an exact emendation of any such difficult phrases as these definitions could not be reached by as large a gathering as about 40 members of this Committee. Prof. Bernheim vigorously expressed his opinion that our knowledge of Animal Magnetism and Hypnotism was as yet too imperfect to allow of our fixing their limits exactly; he was himself inclined to keep the term Animal Magnetism for historic use as describing the phenomena of a past generation; to employ Hypnotism as a newer word to cover a large area as yet imperfectly known, and which it would be premature to define exactly, but which did not necessarily imply any condition of sleep; and to restrict Somnambulism to a condition analogous to sleep and produced by suggestion or hypnotism. Prof. Liégeois wished to give up the use of "Animal Magnetism" in any accurate discussion, as being a term based on an old mistake. After some further debate a decided majority of the Committee voted against the use of "Hypnotism" and "Animal Magnetism" as synonymous terms.

A short discussion on Automatism led quickly to philosophical difficulties and was not pressed to a division. The proposed definition of a subject under Suggestion was that he could not resist the idea or act suggested. Prof. Bernheim, with whom MM. Liégeois and Forel substantially agreed, protested against the assumption of a natural resistance to suggestion which was implied in this definition, but no exact alteration in the form of words was agreed upon.

Thursday morning, August 8th. M. BALLET in the chair.

M. OCHOROWICZ read a paper on "La Sensibilité Hypnotique." There were some people in the world, he said, who were not hypnotisable;

that was a fact which was admitted by all. It led to the question, what qualities made a man a good or a bad subject? His aptitude might be shown in various ways, of which there were at least four which might be clearly distinguished, viz.: (i.) the readiness with which he could be hypnotised; (ii.) the depth of sleep which could be obtained; (iii.) his greater or less sensitiveness to suggestions, and (iv.) the delicate variations and elaborate character of the symptoms. This aptitude seemed to be innate and hereditary. Statistics on this point were wanted. Was it to be called a disease, a morbid diathesis, or simply one form *sui generis* of the nervous temperament? Was there any connection between this hypnotic sensibility and hysteria, anæmia, &c.? Were any perfectly healthy people hypnotisable? It was generally admitted that by being frequently hypnotised the subjects became more sensitive, but it was not determined whether there were other ways by which this might be brought about, and whether any degree of unsusceptibility might be overcome by patient and repeated trial. There were further questions as to the influence of race, sex, and social position on the susceptibility to hypnotism and the ready diagnosis of good subjects from external signs. He showed a hypnoscope which he had himself brought into use. It consisted of a short and broad bar magnet bent into a circular form so as to fit one of the fingers. When it had been worked on one finger for a few minutes it was often found that that finger was stiff and to some extent anæsthetic. In his opinion that symptom was co-extensive with susceptibility to hypnotism and might be accepted as a valid test, whether it was due to any magnetic influence or only to suggestion.

Prof. Charles Richet hoped that the important questions raised would meet with full discussion, and remarked that in his experience he had found some hysterical subjects not hypnotisable, and certainly also many hypnotisable who were not hysterical.

Prof. Bernheim said he had found nearly all persons hypnotisable; but some hysterical subjects were very difficult to hypnotise and some who were not hysterical were most easily hypnotised. He had found hypnotism possible at all ages; it was on the whole more difficult in the educated classes than the uneducated, as there was more personal reserve and self-control in them.

Prof. Charles Richet considered the French and Italians as particularly hypnotisable races, though many further observations were wanted on that and similar points, and agreed with M. Bernheim as to the greater susceptibility of the uneducated classes.

Prof. Delbœuf had found about 75 per cent. of almost all classes in Belgium hypnotisable; colonels and generals as well as the lower classes.

PROF. H. SIDGWICK said he should like to ask as a preliminary question in this discussion whether we had good grounds for considering all hypnotisers of equal power?

PROF. FOREL (of Zurich) remarked that he had not found any difficulty, after a few weeks' practice, in hypnotising about 85 per cent. of the Swiss on whom he tried; and he understood that Wetterstrand in Sweden had found no greater difficulty with 4,000 subjects, and Van Eeden also in Amsterdam. The hypnoscopic test had not been found satisfactory in some Russian experiments.

PROF. CHARLES RICHET said that Prof. Sidgwick's question stood much in need of an answer which it was not easy to furnish. The magnetisers of a previous generation had certainly had a strong opinion that the hypnotising power was much greater in some individuals than in others. In his own experience he was inclined to think he had himself less capacity for hypnotising now than he had had some 20 years ago. It seemed to him to be not a loss of authority but of influence. He could give no reason for it, and personal power was to him a problem of the very greatest complexity. M. Tarchanoff had very recently exhibited at the Société de Biologie some very delicate electrical experiments which went to show that a sensitive galvanometer revealed an alteration in a man's electric condition according as he thought of the left hand or the right. If there was a perceptible electrophysical change produced in this way it was not impossible that in hypnotism one agent might be perceptibly different from another in his physical influence.

MR. F. W. H. MYERS described an experiment which had been devised by Mr. Gurney and repeatedly tried upon a sensitive subject (F. Wells) at Brighton to test the difference of his reaction to different individuals without any opportunity of suggestion by the ordinary paths of sense. The subject was placed behind a tall screen so as to shut him off entirely from the experimenters and his hands passed through the screen and spread out on a table in front of him. No contact or talk was allowed. Over one finger Mr. Smith, who had often hypnotised the subject, held his hand at the distance of an inch or more; the other observers held their hands over other fingers in an exactly similar manner. In nearly every case it was found that anæsthesia and rigidity were produced in the finger over which was Mr. Smith's hand and not in the others. Great care had been taken to eliminate suggestion, and the nearly uniform result pointed to some specific personal influence.

PROF. DELBŒUF related a case in which he had found the delicacy of the sense of touch so greatly increased in a hypnotised subject that she had been able to distinguish every card in the pack by touch alone. He attributed the results Mr. Myers had described to a similar

hyperacuity of feeling which had enabled the subject to tell one hand from another at a distance.

MR. MYERS observed that in the experiments he had mentioned they had tested the subject's hands in other ways for hyperæsthesia, but had found none.

M. GILBERT BALLET was nevertheless inclined to attribute the results to an abnormally developed capacity of distinguishing the temperatures of different hands which were not in actual contact.

PROF. BERNHEIM thought that the electrical changes Prof. Richet had mentioned would be explained by the unconscious muscular contraction accompanying the thought of one hand or the other.

PROF. RICHET replied that muscular contraction would not be an explanation of the electrical change; it was more possible that it might be due to an influence of attention on the sweat glands.

Friday morning, August 9th. PROF. BERNHEIM in the chair.

After a short paper by M. ALLIOT attempting to connect the varying conditions of hypnotism with the electrical conditions of the human body, the discussion was continued by M. OCHOROWICZ, who expressed his opinion that the phenomena of hypnotism were not all explicable by suggestion only, for instance, in the case of infants and animals. He thought that there was more power in magnets than could be explained by suggestion. He had himself observed that motions which did not convey any suggestion had definite effects; for example, transverse passes over the arm of a hypnotised subject diminished its strength whilst longitudinal passes increased it.

PROF. CH. RICHET proposed to classify all the states characterised by an alteration of personal qualities under three headings. viz.: (1) spontaneous conditions, normal and pathological, such as sleep, somnambulism, &c.; (2) conditions artificially induced, either by suggestion, which had been shown capable of producing both mental and physical change, or by physical influences, such as those of magnets or electrical conditions, which it was at present very difficult to estimate conclusively and to divide accurately from suggestion. In addition to these there were (3) the further influences of action at a distance, telepathy, and mental suggestion, the proofs of which were not by any means universally regarded as satisfactory. Their science was at present embryonic, and hardly ripe for discussion, though it needed careful attention.

PROF. FOREL thought it very possible that the results of M. Ochorowicz's experiments might have been obtained by unconscious suggestion from the acts, expression, and gestures of the agent. It was difficult to limit the amount of meaning that might be unconsciously hidden in these without words. He was not at all wishing to deny telepathy, but he could not admit that M. Ochorowicz had proved it.

PROF. H. SIDGWICK hoped that their attention might be recalled to three conditions where he thought suggestion might be excluded, viz.: (1) experiments with animals, (2) with babies, and (3) at a distance.

PROF. BERNHEIM said there were two theories on these points; the first was that of suggestion, which he maintained himself, and the second that of the "fluidists" who were there represented by M. Ochorowicz, who maintained some further action on the person than by the brain of the percipient. That he regarded as possible, but at present unproved. The passes and staring at a bright object brought in some points of suggestion of sleep, by quiet and by tiring the eyes. He did not wish to deny the effects of some similar actions, but he interpreted them by suggestion. In animals he regarded the state produced as one of catalepsy, and similar to the condition of men occasionally seen in some very exhausting diseases, such as typhoid fever. With some babies still at the breast M. Liébeault had considerable influence in stopping pain and digestive discomfort by laying his hand on their stomachs, or even, he believed, by bathing them with magnetised water, or, indeed, any water. How soon children might become susceptible to some suggestion it was hard to say; it might be when they were a day old, very probably before they were a month.

M. GILBERT BALLET was surprised by the use of the word suggestion for what the experimenter did not expect. If there was always a psychical process to be called suggestion between the physical agent which brought on sleep, and the sleep resulting from it, he would ask what it consisted in when sleep was produced by a sudden loud noise or bright light.

PROF. BERNHEIM replied that in these cases there was a fresh awakening of previous suggestions.

PROF. PIERRE JANET cited two cases where sleep was so produced on a first trial.

PROF. BERNHEIM was inclined, if the subjects had never before heard any report of this plan, to call the cases catalepsy, and to doubt the truly hypnotic character of the results.

PROF. CHARLES RICHET had been much interested in the discussion of the limits of suggestion. If the use of the word was confined to its ordinary meaning he thought that important as its agency might be in the results of hypnotism it certainly was not the sole cause.

PROF. DANILEWSKY (of Kharkoff) then went on to read his paper on the study of Hypnotism in Animals. He had obtained hypnotic results in a long list of animals, going upwards from the shrimp, the crab, the lobster, the sepia, to several fishes (among them the cod, the brill, the torpedo-fish), the tadpole, the frog, the lizard, the crocodile, the serpent, the tortoise, several birds, the guinea pig, and the rabbit. He had

generally found it sufficient to place the animal in some abnormal position, *e.g.*, on its back, and keep it quiet with slight continuous pressure. Under these conditions it soon fell into a condition of loss of voluntary movement, and anæsthesia of the skin and mucous membranes, so that, for example, after a time the artificial stoppage of its means of respiration did not excite any appropriate resistance, and the appearance at the same time of some spasms and convulsive movements gave the action the character of an emotional struggle. Repeated hypnotisation lessened the resistance of the animals, so that they became more and more susceptible. In some of the animals and birds if injury was done to the semi-circular canals in the ear so that involuntary circular motion naturally followed, it was found possible to stop this so long as they were hypnotised. When the animal woke from hypnotism and changed its position the circular motion began again. There were two conditions which it was necessary to distinguish: (1) *Catalepsy*, which was a condition of arrest of voluntary movements and of anæsthesia, and was generally brought on by strong and painful external stimulus; and (2) *Hypnotism*, which was induced without violent stimulus. The anæsthesia of hypnotism and the emotions of hypnotism were the result of the inhibitory power of the brain; and if the brain was taken away these results disappeared also. External constraint provoked in an animal a feeling of inability to defend itself and a paralysis of the will followed. That was the first condition for inducing the phenomena of hypnotism in animals and men. Animals got their feeling of irresistible coercion from their skin and their bodily cases; men from psychical causes. Verbal suggestion to a man was analogous to bodily suggestion to an animal from the hands of a hypnotiser.

Saturday Morning. August 10th. PROF. ESPINAS in the chair.

M. BABINSKI was called upon by the Chairman to explain the views of the school of the Salpêtrière upon hypnotism, and began by remarking that these views had been recently put into print[1] and supplied some answers to the objections raised by the school of Nancy. M. Charcot had studied hypnotism in hystero-epileptic patients alone, because he found in them good types for study. He did not deny that hypnotism might be observed in other patients, and that the phenomena observable in the hystero-epileptics might not be observable in all others. Suggestion was admitted by the Parisian observers to be important, but not to be the only source of the hypnotic phenomena. If a patient who was unacquainted with medical facts and entirely ignorant of hypnotism showed when hypnotised the contractures which

[1] *Grand et Petit Hypnotisme.* Archives de Neurologie. 1889, Nos. 49-50.

belonged to the lethargic state, although the hypnotiser had given him no hint whatever by word or gesture, it could not be said that suggestion was the cause. Why should the characteristic muscular state be contracture rather than paralysis, tremor, or any other symptom? And after M. Bernheim had produced hypnotic sleep as he said by suggestion why did he find anæsthesia which he had not suggested? Why did pressure produce contracture in the lethargic state and not in the cataleptic? It had been objected that the three consecutive states which M. Charcot had described,—the lethargic, the cataleptic, and the somnambulic,—were themselves the result of suggestion. But even if that were possible it would not explain their occurrence in the first cases where they were observed. It was said that they had only been found at the Salpêtrière, but some similar observations had been made by Tamburini, Seppilli, Vizioli, David, and Ladame. Hypnotism he regarded as a pathological and not a physiological state, and in character allied to hysteria, for (1) they had certain symptoms in common, (2) the stages of hypnotism were like the stages of the hysterical attack, and (3) there was an interdependence between hypnotism and hysteria such as was seen with some other conditions intimately related. The results of M. Charcot's experiments on hysterical patients which had been published in 1882 had not lost any of their truth or value.

Prof. Lombroso (of Turin) had tried hypnotism on seventy persons in Bologna. He had produced a truly hypnotic state in only a few persons, in all of whom there was some morbid nervous condition, but had noticed what he should prefer to call credulity in many of the lower classes.

Prof. Espinas (of Bordeaux) had observed that whilst suggestion was used without restriction at Nancy, nevertheless, at the Salpêtrière it was said to be very rarely tried, for fear of causing an attack of hysteria. Was that fear well founded?

Prof. Forel considered it possible to make some patients hysterical by hypnotism, but that was only when very wide limits were allowed to that vague word "hysteria," and when hypnotism was used for a long time with the special attempt of producing it. There could be no doubt that when hypnotism was fairly used on a large number of people it was found that it was not confined to, or, indeed, much helped by hysterical temperaments.

M. Babinski admitted that he had not had the opportunity of studying the effects of hypnotism widely on non-hysterical persons.

Prof. Pierre Janet (of Le Hâvre) did not think that to be hypnotisable was in the least a proof of being hysterical. It was rather a sign of mental and moral weakness, of an incapacity of fixed attention; and from such incapacity, which he considered a definite disease (*maladie*), arose the anæsthesia which was to be found both

in hysteria and hypnotism. In his own trials of hypnotism he had succeeded in about 80 per cent.

Prof. Forel had himself succeeded in about 60 or 70 per cent. of cases when he began to practise hypnotism, and in a large number of people who had no such disease as Prof. Janet described, but were perfectly healthy. After more practice he had succeeded in as many as 90 per cent., and he came to the conclusion that fatigue was a condition which rendered the subjects more susceptible. With the insane he had found hypnotism extremely difficult.

Prof. Charles Richet protested against the word "disease" which Prof. Janet had made use of for conditions which, even supposing they were not the most absolutely normal, would certainly not be included in what a doctor would understand by disease. And for his own part he thought some hypnotisable people were absolutely normal.

Prof. Delbœuf quite agreed with Prof. Richet on this point. To be hypnotisable depended on attention, not on disease. He had found himself able to arrest salivation by self-suggestion when under the hands of a dentist; and one of the necessary conditions in his own case was the capacity of concentrating his attention, not that incapacity of fixed attention that Prof. Janet had spoken of. As to the Salpêtrière phenomena, he observed that after he had himself first visited the Salpêtrière he found that his own subjects manifested those phenomena. But when he had learnt from the writings of the Nancy school that these contractures, &c., did not necessarily occur, they ceased to occur in his own subjects.

M. Ochorowicz said his experience during about twenty years for which he had practised hypnotism had shown him that the insane were the most difficult of all subjects. The susceptibility to hypnotism he had found persistent through middle and elder life, a point in which it differed markedly from hysteria.

Saturday afternoon, August 10th. Prof. Delbœuf in the chair.

Mr. F. W. H. Myers described some experiments which he and other members of the Society for Psychical Research had made to test the possibilities of thought-transference when the recognised means of communication through the senses were cut off. The subject was a healthy person who was hypnotised and between whom and the experimenter a screen was in many cases placed. The experimenter then drew a counter on which was written a number of two figures from a large collection of these in a bag, and observing very strict conditions in detail, he fixed his attention on it, asking the subject to let him know if by any means he became acquainted with it. The answer was not correct in every case, but the total number of correct answers in a very long series of experiments was so vastly

greater than would have been the result of chance, which under these conditions could be mathematically calculated, that he could not doubt that there was some other agency at work, which was neither fraud nor chance but thought-transference.

Prof. Charles Richet knew well the experiments described by Mr. Myers, and had himself made some others which led to a similar result. Such experiments, he thought, should be repeated widely and with the greatest care, for if the proof of thought-transference to which they led could be established, without a doubt it would be one of the greatest discoveries of our time.

Prof. Sidgwick remarked that results of a similar character had been obtained with subjects in a normal condition as well as in hypnotism. At the same time the experiments of himself and his colleagues seemed to show that success was rather more likely to be obtained in the hypnotic than in the normal state. He entirely agreed in the view that more experiments were urgently required.

Prof. Delbœuf had paid some attention to these phenomena, but had not been able to satisfy himself of any similar results in experiments of his own. He had been struck with a remarkable power in those who had been deeply hypnotised of making an exact estimate of time, and had noticed many post-hypnotic suggestions carried out exactly to the minute after an interval of several hours.

After some discussion as to the date and place of the next meeting, it was unanimously agreed that the next reunion of the Congress should be held in England early in August, 1892.

It is hoped that a Committee of Reception may be formed in England before that date; but in the meantime a Committee of Organisation was appointed, which is to meet about Christmas, 1891, and consider the subjects to be proposed for discussion at the Congress. It is hoped that a programme of these subjects may be printed in English, French, and German, some months before the Congress actually re-assembles.

II.

AD INTERIM REPORT ON THE CENSUS OF HALLUCINATIONS,

Up to October 24th, 1889.

In England the whole number of answers received is:

	"No."	"Yes."	Totals.
From men	1181	112	1293
From women	1382	251	1633
Unstated	2	—	2
Total	2565	363	2928

Percentage of "Yeses," 12·4.

Of the persons answering "Yes" 64 have as yet sent no particulars.

113 persons have had more than one experience, either the same repeated more than once, or different experiences.

The experiences recorded may be classified as follows:

A.—Experiences Affecting More Than One Sense.

	Coincidental.	Non-Coincidental.			Totals.
		Representing a Living Person.	Representing a Dead Person.	Unrecognised.	
Visual and Auditory	4	1	7	4	16
Visual and Tactile	1	1	2	3	7
Auditory and Tactile	1	1	—	1	3
Visual, Auditory, and Tactile	—	—	1	1	2
Total number of Cases...	6	3	10	9	28

B.—Experiences Affecting One Sense Only.

I.—Visual.

1. Coincidental—				
a. Recognised		14		21
b. Unrecognised		7		
2. Non-coincidental—				
a. Human apparitions:				
α. Of living people		44		
β. Of dead people		20	159	
γ. Unrecognised		91		
δ. Of an arm or hand		4		184
b. Non-human apparitions:				
α. Of animals		7	25	
β. Of inanimate objects		18		
Total				205

24 of these are said to have been collective experiences; viz., 2 coincidental cases (1 recognised and 1 unrecognised) 4 apparitions of the living, 1 of the dead, 16 of unrecognised human beings, and 1 of an inanimate object.

In the above table 30 cases in which the percipient had more than one experience, but did not describe them singly, are counted each as one case.

II.—Auditory (Voices).

1. Coincidental—				
a. Recognised		14		22
b. Unrecognised		8		
2. Non-coincidental—				
a. Trivial and often repeated experiences, generally of the name being called, sometimes recognised and sometimes not			43	
b. Recognised. Of Living Persons:				
α. Calls or voices		9		
β. Name called on two occasions		1		
γ. Short conversation		2	15	
δ. Sentences		1		
ε. Familiar words and phrases		1		
ζ. Song		1		93
c Recognised. Of Dead persons:				
α. Calls or voices		7		
β. Calls twice repeated...		1	9	
γ. Sentence		1		
d. Unrecognised:				
α. Calls or voices		18		
β. Calls twice repeated...		1		
γ. Sentences		4		
δ. Counting		1	26	
ε. Crooning a tune		1		
ζ. Music and faint voices		1		
Total				115

6 of these are said to have been collective experiences; viz., 2 coincidental cases of the name being called (the voice being recognised in one case and not in the other), 2 recognised living cases (1 call and 1 song), 1 trivial experience and 1 unrecognised case (crooning a tune).

III.—TACTILE

1. Coincidental—				
a. Recognised	1	2		
b. Unrecognised	1			
2. Non-coincidental—				
a. Recognised touch of Living person:				
a. Single touch	1	2		
β. Recurring touches	1			
b. Recognised touch of Dead person:				
a. Single touch	1	3	22	
β. Recurring touches	2			
c. Unrecognised:				
a. Frequent touches	6	17		
β. Touch, &c., once	11			
Total				24

One case of a single unrecognised touch is said to have been collective, one percipient seeing a form while the other felt a touch.

In this analysis no account is taken of morbid conditions which undoubtedly existed in some cases, being indeed explicitly mentioned occasionally. But the great majority of the percipients were, according to their own statements, in a perfectly normal and healthy condition at the time of their experiences.

Also no attempt has been made as yet to make more than a rough estimate of the possibilities of error in the accounts through defects of memory of mistakes of inference. In particular, the probability of the figure seen being a real human being, or the sound heard a real human voice, in some of the collective cases requires to be carefully examined.

As regards other countries than England, we heard in August that about 2,000 answers had already been collected in America, and Mons. Marillier reports in October that he had received 633 answers from France and Switzerland as follows:

	"No."	"Yes."	Total.
From men	366	57	423
From women	161	49	210
Total	527	106	633

He had received as yet no particulars from about 50 of the persons answering yes. Among the remainder, 24 of the experiences are said to have been veridical.

The enquiry has also been commenced in Germany, but not yet in Russia nor in Italy.

I may remind my readers that a report on the census is to be made to the International Congress of Experimental Psychology in 1892, and that we should like by that time to have 50,000 answers. Further assistance in collecting is urgently needed, and I shall be glad to correspond with any one willing to help in the work.

HENRY SIDGWICK.

III.

PROFESSOR PIERRE JANET'S "AUTOMATISME PSYCHOLOGIQUE."[1]

By Frederic W. H. Myers.

The name of Professor Pierre Janet has long been familiar to the readers of these *Proceedings*. We have been amongst the first and warmest appreciators of the remarkable articles in the *Revue Philosophique* in which he has for several years past recounted the results of a series of experiments on human automatism, &c., seldom surpassed for care in observation and acumen in interpretation. We shall, therefore, be prepared to join cordially in the welcome which French *savants* are now extending to M. Janet's *thèse présentée à la Faculté de lettres à Paris* under the title of *L'Automatisme Psychologique*, "an essay in experimental psychology upon the inferior forms of human activity." This book contains the gist of the above-mentioned articles, and much more besides; and we consider that it at once places M. Janet in a front rank of experimental psychologists. It ought, we think, to be translated into English and other languages, and studied by all who are interested in researches of this kind.

But when a book is so full of new observations and reflections as this book is,—and observations in so difficult a domain,—it is not by mere general expressions of praise that we shall show it the truest respect. Its greatest merit is that it opens new paths; and in a new path we may walk side by side like explorers rather than follow in a leader's steps like sheep. Much of the book is occupied with criticism,—reasonable and effective criticism,—on views which have been set forth in these *Proceedings;* and much of our limited space must be given to an answer to those criticisms,—such answer as we make to an opponent whom we desire not to confute but to persuade.

The work begins in a manner unusual in psychological treatises, but, in our view, strictly logical. "Total automatism" is the title of the first part, and "Isolated psychological phenomena" of the first chapter. What is implied in these titles is the new, the experimental method of getting at the simplest beginnings of human consciousness and intelligence. No merely imaginary or metaphorical simplicity, such as Condillac's "breathing statue," can be a really simple notion, or afford a true basis on which to upbuild our conceptions of gradually developing personality.

Dr. Hughlings-Jackson (with whose works, little known in France, M. Janet does not seem to be acquainted) has taken *coma* as representing a "lowest level of evolution," and has traced the operation of nerve-centres at different levels as they come into prominence at successive stages of the *dissolutive* process of an epileptic explosion. What we want to produce and watch, however, is of course not the catastrophe, but the evolution of the psychical cosmos;—not the breaking down of one set of reservoirs

[1] *L'Automatisme Psychologique*, par Pierre Janet. (Paris: Alcan, 1889, pp. 496).

of nerve-force after another, but the gradual calling into operation of higher and higher connections. And M. Janet is right, I think, in taking the condition of hypnotic *catalepsy* as the lowest starting-point which can be safely reproduced in practice. Judging both from external indications and from that memory of cataleptic attitudes which sometimes persists into a somnambulic state, the cataleptic subject is in that condition of impersonal consciousness which we must suppose to exist in the animal and in the infant, and which is occasionally experienced and even remembered by the adult, on his recovery from anæsthetisation by drugs, or from a profound fainting-fit. Professor Herzen's description of this latter experience deserves quoting here, for it gives us probably a more vivid notion of "total automatism" than any mere observation from outside could afford.

"During the faint," he says, "it is absolute psychical nonentity, complete absence of consciousness; then one begins to have a vague, unlimited, infinite feeling,—a feeling of existence in general without any delimitation of one's own individuality, without the least trace of a distinction between the I and the not-I; one is then an organic portion of nature, having consciousness of the fact of one's existence, but no consciousness of the fact of one's organic unity; one has, in two words, an impersonal consciousness:—sensations which, from the mere fact that they remain isolated cannot be known, but only felt."

By hypnotic catalepsy is here meant a state in which there is no initiative of movement, but in which an attitude or a movement can be impressed from without upon the subject,—who will inevitably retain the attitude, or repeat and complete the movement. Imagining this state from within, and from a psychological standpoint,—a task which M. Janet has faced more boldly than any predecessor,—we reach the following conclusions (p. 66): "Many sensations and images are accompanied by a bodily movement and cannot exist without producing it; every sensation or image persists in the consciousness until another phenomenon occurs to efface it; every sensation or emotion tends to develop and complete itself, and to manifest itself by appropriate acts."

In the cataleptic subject we witness the play of these isolated sensations and images, not yet collected and correlated under the conception of a central personality.

Here, then, we have a starting-point; what are the next stages on the upward road? From the cataleptic state (it would be usually said) we rise to the somnambulic, and from the somnambulic to the waking condition. But note that our conception of the somnambulic state,—what used to be called "the mesmeric trance,"—is gradually undergoing development, as more prolonged experiments are made. When this state was only maintained (as by the earliest mesmerisers) for a few minutes or hours, attention was naturally directed to its first or superficial aspects,—the habitual anæsthesia,—the *rapport* with the mesmeriser only,—the readiness to receive suggestions,—and, of course, the alternation of memory, and forgetfulness on waking. Further experience has shown that the phenomena of anæsthesia and of *rapport* are by no means uniform, and that *suggestibility* is by no means confined to the somnambulic state, but often exists in waking subjects. We are, in fact, obliged to admit that there is no one phenomenon which invariably

characterises the somnambulic state ; and that all we can say is that the subject is not quite the same as in the waking state, and that there is generally a more or less complete forgetfulness in the waking state of what has passed in the "trance."

There is, I think, a wider conclusion to be drawn from these facts than M. Janet has attempted. But before indicating that conclusion I must note the extremely ingenious observation which our author has made as regards one at least of the conditions accompanying and determining these somnambulic changes of personality. M. Janet's experiments were made on 27 persons, all of them hysterical, epileptic, or insane ; and although this limitation of his experience to diseased subjects has, as we shall presently see, in some ways much cramped his conceptions, it has also had the advantage of concentrating his attention upon certain marked and extreme phenomena, which previous observers had usually witnessed only in a fleeting or accidental way. He noticed, then, in one of his subjects that there had been various *lacunæ* in her memory before she had ever been hypnotised, and that he could not summon back the recollection of these periods even in her somnambulic state. But this was a subject who passed through many forms of somnambulism ; and in a new phase which she one day entered she spontaneously gave an account of what had happened in those blank periods.

M. Janet naturally tried to discover whether this new somnambulism possessed any special characteristic linking it with those previously unremembered periods in Rose's past. He found that,—whereas in ordinary life and in all previous somnambulisms she was wholly anæsthetic,—yet both in this new somnambulism and in those blank periods of life she was only hemi-anæsthetic,—having recovered tactile and muscular sensibility on the right side. Other observations followed,—some of them of a very delicate and ingenious kind,—and M. Janet came to the conclusion (p. 109) "that the alternating memory of somnambules is due to a periodical modification, whether spontaneous or induced, in the state of their sensibility, and, consequently, in the nature of the images which serve as the basis for complex psychological phenomena, and especially for language. This modification finds place particularly in subjects more or less anæsthetic in their normal state, and then consists in the temporary restoration of a certain category of images of which the subjects in their ordinary state have lost possession." Thus—adopting the distinctions with which Mr. Galton has made us familiar,—Léonie is a *visual* in her waking state, an *audile* in her second state (Léontine, now termed Léonie II.), and a *motile* in her third state (Léonore, now termed Léonie III.). Each set of images forms a chain of memory of its own, and the transition from the predominant use of one set of images to the predominant use of another necessarily involves a certain change of personality.

These remarks appear to me to suggest an important field of observation. They do not, indeed, cover the whole ground ; for there are abundant cases of alternating memory where the subject presents no appreciable change in mental habits of the kind here insisted on. And I may add that M. Janet's observations,—in which states of hemi-anæsthesia play no small part,—seem to me to add confirmation to my own view (*Proceedings*, Vol. III., pp. 43 and 99) that alterations in the predominance of one or other cerebral hemisphere

have something to do with these changes of personality, of which automatic writing is now recognised as one of the most instructive manifestations. I can scarcely understand why M. Janet disapproves of this view (p. 415), which seems to me entirely consistent with his own, and which was in fact based in part upon the very same observations. M. Janet refers to Louis Vivé, with his changes of character coinciding with the shifting or disappearance of hysterical paralyses. I also referred to that case; and surely when hemi-anæsthesia and hemiplegia are amongst the most marked of the phenomena with which we are dealing, there is nothing fanciful in assuming that there are coincidental changes in the equilibrium of the cerebral hemispheres. The suggestion—which I owe to Dr. Ireland—that Spiegelschrift may represent the word-vision of the right hemisphere, still seems to me ingenious and probable; and although M. Janet has never witnessed Spiegelschrift among what he calls "*un assez grand nombre de sujets*," I must venture to say that his score or so of writing subjects (for not all his 27 subjects wrote) is not for present purposes a sufficient number; and that I, who have seen more writing subjects than M. Janet has—(though I am far from asserting that I have observed them with care or skill to equal his)—have witnessed this Spiegelschrift in a good many independent cases. Unfortunately I cannot say in how many; for while the inquiry was a mere curiosity of my own, I regarded the incident as too common to need record; and now that the matter has become one of controversial interest, I am afraid of suggesting my own view to any automatist with whom I am concerned.

On one point M. Janet (who is very careful and accurate in his citations from our *Proceedings* and other English sources) seems to base an objection on a misconception (p. 415) of the phenomenon which I am describing. I draw a parallel between the sufferer from verbal cecity and the writing automatist who does not know what he has written, and who writes therefore without the aid of the "word-picturing centres" of his left hemisphere. M. Janet supposes that my automatist is partially anæsthetic—"*le médium n'a pas la sensation des mouvements.*" But he is *not* in any degree anæsthetic in the cases to which I am alluding: he *has* the full sensation of the movements, and he can sometimes guess by the movements what word he is writing, although he has no mental vision of that word in his conscious intelligence. My parallel is therefore a closer one than M. Janet has supposed.

I should have some other rejoinders to make to the criticisms on pp. 415-9. But the discussion may well be left until there are a good many more observations to analyse. Automatic writing occurs, it is evident, under more forms than any single observer has yet noted; and the urgent matter is to get experiments carefully made and recorded in *milieux* as different from each other as can be contrived. Let us not lose the true independence of each experiment by falling prematurely under the *power of suggestion* of any one theory.[1]

[1] It is to me a real disappointment, and I think that it is a real drawback to the attainment of a complete view of the subject, that there should apparently be almost no producible experiments now made by those who believe that these automatic writings sometimes emanate from disembodied (or unembodied) minds. That there should be

I now return to a statement of M. Janet's, already cited ; from which, as I have said, it seems to me that conclusions much wider than his own may fairly be drawn. He says,—and I fully concur,—that there is no specific character which belongs to the "somnambulic state" in itself. "The somnambulic state," he remarks, p. 125, "has only *relative* characters ; and can be determined only in reference to another period of the subject's existence,—the normal or waking state. . . . Somnambulism is a second existence which has no other character except that it *is* the second."

Taken by themselves, and detached from their modifying context, these very words might be used to express what I believe to be a profound truth,—which a great part of M. Janet's book is employed in combating.

I believe, in short, that we have no right to go a whit beyond actual observed facts in any judgment which we may pass as to the relative superiority or "normality" of any of man's different states. I refuse to call my actual waking state "normal" or "natural" in any sense except that of habitual or ordinary. It has been shown that in a very large number of persons,—many of whom (as Mr. Wingfield's Cambridge subjects[1]) are excellent examples of health and vigour,—certain changes of memory, sensibility, character, occur or can be induced, which in cases where they are carried furthest amount to a profound—even a permanent—even a salutary—modification of personality. Taking, then, myself as my example (lest I offend my reader by supposing him capable of being changed for the better), I cannot suppose that I am made on a different pattern from these men simply because the empirical modes of inducing these changes, as thus far discovered, happen to have no effect on me. I conclude that I simply do not know of what modifications the stream of consciousness of which my organism is the basis is potentially susceptible. I know this no more than I know of what modifications the human germ is susceptible. Since the era of my protozoic ancestors the germ which is now human has shown absolutely unpredictable potentialities. Whatever be the part which we assign to external influences in its evolution, the fact remains that the germ possessed the power of responding in an indefinite number of ways to an indefinite number of stimuli. It was only the accident of its exposure to certain stimuli and not to others which has made it what it now is. And having shown itself so far modifiable as to acquire these highly specialised senses which I possess, it is doubtless still modifiable in directions as unthinkable to me as my eyesight would have been unthinkable to the oyster. Nor can we limit the *rate* of change, which, so far as cerebral modifications are concerned, may probably be increasingly rapid as it has an increasingly complex material to work on. All I can say is that I am a momentary link in a chain of organisms perpetually changing in accordance with an unknown path of evolution ; and my present conscious condition represents no norm whatever, but only the historical fact that my ancestors' actual mode of development was sufficiently suited to their environment to keep them alive.

so many Spiritualistic journals in the world, and yet so few attempts either to prove or to illustrate this central article of faith, is to me a never-ceasing wonder. I can only reiterate my own anxious desire to receive records of experiments from observers at every point of view.

[1] See Appendices to this review.

It follows that so long as we are dealing with mankind from a rough practical point of view,—as, for instance, in therapeutics,—we may without serious error treat the ordinary state of health and intelligence as a type to which aberrant specimens ought to be recalled. But if we wish to engage, as M. Janet engages, in a more original, more philosophical discussion of man's personality, we have no longer the right to assume that our common empirical standard gives any true measurement of the potentialities of man.

From among a good many passages of M. Janet's which seem to me thus lacking in width of purview, I take one (p. 137) where, amid much which I hold to be true and important (see *Proceedings*, Vol. IV., p. 225), one phrase occurs which places our point of difference in a clear light.

"The memories which persist in a man's mind are grouped and aggregated round some one leading form of sensation [*i.e.*, as visual or auditory images, &c.], which serves both to express them and to evoke them; and when they are sufficiently numerous they form a system of which all the parts cohere and belong to the same memory. A man perfectly healthy from the psychological point of view would never possess more than one memory of this kind, and since all the phenomena of his thought would be attached to images always the same and always present to him, he would be able easily to evoke them all, and at any moment. But no one is thus perfect; a thousand circumstances,—passion, sleep, drunkenness, illness, diminish or destroy certain images, revive others, and change the whole orientation of his thought. Secondary groups of memories are then formed, in accordance with the same laws, around certain images which are abnormal in his mind; [*e.g.*, auditory images in a 'visual,' &c.]; these new images may vanish and reappear no more; but if they reproduce themselves periodically or are brought back by artifice, they bring with them all the memories which are linked with them, and the *different* memories become *alternating* memories."

The main *truth* in this passage (in my view) lies in the description of the growth of subsidiary mnemonic chains, which may ultimately enter into rivalry with the primary mnemonic chain in the waking individual. The main *originality* lies in the association of each new mnemonic chain with a different set of revivable sense-images—so that a "visual" formed *pro tem.* into an audile (to use Galton's terms) enters by that very fact into a fresh phase of personality. This ingenious hypothesis M. Janet has shown to be probable in some instances;—though I think that he presses it too far. But the main *error* which the passage (as I think) contains, lies in the conception of the psychologically healthy or normal man who has one set of memories only,—say visual; sticks to that set, and is able to reproduce at will all the memories which have been grouped around his stock of visual images,—memories (unless I much mistake M. Janet) of objects wittingly *(sciemment)* observed by our normal man's primary consciousness.

Now I say that such a man's memories may of course be practically adequate, but are certainly not theoretically complete. I hold that every impression made on the organism (above some minimum which we cannot guess at)—be it visual, auditory, or tactile, is in a certain sense remembered by some stratum of that organism, and is potentially capable of being reproduced in the primary memory. If called upon to defend this thesis at length, I should find various experiments of M. Janet's own to add to the converging

mass of observations which this view, and this view alone, serves to explain and to unite.

For the moment I must confine myself to a single concrete illustration. In the paper on crystal-gazing which appeared in *Proceedings* XIV., the author gave the following carefully observed incident. She saw in the crystal,—as an externalised hallucination which M. Janet would doubtless class as morbid,—a printed announcement, as though from the *Times* newspaper, of the death of a friend, as to whose health she was in no way preoccupied. On searching the *Times* of the previous day that announcement was found. But Miss X. had never consciously read it;—never *read* it, in the usual sense, at all. She had simply held that sheet of the *Times* to shade her face from the fire while talking to Mrs. Sidgwick, with whom she was staying at the time. That is to say the words of the announcement had imprinted themselves on her retina, but their meaning had never reached her *mind*, in the usual sense of the term,—that is, her primary consciousness. But when she looked in the crystal,—used, that is to say, an empirical method for facilitating communication between the subjacent and the superficial consciousness,—then that subjacent consciousness was able to convey, in hallucinatory form, this true message to her primary self. Now I say that in so far as any one possesses a power of this sort, and can acquire cognisance, either by artifice or by some spontaneous uprush, of the impressions stored, and the operations proceeding, in strata deeper than his primary consciousness, to that extent is he superior and not inferior to ordinary humanity, more "normal" than the average man—if any norm there be—because he is more fully utilising the possibilities of his being.

In Miss X.'s crystal-gazing the information gained is often trivial, and the upward-flowing messages interesting mainly in their theoretical aspect. But there are phenomena of a more exciting kind which must receive just the same explanation. The differentia (as I venture to hold) of *genius*;— not of the genius which is a mere extraordinary capacity for taking pains, but of the sheer unmistakable creative genius (say for instance) of a Mozart,—lies in this very same thing;—in the capacity for drawing upwards into the primary consciousness the results of operations which have taken place, (with no effort to the primary self, and often beyond its conscious capacity,) in the subjacent strata of his complex intelligence. And if after this the man of genius should suffer from nervous exhaustion, (which is by no means always the case,) I consider that he has accomplished the greater object at the cost of the lesser, and is no more morbid than a champion sculler is morbid because on the day after a hard-won race he has a pain in his back. This mention of the case of *genius* is by no means here a digression. For the doctrine that *le génie est une névrose*,—that there is something morbid and disequilibrated in any extraordinary creative power,—is maintained now-a-days[1] with arguments closely resembling those which M. Janet directs against the soundness of automatists or of hypnotisable persons. Genius, automatism, hypnotisability; these three in a sense must stand or fall together, as representing unworked potentialities of the human spirit; accidental or empirical modes of bringing "the good treasures of the heart" into serviceability to the conscious self.

[1] See Lombroso's *L'Homme de Génie*.

For, indeed, the capacity of being hypnotised—to return thus to the immediate arguments of our author—is surely *not*, as he would have us believe, an indication of something in the subject already morbid, or on the point of becoming so. Actual experiment (as we have seen in Mr. Wingfield's cases) disproves this view as completely as my theory could desire. I offer in exchange the following suggestion: Hypnotisability indicates neither health nor disease; but merely a facility of communication or alternation between different strata of the personality. The facility of such interchange (like other capacities of strong organic reaction to given stimuli) may be harmful or helpful according to the circumstances of each case. It is probable that those who are morbidly unstable to begin with will be hypnotisable also. And thus it is found on the whole (though with considerable divergence between observers) that hysterical subjects are specially hypnotisable. But this fact constitutes no presumption whatever that all hypnotisable subjects will be morbid. As well might one say that because drunken men fall very sound asleep, therefore everyone who falls asleep must be more or less drunk.

We have dwelt long on this important theoretical point; for this too hasty generalisation of M. Janet's from his own experiences with morbid subjects to the morbidity of *all* subjects lies at the root of almost all in his book to which our English experience would lead us to demur. I pass more briefly over his account of *suggestion*,—the artificial retrenchment of the field of consciousness,—which he classes as one of the phenomena of total automatism. In reading M. Janet's *résumé* of the power of suggestion, with his apologies for again treating so well-worn a theme, we, in these *Proceedings*, may be allowed a passing reflection on the extraordinary rapidity with which the phenomena of hypnotic suggestion have taken their place among the common-places of experimental psychology. Hypnotic suggestion, though known to the early mesmerisers, (now beginning at last to receive due honour), in England as well as in France, had, in this generation, fallen almost wholly out of the scientific purview, and was looked upon as a trick of itinerant charlatans. In these *Proceedings*, however, from their very inception, we have dwelt on the reality and the power of this singular agency. I suppose that other English organs must be beginning to deal with the subject now; but during the seven years' life of these *Proceedings* I cannot remember that we have gathered a single illustrative instance from any English periodical, or even any criticism, except the oft-repeated remark that the subjects of suggestion are probably either paid, or duping the operator.

Well-worn though the subject may now be, M. Janet has, as usual, some ingenious contributions to make to it. One of the most striking of these is an experiment—or rather a pair of experiments—which show the convertibility of what I have elsewhere called active and passive automatisms,—of suggested *action* and suggested *hallucination*,—in a quite novel way.

"There are no acts," says M. Janet (p. 148), "without an image in the mind, which, although associated with a movement, is not on that account the less intense. A subject ordered to lift her arm has in her mind an image of the act—an image muscular or visual as the case may be,—which is quite clear and exactly like a hallucination. For instance, I bid Marie lift her

arm, but I straightway seize the arm and arrest the movement. Since she has no muscular sensibility on this side she does not feel my action. A few moments later I ask her where her arm is, and she answers that it is in the air, and that she *sees* it. . . We have thus suppressed the action which under ordinary circumstances masks the image of the action, and have left this image isolated [divorced from its habitual realisation]. It is then seen that the image existed in full completeness, and in this case even amounted to a hallucination. On the other hand, it is easy to show that some movement [surely it would be safer to say some tendency to movement] always accompanies a suggested hallucination. . . It is impossible to give to a visual subject the visual hallucination of the movement of her arm without the supervention of an actual movement. I told Léonie, after bandaging her eyes, to see her left arm rising and waving in the air. [Her left side is anæsthetic, so that its automatic movements could give her no information.] In a few moments she said, 'Yes, I see it; the fingers are parted'; but at the same time the left arm [which she cannot feel] executed just the movement which she declared that she saw."

The value or novelty of each experiment of this type can hardly be judged except by those who have followed pretty closely the long series of such observations which have of late been accumulated in France. It is, I think, rather unfortunate that the work on Hypnotism, written for the International Scientific Series,—though lucid and ingenious as are all the productions of its fertile authors,—should contain at least one series of experiments of very dubious interpretation. I allude to the *transfert*, and especially the *transfert psychique*, or reversal of emotion, supposed to be effected by the agency of magnets on hysterical subjects. The very curious experiments of Messrs. Binet and Féré on this point have received little real confirmation elsewhere; and M. Janet is, I think, probably right in attributing the phenomena to unconscious suggestion, working on some influence of a vaguer kind which the magnet may perhaps exert. I see, indeed, that in his latest paper (*Rev. Phil*, October, 1889, p. 438) M. Binet himself admits as an explanation of this so-called psychical polarisation the view of Ottolenghi and Lombroso that "the principal action of the magnet on the organism consists in suppressing the phenomena previously suggested; so that—this phenomenon once effaced—association by contrast comes into play, and produces in the consciousness a *negative* instead of a *positive* phenomenon":—*i.e.*, a reversal of the hallucinatory idea previously dominant.

But I must pass on to the second division of M. Janet's book,—in which he deals with *partial automatism*;— the subconscious acts performed by persons in a waking state, in obedience sometimes to previous suggestion in the hypnotic trance, or sometimes to commands insinuated into the waking subject *par distraction*;—by whispers or tactile hints which the main consciousness of the subject does not perceive, but which induce (say) her anæsthetic hand to write automatic replies. Automatic we are forced to call these acts, but (as M. Janet justly insists) we must not therefore assume that they are effected without a consciousness of their own. I must not here dwell on the details of these ingenious investigations, of many of which some account has already been given in these *Proceedings*.

Rather let me once more colligate these and many similar experiments in

a single hypothesis, and give to human personality a definition as wide as such observations seem to require. I suggest that every cell in our bodies may have a separate memory, and therefore in a sense a rudimentary personality of its own. Every combination of cells, every nerve, every muscle, every limb or tract of the body, with its brain-connections, may have a more complex memory of its own, and may recollect and give account of incidents of which the ordinary waking consciousness has never been aware. These are separate memories which do not deserve the title of separate *personalities*, except in the sense in which that word may be applied to the brute creation. Above this comes the immense nervous apparatus which corresponds to the human mind: and of this apparatus we habitually use only some such proportion as our English vocabulary bears to all possible combinations of the alphabet. The letters of our inward alphabet will shape themselves into many other dialects;—many other personalities, as distinct as those which we assume to be *ourselves*, can be made out of our mental material. In some extreme case these allotropic personalities may alternate with or supersede the personalities which we have learnt to call our own. But in ordinary cases, where they do not thus emerge, we must not assume that they are non-existent. It may be indeed that they are not shaped into definite chains of memory—a Lucie II. and a Lucie III.—as in M. Janet's subjects. It may be that the very formation in us of anything so narrow and confined as what we know as personality, is in itself a limitation of our essential being,— a mere mode of concentration in order to meet the perils of our environment. But in some way or other—personalised or not personalised—a continuous activity of our whole being goes on, of which the results are in some sense psychical, in some sense permanent. Every impression made upon or within the organism has a psychical counterpart, and this, or the capacity of reproducing this, is somewhere fixed and preserved. The question as to what part of a man's being enters into his ordinary consciousness is like the question what part of his body when he floats on the sea, floats above water. It is necessary for his preservation that a certain minimum should so float; but the submerged portion is living with the same life as the portion exposed.

Our hypothesis, it is manifest, may be carried one step further. Each of the personalities within us is itself the summation of many narrower and inferior memories. It is conceivable that there may be for each man a yet more comprehensive personality—or say an individuality—which correlates and comprises all known and unknown phases of his being. Such a notion can no longer be dismissed as merely mystical; analogy points to it; and although no observation could fully prove it there may well be observations which may make it probable. But here as everywhere fearless analysis is the pre-requisite of any sound construction. We must not shrink from pulling ourselves to pieces if we hope to find indications that there is something in us larger and more perdurable than we had previously supposed.

An important chapter of M. Janet's book,—"Désagrégation Psychologique,"—is devoted to the review of a subject where premature construction has long hindered necessary analysis. It requires some courage,—perhaps more courage in France even than in England,—for a scientific writer so much as to discuss the Spiritistic literature. M. Janet faces the task,

though in the spirit of a chemist studying the records of alchemy. "Experimental psychology," he says, "began by being animal magnetism and spiritism; let us not forget this fact, nor laugh at our ancestors."

His treatment of the problem is careful and candid, and he has little difficulty in explaining most of the facts accessible to him on lines familiar to the readers of these *Proceedings*,—as the manifestation of some disintegration of personality within the medium rather than of some invasion of a personality from without. I say that "most of the facts accessible to him" are explicable in this way. But there are, I know, other facts less easy of explanation. Never could there be a better moment than now for some new champion of the Spiritual explanation of automatic writing to enter the field. He must be someone capable of understanding the essential points as to evidence of outside intelligence on which dispassionate critics are now agreeing, and which it is no disgrace to the earlier Spiritualists that they could not at once divine. And he must be someone really patient, really diligent,—willing to bestow on his experiments—what I much doubt whether any Spiritualist author has yet done—something approaching the time and care which M. Janet has bestowed upon *his*. The few cases which have been sent to myself, by M. Aksakof and others, in response to previous appeals of this kind, are quite enough to show the real importance to science of the fullest possible presentation of that very theory against whose rash and hasty adoption both M. Janet's arguments and my own have thus far been directed.[1]

This long review must now draw to a close. I may perhaps end it by quoting a curious example given by M. Janet (p. 466) in his last chapter, "La Faiblesse et la Force Morales," to show how the tendency to "psychological automatism" is latent in all of us, but gathers force to manifest itself only when we are brought "below par" by fatigue or disease.

"It is commonly said that love is a passion to which man is always liable, and which may surprise him at any moment of his life, from 15 to 75. This does not seem to me accurate; and a man is not throughout all his life and at every moment susceptible of falling in love (*de devenir amoureux*). When a man is in good physical and moral health, when he has easy and complete command of all his ideas, he may expose himself to circumstances the most capable of giving rise to a passion, but he will not feel it. His desires will be reasonable and obedient to his will, leading the man only so far as he wishes to go, and disappearing when he wishes to be rid of them. On the other hand if a man is morally below the mark (*malade au moral*),—if in consequence of physical fatigue or excessive intellectual work, or of violent shocks and prolonged sorrow, he is exhausted, melancholy, distracted, timid, incapable of controlling his ideas,—in a word, *depressed*,—then he will fall in

[1] An allusion made by M. Janet to the Rev. P. H. Newnham (p. 392) gives me an opportunity of repeating my grateful acknowledgment of the kindness and candour with which Mr. Newnham presented me with the original private note-books containing his experiments,—which books I shall be glad at any time to show to inquirers. Mr. Newnham, who had lived for some years in the calm but constant expectation of death, from disease of the heart, has now passed away; and I may repeat the witness of others who knew him more intimately than I, to the effect that a simpler, franker, more sane, more upright character has rarely been met with even in that profession whose duties he fulfilled so earnestly so long as any strength to fulfil them remained.

love, or receive the germ of some kind of passion, on the first and most trivial occasion. . . The least thing is then enough ; the sight of some face, a gesture, a word, which previously would have left us altogether indifferent, strikes us, and becomes the starting point of a long amorous malady. Or more than this, an object which had made no impression on us, at a moment when our mind was healthier and not capable of inoculation, may have left in us some insignificant memory which reappears in a moment of morbid receptivity. That is enough ; the germ is sown in a favourable soil ; it will develop itself and grow.

"There is at first, as in every virulent malady, a period of incubation ; the new idea passes and repasses in the vague reveries of the enfeebled consciousness ; then seems for a few days to have disappeared and to leave the mind to recover from its passing trouble. But the idea has done its work below the surface ; it has become strong enough to shake the body ; and to provoke movements whose origin lies outside the primary consciousness. What is the surprise of a sensible man when he finds himself piteously returning beneath the windows of his charmer, whither his wandering feet have taken him without his knowledge ;—or when in the midst of his daily work he hears his lips murmuring perpetually the well-known name ! . . Such is passion in its reality ; not as idealised by fantastic description, but reduced to its essential psychological characteristics."

It will be seen that this eloquent passage,—as of a modernised Lucretius,—is thoroughly in harmony with M. Janet's opinions, as above discussed, with regard to the normal condition and necessary limitations of the psychical energies of man. It is opposed to the wider hopes and conceptions which I have indicated ; but I shall not here again argue the point in detail. I shall leave it to *voi che avete intelletto d'amore* to consider whether M. Janet's analysis is sound or complete,—whether such words as Plato and Dante have spoken concerning love are "*descriptions fantaisistes*," or living records of profoundest truth ;—whether *that* were a sign of strength or of weakness,—that most overmastering, most irrational of all recorded passions, which yet was as a *Vita Nuova* to one potent heart :—which could prompt to high effort, and soar above desire, and project its passionate ardour beyond the gulf of death. For my part I have some fear lest so soon as we come to disbelieve in the highest facts of man's past, and to despair of surpassing them in man's future,—so soon as we assume that we have already attained our full normal development, and that the obscure strivings of this restless spirit must lead henceforth nowhither ;—then by that very assumption we shall have entered upon our decadence, and invited our degeneration and decay.

This review, with the reviews which follow, must serve for the present as a fulfilment of our promise of a survey of the existing condition of hypnotism in France.

I have touched above on several of the points which excited most controversy at the recent International Congress of Experimental Psychology, in whose discussions hypnotism played a leading part. For the rest, the impression produced by that Congress was that of the increasing acceptance of most of the doctrines of the Nancy school. Readers of these

Proceedings will not be surprised at this result, which represents in fact essentially the triumph of generalisations based on a wider experience over generalisations based on a narrower experience,—*narrower*, I say, in spite of the vast extent and skilled organisation of the Salpêtrière—because the subjects there submitted to experiment have been all of nearly the same type,—hysterical and epileptic invalids. The school of Nancy is gaining ground, with its demonstration that the "three stages" of the "*grand hypnotisme*" under M. Charcot's rule are rarely reproduced elsewhere, and are therefore not a necessary or typical manifestation of the hypnotic state. Nancy is gaining ground with its insistance on the power of suggestion, and its belief in the hypnotisation of healthy subjects. But,—if a foreign observer may repeat the warning which impartial judges like M. Richet are already uttering in France,—I see a cloud on the horizon of Nancy's fame. Its leading men (except the veteran Liébeault) are pushing their theory too far, and insisting that *all* in hypnotism is suggestion, and that there are no physical influences whatever, whether from passes, metals, or magnets. On this point I must adhere to the view which I have often expressed in these *Proceedings*, that passes almost certainly, metals probably, magnets possibly, do sometimes exert a physical influence; and that we are yet far from having exhausted the agencies which operate between one human being and another. Has not the history of hypnotism thus far been a slow but repeated justification of those who, in each successive controversy, took the wider and less exclusive view? of those who recognised most frankly the magnitude, the obscurity, the unpredictable issues of this ever more penetrating inquiry into the hidden mechanism of man?

APPENDIX I.

Mr. Hugh Wingfield, who, when holding a University appointment as Demonstrator of Physiology at Cambridge, had very wide opportunities of choosing subjects from his large classes of medical students, sends me the following statement.

September 6th, 1889.

I subjoin the results of my own experience of hypnotic subjects.

I have hypnotised at first trial over 170 men, between the ages of 17 and 28, having had about 20 per cent. of failures.

I do not know how many I could have hypnotised had I persisted, as, if I failed once, I never tried again.

The subjects, with the exception of 18, were all undergraduates. Considering the extreme rarity of hysteria among men in England, it is utterly incredible that I should have hit upon 170 hysterical men haphazard. Besides, I always refused to hypnotise anyone unless I believed them to be perfectly free from hysteria.

In the only three cases where abnormal symptoms presented themselves during the hypnotic state (I cannot say that the symptoms were hysterical), I rejected the subjects.

In most cases I did not test the subjects for hemi-anæsthesia or other hysterical symptoms, as it was quite superfluous to do so; but in certain experiments on sensation it was necessary to test the sensation of both

hands; and I have also sometimes tested the sensation of both forearms and the two sides of the face; yet in no single instance could I detect any abnormality whatever.

With regard to the other cases, none so far as I know (I have only inquired of a few) had ever had any symptom of hysteria.

It seems distinctly unfair to argue that because hysterical subjects are easily hypnotised, all subjects must be hysterical. Besides which, I very much doubt the susceptibility of *all* hysterical persons. I have found two whom I could scarcely influence at all. Personally, I am quite convinced that large numbers of persons who have no symptom of hysteria whatever can be readily hypnotised.

H. E. WINGFIELD.

APPENDIX II.

On the general question of the comparative frequency of hysteria in France and England, Dr. A. T. Myers sends me the following note:—

"The position of hysteria among the diseases of England and France is very different. The '*grande hystérie*' which French study has of recent years defined and accentuated among nearly all large collections of the young as well as of the sick in France, and more especially in Paris, is hard to find in England even when sought for, and very imperfect in its French equipment of anæsthesiæ and paræsthesiæ and elaborate sequence of four periods of convulsion, so that it offers comparatively little opportunity for testing, for instance, whether metallo-therapy acts purely by suggestion or not. And the '*petite hystérie*,' the possibility of which the French observer can never forget when he is dealing with young people, especially if they are being hypnotised, is out of the question for almost every one of such subjects as have come most completely under Mr. G. A. Smith's influence and furnished the staple of Mr. Gurney's inductions. Cases of it may be found, no doubt, in a few morbid conditions of health and surroundings in all classes in England, but not among the vigorous, hard-working telegraph boys, or apprentices to active trades, who have to spend half their day in the open air and to learn how to use their muscles.

"The diffusion of hysteria among the European races seems to be far from uniform. The widest experience shows that the French have on the whole had the most cases to deal with (Strümpell); and among them some of the most severe type. The Italians, Spaniards, and Greeks apparently suffer more from hysteria than the English, Germans, or Dutch. Among the Sclavonic races there are occasional limited endemics (Hirsch); and the Jews are credited with a large percentage (Grasset).

IV.

BINET ON THE CONSCIOUSNESS OF HYSTERICAL SUBJECTS.[1]

By F. W. H. Myers.

M. Binet is doubtless known to most of our readers as one of the most ingenious and suggestive of modern French experimental psychologists. He has worked mainly in association with Dr. Féré, and at the Salpêtrière; but his range of speculation is wide, and his book on *La Psychologie du Raisonnement*, and his *Études de Psychologie Expérimentale* enjoy a just reputation. The present article is an account of experiments performed on hysterics at the Salpêtrière; and on this point two preliminary remarks must be made. In the first place, one feels that the Salpêtrière has, in a sense, been smothered in its own abundance. The richest collection of hysterics which the world has ever seen, it has also (one fears) become a kind of unconscious school of these unconscious prophets—a *milieu* where the new arrival learns insensibly from the very atmosphere of experiment around her to adapt her own reflexes or responses to the subtly-divined expectations of the operator. One is inclined, therefore, to wait until a series of Salpêtrière experiments have been independently confirmed elsewhere before offering them to an English public, which, from our marked poverty in hysterics, is little likely to have the chance of verifying the results *de visu*.

But in this case M. Binet's experiments are so strikingly in concordance with the quite independent results obtained both by M. Pierre Janet and by some of ourselves in England,—and are, moreover, in themselves so easy of repetition, if only a properly anæsthetic subject can be secured,—that some account of them seems due to the readers of these *Proceedings*.

In the second place, it may be said that these are pathological phenomena; and that our Society is not concerned with disease. To this I answer that these are not pathological phenomena, but pathological revelations of normal phenomena, which is a very different thing. The gearing of the hysteric's inward factory is disconnected; the couplings are shifted in all sorts of injurious ways; some of the wheels are standing still, and some are whizzing uselessly round and round. But the wheelwork is still all there; and by observing the various hitches and stoppages which are now taking place, we can get a better notion of the way the power is applied than the smoothly-working, carefully-boxed machinery of the healthy subject is likely to give us. Above all, we must avoid the assumption that the hysteric possesses any capacity whatever which we do not all of us potentially possess. Is the hysteric hyperæsthetic? Then so do we all potentially possess the acuteness of smell or sight or hearing which she manifests. No fresh anatomical element is added to her ear or eye; no fresh physiological property to any one molecule in her body. What she can do, we can do,—only as that has not

[1] Recherches sur les Altérations de la Conscience chez les Hystériques. A. Binet, *Revue Philosophique*, February, 1889.

been the most useful way of exerting our innate powers, our ancestry has so arranged us that those hysterical delicacies of perception remain in us latent and unknown. Is the hysteric dissociable into two or more co-existent personalities? Then so are we also presumably dissociable; our machinery is made on the same plan as hers; though the belt which for her has slipped from the shaft, in us still keeps its place, and holds our personalities together.

Nay more, if that purely imaginary entity, the normal man, is still held up before us as incapable *ex vi termini* of any change which is not degeneration, we shall reply that after all it is one of the perfections of a complex instrument to admit of the ready disconnection of its constituent parts; and that our true ideal should be,—neither the rigid connections of so-called normality, nor the ungovernable disconnections of hysteria,—but a condition in which we should be able to connect or disconnect any element within us at pleasure. We can at present do this to a slight extent, and we account this power as a gain. It is a gain, for instance, to be able to abstract one's attention,—to become temporarily anæsthetic to noises around one. This may, indeed, be pushed too far; as we know that a soldier cut off Archimedes' head while that philosopher was meditating on the hypothenuse. But our ideal should go beyond Archimedes;—it should be to cut off the soldier's head with one of our personalities, while we meditate unbrokenly on the hypothenuse with the other.

Let us proceed now to M. Binet and his hysterics; remembering that just as, in Mr. Herbert Spencer's phrase, "the mobile in expression *represent* the race,"—give overt manifestation to such slight changes as pass over the moods of all;—so also do these far more profoundly mobile beings "represent the race" in deeper fashion;—sometimes even dissect away our recent nervous acquisitions, and lay bare processes that correspond to a long-past stage of evolution.

The first point to remark is that the anæsthetic limb of a hysteric is almost always capable of certain simple movements, which it executes without the subject's knowledge, or when concealed from the subject by a screen. If the anæsthetic arm, for instance, is moved in a certain way, and then left to itself, it continues the movement. If it is guided into writing a word or words, and then left to itself, it will repeat the word, or continue the sentence. It acts, in short, very much as the subject's planchette-writing hand in Mr. Gurney's experiments acted when fulfilling a post-hypnotic suggestion. Let us see how far this supposed anæsthetic arm is really intelligent, or is really susceptible of pain.

If we merely prick the anæsthetic hand it in no way reacts,—shows no disposition to avoid the pin. Perhaps this is because the pin-prick awakes no definite conception. Let us try a more complex stimulus.

"We place in the right (anæsthetic) hand of Amélie Cle— a box of matches; a large vertical screen prevents the patient from seeing her hand. After a moment's contact the right hand clasps the box; fingers it; seems to recognise it; strikes a match and holds it alight; as the flame advances the fingers withdraw, as if they felt and shunned the heat; and when the flame nears the end of the match the fingers open and the match falls." From this experiment it is not clear whether pain is felt, or whether the whole act is a mere piece of what, in a normal waking person, we ca l

secondary automatism ;—the repetition of a familiar series of actions without conscious attention.

Let us now,—I abbreviate M. Binet's account,—give the match-box to a second subject, L. Lavr—. She opens the box, but having taken out a match imagines it to be a pencil, and tries to write with it. We light the match and give it back to her. She does not realise that it is a match, and holds this and a second burning match till they are consumed or go out, and her fingers are much burnt. This resembles an imperfect instinct ; as when ants store up beads which the observer has sown in their hunting-fields.

The result of the experiment with another subject, Louise St. Am., is still more curious. She drops the burning match, but then at once picks it up again. This resembles the tendency of caterpillars, &c., to go back to the beginning of a series of actions, if interrupted.[1] The Sphex which, after its burrow had been, to its knowledge, emptied of the prey which it wished to wall up there, walled up the useless burrow all the same, before beginning another, was obeying the same instinct as Louise, of continuing the series of actions in the accustomed order, without regard to the special circumstances of the case. Sphex and anæsthetic hand each afforded an instance of "lapsed intelligence," nervous adjustments originally acquired by intelligent effort, but now irrecoverably sunk into routine. "How," asks M. Binet, "can one explain the preservation of tactile sensibility along with the loss of sensibility to pain? Are there two orders of sensibility in connection with different centres? Are there nerves for pain, a centre for pain, distinct from the nerves and centres of sensation? Or does the distinction between these two sensibilities consist in a fact of central perception? If the sensibility to pain seems to be suppressed both for the primary personality and for the secondary personality,—that is to say, for the anæsthetic limb,—are we to conclude that hysterical analgesia, in certain subjects, may be an absolute destruction of sensibility to pain, and not an alteration of consciousness?"

I should reply that we must *not* so conclude in any absolute manner; but that all analogy shows that where there is not actual previous lesion or atrophy of the nerves the injury to them is perceived and the pain is—I do not say *felt*, but *recognised*,—by some personality or other. I must suppose that in Louise St. Am.'s case, just as in the case of Blanche Witt—, (mentioned in the review of Dr. Jules Janet's paper, *vid. inf.* p. 216), there is a yet deeper personality which the experimenter has not reached, and which was all the time mutely upbraiding the folly of the anæsthetic hand in mistaking a lighted match for a lead pencil.

As regards the dissociation of tactile from dolorous sensibility, I may just remark that it is quite possible that our earliest monocellular ancestors may have possessed the power of feeling contact, but not of feeling pain. If sensibility to pain be a protective character acquired in the struggle for existence, the hysterical severance of the two sensibilities is less incredible than it may at first appear.

The next point of interest observed by M. Binet lies in the *automatic writing* of these hysterical subjects. "When a hysteric holds a pen in her anæsthetic hand [concealed by a screen], in the attitude appropriate to

[1] Darwin in Romanes' *Mental Evolution in Animals*, p. 179.

writing, the pen will register the ideas which predominate in her consciousness." If the subject is told to think of a name or a number, the pen—unknown to her primary self—will write that name or number. Or, if the subject spontaneously thinks of a number, and the operator then lifts a finger of the anæsthetic hand several times in succession, the finger will stiffen when the operator has reached the number which the primary self is thinking of. The anæsthetic hand can thus be taught to indicate the subject's thoughts by a variety of gestures, though it is slow in learning to substitute one gesture for another,—*e.g.*, finger-lifting for writing.

And now let us reverse the process; let us give the information first to the anæsthetic hand, and see whether, and in what form, the same kind of subterraneous communication will transmit the intelligence to the primary self. Let us take the simplest form of experiment; which is also one of the most interesting to students of automatic writing.

"The first subject observed was a hysterical woman, Mél—, whose right arm was anæsthetic. She did not perceive the passive movements of a general kind which were communicated to this arm; but if one placed a pen in her right hand, and made the hand write a word, the patient at once guessed the word, with her eyes shut. She nevertheless did not feel, she said, the graphic movement communicated to her hand; but she had a visual image of the word, which appeared to her suddenly, 'as if it were written in chalk on a black-board.'"

M. Binet appears to think that his own are the first observations of this curious co-operation of the motor activity of one phase of personality with the visual perceptions of another. Were he in the habit of referring to English works, he would find the phenomenon noted and illustrated in the Society for Psychical Research *Proceedings*, Vol. III., p. 59, &c., (in a paper read January, 1885), and formulated (as xx′ + ss′ + w′), among a series of kindred phenomena there described.

An interesting variety in the experiment is as follows: M. Binet desires the subject to think spontaneously of a word. Meantime he makes her anæsthetic hand write a certain word of his own choice. She proceeds to utter that word, under the impression that she has spontaneously thought of it. The analogy here with post-hypnotic suggestion is very marked. The anæsthetic hand, like the dormant hypnotic personality, makes a suggestion to the primary personality which that personality innocently accepts as its own spontaneous choice.

Another experiment is curious from the metaphysical question which it suggests as to the distinction between pain and the *idea* of pain. In the case of two hysterics, when the skin of the anæsthetic arm is pinched, behind a screen, "the patient, carefully interrogated, with avoidance of all suggestion, spontaneously declares that she has the idea of a painful sensation. She does not suffer from it, for she is persuaded that she is insensible, but she admits that the idea of this pain is disagreeable to her. There is thus a kind of transformation of physical pain into mental pain, like that which occurs when one imagines or recalls to memory some bodily suffering." The pain, in fact, as I have before said, is *recognised* rather than *felt;* and it is a fair question for metaphysical argument whether that pain existed at all.

The phenomenon (as I at least should say) which is common to these

and many similar experiments, is that communications from one state of personality to another,—what, for sheer lack of a word, I have ventured to call *methectic* communications (p. 48, note),—impress themselves on the percipient personality,—just as telepathic communications do,—by means of visual or auditory images, or obscure perceptions, which may develop into actual hallucinations. The submerged personality is writing ; it gives to the emergent personality the hallucination of seeing words written in chalk on a board. The submerged personality is suffering a definite localised smart ; it gives to the emergent personality a vague quasi-hallucinatory idea of pain.

Naturally it is when visual images are evoked in the emergent personality that these communications are most distinct. Nor is it only so definite a movement as the writing of the anæsthetic hand which can get itself represented in visual form. "With some patients," says M. Binet, "the visual image determined by the peripheral excitation [of pinches, &c.] augments in intensity to the point where it externalises itself as a hallucination. Thus, when one has repeatedly pricked the insensible hand of Lav—, while she is occupied in reading, she presently sees the book become covered with little black points which hide and confuse the text ; she is obliged to give up reading."

Here the annoyance given to the submerged personality was represented to the emergent personality by a hallucinatory vision, symbolical of the points of pain. Compare Mr. Gurney's experiment (*Proceedings*, IV., p. 319), where the stress of competition between the normal and the hypnotic personalities represented itself to the hypnotic personality, when emergent in its turn, as a disturbing hallucinatory figure.

"P—l was told several times, 'It has left off snowing' ; and then, when woke and set to the planchette, he was made to read aloud. The writing which appeared was : It has lfeft sn—, and while this was proceeding the reading was bad and stumbling. . . . Re-hypnotisation afforded a glimpse of the condition in which the secondary intelligence had found itself. Asked what he had been doing, the subject replied, 'Trying to write, It has left off snowing.' Asked if he had been reading, he said, 'Reading ! No, I haven't been reading,' and added, 'Something seemed to disturb me.' How was that ? 'Something seemed to keep moving about in front of me, so I got back into bed again.' Didn't Mr. Gurney hold a book and make you read aloud ? 'No, somebody kept moving about. I didn't like the looks of them. Kept wandering to and fro. Horrible, awful ! I thought to myself, I'll get into bed.'"

And now, before concluding, let us extend our area of comparison a little further yet. All these experiments of M. Binet's have been in the well-known Salpêtrière atmosphere. They have all been concerned with *la malade ;* and it has been taken for granted that this dissociation of personalities through the agency of local anæsthesia could only occur on diseased subjects. It has, of course, been assumed also—it would seem absurd to question it,—that the anæsthetic arm was necessarily less rational, less intelligent, than the primary personality, which had apparently the use of the head. Let us see whether it is really safe to make either the one or the other assumption.

In the *Proceedings* of the American Society for Psychical Research,

Vol. I., p. 549, Professor William James, of Harvard, who is a physician as well as a psychologist, cites the following case from his own observation.

"William L. Smith, of Concord, Mass., student at the Massachusetts Institute of Technology, age 21, perfectly healthy and exceptionally intelligent . . . sat with Mr. Hodgson and myself, January 24th, 1889, with his right hand extended on the instrument [planchette], and his face averted and buried in the hollow of his left arm, which lay along the table. Care was taken not to suggest to him the aim of the inquiry, [*i. e.*, to test for anæsthesia induced in healthy subjects by the mere act of automatic writing.]

"The planchette began by illegible scrawling. After ten minutes I pricked the back of the right hand several times with a pin—no indication of feeling. Two pricks on the *left* hand were followed by withdrawal, and the question, 'What did you do that for?' to which I replied, 'To find whether you were going to sleep.' The first legible words which were written after this were, *You hurt me.* . . . . After some more or less illegible writing, I pricked the right wrist and fingers several times again quite severely, with no sign of reaction on S.'s part. After an interval, however, the pencil wrote: *Don't you prick me any more.* S. then said, 'My right hand is pretty well asleep.' I tested the two hands immediately, by pinching and pricking, but found no difference between them, *both apparently normal.* S. then said that what he meant by 'asleep' was the feeling of 'pins and needles,' which an insensible limb has when 'waking up.'

"The last written sentence was then deciphered aloud. S. laughed, having been conscious only of the pricks on his left hand, and said, 'It's working those two pin-pricks for all they are worth.'

"I then asked 'What have I been excited about to-day?' *May be correct, don't know, possibly sleeping.* 'What do you mean by sleeping?' Answer: *I don't know. You* [distinct figure of a *pin*] *me nineteen times and think I'll write for you.*"

Thus we see that local anæsthesia was produced on the hand of a healthy subject, but apparently only just so long as that hand was writing the messages of a submerged self. And when, on a later day, the pencil was placed in the left hand instead of the right, the left hand took up the memories of the right hand's previous sufferings.

"Here," says Professor James, "as the reader will perceive, we have the consciousness of a subject split into two parts, one of which expresses itself through the mouth, and the other through the hand, whilst both are in communication with the ear. The mouth-consciousness is ignorant of all that the hand suffers or does; the hand-consciousness is ignorant of pin-pricks inflicted upon other parts of the body; and of what more remains to be ascertained. If we call this hand-consciousness the automatic consciousness, then we also perceive that the automatic consciousness may transfer itself from the right hand to the left, and carry its own peculiar store of memories with it."

Here, then, we have an independent experiment,—dating from before the publication of M. Binet's experiments above discussed,—and exhibiting in a "perfectly healthy" subject exactly the phenomena which M. Binet elicited from his *malades*. Perhaps those who hold that automatism is always associated with disease, will say that here the automatism was the sole

manifestation of a diseased tendency which revealed itself in no other way. This argument, however, is plainly liable to be reduced *ad absurdum* by the continued production of healthy automatists. And after Mr. Gurney's and Mr. Wingfield's experiments, there can, I think, be no doubt that healthy automatists can be produced in any quantity, if sufficient trouble be taken. But while in France we see well-equipped physicians experimenting in eager rivalry in hospitals teeming with hysterics, we in England have no such organisation either of researchers or of subjects for research. Instead of summoning obedient *malades* in endless procession, we have to induce healthy independent persons to lay their hands on planchettes which they regard as grossly superstitious, or to hold pencils which they are firmly persuaded that no automatism will ever stir. We must not be surprised if the French report a dozen experiments to our one, until more of us put our hands to the wheel. And now to conclude with a case admittedly bizarre, admittedly abnormal, but which illustrates with even absurd unexpectedness the immense variety which these phenomena of dissociated personality may assume. The report, included in Professor James's paper above cited, comes from the late Dr. Ira Barrows, of Providence, R. I., and is corroborated by his surviving partner, and by the mother and brother of the late patient herself.

This was a case of hystero-epilepsy, in the course of which the patient "complains of great pain in right arm, more and more intense, when suddenly it falls down by her side. She looks at it in amazement. Thinks it belongs to some one else; positive it is not hers. . . Cut it, prick it, do what you please with it, she takes no notice of it. . . She believes it to be an arm and a hand, but treats it as if it had intelligence and might keep away from her. She bites it, pounds it, pricks it, and in many ways seeks to drive it from her. She calls it 'Stump; Old Stump.'"

Now comes the odd part of the story. This paralysed arm, which used to write automatically on its own account, in what may now claim to be the orthodox fashion, showed itself in one way unique among all dissociated arm-personalities. It operated, namely, as a kind of guardian angel or Dæmon of Socrates; it was helpful amid the hysteric turmoil; it was perfectly rational while the unlucky head and trunk were raving in frenzy.

"When her delirium is at its height, as well as at all other times, her right hand is rational, asking and answering questions in writing; giving directions, trying to prevent her tearing her clothes. When she pulls out her hair it seizes and holds her left hand. When she is asleep it carries on conversation the same; writes poetry; never sleeps; acts the part of a nurse as far as it can; pulls the bed clothes over the patient, if it can reach them, when uncovered; raps on the head-board to awaken her mother (who always sleeps in the room), if anything occurs, as spasms," &c.

"Thy right hand," said the Psalmist, "shall teach thee terrible things." He foresaw that the uncontrollable impulse, as against the enemies of the Lord, would outrun even the legitimate thirst for slaughter. But it needed a subtler psychology to teach us that the right hand may moderate as well as madden, may control instead of urging the violent unreasoning blow. For to the unsleeping guardian within us all paths of externalisation come alike; while yet all together are all too few, and glance, voice, hand in unison can show but a fragment of the Self.

V.

"DAS DOPPEL-ICH."[1]

By F. W. H. Myers.

We are glad to welcome this first publication of the Berlin Society for Experimental Psychology, a body whose aims, as our readers well know, have a close affinity with our own. Dr. Max Dessoir, secretary of that society, and author of the tractate now to be discussed, is already known to us as the compiler of an accurate and serviceable Bibliography of Hypnotism and kindred subjects. His present work, while consisting mainly of a careful and competent digest of French and English experiments and theories, which have received frequent discussion in these columns, gives evidence also of independent thought and philosophical insight. It has a special interest as one of the pioneer pamphlets which begin to mark the entrance of German science into a wide region of experimental psychology in which the Teutonic founders of psycho-physiology have for the moment been outstripped by French, and perhaps by English, inquiry.

Merely indicating the lines of thought which the earlier part of the tractate pursues, I shall reserve my space mainly for certain reflections which its conclusion suggests.

"In the course of ordinary life"—I quote a passage (p. 6) which gives the keynote of much that follows—"certain actions occur which presuppose for their origination all the faculties of the human spirit, but which nevertheless work themselves out without the knowledge of the agent. These actions we term automatic. Among them are certain automatic movements, as the act of dressing oneself, or of retracing a well-known path; and some other automatic performances, such as counting one's steps, or adding up columns of figures. These latter acts plainly indicate the existence of a separate train of *memory* employed upon them. And, moreover, although they take place without the agent's *knowledge*, they cannot take place without his *consciousness;* they cannot be truly *unconscious* acts. They must in some fashion belong to a *sub*-consciousness which, in its relation to the far more potent *upper* consciousness, may best be understood if we consider it as a *secondary* consciousness. And if we regard Consciousness and Memory as the essential constituents of an Ego, we may boldly say that every man conceals within himself the germs of a second personality."

The experiments of the Berlin Society seem to have thus far been made on healthy subjects; and Dr. Dessoir is decidedly opposed to the view that severance of personality is the special characteristic of hysteria. "In dreams,"

[1] *Das Doppel-Ich*, von Max Dessoir. (Karl Siegismund, Berlin.) This forms the *first* fascicule (it is numbered II., but a subsequent notice corrects this) of the "Schriften der Gesellschaft für Experimental Psychologie zu Berlin." A second fascicule has since been published, containing two papers, by Professor Bastian and F. von Hellwald.

he says, p. 13., "in states of intoxication, in accesses of somnambulism or of epilepsy, a consciousness distinct from the habitual consciousness assumes the sway; and, moreover, mnemonic chains, more or less coherent, are wont to connect these isolated periods of abnormality.[1] The secondary memory thus originated is not always wholly shut off from the primary train of existence,—as it was in Macnish's patient, the American lady,—rather there is generally some connection between the two memories, as in the case of Félida X. But in either case there may be a manifest change of character in the transition to the secondary self, so that two personalities[2] in every way disparate may inhabit a single body. In the case of hysterical patients the dual Ego is much less fully developed. But the careful study of their automatic movements leads to the same conclusion as to the existence and nature of a submerged consciousness as is suggested by the inward experience of healthy men."

A very felicitous experiment (p. 19) serves to illustrate the persistence, throughout healthy waking life, of a submerged consciousness which may at any time rise to the surface if the hypnotic state be induced.

"Several friends were in my room, one of whom, Mr. W., was reading to himself while the rest of us were talking with one another. Someone happening to mention the name of Mr. X., in whom Mr. W. is much interested, Mr. W. raised his head and asked 'What was that about X?' He knew nothing, he told us, of our previous conversation; he had only heard the familiar name, as often happens. I then hypnotised him, with his consent, and when he was pretty deeply entranced, I asked him again as to the conversation. To our great astonishment, he now repeated to us the substance of our whole conversation during the time that he was reading to himself. In this case, then, there was a perception of sensory impressions, but not in the consciousness with which the waking man worked;—rather in another consciousness which found its first opportunity of revealing itself in the hypnotic trance."

In this case, as in some of the experiments with crystals reported in the last Part of these *Proceedings*, we find the unconscious Self noting, treasuring, and reproducing certain information, conveyed indeed through the channel of the ordinary senses, but so conveyed that it never reached the emergent or ordinary consciousness of that same percipient in whose depths it was all the while being registered. From this it is an easy step to the supposition that the submerged consciousness may stand "*im innigsten Zusammenhang mit dem Körper*," and that the somnambule may thus possess a deeper insight into his own organic processes than belongs to him in the waking state.

"Perhaps," continues Dr. Dessoir (p. 31), "the secondary Self presides also over those powers of perception and action at a distance which only a few observers have as yet admitted. In experiments on thought-transference it is observable that the percipient frequently is not conscious of the transferred impression, but reproduces it by automatic word or drawing; and there is no doubt that in certain subjects the receptivity is heightened by the induction of the hypnotic trance, or of some analogous state. The

[1] (*Cf.* SPR. *Proceedings*, Vol. III. p. 225.)

[2] Dr. Dessoir's word is Individualitäten. It is to be wished that a general agreement could be reached as to the use of these two words. The meaning of *persona*, a mask, suggests that personality should be used for the lower or less persistent unity.

hypnotisation at a distance, moreover, which French savants have established as a fact, is best thus explained, by ascribing to the unconscious Self a far-reaching range of perception, and a power of developing an impression telepathically received as freely as though it had arrived by the ordinary channels of sense."

Dr. Dessoir, however, seems to suppose that in experiments on thought-transference with numbers, the "number-habit" constitutes a risk of error difficult to exclude. Perhaps a few words on this subject may here be in place; since the number-habit seems sometimes to be regarded as more of a *mystery*, and sometimes as more of a *discovery*, than it is in fact. Every psychical act or incident of any kind,—perception, image, choice, motor impulse, or what you will,—is an extremely complex thing. It is the result of the co-operation of a great number of nervous elements, which cannot possibly work in exactly the same way in any two persons, or even for the same person at different times. In the first place, for each of these complex acts there will be a *limit of attainment* beyond which each person cannot go; as each man is found to have his "personal equation" when the object is to observe as promptly as possible the transit of a star. In the second place, whenever a choice between acts at all dissimilar has to be made, there will be a *path of least resistance* common either to all mankind, or to some special section of mankind. Thus it is easier to read the letter W than the letter E, &c. A great variety of such experiments have been made; and we may safely say that even between such small efforts as the reading, writing, uttering, or mentally picturing any given Arabic numerals there must be some difference in the effort required; and consequently some *general* number-habit which indicates what is the path of least resistance for the majority of men. But where the difference of effort is so slight, the general or popular number-habit will be very weak, and it may easily be over-borne in any given man by some idiosyncratic preference. For in the third place,—and this perhaps has not always been clearly seen,—there is liable to exist in each man an idiosyncratic preference for one of two efforts demonstrably equivalent,—such idiosyncratic preference depending on some asymmetry in his own mental images. Let there be two hazards at billiards which are mathematically of identical difficulty, the object-ball needing to be struck within the same limits of accuracy in each case, although in slightly differing ways,—and you will find A choosing one hazard and B the other, not at random, but in accordance with some asymmetry in their respective mental pictures of table, balls, and probable results of impact. And of course this idiosyncratic preference—depending perhaps originally on some inequality of early experience—will tend to intensify itself, if yielded to, until a real muscular preference is superadded to the preference based upon mere conceptual asymmetry.

There is *no* choice, I say, however simple or arbitrary—not even the choice between heads and tails or odd and even—which the human mind can be trusted to make as impartially as the spun penny or the roulette-ball would make it.

There will presumably therefore be idiosyncratic number-habits, as well as general number-habits, and although these are not likely to become strong without being observed, still less to become so potent as to

explain coincidences in *double-numbers* thought of by two separate minds, it is undoubtedly proper to eliminate this possible source of error from experiments in thought-transference. We have made it a rule, since our first few experiments, to replace numbers in a bag, or cards in the pack, and shuffle between each trial, and draw at random; as described, for instance, in *Phantasms of the Living*, Vol. I., p. 34; Vol. II., p. 653.[1]

A moment's thought will show, however, that if we thus annihilate the influence of the number-habit in the *agent*, or person who offers the numbers for thought-transference, the existence of a number-habit in the *percipient*, or person who guesses the numbers, will in no way diminish, but may possibly even improve, the evidential value of any excess of coincidences between the numbers offered and the numbers guessed.[2] Suppose, to take an extreme case, that the percipient's number-habit were so strong that he always guessed a 3, then if he deserted his 3 and guessed a 5 just when the agent thought of 5 the coincidence would be much more striking than if he had had no number-preference of his own to overcome. Of course, in practice there are no such gross effects as this; and for evidential purposes we may simply neglect the percipient's number-habit if we take care to neutralise the *agent's* number-habit or card-habit by making him draw his cards or his numbers at random.

From this topic - on which Dr. Dessoir touches rather by the way—I pass on to a more complex problem. Recognising our personality as no single or simple thing, are we to regard it as potentially *multiplex*, or *duplex* only?[3]

"While there are abundant examples," says Dr. Dessoir (p. 26) "of a *double* consciousness, in the waking life, the dreams, and the abnormal states of every one of us, we find, on the other hand, that very few observers enter the lists in defence of the *multiplicity* of the Ego. A mere *triplicity*, indeed, would not suffice. Were we to discover, in some subject, with a third condition like Madame B.'s, that there were still intelligent acts which accomplished themselves below the level of that third consciousness, we should then strike down on a yet deeper layer of consciousness, and so on *ad infinitum*. We should arrive at a kind of onion-structure of the Soul! But since the facts are there, and refuse to be explained away by the facile hypothesis of suggestion, we shall need the most patient psychological analysis to bring us to our goal. In the present position of our knowledge I think that the wisest course is to suspend our judgment, and to be satisfied with the provisional hypothesis that in certain cases a further division of the secondary Self has been established. That a *consciousness* deeper than the

[1] I mention this because Professor C. S. Minot has animadverted in the American Society for Psychical Research *Proceedings* on our early omission (rectified long before his article appeared) to take this precaution. See American S.P.R. *Proceedings*, Part IV., for his criticism and Mr. Hodgson's reply.

[2] Professor Minot hardly seems to have caught this point. "The two minds," he says (American *Proceedings*, Vol I., p. 86), "were working differently, each according to its own habits; hence it is extremely improbable that the excess of right guesses was due to anything but chance coincidence." In reality, the habit of the percipient's mind, if different, as here stated, from the agent's habit, would not diminish, but increase, the evidential value of the coincidences.

[3] On this point see Mr. Barkworth's letters in the S.P.R. *Journal*, March and April, 1889.

hypnotic can be artificially created is shown by the well-known negative hallucinations of hypnotised subjects [where certain objects are kept out of the hypnotic consciousness by some still subjacent intelligence]. But for the formation of a new personality we need a new mnemonic chain [as well as a new consciousness],—and this seems seldom to be found in existence."

I do not disagree with this; but I think that we may probe the matter still deeper. It is not by a mere counting of heads (to use a somewhat inappropriate metaphor) that we must decide the question as to how many potential personalities we carry within us. "Man never knows how anthropomorphic he is"; and we have still to guard against anthropomorphism even while we are frankly contemplating ourselves in a state of segmentation. I mean that we must not let the unavoidable use of the word "personality" deceive us into supposing that any separate consciousness, any distinct chain of memory which rises within us must necessarily form a constituent of a secondary personality of somewhat the same scope and stature as the first. What seems really to happen is something far more complex than a mere fission into two personalities,—the second as good as the first, or better. There is no persistent plane of cleavage; we split asymmetrically; and the new personalities thus formed are by no means necessarily homologous with the old. There is every gradation from a secondary state like Félida X.'s, more stable than the primary, to the week-long or hour-long "controls" which sway the hand and sign the messages of the graphic automatist.

Or take the class of cases mentioned by Dr. Dessoir himself. The hypnotised A is told that B has left the room; and, consequently, cannot see B.; —*i.e.* (as has been amply shown by Liégeois and others), he does physically see B, but he receives a constant, watchful, dominating suggestion from somewhat within him that B cannot be seen. This is what they call at the Salpêtrière, a "systematised anæsthesia." The name is good; but who systematises the anæsthesia? What intelligence is it which thus prevents A from "psychically seeing" B, who is standing in the room before his eyes? The suggestion must come, as Dr. Dessoir justly suggests, from a still subjacent consciousness. But where, he inquires, is the chain of memory belonging to that consciousness, and needed to *complete* a subjacent personality—if such personality could exist? In answer, I would say that I believe that by proper artifices that third inhibitory personality could be tracked in other moments of the subject's life. But waiving this point, I will suppose that the hypothetical third personality comes into being with the experiment and vanishes at its close. Well, at any rate, it has existed *during* the experiment; it has fulfilled a task which needed memory, or at any rate continuous attention, prolonged over an hour. And what hard and fast rule can we make as to the necessary length of a chain of memory which is to constitute a personality? Must it last all life long? Then, if a man's brain is destined to soften next year, he is not a personality to-day. Or, again, with what definiteness of exclusion need the new memory be shut off from the old? It sometimes happens, as Delbœuf and others have shown, that a subject who on waking from the hypnotic trance remembers nothing can be led by artifice to recollect all that he has done. Is his hypnotic personality annulled when this fusion of memories is effected?

I have said that there is no persistent plane of cleavage to which we can point as separating two or more personalities within us. But, of course, there are certain planes of cleavage within us which (as Hughlings-Jackson has shown) we can in imagination distinguish with fruitful results. We can conceive of our nervous system as consisting of three strata, or three levels of evolution, and we can trace in dissolutive processes the results of the cessation of the activity of one stratum after another.[1] But this is not the kind of cleavage which will make a fresh personality. For that purpose the cleavage must not be horizontal, but to some extent at least vertical; that is to say, that each personality must include a certain amount of work done by the highest centres of all;—as well as much work done by the middle centres, and *all* the work done by the lowest centres,—as heart-action and vegetative processes.

The lowest centres, I say, must go on working throughout every change of personality, or the machine will stop altogether. The middle centres—sensory and motor arrangements—may divide their activities between several personalities, as in the hysterical cases which Messrs. Binet and Janet discuss. We can, to a great extent, trace their lines of division, and we can draw our schemes of personalities, each possessing such and such sensory activities, motor activities, &c. But when we come to the higher centres the difficulty is much greater. We do not know what proportion of activities of higher centres is needful to constitute a new chain of memory, a separate consciousness. And, moreover, it is by no means clear that the centres which for our waking life are the highest are also the highest or ruling centres for some of these secondary states. Dr. Dessoir seems to me to discern this fact, but not fully to apprehend its bearing on the ultimate question as to what is the deepest or original form of our Ego. He traces, in language to which we may fully assent, the rise of our personality, as now known, from the combination of the elementary or segmental egos of which our "colonial" ancestors were composed.

"If then the perfection of the animal organisation consists in this;—that from an original multiplicity of groups the individual is developed;—we are entitled to regard the lower nerve-centres in men as vestiges of an earlier system of consciousness. Little of the work done in those centres now arrives at complete consciousness; and thence we may infer that the efficacy of the mechanism is synonymous with its automatism. And if herewith we compare the fact of common experience, that every psychical activity becomes unconscious in proportion as it is fully developed—as reading passes from spelling to the glance over whole sentences at a time—we shall have to consider [normal or waking] 'consciousness' as the subjective expression of the work of acquisition which the mind is carrying on, as the accompanying indication of an incomplete co-ordination of nerve-pathways, or, in strictly psychological language, as the defect of habit."

Regarded either from the psychical or from the physical side, our highest waking consciousness represents unstable equilibria, processes maintained with difficulty, the τελευταῖον ἐπιγέννημα of many complex combinations-

[1] See Dr. Hughlings-Jackson's *Remarks on Evolution and Dissolution of the Nervous System*, p. 12, &c. (London: John Bate and Sons. 1888.)

As Dr. Hughlings-Jackson has said[1]: "There is no autocratic mind sitting at the top to receive sensations as a sort of raw material, out of which to manufacture ideas, &c., and then to associate these ideas. Answering to the constitution (mainly inherited) of the anatomical substrata of subject-consciousness, ideas rise up combined, in association, &c., and coming out of subject-consciousness they then constitute the object-consciousness of the moment. . . . There are different degrees of fixity of nervous arrangements, from those strongly organised, very automatic, and comparatively settled and unalterable, up to those *now making* (nerve-stuff being for the first time traversed by nerve-currents developed by the more and earlier organised nervous arrangements); those *now making* will be, of course, least organised, least automatic, and capable of most modification. The order from most strongly organised to least organised is the order from lowest towards highest layers of the highest centres. . . . Many of the new recently-made nervous arrangements will be evanescent; I mean that they will soon cease to be even the 'potential' nervous arrangements I spoke of. I suppose that one of the uses of sleep is to sweep the higher layers of the highest centres clean of many such nervous arrangements."

Now I maintain that the sub-conscious Self, on the other hand, does not attain manifestation through these recent and unstable nervous arrangements. Its emergence does not seem to depend upon its securing a larger share of the highest nervous activities of the conscious self. It attains its development—advances to the exercise of its characteristic powers—in a different way. It advances, not by passing into a phase of mental stress and friction, such as that which corresponds to the most complex waking thought, but by an apparently effortless improvement in the veridicality of its characteristic hallucinatory content. It begins—not to rack its brains for arguments—but placidly to image forth no longer false things, but true. And this (as I have often said) I believe to hold good both for the subject's own creative power or "genius," and for the influences telepathically transmitted to him from other minds. So far as the creations of genius are concerned, I can adopt Dr. Dessoir's statement.

"The new Psychology," he says (p. 37), "has convincingly demonstrated that in every conception and every idea, an image or a group of images must be present. But since these images, like the original perceptions of which they are the recrudescence, are always endeavouring to externalise themselves, they would always eventuate in actual hallucination, did not the competition of other memory-pictures and of new sensations hinder their development. When these checks are removed,—as in sleep, the hypnotic trance, and certain pathological states,—the hallucinatory germ can unfold itself freely; while, on the other hand, in ordinary waking thought we have to deal with a succession of uncompleted hallucinations. That state which is usually taken to be fundamental in us is in effect the suppression of our natural tendencies; and hallucination—commonly regarded as a merely morbid phenomenon—represents, at least in its nascent condition, the main trunk of our psychical existence. The fully-conscious life of the spirit seems to rest upon a substratum of reflex action of a hallucinatory type. . . It is only when Imagina-

[1] *Op. cit.* pp. 9, 10, text and note.

tion is comprehended as a function of the secondary Self, and Hallucination, Inspiration, Change of Personality, are understood as projections from within outwards, with more or less of sensory clothing,—manifestations, in short, of that *externalising* process which is always at work within us;—it is only then, I say, that the creative imagination of the artist is understood and traced to its root."

With all this I concur, and I have urged elsewhere that the truest way of regarding hallucinations is to consider them as messages addressed by a submerged to an emergent stratum of the personality. These messages may be true or false, meaningless or of weighty import, according to the stratum of the personality from which they rise. But messages from the sub-conscious, of one sort or another, they are; and for that reason alone they would deserve our most careful analysis. Note, moreover, as an indication of the way in which the unconscious Self works, that whereas hallucinations—visual hallucinations in particular—often represent the highest creative power to which the percipient's mind ever attains, they are developed, nevertheless, without his conscious effort, and as though by the mere act of releasing somewhat that was already formed within. They come to us unexpected, confusing, enigmatic; but as with the golden figures on Achilles' shield, the hidden Power which forged them was master of its art indeed:—

ποίει δαίδαλα πολλὰ ἰδυίῃσι πραπίδεσσιν.

And thus we come to the question of the relative dignity, the relative reality of the emergent and the fundamental Self. "From the foregoing discussions," says Dr. Dessoir, "it might perhaps appear as though the dominance of the sub-conscious indicated a higher condition of spiritual activity. That is by no means the case. Such dominance can indeed give facility for the highest creative production, but without itself representing a high psychical level. It is man's *original* condition, no doubt, but so also is it his most *primitive* condition; it works in the *completest* manner, but not in the manner most in harmony with Reality and the End of Life."

Now, if Genius and Ecstacy (as has been here implied) belong to the realm of the Sub-conscious, then I say that you must first tell us what *is* Reality, and what *is* the End of Life, before we decide whether Genius and Ecstacy are out of harmony with these. What is undoubtedly true is that our waking-emergent personality is that which is best suited to carry on the struggle for existence. Itself, as I believe, the result of natural selection, it inevitably represents that aspect of our being which can best help us to overrun the earth. More than this we cannot say. If, as we get deeper down, we come on ever more definite indications of powers and tendencies within ourselves which are *not* such as natural selection could have been expected to develop, then we may begin to wonder on *what* it was that the terrene process of natural selection, as we have it, began at the first to exercise modifying power. To such a question no answer whatever can be given which is not in some sense mystical, or rather metempirical, as dealing with hypotheses which no experience of ours can test. But it should be understood that there is no metaphysical, no physiological answer in possession of the field; the competition is open, the course is clear. In the present disintegration, as it may be called, both of the metaphysical and of the physiological conception of man's being, Dr. Dessoir urges the loss sustained on the

metaphysical side. "Many facts," he says (p. 40), "which Philosophy is wont to adduce as proofs of the existence of an immortal soul, may be equally well explained by the existence of an empirical secondary Self; and to this Self must Occultism transfer the supersensory faculties of man." So be it; to the secondary, to the submerged Self must, not decaying Occultism, but advancing Science refer whatsoever faculties are not accounted for by what we call normal development, terrene and traceable evolution.

But the question of origin will still remain; and it is not really a hypothesis wilder than another if we suppose it possible that that portion of the cosmic energy which operates through the organism of each one of us was in some sense individualised before its descent into generation, and pours the potentialities of larger being into the earthen vessels which it fills and overflows.

On points like these all that anyone can fairly claim is that the one speculative opinion should be accorded as full a right of existence as the other. And—to take leave at length of our author—there is no lack of fairness or candour in Dr. Dessoir's statement of opinions not his own. Agreeing with him as I do for the most part, I feel in disagreement as fully as in agreement the value, along all our range of inquiry, of so capable and painstaking a fellow-worker.

VI.

DR. JULES JANET ON HYSTERIA AND DOUBLE PERSONALITY.

By F. W. H. Myers.

"*L'Hystérie et l'Hypnotisme, d'après la Théorie de la Double Personnalité,*" is the title of a paper published by Dr. Jules Janet—brother of Prof. Pierre Janet, and nephew of the well-known Prof. Paul Janet,—in the *Revue Scientifique*, May 19th, 1888.

Though brief, the paper is remarkable from several points of view. In the first place, it shows by a striking example how far we are from having exhausted the possibilities of hypnotism,—even as applied to a subject who has been for years the object of hypnotic treatment. And in the second place, it affords a strong confirmation to the old view on which Elliotson and his group insisted,—and which one or two writers in these *Proceedings* were for some years practically alone in supporting,—that there is something in the effect of "mesmeric passes" which is specifically different from the effect of Braidian or other forms of stimulation.

Blanche Witt- is one of the best known personalities—or groups of personalities—in Paris. A hystero-epileptic of the most pronounced type, she has never been able for long together to meet the stresses of ordinary life. She has long been an inmate of the Salpêtrière; and some of my readers may have seen her exhibited there, at Prof. Charcot's lectures, or by the kindness of Dr. Féré or other physicians, as the type—I may almost say the prototype—of the celebrated "three stages" of lethargy, catalepsy, and somnambulism, of which she realised every characteristic detail with marvellous precision. Arrived at somnambulism, her state could be no further changed by the various means employed,—closing or opening the eyes, rubbing the top of the head, startling with lights or sounds, &c.—and she was led back to waking life through the stages in inverse order.

She was treated, it is needless to say, with great care and kindness; and her hysterical "*crises*" were frequently averted by hypnotic suggestion. But in spite of all the skill and experience brought to bear on her case, no one succeeded in removing,—except for a few minutes at a time by the action of gold, magnets, or electricity,—the various permanent "*tares*" or defects of sensibility, which signalised her deep-seated hysterical trouble.

In all her states she was without feeling of contact, feeling of position, or feeling of pain. When her eyes were closed (in the waking state,) she could not stand upright, nor close her hands completely, nor hold a heavy object. She could not hear with the left ear, nor see colours with the left eye, whose visual field, moreover, was greatly restricted.

Such was her condition when she came under Dr. Dumontpallier's charge at another hospital,—the Hôtel Dieu,—and was hypnotised by M. Jules Janet. She passed as usual through the three "classical" stages. But M. Janet,—

without, as I understand, any preconceived theory as to the result,—determined to try what a prolongation of passes would effect. Instead of opening the subject's eyes in the lethargic stage,—the regular method for inducing the cataleptic stage,— he continued to make passes, and presently found that she passed into an absolutely inert state,—"the deep state" of our English experiments, in which no muscular contraction could be obtained by pressure, nor did opening of the eyes induce catalepsy. After some futher passes the subject re-awakened into what seemed at first sight simply a more alert somnambulism than ever before.

But on examining this new condition it was found to be no mere slight modification of states previously obtained, but a state reconstructed, so to say, from top to bottom. In the first place, Blanche Witt- was now perfectly possessed of the senses of touch,—capable of perceiving contact, position, heat, and pain. She could now close her hands perfectly, and compress the dynamometer with normal power. She heard perfectly with her left ear, previously deaf, and saw normally with both eyes. It was no longer possible to inspire in her any hallucination. In one point alone did she differ from a normal person ; namely in her excessive *electivity*, or determination to attend to her hypnotiser alone, although she was perfectly capable of hearing and talking to other people.

In this second state, "Blanche 2"—as the reader will doubtless expect—had a full remembrance of the life of "Blanche 1," while Blanche 1 knew nothing of Blanche 2. A further point of interest was the determination of the true position of the "three classical stages" in Blanche's personality. It was found that when she was in her first or ordinary somnambulism her memory extended over the fully-developed state of Blanche 2,—so that we may consider the "three classical stages" as incomplete manifestations of Blanche 2, who had never till now been able to come fully to the front.

Furthermore,— as the reader either of Mr. Gurney's or of Prof. Pierre Janet's experiments will expect,—it was not difficult to show that Blanche 2 really existed throughout the whole life of Blanche 1. If colours were shown to Blanche 1 (with her right eye blinded) and she failed to distinguish them, Blanche 2 nevertheless saw them perfectly,—with the same eye and at the same moment,—and, when summoned, could describe what she had seen. Or if Blanche 1 were pinched or pricked, to demonstrate her insensibility, Blanche 2 felt everything, and, when summoned, began to complain. It is strange to reflect for how many years the dumbly-raging Blanche 2 has thus assisted at experiments to which Blanche 1 submitted with easy complaisance. It reminds one of the difficulty of pleasing both personalities at a time which is sometimes found when it is a question in which state to feed a hypnotised subject. There is an old case in the *Zoist* where a young woman used to insist so strongly in the hypnotic state that *then* was the time to give her her dinner, that the kind doctors consented. But when she awoke and saw the empty plate, she would burst into tears.

Once more, it appeared that the chloroformed condition, and in some sense normal sleep itself, belonged to Blanche 2 rather than to Blanche 1. Blanche 2 could remember what had happened during the chloroformic trance, and could recount ordinary dreams of which Blanche 1 had no knowledge.

On the whole, then, we may say that Blanche 2 represents—not, indeed, the *complete* personality, for that is never represented by any state of any of us,—but at least a pretty complete group or co-ordination of the various elements which go to make up a normal human being. Blanche 1, on the other hand, is scantily supplied with these elements ; she has only just enough to get on with ;—namely, motility, speech, vision of one eye and hearing of one ear. Blanche 2 adds to these vision of the other eye, hearing of the other ear, and general and muscular sensibility. And M. Janet urges that we may regard this incomplete endowment of the primary personality, (primary here only in the sense that it is the *habitual* one), as the differentia of hysteria.

"In short," he says, "every man presents two personalities, one conscious and one incognised [he justly urges that this second personality is *bien plutôt inconçue qu'inconsciente*] : in the normal man these are equal, equilibrated, each of them complete ; in the hysteric they are unequal and disequilibrated ; one of them—generally the primary—being incomplete, while the other remains perfect. . . Let us give a form to these two entities constituted by the two successive consciousnesses ; let us represent them by two persons, walking one behind the other. The person who walks in front knows himself but has no notion of the person who follows him. The person who follows knows himself—and knows also the person whom he sees walking in front of him. In a normal man, these two personages are both of them vigorous and are of equal stature ; the second cannot manage to knock down the first, and show himself openly ; in order to do so he must await some temporary feebleness of the first personage,—as in sleep or intoxication. Sometimes, however, as in the case of the madman, he can abolish the first personage and substitute himself. It is then that, proud of the exploit, he performs the impulsive actions with which we are familiar in some cases of nervous disease."

There seems to me to be some confusion here. The second personality, represented as being the equal of the first—*de taille égale*—ought hardly to be credited with performing mere mad acts if it succeeds in obtaining the mastery over the first. This is to attempt more simplification than the facts admit of. These *actes impulsifs* must be regarded, I think, in many cases as being the self-manifestation, not of any combination of nerve-centres (or their mental correlates) extensive enough to be the basis of a personality, but rather of some hypertrophied group of nervous elements,—some *idée fixe*, existing—like a tumour—in quasi-independence of the mental organism as a whole. "In a hysteric," M. Janet continues, "the equilibrium is overthrown. The two personages who walk in procession are of very unequal strength. The first is feeble, dwarfed, degraded ; he can scarcely stand upright ; the second is vigorous and of normal height ; he can easily show himself ; in order to do so sometimes he takes advantage of the natural sleep of the first personage and takes a stroll along the roofs,—that is spontaneous somnambulism ;—sometimes in mid-day he confuses the feeble personage who walks in front of him, and rolls himself on the ground in frantic gymnastics,—that is the hysterical crisis."

Here again I must protest against the ascription of these senseless habits to a secondary personality in the hysterical subject, which is *ex hypothesi*

stronger and saner than the first. Why should it behave thus wildly? Blanche 2, whom M. Janet has been holding up as the type of a hysteric's second personality, shows, when fully developed, no inclination whatever to violent pranks. She may indeed—though M. Janet does not state this—*remember* the contortions of the *crise*; but that does not prove that she originated the *crise* any more than she originates any foolish act of the first personality's doing. Again I say that our metaphor cannot be thus simplified; the *crise* does not strictly form a part either of the first or of the second personality; it is the explosion of a group of elements insufficient to form the basis of any stable personality at all.

But putting aside this confusion of language into which, as it seems to me, M. Janet has been led by attempting too great a simplicity of metaphor, and trying to force all the phenomena which Blanche Witt- exhibits into the *cadre* of her first or of her second personality, let us consider the definite result, scientific and practical, which M. Janet has attained. He has shown once more—as Elliotson again and again insisted—that the mesmeriser who wants to produce a complete effect, must go on unweariedly with his passes; and not assume that because one state, or several states, are readily producible, and constantly recur, there is therefore nothing to be attained beyond. And on the practical side he has shown that no amount of hysterical disturbance, however prolonged and profound, need be regarded as incurable. Hysteria is not a lesion but a displacement; it is a withdrawal, that is to say, of certain nervous energies from the plane of the primary personality; but those energies still potentially subsist, and they can again be placed, by proper management, under their normal control.

M. Janet tells me that last year he kept Blanche Witt- for months together in her second state, with much comfort to her; and that now, though he has ceased to attend her, he understands that her condition in the first state is much better than of old.

Another case,[1] treated also by M. Jules Janet, and which he has kindly given me the opportunity of seeing, is even more remarkable in a therapeutic aspect. It is perhaps the most marked among those very rare cases where it can be said with confidence that death itself has been averted by a hypnotic change of personality.

From the age of 13 the patient Marceline R. had been subject to a miserable series of hysterical troubles—chorea, *crises*, anæsthesia, &c. In January, 1886, the hysterical tendency took its most serious form,—of insuperable vomiting, which became so bad that the very sight of a spoonful of soup produced distressing spasms. Artificial means of feeding were tried, with diminishing success, and in June, 1887, she was paralytic and so emaciated that (in spite of the rarity of deaths from any form of hysteria) her death from exhaustion appeared imminent.

M. Janet was then asked to hypnotise her. Almost at once he succeeded in inducing a somnambulic state in which she could eat readily and digest well. Her weight increased rapidly, and there was no longer any anxiety as to a fatal result. But the grave inconvenience remained that she could only

[1] The earlier part of this case is decribed in M. Jules Janet's paper, "Un Cas d'Hystérie Grave," *Revue de l'Hypnotisme*, May, 1889.

eat when hypnotised. M. Janet tried to overcome this difficulty ; for a time he succeeded ; and she left the hospital for a few months. She soon, however, returned in her old state of starvation. M. Janet now changed his tactics. Instead of trying to enable her to eat in her first or so-called normal state, he resolved to try to enable her to live comfortably in her secondary state. In this he gradually succeeded, and sent her out in October, 1888, established in her new personality. The only inconveniences of this change seem to be (1) that when she has been left some months without re-hypnotisation a tendency to hysterical mutism sets in ; and (2) that whenever she is "awakened" into her *first* personality she has lost (like Félida X.) all memory of the time passed in the *second*.

After some shorter trials, M. Janet hypnotised her November 12th, 1888, and left her in her secondary state till January 15th, 1889. He then "awoke" her, but the vomiting at once returned, and she again applied to M. Janet for help. He hypnotised her, and left her in her second state till March 31st. He then again "awoke" her, with the same result. Again he hypnotised her ; and when he took me to see her on August 10th, she had been in the hypnotic state continuously for three months and ten days ;—during which time she had successfully passed a written examination for the office of hospital nurse, which she had failed to pass in her normal state.

When we saw her, August 10th, she was normal in appearance and manner, except for a certain shortness of breath, or difficulty of speaking, which M. Janet explained as likely to develop into hysterical mutism, if hypnotisation were not renewed. She was fairly well nourished, and her expression was open and contented.

M. Janet resolved not merely to re-hypnotise her, but to wake her and leave her for a time in her first state, in order to see whether the dysphagia had disappeared,—and at the same time to observe whether the loss of recollection of the events of the secondary state was really complete. He woke her—in the old Elliotsonian fashion—by "reverse passes." Her change of expression was very noticeable. The look of easy content was replaced by a pained, anxious air. Her attention was at once arrested by some masons at work in the courtyard,—who apparently had pulled down a wall, or made some similar change, since her last wakening. Asked what she was looking at, she said in a low, timid voice, "I had not observed the alterations." Asked what day of the week it was, she said "Sunday" ;—and in fact March 31st was a Sunday. "What day of the month ?" "March 31st." "How, then, is this oleander in the courtyard in flower ?" "O, sir," she said, "those flowers are only paper." "Feel them !" She felt them timidly, and said nothing more. "What had you for breakfast this morning ?" "I tried to take some milk." This again referred to March 31st ;—on August 10th, she had breakfasted on ordinary solid food. "Drink a little now." She attempted, but spasms at once began, and she could not retain it. We then left her ; but Prof. Pierre Janet (who was also present,) tells me that during the two or three days for which she was left in her first state the alarming vomiting continued and she began to spit blood. "My brother was sent for, and determined to re-hypnotise her. She was calmed as if by enchantment, and is now in excellent condition. During her two 'waking' days she made a number of serious blunders not only as regards

her mother, but with lodgers in the house. Her conduct absolutely proved a complete forgetfulness of the preceding months. After making inquiries from the various persons who saw her, my brother told me that he could retain no doubt as to her forgetfulness." M. Jules Janet adds that since she has been replaced in the second condition the loss of flesh has been rapidly repaired, and she is again comfortable.

The future of this case will be interesting to watch. Will the secondary personality fade away again, and leave her exposed to the dangerous sufferings from which she has now been for nearly a year delivered? Or will she, like Félida X, thrive on her radical reconstruction, and live out her natural life—*whose* natural life?—in her secondary condition, in peace and quietness? And if so, are there any of the rest of us who might be made much better by being made quite different?

VII.

PROFESSOR LIÉGEOIS ON SUGGESTION AND SOMNAMBULISM IN RELATION TO JURISPRUDENCE.[1]

By Walter Leaf.

Five years ago, in April, 1884, Professor Liégeois read before the Académie des Sciences Morales et Politiques a memoir on "La Suggestion Hypnotique dans ses Rapports avec le Droit Civil et le Droit Criminel." The experiments there recounted, and the extraordinary conclusions to which they obviously led, created a sensation in France; they were introduced to English readers by Mr. F. W. H. Myers in an article in the *Fortnightly Review* for November, 1885, entitled *Human Personality*, which attracted hardly less attention. Many will remember the shock with which they first read of the simulated crimes which M. Liégeois could by a word induce his subjects to commit. A daughter fired point-blank at her mother's breast a revolver which she believed to be loaded; a young man dissolved in water a powder which he was told was arsenic, and gave it to his aunt to drink. When questioned as to his act, he showed the most complete ignorance of what he had done. Hardly less astonishing and disquieting was the development given to the already known facts of post-hypnotic suggestion. Hallucinations had been produced which worked themselves out in action at a distance of days, weeks and even months, at the precise place and hour which it pleased the hypnotiser to suggest. It appeared impossible to set limits to the power possessed by the hypnotiser over the future as well as the present, over the character as well as the momentary acts, of a really susceptible subject.

Five years have expanded this pregnant memoir into a bulky but eminently readable volume of more than 700 pages. But they have only confirmed and extended the conclusions therein arrived at. The criticisms to which he has been exposed in France are examined by Professor Liégeois, and in our opinion are triumphantly refuted. Experiments in England, Switzerland, Belgium, Italy, Austria and Germany have combined to establish the views of the Nancy school against the great names of the Salpêtrière.

The additions which the treatise has received are not, however, wholly or even mainly polemical. It opens with an excellent review, which seems complete so far as France is concerned, of the past history of the suggestion-theory; that work done in other countries should be to a great extent ignored, is only what one has learnt to expect. But with these limitations it will be found an excellent introduction to the study of hypnotism in general. The chapter on processes of hypnotisation is thoroughly practical. The therapeutic aspects of the question, Professor Liégeois, as a lawyer,

[1] *De la Suggestion et du Somnambulisme dans leurs Rapports avec la Jurisprudence et la Médecine Légale. Par Jules Liégeois, Professeur à la Faculté de Droit de Nancy, 1889.*

leaves to his medical colleagues, Drs. Liébeault, Bernheim and Beaunis, with the exception of one chapter, where he relates at length some extraordinary experiments on vesication by suggestion, carried out by Focachon, the production of stigmata by MM. Bourru and Burot, and the use of hypnotic anæsthesia in surgery. It is with Chapter xii. that he begins the practical part of his subject, the influence which the new view of suggestion may have upon jurisprudence.

The facility which suggestion may give for the commission with absolute impunity of the most terrible crimes is so obvious that one might at first be almost tempted to wish that such knowledge had never been published to mankind. But, apart from the question of pure science, it will be enough that the reader should glance through the legal cases collected by Liégeois to see that such a wish is wrong. He makes it clear enough that such crimes have already been sporadically committed, and that miscarriages of justice have taken place, which a mere state of ignorance would certainly bring about again in the future. It is essential in the interests of the innocent victims themselves that we should know all that we can learn, in order to save them from themselves. One great step has already been made when we find that the most suggestible are precisely those who can best be helped, by the suggestion that no one can have any influence over them in future but a single hypnotiser, who is, of course, to be sought in a trustworthy person. M. Liégeois goes on to consider the steps to be taken when it is suspected that a crime has been committed by an innocent person in a state of suggested somnambulism. His method is ingenious, but we are afraid delusive. The first thing, he says, is to appoint a commission of doctors to decide whether the person accused is suggestible. This being ascertained, we have to endeavour to circumvent the suggestion given, *ex hypothesi*, by the real criminal; "You will entirely forget that I have given you this suggestion; you will in no case be able to denounce me, however much you may wish to do so." Liégeois has made experiments which show that such a device may be successfully turned. He gave one of his subjects the suggestion that she had committed a murder, but could not denounce him as the real author of the crime. Dr. Liébeault then hypnotised her and gave her the suggestion that when she saw "the author of the criminal suggestion, whoever he might be," she would fall asleep and perform a preconcerted series of acts with regard to him which would not be naturally associated with the idea of denouncing him. When Dr. Liégeois enters the room she goes through this series of acts, and would thus in a real case have identified him as the author of the crime—whether clearly enough to satisfy a jury remains to be seen. The experiment was successfully repeated on another subject, and Dr. Liégeois concludes: "It is possible to give a hypnotic subject any suggestions relative to the author, whoever he be, of the criminal impulse, which are not expressly and directly contrary to the amnesia which he has called forth. The real criminal will thus fall into the hands of justice, because it will have been impossible for him to foresee and remove all dangers by a suggestion of amnesia, however large and comprehensive." But we must ask Dr. Liégeois one question, which he seems to have forgotten: Suppose the real criminal has after suggesting the supposed amnesia added "No one in future can hypnotise you or give you suggestions but

myself"; how would he then begin his investigation? His commission of doctors would report at the outset that the ostensible criminal was not susceptible to suggestion, and what would be called "justice" must take its course.

We may end by calling attention to the curious experiments in negative hallucination described on pp. 701-711. This particular development is, we think, quite new. M. Liébeault begins by giving his subject, Camille S., the negative hallucination that she is unable either to see or to hear M. Liégeois. She is awakened, and is absolutely unconscious of his presence, even to the extent of showing no sign of pain when he pricks her with a pin, though she feels at once if anyone else does so. He speaks to her in his own name, but she takes no notice. Now comes the strange part. "I now proceed impersonally, speaking not in my own name but as though there were an inner voice addressing her from her own consciousness. Then somnambulic automatism appears as complete in this novel and unknown form as in any of the shapes with which we are already familiar."

"I say to her aloud, 'Camille is thirsty; she will go to the kitchen for a glass of water, which she will bring in and put upon this table.' She seems to have heard nothing, but, at the end of a few minutes, she does what has been indicated, and that with the lively and impetuous manner so often noticed in somnambules. She is asked why she has brought in the glass of water which she has just placed on the table. 'What do you mean? I have not stirred. There is no glass.' I then say, 'Camille sees the glass, but it is not water, as they would have her believe. It is a glass of very good wine; she will drink it, and it will do her good.' She executes at once the order given her, and has immediately forgotten all about it."

M. Liégeois goes on to give an account of a conversation between Camille and the other persons present, in which she repeats mechanically as her own every answer to their questions with which he himself prompts her. Finally, by a suggestion given in his own name he wakes her up—or rather, for she is already awake except as regards himself, he abolishes the negative hallucination, and she has completely forgotten all that has passed.

The conclusion which M. Liégeois draws is strikingly in harmony with views which have been developed at length in these pages by Mr. Myers. "This shows that during a negative hallucination the subject sees that which he seems not to see, and hears that which he seems not to hear. There are in him two personalities; an unconscious Ego which sees and hears, and a conscious Ego which neither sees nor hears, but to which suggestions can be made, passing, if I may so express myself, through the channel of the first Ego. This duplication of personality is no more surprising than that which has been established by Dr. Azam in the case of Felida X.," and one or two similar cases. The experiment is evidently crucial as proving that the phenomena of negative hallucination are purely psychical, nor physical, as MM. Binet and Féré would have it. The further conclusions which might be deduced are more than can be considered here. Suffice it to say that no student of hypnotism can afford to neglect this important work.

VIII.

TWO BOOKS ON HYPNOTISM.

By Walter Leaf.

Der Hypnotismus, seine Bedeutung und seine Handhabung, in kurzgefasster Darstellung. Von Dr. August Forel. Stuttgart, 1889.

Die Suggestionstherapie und ihre Technik. Von Dr. Ed. Baierlacher. Stuttgart, 1889.

These two short treatises have substantially the same object ; that of assisting medical men in the employment of hypnotism in ordinary practice. They take the same view, both authors being—one may almost say "of course"—thorough-going adherents of the Nancy school. Dr. Baierlacher is a practising physician in Nuremberg ; Dr. Forel, as it is hardly necessary to remind a student of the subject, is the director of the important cantonal lunatic asylum at Zurich. Neither of them aims at making any addition to the theory of the subject, but both supply interesting evidence from their own experience.

To take the common matter first, it will be noticed that both, like their Nancy teachers, employ suggestion alone for producing the hypnotic sleep, without any passes or prolonged gazing at bright objects. Both recommend Bernheim's *modus operandi*. "You place the patient in an arm-chair, and make him look for a few seconds up to one or two minutes into your eyes, and meanwhile tell him in a loud and confident but monotonous tone that he is going on famously, that his eyes are already swimming, the lids are heavy, that he feels a pleasant warmth in legs and arms. Then you make him look at the thumb and first finger of your left hand, which you gradually lower, so that the eyelids may follow. If the eyes now close of themselves the game is won. If not, you say, 'Shut your eyes,'" and proceed with suggestions of catalepsy, &c., following up those which appear to be accepted. The success which attends this method is rather surprising in comparison with English experience. Baierlacher advises that the sitting should be interrupted and a further trial postponed for a time, if sleep, or at least some sign of influence, is not produced in half a minute, or at most a minute. He has attempted hypnotism in 146 cases, and failed in only 25. Dr. Forel's percentage of success is still higher ; of the last 105 persons whom he has attempted to hypnotise only 11 were uninfluenced ; a figure which shows a decided improvement on the 80 per cent. who should, according to Bernheim, prove susceptible, and seems to dispose of the often-asserted view that the Latin races are easier to influence than the Teutonic. Indeed, Forel lays it down as a principle that "every mentally healthy man is naturally hypnotisable ; it is only certain transitory psychical conditions which can prevent hypnosis." It is unfortunate, as he remarks, that his own position gives him little opportunity of wide experiments with the mentally healthy. With his insane patients he has had little encouragement. One of them, Mrs. X.,

believed herself to be Mrs. Y. "I was able to hypnotise her, and to produce by suggestion sleep, appetite, and even post-hypnotic hallucinations. But when I told her with all possible emphasis during hypnosis that she now knew herself to be Mrs. X. and not Mrs. Y., that her idea was only an illusion at which she could now laugh, she kept on shaking her head so long as I continued my assertions, in order to show me that she did not accept the suggestion." "In suggestion," he adds, "one uses the brain of the subject as a machine. In the case of the insane the machine is out of gear and will not work."

In spite, however, of his primary occupation with these far from hopeful materials, Dr. Forel has collected a large amount of interesting evidence. We may quote one or two of his more important experiments. Here is a curious case of post-hypnotic hallucination.

"I told Miss Z. while hypnotised that she would on awaking find two violets in the bosom of her dress, both natural and pretty, and that she was to give me the prettier. At the same time I put one real violet into her dress. When she woke she saw two violets; one was brighter and prettier, she said, and she gave me the corner of her white handkerchief, keeping the real violet herself. I now asked if she thought that both violets were real, or if one of them was not one of those fugitive presents which she had on previous occasions received at my hands. She replied that the brighter violet was not real, because it looked so flattened upon the handkerchief. I now renewed the experiment, suggesting three real violets, equally dark, sweet-smelling, not flattened out, but tangible, with stalks and leaves; but I gave her only one real flower. This time Miss Z. was completely deceived, and quite unable to tell me whether one, two, or all three violets were real or suggested. She thought that all were real this time, while at the very moment she was holding in one hand a flower, in the other nothing but air. It is clear, therefore, that when the suggestion is made to all the senses at once it is completer."

The following very important case, from the practical point of view, is slightly abbreviated from Dr. Forel's account. "An old drunkard of 70 years of age, after twice attempting to cut his throat, had been kept in my asylum from 1879 to 1887 as a hopeless sot. He took every opportunity of drinking himself into a state of dangerous hallucination. At the same time he led all the plots against my endeavours to reform the drunkards in the establishment, and, though not generally malicious, incited the patients against the Temperance Society. He could not be allowed the least freedom without using it to get drunk.

"I had long given him up, but in 1887 tried to hypnotise him. He proved very suggestible, and in a few sittings he was brought into a surprisingly serious state of mind. His plots ceased as though by magic, and after a time he himself asked that the small quantity of wine which I had allowed him as a hopeless case might be cut off.

"The patient soon became one of the heartiest abstainers in the institution. I long hesitated to allow him any liberty, but finally did so in the summer of 1888. His freedom, though he was always allowed some pocket money, was never abused. He kept absolutely true to abstinence, became, by suggestion, a member of the Temperance Society, of which he

remains an active adherent, and on his trips to town drank nothing but water, coffee, or the like. His susceptibility to alcohol was such that it would have been impossible for him ever to drink without detection. . . In the course of the last nine months he has been only occasionally hypnotised for the purposes of demonstration, but requires no further anti-alcoholic suggestions."

Dr. Forel's attitude towards the developments which have formed the chief study of the Society for Psychical Research is one of reserve. He says: "A number of apparently supernatural phenomena are brought up again and again by trustworthy and honourable persons, which would seem to support a theory such as that of Mesmer. I refer to so-called thought-transference or *suggestion mentale*, clairvoyance, so-called presentiments and premonitions.

"A remarkable book in this point of view is *Phantasms of the Living*. . . . No fewer than 600 observations on visions, dreams, presentiments, &c., are collected. Exact information is supplied as to the trustworthiness of the evidence, and only clear statements of credible persons are admitted." He then refers briefly to M. Richet's and our own results in thought-transference, and concludes: "It is excessively difficult in all these experiments, apart from chance and cheating, to exclude the self-deception of the subject, and in the last resort, of the hypnotiser himself, and above all to be sure of the absence of slight unconscious suggestion and auto-suggestion. These results must therefore be taken with the greatest caution." This is an utterance with which we can hardly quarrel.

To turn back briefly to Dr. Baierlacher's book, the chief interest of which consists in a selection of cases from his own practice. The most striking of these is perhaps the first—a case of cancer of the stomach where he claims to have succeeded in entirely relieving pain during the last two months of life, for periods varying from a few hours up to (apparently) two days or more, obtaining natural sleep, which up to the time of his first attempt was only imperfectly induced by one to two cg. of morphia. At the same time he facilitated the taking of food by suggestions to the perverted appetite. The remaining cases are of a more familiar type—chiefly neuralgia and chorea. Dr. Baierlacher has the courage to mention at the end more than a dozen cases of complete or partial failure, a practice which deserves much commendation now that cure by suggestion is beginning to afford matter for sensational newspaper articles.