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OF THE

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VOLUME XXXIX

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PROCEEDINGS

OF THE

Society for Psychological Research

PART 112

AUGUST, 1929

THE TONY BURMAN CASE

BY NEA WALKER

INTRODUCTORY

ANTHONY HURDIS BURMAN, whom I shall generally call "Tony" in this paper, is the communicator for whose identity I shall try to set forth some evidence. He died in hospital on November 13th 1926, at the age of 20 in consequence of an accident on the previous day, his motor cycle having collided with a motor car. I did not know him, but as the value of the evidence largely depends on things of which I had no conscious knowledge said to me by Mrs. Garrett's control "Uvani" on November 8th 1927 and by Mrs. Leonard's control "Feda" on January 7th 1928, at sittings when I was the only sitter, it is important that, before describing these sittings, I should make clear what my connexion with his family and my knowledge of facts concerning him amounted to. To an important extent the evidence to which I wish to call attention centres round those two sittings and depends, like much of that in *The Bridge*,¹ on the fact that a number of true statements communicated cannot have been derived from the sitter's conscious mind.

My first introduction to Tony Burman's people came through a friend of theirs, a Mrs. X. living in Birmingham, who wrote to me on September 7th 1926, saying that friends were going to try to get a little circle together to sit regularly on Sunday evenings, and asking whether I knew of anyone who could help. I had already, since 1925, been in occasional correspondence with Mrs. X. owing to the fact that she was enquiring into psychic matters, and I thought

¹ Miss Nea Walker's book of that name. Ed.

that the circle must be in her own family. But her next letter indicated that the circle was to be at Mrs. Burman's house. I did not know Mrs. Burman at all, except by sight. I knew her by sight because I had seen her act (under her theatrical name of "Isabel Thornton,") at the Repertory Theatre. Of her family I knew nothing at all, although I had once, as long ago as 1918, seen her sister, Miss Mabel France, on business quite unconnected with psychic matters. Mrs. X.'s third letter, written on September 27th 1926, written, it should be observed, more than six weeks before Tony Burman died, contained the following remarks on this subject :

Extract from Mrs. X.'s letter.

I have sat several times lately with the Circle I mentioned—it consists of :

Mabel France,

Tony Burman (a boy of about 20),

Sometimes Mrs. Harry Burman (*i.e.* Isabel Thornton), and sometimes her husband.

We usually get short messages on the Ouija Board, and also the table raps out messages.

The other night it demanded that "Steve" [Tony's brother] who usually takes notes should come to the table, and we then got messages regarding his football playing from a man who said he had been a football Blue at Cambridge.

He gave a name none of us recognised, and subsequently another name of a school-fellow of Steve's who had been killed a year or two ago in a motor accident but who was *not* a great footballer : we felt it rather puzzling.

A great many names were spelt out of people who wanted to send messages last night, but we could get no clue as to who they were. . . . We did not get much in the way of a 'message' last night as someone, whom no one knows, called "Geoff" plagued us to sit holding hands and let him speak through a trumpet.

We sat and sat, and the trumpet waved wildly¹ and hit our heads, but we could only get a faint tapping inside it, no voice. . . .

¹ The trumpet did not, I learn, leave the table ; it was a gramophone loud speaker set up on the *narrow* end, hitting the sitters as it swayed, but not leaving the table.

A further letter, October 13th 1926, indicated that the circle was in difficulties, getting messages purporting to be evidential which turned out incorrect upon enquiry. Mrs. X. then said,—

The Burmans asked me to ask you whether, as you have experience, you would ever be willing to come to their house and “sit” one evening? We thought you might be able to give us some help.

Being interested in the account of the trumpet, which was said to have moved independently and to have yielded taps although not voices,—these I was amused to find the circle expecting immediately,—I was keen to see for myself what happened; so I replied that, though I was not an expert on physical phenomena and did not wish them to think so, I should like to go presently—at the time I was just recovering from an illness.

I next heard from Mrs. X. on Nov. 14th 1926 giving a short report on an anonymous Leonard sitting which she had had. In the course of the letter she said, in reference to a communicator:

My husband's brother¹ was very clearly described. He it was who came first to the Burman's circle table-tilting. Their name was tried by Feda—at least it seemed to us she meant them.² But we have just had a great grief. Tony, the Burman's son, who had certainly very great psychic gifts, had an accident on his motor bicycle on Friday and died yesterday. We were so fond of him, especially my husband. Such a dear jolly boy. It seems, in a way, as if God had been tempering the wind to them a bit by starting him and his Mother in this interest in The Beyond. But we shall not be able to do much in the circle without him I think.

¹ I learn that Mrs. X.'s brother-in-law, though dead many years, had, when alive, been a close friend of Mrs. Burman.

² *Mr. and Mrs. X.'s anonymous Leonard sitting. 11th Nov. 1926:* “He [apparently the brother referred to above] says there is a brud—pause—he has used up the power. Name starting BR—he is trying to make a cross-correspondence. B, R, and 7 or 8 letters—M. No, not enough power left—you will hear something which will make you understand. He is going to have an opportunity soon of speaking again, not here, but under other conditions.”

And so it happened that I never met Tony Burman when alive here, nor did I ever join the Burman family circle. I replied asking Mrs. X. to tell Mrs. Burman, that if she wanted my help it was at her service. My only other step was to write to the D.W. Group—*i.e.* the group of supposed spirits communicating through my sister Damaris—on November 15th 1926, and to ask for the help of one of them—Bunny—for Tony Burman, if he needed any; I asked for Bunny's help as I knew that Bunny had a link with people known to the Burmans which might, if such things mattered, make it easier.

In another letter on November 20th 1926, Mrs. X. wrote :

Mrs. Burman was at a séance at Mrs. Garrett's on the very day the boy had his smash, and the medium there said something which *now* looks like foreknowledge. She [also] described most accurately the friend who is said to have 'lent his power' to operate the Ouija Board. [At the private sittings.]

After this Mrs. X. went abroad for several months and I heard no more from her in connexion with the present case. Details of this sitting of Mrs. Burman's with Mrs. Garrett I did not hear till much later. I heard them in conversation with Mrs. Burman, and afterwards, in a letter dated April 17th 1928, she wrote :

As you know I took no notes, but so far as I remember the exact words used by Uvani [Mrs. Garrett's control] were 'I see with you, Madame, your littlest son, there is some trouble but he wants to say don't worry.' Stupidly, the word "littlest" put me off, and I said I had no "little son." But it might quite well have been Uvani's way of saying "youngest." I said I could not place it at all, my sons were both well, and in no trouble, so he [Uvani] said,—'We will leave it then, Madame, but I think I am right.' . . . The sitting was from 3.30 p.m. till about 4.45. Tony's accident took place at 2.30, and the message relating to him was given by Uvani towards the end of the sitting. I did not get to the theatre till about 8.30 p.m. when I found Steve's wire.

The coincidence was certainly a remarkable one. It is of course unfortunate that no notes were kept, but it may

be mentioned in corroboration (besides Mrs. X.'s letter quoted above) that Miss France remembers clearly that on reaching home in response to the telegram, Mrs. Burman's first words were,—“I've been told about this.”

This striking success of Mrs. Garrett's was doubtless the reason of further experiments with her by Tony's family. They had sittings on December 17th 1926, April 16th 1927, and November 17th 1927—this last being after my own sitting. Miss Mabel France, Mrs. Burman's sister, was present on all these occasions and she was accompanied by Mr. Stephen Burman on the first, by another sister on the second, and by Mrs. Burman on the third. There were some good points given at all three. I do not, however, propose to deal in this paper with the evidence they afford, though I shall have to refer to them again presently. All the three sittings were held at The British College for Psychic Science, whereas my own was at The London Spiritualist Alliance rooms, and there seems no reason why Mrs. Garrett should have connected me (I sat anonymously) with any of Tony's friends or relatives.

As the Burman family were accustomed to having sittings at home in Tony's life-time, their first attempts to reach him were naturally made there, sometimes with a table, sometimes with an Ouija Board. Careful records were kept of these sittings, and I have now (May 1928) had the opportunity of reading through the book. There is nothing evidential in the strict sense, but I was struck by the feeling one got that it was the same personality whenever Tony Burman purported to communicate, and I have placed in the full Report¹ of the Case a few extracts. Whatever the value, or otherwise, of such domestic efforts at communication without strong mediums, they seem to me, in many cases I know of, to have the effect of making it comparatively easy for those left on earth to reach the minds of such as Tony Burman and to direct them to come, for example, to some stranger like myself and to give evidence of survival, even through a medium strange to the communicator. In the present case one should probably also bear in mind the fact that the boy

¹ Filed with the S.P.R., but not published.

himself had a psychic faculty when alive and had been keenly interested in his own experiments then.

Other attempts to get into touch with Tony were made through the automatic writing of his aunt, Miss Mabel France. She had begun writing automatically after some conversation on the subject on September 29th 1926, that is, about six weeks before Tony's death, and continued it afterwards. I have recently (May 1928) been allowed to read her script from beginning to end.

There is a good deal of it, and perhaps nothing evidential, though there were one of two small tests which proved correct. Once more the value seems to lie in the channel which helped with the later sittings with professional mediums; and also in creating in the minds of some of the surviving relatives a sense of the boy's nearness.

So far as I could judge, the personality purporting to be Tony Burman seemed very like the Tony B. which communicated through the Burman Ouija and Table Circle. He also seemed consistently like himself all through Miss France's attempts at automatic writing.

A feature of the script was that each time I was taking a sitting, the "writing" seemed to show a dim knowledge of the event. And, a few days before I sent this report to the Burmans to examine—quite unexpectedly as far as they were concerned—Miss France says she had a long communication from Tony Burman, telling her it was coming, and a good deal about it. Unfortunately she was feeling disgusted with the quality of her faculty, and tore up these last scripts before my packet arrived.

As regards my own knowledge of the Burman family, it will of course occur to the reader that I must have seen accounts of the accident and inquest in the newspapers. As a matter of fact I do not think I did see them, but if I did, the one thing apart from the accident which I should have learnt was Tony Burman's profession—that of a mechanical engineer. I certainly, however, had no conscious knowledge of this when I sat with Mrs. Garrett or with Mrs. Leonard.¹

¹ I have collected some newspaper accounts and filed them with the fuller documents concerning the case which I shall lodge at the S.P.R.

My first direct communication with Mrs. Burman arose from her reading my book "The Bridge" published in September 1927. She wrote congratulating me upon it, but from her letter I need for the purpose of this paper only quote the following :

My only slight introduction to you was through Mrs. X. of — when you very kindly told me, through her, of Mrs. Garrett, the trance medium at the Psychic College in London.

And now to the purpose of this letter. I have just finished reading your engrossing book "The Bridge" . . .

She then went on to enquire as to the identity of one of the members of the D.W. Group (my sister Damaris' group of communicators), "Geoff," since at her table sittings she had had a communicator of that name. But, as "Geoff" of the D.W. Group in "The Bridge" was a pseudonym, that enquiry led nowhere.

Meanwhile, her sister, Miss Mabel France, had written to me, on October 22nd 1927, also expressing interest in "The Bridge," and asking me if I could help her to get some message, through Miss Bazett, for the Burmans, in time for the anniversary of Tony Burman's death, of which she gave me the date for Miss Bazett's use, together with his full name. Miss Bazett kindly made three attempts with no sitter connected with Tony present, but the results were not considered good, and I only mention them because, in commenting, Miss France wrote the letter from which I quote below, and the mention of Alpine Climbing in it gave me a point about Tony of which I was ignorant before.¹

Extract from a letter from Miss France.

On the whole, I am inclined to think that it was Tony who was trying to get through, but I do not consider that the evidence is good. Perhaps I am expecting too much. The rooms, in case any student cares to consult them. I shall give here all the information I think really important.

¹ While writing this report I have read the annotated records of these sittings again and do not think they gave me any other information. Mrs. Sidgwick, who also read them, agrees in this.

only real points that he got through were the hint of his accident, the question that he is always trying to get tests through, and his sympathy.

The reference to ice was most interesting.

The one thing that absorbed him was Alpine Climbing.

He talked and thought of little else, especially during the last year of his life; *but* Miss Bazett seems to make it clear that this [reference] is only to give relative significance to something which I cannot trace.

Finding that Miss France had not, through Miss Bazett, achieved anything which she thought likely to be helpful for the anniversary, and having myself been offered a free anonymous sitting with Mrs. Garrett by the Council of the London Spiritualist Alliance I decided to use it for the purpose of trying to get something for Mrs. Burman. My personal reason for choosing this case in preference to any other was that I had seen Mrs. Burman act soon after her boy was killed; and, knowing her by sight, had observed her going about with no outward signs of grief and depression, and had greatly admired her pluck. Also, having been indirectly interested in Tony Burman's own psychic experiments when he was alive here, I had an interest in him. So I thought that all this would probably count for something in achieving success, and perhaps justify an experiment rather difficult for a first sitting.

Before I go on to describe this sitting which took place on November 8th 1927, I must explain that though before October 31st 1927 I had seen no records of the sittings which the Burman family had had with Mrs. Garrett nor heard any details about them, I had learnt from Miss France's correspondence with me *à propos* of Miss Bazett that, on April 16th 1927,—their second sitting with Mrs. Garrett—Tony had seemed to communicate; and so I wrote a note to Miss France, on October 31st 1927, asking her just to tell me "whether the boy [Tony] had come through Mrs. Garrett *by name*," since I had been "offered a sitting with her, meant to try her when in London, and thought perhaps I might see what luck I had in trying to get anything about 'Tony.'"

I asked her to tell Tony, and said that I had tried to tell my folk "over there." I also asked her not to give me information, but, if there were a name which might explain Tony to the control, to tell me that. Unfortunately, instead of doing so, she sent me a copy of the sitting in question. The correct points were only indicated in the margin.¹ I had no time to read it and take it in, I merely glanced down the pages to see whether the name Tony occurred, and found it. So I put the sitting away and did not study it until after April 1928. But this complicates my work in presenting the material considerably, for I have to take that sitting into account, since I do not know what information my subconscious mind managed to acquire in skimming down the pages to see if the name occurred. I can only say, truly, that the glance which I gave the record was *extremely* hasty. I looked for the name, found it, and left it at that. I do not myself think that there is much in my sitting which is directly derived from a glance at Miss France's. The best material certainly is not. But in the fuller account already mentioned there is the report, as I saw it, and anyone disposed to read that can judge for himself.

Miss France thus knew from October 31st that I was to have the sitting and try the experiment. And she also knew that I was going away from home on November 3rd for a week. She may therefore have guessed that the sitting would take place during that week though she did not know the day. I had avoided letting other members of the family know about the experiment as I feared it might prove a complete failure.

A sitting with a fresh medium, unaccustomed to oneself and to one's communicators, is of course always rather confusing, and everyone concerned is at a disadvantage. This I think should be remembered in comparing the result of this sitting with that of the sitting which I took later with Mrs. Leonard.

The sitting was anonymous as far as Mrs. Garrett was concerned; if I had previously had any doubts as

¹ The *annotated* record of this sitting was first sent to me on October 5th 1928. N. W.

to whether she knew me, and in fact I had none,—the internal evidence of the sitting clearly shows that she did not know me. I had never before seen Mrs. Garrett, though I had heard reports of her which were good on the whole. Her manner is dignified, and she did not talk to me either before or after the sitting. She sits in broad daylight or electric light. There is no fuss, and the “trance” comes on quickly and easily, taking, in this particular ease, just five minutes. The control is supposed to be an Arabian, Uvani by name.

REPORT OF SITTINGS WITH MRS. GARRETT

For the convenience of readers I will here give a list of the best points of evidence in this sitting :

	PAGE
1. A memorial and inscription - - - - -	12, 13
2. His mother's health and worries - - - - -	13
3. Renovation of the house . . . - - - - -	14
4. Oblong box - - - - -	14
5. A watch causing <i>amusement</i> - - - - -	14
6. Picture of his mother “listening-in” - - - - -	15
7. Portrait which used to be “laughed at” - - - - -	15, 16
8. Portrait of his sister - - - - -	16
9. His mother and an intention to attend a service of remembrance - - - - -	17

The full report of the sitting with annotations is filed with the documents concerning the case at the S.P.R. rooms. The earlier part of the sitting, though somewhat vague and confused, may have referred to my own people at least in part, and was not without interest; I was of course as encouraging as I could be in recognising likenesses, but I shall not attempt to describe or quote here any part of the sitting except those statements which, after I had seen the annotations on the whole record by the Burman family, appeared to me to refer to Tony Burman. I should like to say here that I have had very considerable help from the Burmans as annotators. If they will allow me to say so, I found them most kind, very sensible, and not at all anxious to find Tony when he was only doubtfully present, although they put all the possibilities before me.

After the above mentioned talk, which may have referred to my people, Uvani went on to describe two other people in such a way as to suggest to Mrs. Burman when she saw my record, her uncle Edwin Jones and her father. Having now discussed the details with her, I can see that the recognition is legitimate. Now, in the Burman home circle, these two men, Uncle Edwin Jones, and Mrs. Burman's father, Henry Charles, belong more or less together. And they have each the same sort of relation to Tony Burman. Both had communicated at home, and Mrs. Burman's father had been well described through Mrs. Garrett to Miss France and another sister on April 16th 1927.

Again, showing that this material perhaps related to the Burmans, were given the names, "May and Marion, on the maternal side." It is a fact that Mrs. Burman's name is Marion Isabel, commonly called May; and that her mother's name is Marion. So that I feel that the Burmans were reasonable in thinking that this material probably referred to their communicators.

Meanwhile, just before May and Marion were mentioned, but *not* before the two men were described, I had asked, rather in despair, whether Uvani had "ever had a boy communicator called Tony? Did he remember?" The reply was—"I don't remember. My people come and go. Strange I can get so little light"—I of course having been obliged to say that I could recognise none of these later descriptions.

There was then some confusion and apparent return to my own communicators, and then, quite suddenly, the control said :

There is one over here near him [*i.e.* apparently near the D.W. Geoff] Anton—Antony. Then *he* would be Tony ?

I replied :

It sounds possible. Has Tony any messages for his mother ?

The supposed Tony at once referred to his mother's state of health, and to worries (see pp. 13 and 14); these

references were understood by the Burmans, not by me, and referred to matters cropping up *since* the date of the last sitting taken by the Burmans. Uvani then said that Tony told him that his mother had actually spoken to him through "this condition" twice. Now, I did not then know how many times Tony had communicated, nor to whom. It turned out that Tony had communicated through Mrs. Garrett twice, but *not* to his mother; once to two aunts—April 1927, and once to the brother and aunt together, December 1926. His mother and aunt are however much alike; and this might link them up normally in the mind of the medium.

Tony then referred to a "Hilda" and to his sister; it almost sounded as if Hilda *were* his sister. I learnt, afterwards, that Hilda was a favourite cousin, and a great friend of his sister. This Hilda, I find, had been referred to (in connexion with a small evidential point) at the Garrett sitting attended by Tony's brother and his aunt—namely the one through which I had glanced, *unannotated*, to see if the name Tony would help the control if *I* used it.

Discounting references to the accident, which went no further than my knowledge, there were afterwards the following items of value, *i.e.* which were unknown to me, and also *not* repetitions from previous Burman-Garrett sittings. I quote some of them verbatim with the annotations supplied later, as this is really the simplest method where the evidence is clear and not tangled up with material belonging to other communicators.

FURTHER EXTRACTS FROM SITTING WITH MRS. GARRETT,
NOVEMBER 8TH 1927.

There was something to do with a tablet or a memorial. He knew she spent some time and thought over this memorial, with the inscription and the design.

Note by Stephen Burman.
Yes.

N. W. Tell me anything you can about it. [My knowledge of it is exactly nil. N. W.]

A small kind of memorial.

She must have been spending some time about it. He says she hasn't put anything about *Death*, something to do with Resting and Life, or Resting in Life. He speaks of that. And of knowing about it all, and of what she did about that.

If you could thank her for that?

He knew how ill she was.

Is his mother a little rounded of body?

[I know her by sight, and on the stage. N. W.]

A jolly woman, a most charming woman.

She's been suffering a little with the foot and the limbs. Tell her he knew. Much weakness there (back about waistline) Very busy, very active. But her back has been troubling her.

Also something about her being off sleep.

She was worried about somebody.

October, the first two weeks, she was rather worried. And she's been off her sleep.

She gets very short of breath.

Note by Stephen Burman.

Tony's mother was much concerned during the last week as to whether she should insert an "In Memoriam" in "The Birmingham Post." She even went so far as to consider the wording of such an announcement.

Note by N. W.

She tells me, 14th Nov. 1927, that she would certainly *not* have mentioned *Death*.

Note by N. W.

See earlier references to health.

Note by Mrs. Burman.

More than correct, unfortunately.¹

Note by N. W.

I found this true also.

Note by Stephen Burman.

Correct.

Note by Stephen Burman.

Quite correct. Theatrical difficulties as mentioned previously, and consequent effect on sleep.

She does not now, thanks to an operation, 18 months ago.

¹ The aunt is of the same build, see p. 12. N. W.

[I knew this. N. W.] [*Later* : I had not known of the operation. N. W.]

He says he helps that as much as possible.

There was some change with a house. If not a change, a renovation.

Again he speaks of Hildred or Hilary with love, and in connexion with a sister.

He's giving an impression too of—tell her that she has a box connected with him. Almost like a wooden box. Oblong, not small. She's got prize books and other things put away. But in that box she's got some things belonging to him, and amongst it although I'm not sure—there's a lot of personal stuff in that box—there's something to do with a cap of his when he was smaller. She kept that by her.

Did she get a watch? There was a watch to which he attached value. He doesn't know whether she's got it. But he attached value to it, and she will laugh over that.

The next few items were either known to me, or incorrect for Tony Burman. Then Uvani says that

He [Tony] must have spoken to her [his mother] in July. Also about this time last year.

Note by Stephen Burman.

We rebuilt a wing of the house between Christmas and Easter 1927—*i.e.* since Tony's death.

The cousin Hilda, again, see page 12.

Note by N. W. 14th Nov. 1927.

Mrs. Burman told me that his sister (Hilda's friend) had an oblong box which she prized very much though it was nothing of value. It is wooden. When closed it looks like a box; open, it is an old-fashioned desk. Size—length 20½"; width 10½"; depth 7½". It never belonged to Tony. She did have snaps of Tony in it, nothing else of his but "personal stuff" of her own. No Cap.

Note by N. W. 14th Nov. 1927.

Mrs. Burman told me, today, that Tony had bought himself a 2/9 watch of which he was very proud. And that the family always teased him by asking him the time, as the watch was never right!

There was no professional sitting in July, 1927; but the sitting at which Uvani tried to warn Mrs. Burman of the trouble about her "littlest son" was on 12th November 1926. And Uvani added—"He tried to send her a warning that all was not well." This of course I had heard of, see page 4.

It is curious that, among these remarks applicable to Tony Burman, Uvani refers to someone in the physical body called Humphrey, connected with Tony—just as a passing remark. Now I discovered, *after* the sitting, that three people whom I had sent anonymously to Mrs. Garrett had "Tony" as communicators. Two of these were *children*; and, one of these children *had* a living brother, Humphrey. So that this looks rather like the control's or medium's memory working. On the other hand, when I asked for a Tony, I did not realise that there were three to choose from who might be connected with me in the medium's mind. How was it that Uvani produced, except for this reference to Humphrey, only evidence of the Tony I wanted?

A little later in the sitting Tony Burman seems to become clear again, in a reference to wireless:

He tells you to tell her that he likes the wireless in the house, and also the music

And will you say that, in the evening, when sitting in her own particular chair, busy as usual, he is always there. He listens in with her. He is interested in long-distance communications more than anything in the world, because he has a theory that . . .

Note by Stephen Burman.

Tony was very keen on wireless.

And, his mother *always works at raffia* in a *particular corner of the sofa* while listening to the wireless.

There followed some theoretical remarks on the analogy between wireless and psychic communication, of no particular interest. Presently Tony announced that there was "a portrait of him which they used to laugh at, taken with a brother, somewhere in a bedroom now." This I

was told was correct, including the laughing. More details as to photographs were given, so far as I can learn, correct, but rather too much confused for this account. Then the communication seemed to improve again, and the following is worth quoting :

Somebody connected with the family, John, an Unele John. She will understand who he means.

Is there an Edith, an Aunt, or somebody in the physical body, Edith, he says—She hasn't altered a serap.

There's one sister to whom he's very devoted. They were great friends. That sister was thinking of some trip abroad, or over water. I think she took that trip—no, I don't think she went abroad. But there was some thought of it. She went near water. During the time she was near this water, she had a small portrait done which he liked awfully.

The beginning of September.

He had a friend who went out during hostilities.

His Mother is going to some kind of remembrance service. She's to remember he will be

Tony has an Unele John.

Note by N. W.

I learnt, in conversation with Mrs. Burman, that there is an Edith, *not* an aunt, but of aunt-like age in regard to Tony, who has sat with the Burmans.

Note by Stephen Burman.

Tony had a sister to whom he was devoted.

We cannot trace "a trip abroad."

She spent her summer holidays at the seaside.

During this time a very excellent snapshot was taken of her—

At the end of August.

Note by Stephen Burman.

No. [But I am told that the Geoff of the Burman psychic group, who thus "made friends" with Tony, died so. N. W. 14th November 1927.]

Note by Stephen Burman.

Quite correct. For a week she had considered going to

there too, although it's not a the Albert Hall Remembrance day for him. Service on November 11th.

Assure her he knows she's been talking about something to do with the service.

Again the medium enlarged a little on the question of "a grave or a garden of remembrance," and went wrong, for Tony had no grave—his body was cremated, I learn, and the ashes scattered. This part seems to be a thought roused by the previous references, and, as it did not apply, I suspect that the medium's own mind began to take control, and that confused recollections of other sittings were consequently introduced. I fancy that this must often occur.

Then Tony Burman disappeared from the sitting almost as abruptly as he entered it.

Subsequently, as already stated, Mrs. Burman and Miss France had a joint anonymous sitting with Mrs. Garrett, on 17th November 1927, at the British College. This sitting contained only one point unknown to the sitters, but was good as a sitting of the other type. For instance Tony remarks, characteristically—"Tell *the men of the family* that the garden is awful at the moment." And "ask Joan [his small sister] about Marjorie; she's another sprat"—Tony's generic name for Joan and her small friends. And again, referring to his motor cycle: "there's not even the ghastly remains."

And also:

MEDIUM. He was jolly-looking, laughing, tall, full.

Mrs. B. Yes, he was fat.

MEDIUM. Tony says, 'I didn't mention the stoutness, so I asked the control not to mention it. I was never fond of my girth. Thank heaven it's not there now. I'm much more graceful—round—jolly—it's most unkind of you to refer to it.' Everyone forgave him his size because he was so jolly. He is laughing again.

I quote the above two small items because they seem to me to make Tony "live" rather better than he can in the sort of evidence with which we are chiefly concerned.

It was in connexion with the annotations to my Garrett sitting that I met Mrs. Burman for the first time. She and her surviving son Stephen had annotated the sitting, but she wanted to meet me and explain the annotations. So we met, on November 14th 1927, for about three quarters of an hour at one of Kunzle's cafés in Birmingham. We talked only of the sitting and of matters directly bearing on it, and I saw no other member of her family at that time, nor did I go to her home; I knew where it was, but had never been inside.

I met her again, at the same place, and for the same length of time, about a week later—after she and her sister had had their joint sitting with Mrs. Garrett on 17th November 1927 (see above); for Mrs. Burman wished me to see the reference to *my* attempt through Mrs. Garrett on the 8th. In the course of their sitting Tony had remarked that he had

got in one morning and been able to send messages, lady communicator; didn't get much of her own, but I got through all right. And you said—'I wonder if Tony will mention it.'

This evidently, I think, referred to my sitting, at the London Spiritualist Alliance, on the 8th November 1927.¹

Mrs. Burman also read me the rest of the record of the sitting of the 17th November 1927 with comments. There is no evidence in it which affects anything I quote out of my subsequent Leonard sitting. Some of this Garrett sitting

¹ I am kindly allowed both by the L.S.A. and by the British College to send sitters, anonymously, to Mrs. Garrett at their rooms. I give a note saying that the sitter is a *bona fide* enquirer; the sitter presents that, and is asked no further questions by anyone.

Mrs. Garrett thus knows that I send sitters to her. But, she could not know me by sight, and therefore could not, when I sat anonymously with her, have known that I was the individual who sometimes sent sitters to her at these institutions.

was almost a repetition of mine. The one point of real evidential value was the following short sentence :

There's Billy, a girl, ask Joan, Billy B.

This meant nothing to the two sitters until they consulted Tony's small sister, Joan, aged 10, and discovered that a child whom the sitters knew as Louise Morgan (pseudonym) was called, by Joan, Billy. The "B" is perhaps an addition by the medium—probably due to some other association. Small as it is, this is a good little point of evidence. It is however open to the objection that either the aunt or the mother *may* have heard the name used without consciously noting it, since they have access to Joan. But their surprise, I think, indicates that this really was not the case. They feel sure of it. At any rate they consider it one of the points in the sitting.

Later in the same month, November 26th 1927, Miss France (Mrs. Burman's sister) spent a couple of hours in my home as she wished to meet us. It was a first meeting, except for the business appointment many years before, and the talk was a little on psychic matters generally, and on general social lines otherwise. No information about Tony Burman or his family was given away to me.

After seeing the annotations on my Garrett sitting, I began to feel that Tony's efforts might lead to something good which should be helpful to his people, some of whom, I learnt, were feeling very sceptical. So after this I refused to see any more of either Mrs. Burman or Miss France, since I planned to try Tony Burman as communicator at my next Leonard sitting, *the date of which I did not tell his family*. So far nothing had been given away to me, but I feared lest, as we got friendly and conversation consequently easy, I might learn a good deal.

I therefore repeat that when I had my Garrett sitting on November 8th 1927, I had met no one of the family, except for the brief meeting with Miss France years ago. Also that, by the time I took the *Leonard* sitting for Tony Burman, on January 7th 1928, I had had only the

above-mentioned three talks with Mrs. Burman and Miss Francee.

REPORT OF A SITTING WITH MRS. LEONARD

I now proceed to describe the result of my sitting with Mrs. Leonard on January 7th 1928, in the light of the annotations supplied by the Burman family. The full record is filed at the S.P.R., and I will quote here only the more evidential portions. Unless I state points as known to me, readers can be quite certain that the material had no meaning for me nor for the medium.

But before summarising, I have a few points to note.

I had taken a Leonard sitting on December 2nd 1927 by proxy for someone who was prevented going at the last moment; at the very end of the sitting I had asked the D.W. Group whether they were "in touch with a boy called Tony whom I know, not a child." The answer was that they were, and I then asked if they could bring him to my next Leonard sitting. Nothing else was said. I specified "not a child" purposely, because I knew of a child of that name and that in October he had communicated, through Mrs. Leonard, to his mother, and therefore I rather feared confusion. Curiously, the Christian name Tony was never mentioned in the course of my Leonard sitting, although an attempt was made to give Tony Burman's surname.

Once more the Burmans did not know the date or time of my sitting. I could attach meaning to very little of what Feda told me; where I can, I say so. It has been suggested to me that a list of the best points might be helpful to a reader inexperienced in what constitutes evidence. So I append one. But it must be remembered that, although the following points are perhaps the best evidence, they are drawn from a continuous whole, and that many of these points are best read and considered *in their contexts*.

	PAGE
1. The position of a hand - - - - -	22
2. A frustrated plan to go somewhere near water -	23, 24
3. A Family name Roy. See Point 15 also - - -	24

	PAGE
4. An indication of a certain leather motoring coat -	25
5. The effect of Tony's death on the mind of his brother - - - - -	25, 26
6. A bank-book and Fund - - - - -	26, 27
7. A particular road along which Tony enjoyed "whizzing" - - - - -	28, 29
8. A reference to a photograph of him riding at speed	29
9. Stamps - - - - -	31
10. A reference to his poisoned hand, with details not in the conscious mind of his family in that connexion - - - - -	32, 33
11. Happy Saturday afternoons - - - - -	35
12. One particular Saturday afternoon spent "hammer- ing a pole" into the ground - - - - -	35
13. The metal figure - - - - -	36
14. A reference to an argument about a hat - - - - -	37
15. Followed by an attempt at the communicator's surname. This name was known to the sitter; but it came in the correct context. And, the other name given (No. 3, Roy), was also a family name, and that too came in a correct connexion and was <i>unknown</i> to the sitter - - -	37

The sitting begins with the usual preliminary conversation, greetings, etc., and presently Feda (Mrs. Leonard's control), after alluding to a previous communicator, speaks as follows:—

But there's someone here, not a lady, a man. . . . He's not an old man, but like someone cutted off.

N. W. Please ask him to come forward and speak to me.

I think he was one of those that went unexpectedly—it's as if it had been rather a shock to him and to other people too.

Note by N. W.

I knew this was true of Tony Burman.

Some rather vague references to his activities just before the accident and to his physical feelings after it follow, and Fedá continues :

But, Mrs. Nea, I don't think he could really have suffered at passing, he had gone unconscious quickly. He wanted me very much to say that. 'Quickly unconscious,' he keeps on saying.

On this, Mr. Stephen Burman, his brother, who was with him immediately after the accident and until the ambulance came to remove him to the hospital, comments:

Quickly is a relative term. He was fully conscious and in considerable pain for an hour—after which he was given morphia.

The assertion therefore that he was quickly unconscious and suffered little was not true ; but, curiously enough, he persisted in it through all the channels he apparently used for communication. I shall make some remarks on possible reasons for this in my final section.

Fedá then continues :

And do you know if he would have had his hand in a peculiar position ?

I have a feeling he would be holding something, or trying to keep hold of something, I want to keep hold of something, but it is as if my hand is in an awkward position. There's something important about this. His fingers was gripping this, and his thumb sticking out a bit (indicating upwards and slightly outwards from the angle of the wrist N. W.). And he was pressing.

Note by N. W. September 1928.

Based on information supplied by the family.

Tony was not unconscious all the time in hospital, although most of it, and this probably refers to his position lying there after the accident.

When his mother went on the morning of the day of his death, he spoke a word or two. And she noticed that he had his *right hand up above his head, grasping a pulley*, to ease breathing. As he spoke, she saw him take his hand down ; the nurse took hold of it and replaced it. No one is

I feel as if he was rather worried when he first got over. He was worried about other people here, and he wouldn't be quite happy at first. He wasn't, because he says there were circumstances that were particularly hard for people who were left here.

He had what he would call two groups of people. It's as if there was a little lump of people very much in one condition, and somebody else he was interested in and loved rather differently.

He thinks of two people, and of a lady.

It's the lady specially he's sending this message to. Had she been away from him a little while before he passed? I think so. Not quite together for a little while. But it couldn't be helped, it wasn't anybody's fault, just like conditions. I think that that would be just before he passed over.

I get a feeling that he had been thinking about water, of

sure they noticed the position of the thumb; but, if one holds on to a bar in that position, the thumb goes outwards automatically.

Note by Stephen Burman.

Except for the circumstances of his death itself, there were only two minor cases where his passing was inconvenient. One was that his mother was in London at the time; the other was that he was taking an important part in some amateur theatricals due for production in a few days. He particularly mentioned the latter to me immediately after the accident. S.F.B.

Notes by Miss France.

His mother.

Yes, in London.

Note by N. W.

I know that his mother was in London when the accident occurred.

Note by Stephen Burman.

The first remark Tony made

going somewhere where there would have been water. He hadn't *been* there; but a little before he passed he had been planning about going somewhere where there was water. Not a river, I think it looks too wide, more like the sea.

Almost immediately afterwards, and after giving a letter **B** connected with the communicator, Feda says:

Had he to go up and down ladders? I had better be careful. I get a feeling of him sometimes going up something awfully steep, not ordinary stairs, but almost straight up. It was something he had been rather in the habit of doing when he was here.

This made no sense to me at the time. It was later annotated as "a reference to rock-climbing of which he was very fond," and it may therefore have been suggested by the thought of the proposed visit to Ogwen. As I have already stated, Miss Francee had mentioned to me his keenness on "Alpine Climbing"; but, that conveyed *snow* to me!

Feda presently mentions "an older man, passed over" who was "waiting for him and over-joyed to see him, although he hadn't expected him so soon." This might be Tony's maternal grandfather who had apparently been described by Mrs. Garrett both to the Burmans and to me. Immediately, though not actually stating that it belongs to this "older man," Feda makes attempts at a name

Roy, Rory—not very long, Ruddy. I haven't got it right.

This, I find, was pretty correct, a family name being Roy: Tony's great-grandfather was William Roy, his grandmother Marion Roy, and his aunt is called Mabel Agnes Roy France; all these connexions *are* on the mother's side.

Then begin references to someone young,

A he . . . that he's left. He's upsetted when he thinks of having left the young man—that upset him as much as anything . . . All he's saying is in a way for The Young One—to help him to grapple with things. Because, in some ways, it's been the end of the world for him.

I guessed that these references must be to his brother, Stephen Burman, although Tony was the younger of the two. I knew by this time that the death had been a great blow to him. The family annotate that “there was only 18 months between them, they were devoted.”

So far, my mind might again be the source of the remarks; but in this connexion—and I think it is worth remarking upon the appositeness of the *connexions in which* the evidence quite outside my knowledge occurs all through this sitting—the record continues:

He's got something of mine that he very earefully fastens, and buekles and unbuekles, something with a strap. He threads it through a buekle-thing, and that he likes having because it belonged to this spirit.

Note by Stephen Burman.

A leather motoring coat, with straps and buekle, which was presented to me on my 21st birthday by Tony. It is the only thing I have which unfailingly reminds me of him. It was a most expensive present whieh at the time he could ill afford.

Mrs. Nea, this isn't a boy I am talking to, a man, not a ehild. This is a person what's growed.

Don't forget about the strap because that's rather important.

I think he wants—

Feda—Oh dear!

This young one has been terribly upset, nor ordinarily upset, but he's been almost afraid for him, afraid that it

Note by Stephen Burman.

These remarks are more or less true.

might have an effect on his mind. He's sensible and clever, and all that, but he says—It's been *such* a blow. And, he says—You know I *know* him, and I *know* how he feels things. He doesn't always let other people know, But *I* know how he feels things.

Contemporary Note by N. W.

[The next bit went straight on, but it sounded to me as if Tony himself was feeling he couldn't stand saying any more on this topic.]

It is strange that what followed were references to a Bank Book and a little special fund therein, which appear to have been entirely wrong. The communicator indicates that there has been discussion as to doing something with it. The annotators say that Tony's bank book "was surrendered for Probate purposes, and was not the subject of any discussion." I understand that the only thing like the bank book in colour and size, would be his passport, which should have been returned to the Foreign Office for cancellation, but which the family still have. It would be interesting if one could trace the source of this error: possibly—I only suggest this—Tony saw the passport and heard some discussion about it, and mistook it for the similar bank book—which made *his* mind turn to the idea of the "fund." I have experience in other cases of "their" seeing and hearing things in a rather distorted or vague manner, and consequently drawing wrong conclusions.

In submitting all this material to the Burmans, so that they might check my statements and criticise my points, I learn that :

Yes, there was a good deal of discussion as to whether it (passport) should be surrendered to the Foreign Office; but nothing was done.

Rather a striking corroboration of my surmise, for, in annotating about the Bank Book, the Burmans had said nothing about this *discussion*.

Having learnt this, I enquired about the "little special fund," and discovered that, at the time of his death, he *had* a deposit account at his bank of £13 odd—money he was saving up for another climbing holiday.¹

I quote what follows the Bank Book incident verbatim with the annotations.

What is it ? Wheels ? Wheels.
I don't know if he would be interested in wheels, but he's trying to build up something, and I saw wheels. He was pretending to, like run them along.

Notes by Stephen Burman.

He was engaged in the motor cycle trade, and was intimately connected with machinery.

I think it was something like that he was trying to show me earlier in the sitting. I get machinery all mixed up with it.

Mrs. Nea, did the wheels all get mixed up ? I get a feeling that something had happened to the wheels.

He says—It wasn't the *wheels* that mattered, it was the rest of it. He got so impatient ! He says—Perhaps something did happen to the wheels, but that doesn't matter. But I feel he wanted me to notice

¹ On reading proofs of this case and talking over points with her husband, Mrs. Burman wrote to N. W., June 16, 1929 :—

"My husband now tells me he took Tony's Bank Book down to the Works, and used it to jot down particulars as to what should be done with the money due as Tony's life-insurance . . . There was no real 'discussion' except that Harvey and Stephen talked over how the money should be divided and invested."

the wheels. The way that they is setted, it looks like the bottom of a car; but I got to be careful, in case it's the thing you calls a bicyele! He pushed them along, and then he suddenly stopped them, and mixed them up. I think it's something that happened, something he was interested in and concerned in. I think he must have some reason in showing me this.

I see a very beautiful smooth road, also. Oh, a beautiful road! All smooth and clear. And I feel he is moving something along on this beautiful smooth clear road, and going rather *quick* on it! As if whizzing a bit. Not just the way Gladys (Mrs. Leonard) takes them round; but going SHSHSH—flying round more than walking round, Mrs. Nea!

Was the road a little bit—I told you it was smooth—but was it a tiny bit humpy? It's beautifully smooth, but not quite flat, on the slope, like that. (Feda indicated a slant downwards away from herself; I could not of course know *how* the slant was supposed to be in relation to the road, and didn't like to stop her to ask. N. W.)

He felt as if he was '*joying* this! He felt so *pleased* and so excited about it.

Notes by Stephen Burman.

He was on a motor cycle at the time, and when he hit the ear, the two wheels of the motor cycle were jammed between the two wheels *and under* the front of the ear.

The accident.

This all refers to the road on which the accident happened, Pakenham Road. This is on a slope and looks very smooth. Actually it is full of small transverse ridges, very noticeeable at speed.

We had to go along this road every day on our way to work. Usually unfrequented, it offers great opportunities for speed, the feeling when descending the one hill and commencing the ascent of the next being particularly exhilarating.

At the side of it there was something like ledges, at the side of the road. It can't be a cliff, I think! But something sticking up on one side.

Not a cliff! Nothing of the kind at all, he says.

I think you call it sometimes like a ridge.

This wasn't something he had done just once, I think he was in the habit of doing it, *loved* doing it, felt he was *living* doing it.

Did you know he was photographed doing it?

Now B. comes up again, that word he's been showing me. Not a short word, several letters. Not like "boy," longer than that. I can't see any more of the B. at the moment.

Mrs. Nea, I feel he had done this in this car, specially enjoyed doing it, *at two quite different places. Not near to each other, rather a distance apart.*

He must have been doing this only a little while before he passed, because it's so fresh in his memory, and he thought of that when he first woke. He was wondering where he was, and pictures of that began to come up.

Notes by Stephen Burman.

Hedges.

As I pointed out before, we knew this road intimately.

We have an excellent photograph, taken from a car, of Tony riding at speed on his motor cycle. [This photograph is filed with the other records of the case.]

Probably Burman, or Bike? Tony used the term.

In a car, yes. See below.

Cannot trace this.

Is he referring to the T.T. Races at the Isle of Man where he went May-June 1926 as a representative of the Firm. M. F.

Later Note by Stephen Burman.

This now occurs to me: About 6 months previously Tony and I had gone on a climbing expedition to Capel Curig in a Morgan runabout¹ recently bought by me. It was a most enjoyable run.

¹ A car, not a cycle. N.W.

The ear was a new acquisition, and the fact that we had considerable trouble with the car on the way back would have remained in his memory. Later we went in *the* same car to climb in Derbyshire, and had trouble again on the return journey.

He won't say he passed over like that (snapping her fingers together), he didn't do that, and it's *because* he didn't do that, that he doesn't want them to think that he was suffering. It wasn't what he calls 'stantaneous'; he says, he *didn't* suffer so; he would have done if he had lived a little bit longer. But I became unconscious, and it saved me from feeling.

I think there's something there (head to shoulder again) that he would have felt. He felt a little cold feeling up his back. The doctor would know he would feel that. Not a pain, but a numb, cold, feeling going up his back. He would feel this numbing more than pain.

After this Feda made an attempt to give the name Burman, and got as far as Borrowman (I spell phonetically).

A reference then seems to be made to the famous watch which had been mentioned via Mrs. Garrett (p. 14) and to its telling the time correctly!

Notes by Stephen Burman.

I think this is all an effort to save his family pain. There is not the slightest doubt that he was in great pain until he was given morphia, which was not until an hour after the accident.

These would have been his sensations exactly.

Is the young one going to have a watch of his? Because, he says—He'll be pleased with the watch. The works is all right, to the watch, it tells the time all right; but why does he want to alter the opening and shutting part to make it perfect? For the Young One, that should be seen to. Tell him that, he will be interested.

And, oh, the stamps. He had a good many.

He says he had them in two places. He had some that he put in a book where they might ordinarily be kept, but he had got a few more he had forgotten about, and he put them in an envelope, he thinks. He's sure he did. So he expects they will be found.

Note by N. W.

His *mother* had the 2/9 watch that was never right, about which there was a family joke (see p. 14).

In the Garrett sitting it seems to be realised as a joke, but, though it may have been meant as a joke here, there is no indication of it. I of course knew about the watch by this time, Mrs. Burman having told me about it when discussing my sitting with Mrs. Garrett. I understand that the sentence about the opening and shutting part appears to have no meaning.

Note by N. W.

Tony Burman was not interested in stamps, the annotators say, though his brother ("the young one") had a large collection, with which Tony sometimes helped him.

I, on first seeing the annotation, counted this as one of the distinct errors to be noted. But on looking at the record again, there seems no reason why the "he" of the stamp paragraph should not be the same as the "he" of the previous sentence.¹ In fact that is perhaps the more natural reading and only escaped us because in my record

¹The interpretation of pronouns in passages in which the control, e.g. Feda, speaks for the communicator, sometimes quoting him and sometimes describing what he says, is often rather doubtful.

I had interpolated between the two paragraphs a note concerning my knowledge of the 2/9 watch.

On realising this I made more enquiries, and learnt that when interesting stamps came to the Works, Tony, or his brother, put them in an envelope and brought them home. Also that odd stamps were kept in an envelope at the back of the stamp book. By this time however, 9 months later, there were none in the envelope.

After this there are general remarks about psychic power and about Tony's attempts to communicate. He emphasises the fact that his small amount of practical experience while still on earth had been a great help to him.

There are then references to the family, and apparently, in that connexion, again to Mrs. Burman's father. All this was more general and less striking than the passages I have selected for special comment. Presently, about the middle of the sitting, occurs a reference to another accident. Fedá says :

A big H. comes up awfully close.

N. W. Yes? [Thinking of a Hilda I had heard of before. See p. 12 N. W.]

That comes up very near him.

(Fedá—Try and say it again.)

Qualms, qualms. Nothing had happened the time before. Nothing had happened the time before. I had been in a very dangerous position a little while before I passed over, which he had got over awfully easily. And it might have been an awful thing! He got out of that awfully easily. Someone else didn't get out of it so lightly as I did, but mine was really a miraele. It hurt someone else

to a certain extent, but it was really wonderful that it wasn't more serious.

[Note. Greek to me. N. W.]

Now, as I learnt only after the sittings referred to in this paper were over, there was in a sealed envelope¹ prepared by Tony's family in view of this sitting, a request that Tony would refer to a poisoned hand from which he had suffered about a year before his death. It was a very serious matter and might have cost him his hand and arm, and its occurrence was due to his own carelessness in not wearing rubber gloves as warned to do by his father. The passage just quoted, and another considerably later in the sitting referring to gloves,² naturally suggested to the Burman family that Tony was trying to fulfil the request in the sealed envelope, in spite of his not actually mentioning the nature of the accident, and in spite of evident confusion in the pronouns. But one thing seemed to them a definite error, for the poisoning of Tony's hand had hurt no one else. No one else was involved. It occurred to me, however, on re-reading the record after annotation by the family, that possibly the thought of his own poisoned hand recalled to Tony's mind some case known to him in which the consequences of similar poisoning were much more serious than in his own, and that a rather muddled reference to this other case accounted for the apparent error. I was therefore greatly interested when on reading this report for the purposes of checking, the Burmans told me that Tony *did* know of a similar case which had occurred at his Works a year or two previously, in which a foreman *lost his finger*. This had not previously occurred to them as the explanation of Tony's remarks when they were holding him and his accident only in their minds in

¹ In replying (on Jan. 2nd 1928) to an enquiry from Miss France as to whether I would take the sealed envelope with me to my sitting with Mrs. Leonard, I had said no, and that I thought she could not "do more for Tony than tell him you want him to meet me at Mrs. Leonard's some time soonish." The envelope is now filed, unopened, with the Full Report at the S.P.R. at the end of the Report of the Leonard sitting.

² It was a shame I didn't wear the gloves, wasn't it? Ought to have made more use of them.

annotating. So that my surmise, based purely on what seemed possible from a study of the record, has turned out to be correct. I leave it thus, as I feel it may be more useful to see that the Burmans had not the poisoned hand and *all* its connexions as clearly in mind as the communiicator himself; while I knew nothing.

I may here digress in order to mention what seem to be cross-correspondences between Miss Francee's automatic script and the Leonard sitting we are dealing with. In Miss Francee's script of Dec. 26th 1927, in which Tony purported to say what he would tell me at my Leonard sitting, occur the words "I will speak of the day when I had my poisoned hand," a statement again referred to in the automatic script of Jan. 1st 1928, with the addition that he would try to "spring it" on me early in the sitting. The above passages from Mrs. Leonard's sitting quoted as apparently referring to the poisoned hand did *not* occur early in the sitting. But, quite early in the sitting (see above p. 22) and in connexion with his fatal accident, Tony had referred to an awkward position of his hand. This made Miss Francee think not only of the position of the hand in hospital, but of the poisoned hand which had to be held in an awkward position, upright and with thumb stretching out owing to bandages. It is of course possible that the two were associated in Tony's mind and that the desire to speak early in the sitting of the poisoned hand, led to the reference to the position of the hand in hospital.

Towards the very end of my sitting with Mrs. Leonard, I got my first opportunity of asking a question without interrupting, and reminded Tony that I had heard his people had put something in a sealed envelope to which they wished him to refer. The reply was—"I have made an allusion to it." I asked "Which is the allusion?" and Tony said "Rather early in the sitting . . . I can't get it now."

In Miss Francee's automatic script of Dec. 26th 1927, is also stated "I will tell her [N. W.] about my ice"—meaning his hobby of climbing (cf p. 8 above.)

None of this automatic script was seen by me till May 1928, so that cross-correspondence between it and my Leonard sitting of January 7th 1928 were not due to my conscious knowledge.

Returning to my account of my sitting with Mrs. Leonard on January 7th 1928, I will select only the most outstanding points in the rest of the sitting. The matter I omit at this stage was not irrelevant or incorrect, simply less striking than what I quote, which is strictly evidential.

The first quotation relates to the Saturday afternoon before Tony Burman's accident :

Saturday afternoons, happy.

Happy Saturdays.

N. W. I hope this is one too, then.

It is. But it *was* when I was here. But a pity I took myself off sometimes. But happy. Happy.

Row, Roby, Ro (oh-sound). There's a name beginning with R, to do with the Saturdays.

Note by Stephen Burman.

He used to play *Rugby* football every Saturday.¹

(Feda—What were you doing? Were you putting up a post or a pole at home? Not long before you passed over?)

This is also connected with a Saturday afternoon. He's pretending he's got a long pole in his hand, and he's trying to make it stick in the ground, and it's wobbling. And he was getting cross with it! And hammering it into the ground.

Notes by Miss France.

This refers to building a bonfire and scarecrow on a Saturday just about November 5th (the Saturday previous to his accident.)

Quite true!

¹ Notice how Feda mis-hears apparently. I have several interesting instances of this indication of *process* in other cases. N. W.

Immediately after this came the following :

And please will you ask them if they have got something of his which is a figure, a figure like a little statue. Of course statue's the right word in one sense and wrong in another. If you say metal statue—or figure is better, he says.

The metal figure is leaning forward.

And is there a spike on it ?

Something rather long and pointed sticking out of it.

Then he says—Don't make too much of the pointed part, it's not like a needle.

Tapering a little, tapering.

This is something that he would have thought a lot of, given a lot of thought to, and been much mixed up with and connected with when he was here.

Note by Miss France.

An enlargement of Tony was made from a snapshot group.

This enlargement was cut out and glued to a piece of *metal cut* to the same shape and about $\frac{3}{16}$ " thick. The height of the figure is approximately 9".

Tony is *stepping forward* in the photo ; but the actual figure stands upright.

The back of this "metal figure" was painted black, and a "tapering" support of metal of the same thickness was soldered to the back to enable the figure to stand on a mantelpiece.

See illustrations.

Note by N. W. April 1928.

I learn that the original "snap" from which the enlargement was made, was taken at a race-meeting which the two brothers and the sister attended together and *enjoyed very much*. The figure therefore connects with "something he gave a lot of thought to and was much mixed up in when here."

But, Tony knew nothing of this method of dealing with photographs, and the figure was only made *after* his death.

PHOTOGRAPH OF
THE "FIGURE LIKE A STATUE"
"METAL FIGURE"

Back view



Front view



The "spike on it. Something rather long and pointed sticking out of it . . . it's not like a needle. Tapering a little, tapering."

Note.

This "spike" is the prop which enables the "metal figure" to stand on a mantelpiece or elsewhere like a photograph. It could not stand on its own base, having no breadth. N.W.

Tony is actually stepping *forward*, in the group from which this photograph was enlarged before being cut out and mounted on metal as a figure by itself. The metal figure stands straight, but one has the impression with the original that Tony is coming towards one.

It is not, however, a stereoscopic photograph. This is merely the effect of his "stance" at the time. See print of the group photo.



A copy of an enlargement of the photograph which shows the forward movement better than the figure; and also the face better. Tony is the prominent figure, altogether, in the group; his prominence in a photo is mentioned in a sitting the family took.

The next item I shall mention is not so vivid as the little "metal figure," but is worth quoting, partly because it is a small point unknown, until after the sitting, to the only two members of the Burman family whom I had met. It introduces also the idea of an argument (about wearing a hat), and immediately precedes the reference to wearing "gloves" already quoted (page 33), about which there had also been an argument, with the *same person*.

There had been an awful argument, not long before [his death] about what he was to wear on his head.

Did he want a flat hat?

Something that would stick on.

A lot of argument about that, which he thinks will be remembered.

It was a shame I didn't wear the gloves, wasn't it? Ought to have made more use of them—someone was saying so a little while ago. They were only sweet about it, not blaming me.

An attempt at his name follows this:

Burry, Burry, Birry, Burry, [See p. 30 for an earlier attempt. N.W.]
Bur, Bur. a name that's not
Bro' but is *Bur* Burnam.

Some less interesting material followed; and then Feda says:

He's trying to tell me how pleased he is about you coming and doing this for him.¹ . . . He says [the D.W. Group] has helped him. They helped him early, quickly, not just for to-day, but when he first passed over, they

¹ He had told his aunt, in script, actually *on* the 7th January (time not noted and not remembered), that he "would thank" me; and on another occasion, he had said that he had asked my Group to help him.

collected him. He doesn't mean that they knew he was passing, it was afterwards . . . And he's got a link with Bunny he says. Not with the others . . . It's not a link with Bunny exactly, but with people that Bunny is interested in on the earth . . . a link composed of people, and you, and Bunny.

This I knew of, and had mentioned in my recorded private request to the D.W. Group to help him, November 1926 (see above p. 4). All this about the Group was known to me, but it came in very naturally at the end of the sitting and with no prompting from me.

As I have not quoted the whole report of this sitting, I had better before leaving it give here a list, based on the full report, of the points which were either (a) apparently definitely wrong, or (b) have not been traced.

Wrong Points.

- | | |
|------------------------------------|-------------------------------------|
| 1. "Press Cuttings in his pocket." | None. |
| 2. "Good at running and jumping." | Never any good at either. He boxed. |

Not Traced.

- | | |
|---|---|
| 1. "There was somewhere else where he did it [motor-cycling or motoring] Up and Down—Oh, Up and Down. And there's again photographs of that too." | There are apparently no photographs of this. His brother does not pass it. The only information I got was that Tony attended the T.T. Races, to see to the Firm's gear boxes, in use there. The track is, I understand, hilly. But Tony was not there to <i>race</i> , though he had a machine with him, and rode. This follows soon after references to his accident (p. 28) and to his being "photographed doing it." I suspect that Tony |
|---|---|

never rode slowly! *Vide* the argument about the hat. But we cannot get at corroborations of this bit. N. W.

2. "Name beginning with E."

Not traced.

3. The figure 7, in connexion with what seems to be otherwise correct of his christening mug, except that it was not large, as Feda said.

Not traced.

4. "Help someone through an illness."

Only vaguely traced.

In a sitting during which Feda was talking hard for two hours, practically altogether about a communicator with whom I was unfamiliar, these are the only real flaws; and there was, as can be seen from the portions quoted, much that was first-class from an evidential point of view. Taking everything into consideration—relationships, etc., etc.—I think it the best sitting I had ever had up till that date.

Before seeing the annotations on the January Leonard sitting, or meeting any of the Burman family again, I tried once more, on the 3rd March 1928, through Mrs. Leonard. But, as regards evidence from Tony Burman, this sitting was practically a failure. In places Tony apparently broke through; and there seems to have been a rather good reference to his grandfather *and* another older man, whom, from the apposite remarks about the kind of regard he had for Tony, and the period at which he had known him, the Burmans recognised as the Uncle Edwin already referred to, both at their own sittings and to me at Mrs. Garrett's. But there was nothing else of real interest.

Possible reasons for this failure may be that

(a) *I was very tired*, and I believe that the sitter has something to contribute at a sitting.¹

(b) That perhaps some unrecognised communicator mono-

¹ Curiously, on the very day of the sitting (I do not know the hour, nor can I now learn it), I find that Tony purported to write through

polished most of the sitting. My knowledge was too slight for me to be able to tell, at the time, whether Tony was communicating or not. It was only afterwards that I learnt that his family felt that someone else had got in, and that they could only recognise bits here and there as clear references to Tony.

(c) My third suggestion is that Tony himself may have been less eager about the second experiment after the very successful first one.

I will conclude my account of evidence for communication from Tony Burman with an extract from a sitting of Miss France with Mrs. Dowden which explains itself.
Date. 10th March 1928. Saturday.

The appointment was made by Miss France herself, by letter, in her own name.

Johannes (Mrs. Dowden's control), after explaining that Tony wished to send messages to "Dad and Stephen" reported by automatic writing, that Tony said,

I want specially to send Steph a message.

I was watching him the other day.

He was thinking of me the other day.

He took up that photograph, you know the one I mean.

[There are two, one of them the one which also made the metal figure: S.B. took that one, I learn. N. W.] and I touched him on the head. He knew it was me, ask him.

Miss France. Which day?

Monday I think, but I can't be sure.

On Miss France's return to Birmingham next day, Sunday 11th March, she asked Stephen Burman if he had "done anything with Tony's photo recently," specially *omitting* to mention any day—Stephen Burman corroborates this omission. He replied that, on the previous Monday evening, 5th March, he had picked up "the one that always stands on the mantelpiece in the dining-room,"

Miss France, *à propos* of my sitting, the date of which might have been any time in February or March, as far as Miss France knew—:

"I shall be there when she wants me . . . but I hope she will go *when she is quite fresh in her mind* because I can help her much more easily."

and carried it into the next house to show the inmates, there having been a discussion about a psychic photograph. He did not say he had felt Tony's presence. Miss France states that as far as she can remember, she had not seen her nephew to speak to all that week; and is quite sure she knew nothing normally of the episode in connexion with the photograph.

A FEW NOTES OF PERHAPS GENERAL INTEREST, MADE AS A RESULT OF MY STUDY OF THE WHOLE MATERIAL, AND WHILST THAT IS STILL FRESH IN MY MIND.

It is of course unwise to draw many inferences from a single case. But if these notes are regarded as merely suggestive, they may be useful when more material of a similar kind is available for comparison; and they are certainly best written, and possibly read, while the case is fresh. Moreover, this case does not stand alone, even in my experience, for we have the White Case, recorded in "The Bridge," with which to compare it. And I will make that the starting point of these remarks, although I have no intention of making a detailed comparison.

In the present effort to get evidence unknown to the sitter, I should like to point to the fact that I was not dealing with a pair of lovers, as I was in the case of Mr. and Mrs. White. One might in that case point to the strongly emotional link; and I suppose it would be one's duty to do so. In the case of Tony Burman, as far as I can judge, having now met the family, there is very little trace of the kind of emotional sentiment which was prominent between the Whites. Mrs. White was certainly exceptional, and had, I felt, a temperament which must have played a considerable part in leading to success. In the case of Tony Burman there is *none* of that kind of temperament, as far as I can tell. Family love there is in abundance; but everyday life holds its own; there is full interest in and occupation with the affairs of this world. We are not dealing, on this side, with a solitary figure bereft of all that made life here worth while. So that that aspect, which to my mind was a point perhaps

to weigh against the evidence in the White Case, does not enter into this case.

In regard to the two ostensible communicators, there are two very obvious differences. Gwyther White was about twenty years older than Tony Burman; and, he had not the same happy-go-lucky type of mind, which Tony seems to show.

Then there is the fact that I myself was full of other work, and simply took this little experiment "in my stride," without devoting much thought or attention to it beforehand. In consequence, I have felt pleased, and rather surprised, at the success; for I think that the Leonard Sitting of 7th January 1928 is quite equal, from the point of view of pure evidence, to anything in "The Bridge."

Probably the confidence I have gained as a result of experience of what was possible has helped. And perhaps that balanced the fact that my feelings of sympathy were not being drawn upon to anything like the same extent.

At any rate, we have had in this case, as far as we know them, different conditions and relations all round—in regard to the bereaved, in regard to the communicator, and in regard to the sitter. Most certainly a difference of type and degree of bereavement—and it was on the question of the degree that I felt anxious in respect of the White evidence.

After studying the full record of my sitting with Mrs. Garrett on Nov. 8th 1927, with annotations, a point seems to me to emerge which is perhaps worth noting, and which might not be evident, unless one were very familiar with all the details. I think that it is rather likely that the various communicators may have been carrying on simultaneous conversations, independently, and, possibly, unknown to each other; the medium tapping now one communicator now another, without realising the change. A good deal of confusion and error would naturally result. Moreover, in the course of my work in collecting the White records, I had also noticed that it seemed possible that something of this kind had occurred on various occasions. So that

I was interested to see somewhat the same effect at this sitting with Mrs. Garrett.

Since meeting the Burman family, I have one or two points to submit in regard to the communicator Tony. It appears that he was an amateur comedian of very marked ability. During one sitting he seems to refer to his exploits, and outsiders had commented very favourably on his gifts in that direction. Now, in view of the pictorial method in use at sittings, at times obviously, perhaps often, when the sitter does not realise it—has this gift of Tony Burman's been an element in the success achieved. In the course of the White Case, Feda remarked that Gwyther White made a good communicator because he was very clearheaded, knew what he meant to say, and was not to be turned aside by doubts on the part of sitter or control. Here, we perhaps have another natural faculty influencing results, although not remarked upon by the control. In each case that fact, if it be one, should be an argument on the side of survival.

Another feature which may have contributed to Tony Burman's success, is his gift for and practical experience in psychic work while still alive here. This has been already referred to.

Again, a characteristic which Tony's mother mentioned to me, and which might have an adverse effect, was that Tony was not a plodder: he would, I learnt, start things keenly, but it was often his brother who finished them off and did all the detail work. Tony had not patience in that way, and was apt, when alive, to lose interest after the first successful experiment. Whether this has a bearing on the failure at the second sitting with Mrs. Leonard, I cannot say, since there were other contributory factors. But I mention the characteristic as perhaps of interest in that connexion.

So far as I have been able to learn, the above are the characteristics most likely to have had a bearing on the results.

In reference to the beginning of the Leonard Sitting of January 7th 1928 (pp. 22, 30), the supposed Tony Burman makes a special point, which he persists in through all the

channels used—namely that he was “quickly unconscious” and suffered little. This, as already shown, is not, from the point of view of the brother who was with him, true. But the constant repetition on Tony’s part (if we assume that his personality is at the back of the communications) has its interesting aspect, although said to be inexact, and thus rather irritating to the brother, on whose mind the impression of the suffering and the length of time before it could be relieved, are vivid. I feel that these remarks may have originated in one or two ways perhaps worth indicating, for the sake of other people troubled by the same kind of thing.

(a) General mediumistic stock in trade to comfort relatives.

(b) The communicator’s desire to relieve the agony in his brother’s mind about the period of suffering through which he passed while the two waited, longingly, for the ambulance.

(c) The fact that to Tony, *now*, the period may well seem relatively short. We all know the experience of forgetting pain as other experiences crowd out the memory.

Personally I do not feel that (a) explains Tony’s persistent remarks. I fancy that the tendency to improve aspects is rather on the side of the communicators than on Feda’s—as far as my experience of Mrs. Leonard’s mediumship goes. But I do feel that it is not an argument against the identity of Tony, rather one in its favour, that either for reason (b) or (c)—possibly partly for both—he should reiterate this same statement so continually. I knew nothing about it either way, at the time, although I knew that he had died fairly soon after the accident. And if the source of most of the material in this record be the mind of some member of the Burman family, then the constant insistence that Tony did not suffer must surely have a different one.

In connexion with my referenees to the confusion between the Bank Book and the Passport (p. 26), a few general remarks, based on experience of apparent mistakes the sources of which I happen to have been able to trace, may be suggestive. I fancy that often we seem to “them”

quite as hazy and far away as "they" do to those of us who have the faculty of feeling and seeing "them" just a little. Granting the possibility of survival and communication "they" have the advantage over us of having had *our* experiences, and can therefore probably interpret better what they see and hear than we can; also "they" appear to have more easy access to us than we to "them". But, on neither side is the contact complete, or constant, or always to the same degree. A friend of mine once suggested a simile—viz., that it was, for us at any rate, rather like looking at a stone at the bottom of a pool; if one could reach it and handle it, it would be found to be real and there; but the shape, size, colour, surroundings, etc., would be found to have been considerably affected by the medium through which it was seen, and by the various elements acting on that medium.

To my mind, to allow for such errors, provided one does it cautiously and never dogmatizes on that line, is not to be uncritical but to be fair, and, as in the present instance, possibly to open the way for a sound explanation of some point. Tracing errors and their cause is complicated so often by the fact that one does not know what, in the mind of the medium, is impeding the communicator. I discovered that fact in working with my sister; for, in her case, I could often trace the reason why she made certain mistakes, or "heard" certain words as something rather different. In working with a strange medium, one cannot know those reasons. I feel sure that the more we can study that side of communication, the more we shall understand, and the less we shall be able to say bluntly "a bad shot, quite off the mark." And so it came that I could not believe that the reference to a bank book was a bit of communication without foundation and entirely erroneous, although it was at first counted by the Burmans, and in consequence by me, as a definite error. We had not, I felt, the key to the form in which it had been given, that was all. For one thing it had not "come through" in the groping manner in which, sometimes, material comes before the medium has got on to the correct "wave length," if one may call it

so. It had come quite definitely and crisply. There were one or two other points in the same category—*e.g.* the Stamps, the Poisoned Hand. And I reached the real explanation of apparent incorrectness or confusion, in each case, by trying to find out, from the context, and from trying to imagine myself as communicator, how, if the mistake *had* been made by the communicator, it had come about. I did this without in the least knowing whether it would clear matters up. But, as I have often found, it did. And I think that this could frequently be done if the investigator had patience, and, still more, if the annotaters were as kind and long-suffering over the resulting enquiries as I found the Burman family.

Jan. 17th 1929.

REPORT ON THE INVESTIGATION OF SOME SITTINGS WITH MRS. WARREN ELLIOTT

BY H. F. SALTMARSH.

(This paper was read in part at a Private Meeting of the Society on March 6th, 1929.)

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PREFACE.

I HAVE thought it advisable to give as a preface to this report an account of some control experiments undertaken for the purpose of determining whether the results described can reasonably be ascribed to chance alone.

These experiments have not yielded an unequivocal result.

While it is possible to argue that, taken on the average, the score for chance¹ is so much below that of the real, sittings, that it can be excluded as a possible explanation, on the other hand one may point to two or three instances where chance has scored little below real, and claim that the existence of these cases demonstrates that chance could account for the phenomena.

¹ The meaning here given to the words "chance" and "real" will be apparent when the method of experimentation is described below.

The reason for giving these results in a preface is that those who may adopt the view that chance alone is responsible for the coincidences observed, and that supernatural phenomena are absent, may be spared the labour of reading the report. It would be a just cause for complaint if any should be induced to expend a considerable amount of time and energy in considering the discussion of a series of sittings, only to find at the end that they contained nothing of positive value, in their opinion, for psychical research.

The experiments were as follows :

Copies of the records of certain selected sittings were sent to persons totally unconnected with the real sitters. These persons were asked to imagine that the sittings referred to themselves, and to annotate accordingly. Any coincidences which might occur would, therefore, be attributable to chance alone.

A system of scoring or marking was devised, and the chance sittings were scored in exactly the same way as the real. It was argued that if the score for the real sittings was considerably in excess of that for chance, when taken over a fair number of cases, it might be claimed that chance as a possible explanation was excluded.

The figure for the ratio by which real should exceed chance in order to afford complete proof was fixed at eight times.

A more detailed account of the methods adopted and some discussion on certain points arising will be found in the body of the report.

One or two of these points must be briefly mentioned here.

Nearly all those who have participated in these control experiments have had some experience in psychical research, and it has been suggested that this would tend to make them critical in annotating and thus reduce the chance score.

Also, it is said that the chance annotator would not feel the same interest in the sitting as the real sitter and would thus be liable to overlook coincidences.

Further, where the real sitters have been convinced of

survival and of the identity of the ostensible communicator, as was the case in a good many of the sittings selected for this experiment, the real annotations would tend to be more favourable than the chance.

The personal factor comes in very largely in the whole investigation, some annotators exhibiting the "will to believe" rather markedly, while others display an equally strong "will to disbelieve."

It was to cover these and similar factors that the ratio of superiority necessary to exclude chance was fixed as high as eight times.

The choice was to some extent arbitrary, and it must be left to the individual judgment of the reader whether it be reasonable or not.

The first experiment was as follows :

The sittings all related to one ostensible communicator. It was a case in which a young flying officer, who had been killed in action, purported to communicate, and the chance annotators were all persons who had suffered a similar loss, that is to say, persons who had lost a son or brother of about the same age and in somewhat similar circumstances. The idea was to try to find out how much of what was said of the ostensible communicator would be true of other men of similar age and circumstances.

The figures are as follows :

There were six sets of chance annotations, comprising fifty-three sittings in all.

<i>Total possible score.</i>	<i>Real score.</i>	<i>Chance score.</i>
5642	4107 = 72·8%	452 = 8%

The average score for real is thus nine times that for chance. Taken by itself this might seem sufficiently great to exclude chance, but there are two instances where the chance scores were more nearly comparable with the real than is shown by the average. For one the total possible was 850, real score 533 = 64%, chance score 141 = 17%, giving a ratio of superiority over chance of 3·8; in the other, total possible score 939, real score 757 = 80%, chance score 163 = 17%, giving a superiority of 4·6 times.

The second experiment consisted of a number of sittings selected at random from those which had given good results for real annotators.

There were fifteen sets, comprising eighty-five sittings in all.

<i>Total possible score.</i>	<i>Real score.</i>	<i>Chance score.</i>
5554	3226 = 58.1%	487 = 8.75%

The average real score is thus 6.6 times greater than the chance score. Taken by itself this might seem a superiority sufficiently large to give a very good case for the exclusion of chance, although it did not quite come up to the standard fixed. There are, however, three sets of five sittings each which show scores much higher than the average, and in one case approaching the real score. These are :

	<i>Total possible.</i>	<i>Real score.</i>	<i>Chance score.</i>
(1)	374	195 = 52%	86 = 23%
(2)	353	218 = 59%	101 = 28%
(3)	404	185 = 46%	136 = 33%

Out of these fifteen sittings two only gave no chance score at all, in one case the chance score was slightly in excess of the real, in five it was only slightly less.

Now it might be said that these facts prove that it is quite possible that chance alone may account for the apparent veridicality of the statements, as in actual fact it has done in a fair number of instances.

On the other hand it may be argued that while one knows antecedently that it is theoretically possible for chance to give some scores comparable with the real sittings, provided that a sufficient number of tests are made, the real basis of comparison should be the average score.

In a very large number of tests one would expect to find a certain number of high scores by chance, scores, in fact, which might be much greater than the real: the question is whether the number of high real scores found in this experiment is so much in excess of that which might be expected from chance that some other cause of this excess must be sought. Whether this be so or not must be left to the individual judgment of the reader.

REPORT.

INTRODUCTORY.

THANKS to the generosity of an anonymous donor, it was possible to arrange for a continued series of sittings with a trance medium under conditions which permitted the hope that some advance in our knowledge of the phenomena might be attained.

The Society was fortunate enough to persuade Mrs. Warren Elliott, the well-known medium, to agree to give three sittings per week over a period of one year under the conditions desired, and it is only fitting that this report should commence with an expression of appreciation of the manner in which Mrs. Elliott has performed her part in the experiment.

She has been most assiduous to co-operate in every way possible to her, and has cheerfully submitted to conditions which, particularly in one class of sitting, must have been extremely trying.

As to the nature and quality of her mediumistic gifts nothing need be said at this opening stage. The meaning and interpretation of the results and the deductions to be drawn therefrom will be discussed later.

The gratitude of the Society and all others interested in the work of Psychical Research is due, therefore, in the first place to the generous donor, who has made the experiment possible, and in the second place to Mrs. Warren Elliott, whose co-operation has enabled the Society to carry it out.

The primary object of the experiment was to attempt to throw some light on the psychological mechanism of trance mediumship.

At the present stage of Psychical Research there is available an immense amount of material, records of sittings, accounts of spontaneous phenomena, evidence of various kinds of supernormal knowledge, much of which is of unexceptionable quality: there is also no lack of hypotheses put forward to explain the facts, but so far our knowledge of the psychological mechanism involved is small. It seems fairly obvious that until this defect is

remedied the progress of Psychological Research, as a science, can hardly be other than slow and uncertain.

The accumulation of evidence is, of course, a matter of great importance; it is impossible to have too much, but an understanding of the psychological conditions is an indispensable pre-requisite to the fruitful employment of that evidence.

The sittings described in this paper are of two types: viz. the Sitter Present, known as S.P. sittings, and the Absent Sitter, called A.S. sittings.

S.P. Sittings.

In the first the sitter is present at the sitting with a shorthand writer supplied by the Society, who takes down all that is said, both by the medium and by the sitter. Every care is taken to preserve the anonymity of the sitter, and this, so far as can be ascertained, has been successful in nearly every case. There are only two instances in which Mrs. Elliott has normally known the identity of the sitter. 1st. A single sitting with Miss Newton, undertaken as a special experiment. 2nd. A series of sittings with the Rev. W. S. Irving, who, having previously had several private sittings with Mrs. Elliott, had become known to her by name, though originally introduced under a pseudonym.

The notetakers were, with one exception, also introduced under pseudonyms, though, of course, Mrs. Elliott was aware that they were connected with the Society. The one exception was Dr. Woolley who took notes in a few instances.

The various notetakers have performed their laborious task in a most praiseworthy fashion, the notes are extremely full and clear, care has been taken to record everything said, both by the medium and the sitter, and to note all relevant details.

As is customary with Mrs. Elliott, the sitter handed her a relic, that is to say some object associated with the deceased person concerning whom it was hoped to obtain communications.

In a few cases this practice was varied in order to try special experiments: these will be referred to later.

A.S. Sittings.

The second type of sitting, *i.e.* the Absent Sitter sittings, constitute a new departure in Psychological Research, and it is from these that the most valuable evidence is obtained.

For these sittings a number of persons were invited to send to the Society relics associated with a deceased relative or friend, and precautions were taken to prevent any normal knowledge from reaching anyone concerned in the sitting, either the medium, the notetaker or anyone else.

The relics were sent to an officer of the Society. They were then enclosed in separate envelopes, numbered, and the numbers entered in a book against the name of the sender. The various packages were then placed in a locked cupboard. On the morning of a sitting the notetaker, who went *alone* to the medium, was allowed to select at random a package from those in the cupboard.

In Miss Newton's own words, "The majority of the relics were sent to me open, and I did them up and sealed them, and handed them to Mr. Dingwall, who numbered them and entered the number in a book against the name of the sender, of which he was informed by me. . . . After the packets had been numbered they were put into the locked cupboard, and neither Mr. Dingwall nor I saw them again until they had been used; the notetaker bringing back and handing to me the used packet."

A second relic was taken in reserve, to be used if the first failed to give any result.

Thus the senders of the relics had knowledge of the identity of the person to whom they referred, but did not know when they were being used. The Officer of the Society who received them knew the name of the sender, and in some cases might have known or guessed the name of the person with whom the object was associated, but did not know when any particular relic was being used. The notetaker would know the number of the relic, but, not having had access to the book, would not know to whom the number referred.

The medium would, of course, possess no normal knowledge whatsoever.

In a good many cases the package containing the relic was not opened by the medium, and there is nothing in the sittings to indicate that this factor had any determining influence on the quality of the evidence given, that is to say, evidence of as good a quality has been given in cases when the package has been unopened as in those where it has been opened.

Mrs. Elliott's Control, Topsy.

In all cases of S.P. sittings, and in the majority of the A.S., the medium went into trance, and the communication was through her control, Topsy; in some, however, of the A.S. sittings the medium has remained in her normal state and has dictated the communications to the notetaker. There are one or two instances where the relic was left with the medium, who later on dictated her impressions to her husband and handed the record to the notetaker at the next sitting.

No difference in the quality of the communications given by these different methods can be detected.

In the trance sittings the characteristic child-like diction of Topsy is always employed. It must be admitted that this is not altogether convincing; a child who would talk as Topsy does, and make the grammatical mistakes that she makes, would hardly know the meaning of many of the words she employs, *e.g.* "trivialities," "inspiration," "over-enthusiastic," "mental distance between them."

She is, on the whole, consistent in her manner of speaking, but some of the errors of pronunciation and grammar appear hardly natural. This remark is not intended as a criticism, nor could it be reasonably taken as such, for Topsy, whatever she may be, cannot be considered as psychologically comparable to a normal child. If, as assumed by spiritistic hypothesis, she is an independent entity who acts simply as a mouthpiece for others, then it may be supposed that she would occasionally use words borrowed from the more mature

intelligences whose messages she is transmitting. On the alternative hypothesis, *i.e.* that Topsy is a personification of some stratum of the subconscious mind of the medium, the difficulty does not lie in the employment of words not natural to a child, but in the assumption of a childish personality. This, however, appears to be a quite usual phenomenon with trance mediums, and various reasons can be suggested for it, none of which need be here discussed.

Whatever Topsy's defects in grammar and pronunciation may be, she certainly possesses the faculty of graphic description. She frequently sums up in a few words a situation which would take perhaps two or three sentences in more orthodox grammatical style to elucidate. For example, "Gives feeling doesn't feel ill and yet him ill for a long time," seems to describe accurately the condition of a man who had been confined to an invalid chair for some years owing to an accidental injury.

Her descriptions of character are particularly good.

So far as can be judged from the records of the sittings a large part of the material appears to come to both Topsy and Mrs. Elliott in her normal condition in the form of visual impressions, although there is a certain amount of what appears to be auditory. This point will come in for full discussion later.

Method of dealing with records.

The methods adopted for dealing with the material were as follows:

After the records had been annotated by the sitter or contributor of the relic, the various paragraphs or separate items were numbered, each record being kept in a separate big envelope on the front of which a complete list of the separate items and their numbers is written. By this system any reference can readily be found if its number is known.

On the backs of the envelopes are endorsed the various scores for the sitting, also any remarks or notes.

When the records were being examined for the purpose of doing this work it was noticed that certain topics tended

to repeat themselves with what seemed to be abnormal frequency. For example, a large number of ostensible communicators were said to have had an injury to the leg or foot; a ring with black and an animal standing up on it was also mentioned several times. These topics are called clichés.

A selection of about one hundred clichés was made, and the entire set of sittings was gone through and each mention entered in a card index.

In the same way all names and initials given were indexed.

Where a sitter or group of sitters has had more than one sitting the records were collected into a sub-case, and this sub-case was treated separately. A complete index of every statement or item of information given was made for each sub-case: by this means all correspondence between the sittings was noted.

A separate preliminary report on each sub-case was drawn up and kept for reference.

Some of the sub-cases were, however, of so poor a nature from various causes that this detailed work was not undertaken in connection with them.

The primary problem in all researches of this kind is to find some basis of comparison, some method of estimating the relative quality of the evidence. This is necessary for many purposes, *e.g.* to determine whether trance or normal sittings give better results, whether the envelope containing the relic remaining unopened by the medium affects the evidence, whether the nature of the relic has any effect on the communications, to compare the quality of the S.P. and the A.S. sittings, and, finally, to provide a basis of comparison with the result of pure chance. The methods adopted for this last and most important comparison have been briefly referred to in the preface, and will be explained in more detail later.

It must be emphasised that the scores arrived at are not to be considered as giving the absolute value of any sitting, they are for purposes of comparison only. This point is of the utmost importance, and must constantly be borne in mind in order to avoid misapprehension. In almost all cases the scores

allotted show a far higher figure than could be considered as representing the real evidential value. Thus, for example, there are several S.P. sittings with scores of over 90% ; were this taken to represent the absolute value we might reasonably suppose that they afforded almost conclusive proof of the identity of the ostensible communicator, etc., but this is not the case ; the high score may be due almost entirely to a set of commonplace or vague statements which would be probably true of most sitters.

The problem of scoring is one of very great difficulty, the statements made are generally vague and indefinite, it is as a rule impossible for the annotator to answer with a plain "Yes" or "No." The closest analogy seems to be the marking of examination papers on some literary subject where differences of opinion, individual idiosyncrasy, nuances of meaning, all complicate the matter. It is impossible to eliminate completely the personal factor in both the annotator and the scorer. The ideal method would be to have each record scored independently by several persons, who would be provided with a set of rules for their guidance ; a discussion between them of each disputed point would in all probability then result in some compromise, and the average of the final scores might be taken as a fairly reliable estimate.

This, however, would entail an enormous amount of labour, and it has not been found possible to undertake it.

The scoring in this experiment has been done by one person, *i.e.* the writer of this report, and he has attempted to do the best possible in the circumstances by revision at various intervals of time and by striving to preserve the same mental attitude in all cases. The sole criterion adopted in scoring an item as veridical is, "Can this be said to be veridical from the information given by the annotator?" As far as possible the critical faculty has been suspended altogether, the only exception being the classification of the statements under the classes of "vague," "definite," or "characteristic," and the small deductions made from the total score for "all vague." (These terms will be explained presently.)

This attitude was adopted in order to reduce to the minimum the factor of individual judgment on the part of the scorer; if he had attempted to assign to each item its evidential value from the point of view of some particular theory or hypothesis, the utility of the scores as a basis of comparison would be seriously vitiated, they would have been tinged with his personal predilections and idiosyncracies. The scorer has had the advantage of having had to annotate some sittings with articles contributed by himself, and this made the difference in mental attitude very apparent.

Method of Scoring.

The method of scoring adopted was as follows. The various statements were classified under the three headings:

1st. Vague or commonplace statements, that is to say, statements which might be true of a very large number of people; *e.g.* a communicator is described as fairly tall, blue or blue-grey eyes, and so on, lived in the country or near water, used to write a lot, sitter was buying boots, was going for a holiday, ordinary names, etc.

2nd. Definite statements, *i.e.* statements which though possibly true of a good number of people are not of so commonplace a nature as those of the first class; *e.g.* communicator described as "though very gentle, he was frightfully daring," "used to fly in the air," "sitter sleeps in a bed too short for her," "Sitter had a quarrel with his father," etc.

3rd. Characteristic statements, *i.e.* statements which are so characteristic that the probability of being true by chance is practically negligible. These are very scarce.

Number 1 or "V" statements are by far the most numerous; they count 1.

Number 2 or "D" statements occur as a rule about five or six times in a good sitting; they count 5.

Number 3 or "C" statements count 20.

The total possible and the actual scores are computed by addition and the percentage worked out. When the number of D or C statements is very small a deduction

is made from the actual score based on an estimate of the general character of the sitting; this deduction is rarely more than about 10%, except where the total possible score is very small. In the figures given it will be observed that there is a gross and a net score, the net score is after the deduction, if any, has been made.

Where a V statement is not only not true but definitely opposed to the truth, *e.g.* where a person is described as tall when, in fact, he was particularly short, a minus score is given; where a D statement is partially true, 2 is allowed in actual score; in the total possible score the full amount, 5, is counted.

As above remarked, the total possible and the actual scores are computed by addition. This is, of course, not strictly correct. An example will make this clear. If out of 100 statements, 99 V and 1 D, 49 of the V statements and the D statement are found to be veridical, this would give a score of 54 out of a possible 104, or 51.9%.

But if, out of these 50 veridical statements, 5 were D statements and 45 were V, this would give a score of 70 out of a possible 120, or 58.3%.

It is clear that the addition of four more definite statements add far more to the real value than is indicated by the increase of 6.4%.

To arrive at the correct *absolute* value by scoring would, assuming it to be possible, entail an enormous mass of calculation. It is only with such phenomena as telepathic experiments with cards, where the veridicality can be accurately gauged, that mathematically correct values can readily be obtained. However, a suggestion for a more accurate method will be made in an appendix. This unavoidable error is to some very small extent compensated for by the deductions made for "all vague" sittings.

However, as the scores are not intended to be a gauge of the absolute value, but are used for purposes of comparison only, the point may be passed over: the essential thing is that the same methods should be used in all cases. The most important of these comparisons is that

with pure chance, and an account of some control experiments designed to assist this comparison has been given in the preface.

A question arose in connection with the selection of the sittings to be used for the second of these control experiments. There were two alternatives open: 1st, to select sittings entirely at random; 2nd, to select those which had proved to contain a fair amount of veridical matter for the actual sitter.

Strictly speaking, the first of these alternatives is the correct procedure, but to make the test a fair one a very large number of records would have to be treated; it was not found possible to obtain a sufficient number of records and pseudo-annotators, so the second alternative was adopted. It is obvious that only A.S. sittings could be used in this experiment as the conditions of an S.P. sitting, viz. the presence of the sitter and the interchange of remarks between the sitter and the medium, could not be duplicated in a pseudo-sitting.

Scoring as applied to physical descriptions of communicators.

One of the most prominent features of all trance sittings is, of course, the physical descriptions of the ostensible communicators. In very few cases are these sufficiently definite to possess any considerable evidential value, their chief use is to provide means of *prima-facie* recognition; however, it seemed possible that, in spite of their general vagueness, they might present some evidence of identity if it could be shown that their general veridicality was much in excess of chance. To test this a further experiment was devised.

A rather more elaborate system of scoring was adopted. The various items of the descriptions, which in Topsy's case are more or less standardized, were classified under five headings, viz. General Congruity, Partial Congruity, Not Inconsistent, Inconsistent, and Not Mentioned. All the descriptions which were recognised by the annotator as probably applying to one ostensible communicator were collated and scored in this fashion and percentages worked out.

A number of records were selected at random by drawing the envelopes containing them from the pile, and the descriptions therein collated and scored in the same way. Where there was more than one description of ostensible communicators in any sitting, that which corresponded most nearly to those already collated was selected, thus favouring the chance score to some extent.

A comparison of the percentages thus obtained affords a gauge of the value of physical descriptions.

The chance score for physical descriptions thus obtained, as compared with the actual score, is considerably higher than in the other chance experiments with whole sittings. This is only what might have been expected. In the case of "whole" sittings the number of statements which would be veridical for any sitter is undoubtedly large, but the total possible number of such statements, both veridical and non-veridical, is practically unlimited: it includes everything which could possibly be said about human affairs.

On the other hand, the total possible in physical descriptions is comparatively small. There are at most only about eighteen or twenty items in a physical description, for many of which there are only three or four possibilities, *e.g.* a man can be described as tall, fairly tall, medium or short, which gives a three-to-one probability against being correct by pure chance. The maximum number for any item would not exceed fifteen or twenty possibilities. If the average be taken at ten, it would be a fair estimate. Thus the average probability of any item being correct by pure chance is one in ten, while for the other statements in the whole sittings the probability would be very small.

It must be borne in mind that these scores do not pretend to be any gauge of absolute value. This has been said before, but it cannot be too strongly insisted upon. The totals are arrived at by addition, whereas to estimate the probability for any set of statements the total should be a function of the product of the separate chances. Averages of probability can be employed solely as a rough method of comparison.

Although physical descriptions, taken by themselves, have little evidential value, except in those rare cases where some peculiar characteristic feature is mentioned, they may possess some importance when combined with other veridical statements.

For example, a description of a young man as tall, fairly broad, fair complexion, oval face, blue eyes, well-marked eyebrows, nose of medium length, fair hair and small moustache, is by itself, even if correct, of little value—there are some millions of boys whom the description would fit; but if it be said that this young man was the elder of two boys the field is narrowed somewhat; if it be further said that he was very fond of music it is further restricted, for out of the number of boys of this description who were the elder of two boys not all would be fond of music; if it goes on and further describes him as being as gentle as a girl yet wonderfully daring, etc., etc., and also mentions incidents in his past and details of present happenings among his family and friends, then the probability of all this arising from pure chance is comparatively small.

The figures giving the results of these various control experiments are given elsewhere. Some further remarks will be found on page 145 *et seq.*

Other modes of classification.

In view of the various hypotheses which have been put forward from time to time to account for the apparently supernormal knowledge in mediumistic communications, further kinds of statistics have been collated. In sittings which have been selected for the purpose the statements have been classified under the headings of Ante-mortem or A.M., these being statements which purport to refer to events or conditions prior to the death of the ostensible communicator; Post-mortem or P.M., *i.e.* statements referring to events or conditions subsequent to his death.

A further subdivision has been made, each class of statement has been divided into what are called physical or P. events or conditions and non-physical or N.P. events or conditions. The criterion used in this classification was

as follows: "Had an observer arrived suddenly on the scene, could he have obtained knowledge of this fact?" If he could have done so, it is classed as a physical event; if not, as a non-physical. The physical events or conditions are, therefore, mainly of a material nature, overt actions and obvious conditions; the non-physical are chiefly mental states, emotions, thoughts, feelings not patently expressed, relations between the actors other than physical or family relations, and so on.

From the indefinite and rather nebulous nature of the subject-matter these classifications are, of necessity, somewhat rough; it is frequently very difficult to decide to which class an item should belong. However, as the number dealt with is comparatively large, and the results intended solely as approximate estimates, the unavoidable errors are not such as to deprive the statistics of their usefulness. Mathematical accuracy is unobtainable and, as will appear from the subsequent discussion on the bearing of the figures on the question of theory, it is not indispensable.

Proper names, initials and physical descriptions are not included in these statistics.

There is one further classification. The non-physical statements have been classified under the heading of those relating to the ostensible communicator and those relating to the sitter.

The following is a glossary of the special terms and signs used.

Glossary of terms used.

Trance sitting. When the medium goes into trance and the communicated matter is given through Topsy.

Normal sitting. When the medium does not go into trance but speaks in her normal manner.

A.S. sitting or Absent Sitter sitting. A sitting where the notetaker and the medium are the only persons present. The sitter is represented by a relie.

S.P. Sitting or Sitter Present sitting. A sitting where the sitter attends in the usual way. It is customary with Mrs. Elliott for the sitter to hand her a relie.

Intrusion Sitting. An A.S. sitting where communications appear to come from a recognisable communicator unconnected with the relic or the contributor thereof or the notetaker.

Pseudo-intrusion Sitting. A sitting, either A.S. or S.P., where the communications purport to come from an ostensible communicator unconnected with the relic but connected with the notetaker. These are analogous to the ordinary S.P. sitting.

Deferred impression. A statement which appears to be derived from an impression received by the medium at an earlier sitting, but which has remained latent.

Reminiscence. A statement which appears to be derived from the memory of an impression received and communicated at an earlier sitting.

Total possible. The amount of the score for a sitting if every statement made had been veridical. Arrived at by addition of the various marks for the separate items.

Gross score. The amount of the score for a sitting prior to any deduction for vagueness. Arrived at by addition.

Net score. The final score for a sitting, *i.e.* the gross score less the amount, if any, deducted for vagueness, etc.

Vague statement. Sign V. A statement which is of a vague or commonplace nature, likely to be true of a large number of persons.

Definite statement. Sign D. A statement, that, while possibly true for a number of persons, is not so commonplace as to be as likely to be true as not.

Characteristic statements. Sign C. A statement of such a nature as to be unlikely to be true by pure chance.

Ante-mortem statement. Sign A.M. A statement referring to an event or condition prior to the death of the assumed communicator.

Post-mortem statement. Sign P.M. A statement referring to an event or condition subsequent to the death of the assumed communicator.

Physical statement. Sign P. A statement referring to a physical or material event or condition.

Non-physical statement. Sign N.P. A statement referring to a non-physical event or condition, usually a mental event or condition.

Sitter. Sign S. A person who actually attends a sitting. The sitter is also the annotator in almost all cases.

Contributor. A person who has contributed a relic for an A.S. sitting. The contributor annotates the record.

Communicator. Sign C. A deceased person who is recognised by the sitter or annotator, either from the physical description or otherwise, and assumed to be the ostensible communicator.

Pseudo-contributor or Pseudo-annotator. A person who has annotated a sitting with which he has no connection in order to take part in the control experiment undertaken to ascertain the possibility of correspondences by pure chance.

As an example of the use of signs a statement might be classed as S.P., A.M., N.P., C. This would mean a statement in a sitter-present sitting referring to a non-physical event or condition happening prior to the death of the ostensible communicator and relating to him.

Sittings with Mrs. Leonard	are marked	L.
„ „ Mrs. Dowdall	„	DI.
„ „ Mrs. Barnstaple	„	B.
„ „ Miss Bazett	„	Bt.

These sittings are introduced solely for the purpose of noting correspondences.

I.

EXTRACTS FROM RECORDS OF SITTINGS ILLUSTRATING THE TYPE OF EVIDENTIAL MATTER GIVEN.

For the purpose of rendering these extracts more intelligible, and presenting them in the form of a continuous narrative, a number of separate cases have been extracted, each case comprising all the sittings relating to one sitter or set of sitters.

The references are to the number of the sitting and its class.

1. *The A Case.*

The first case from which extracts will be quoted is one in which there are in all thirteen S.P. and seven A.S. sittings of the ordinary type; there are also six Intrusion

sittings, *i.e.* sittings with a relie contributed by some one connected with this group of sitters, in which the chief communicator of the group appears to intrude sufficiently to be recognised and to give veridical communications. This communicator is here called A. He was born in America on 2nd August, 1887, was taken to London when a child of fifteen months, and lived there until he was eleven years of age. He was then on the Continent in various countries for a couple of years. In 1902 the family returned to London for a year, after which they went back to America. They lived in a house which they had built on the slope of Mount Tamalpais overlooking the Bay of San Francisco. After the earthquake of 1908 the family returned to London, and A. never left England again until he went to France as Second-Lieutenant in the R.W. Surreys in November 1916.

He had been in the University O.T.C. whilst at Trinity College, Oxford.

He became a naturalised British subject in order that he might join the British Army.

He was attached to the Flying Corps, and trained at various centres, receiving his Observer's Wing in November 1916.

As above stated he went immediately to the French front, and had six months' continuous service there. In June 1917 he started his training as a pilot, and obtained his wings in April 1918. In June of that year he went out to Italy with the 34th Squadron R.A.F. On 23rd July he was out on a reconnaissance with four other aeroplanes. On the return journey his machine dropped behind the rest; it was thought that he probably did so to engage in a fight with an enemy aeroplane. He failed to return, and a note was subsequently received from the Austrians saying that his machine had been shot down and both occupants killed.

His body is buried in the British Cemetery at Tezze di Piave.

The principal sitters are his Mother and Father and a friend of theirs, Miss H.; his younger brother, J—, had one S.P. sitting.

Miss H.'s connection with the case requires explanation. She has been a close friend of A.'s mother for some years, but they were not acquainted until after his death.

Before the beginning of this experiment they had held a number of private sittings with the Ouija board among themselves at which A. and Miss H.'s father purported to communicate. Her connection with A., therefore, is entirely posthumous—they never met in the flesh.

Physical Descriptions of A.

The following is a comparison of the physical description of A. given by his Mother and as given in the sittings. The figures in brackets give the total number of sittings of both types in which the item is given.

<i>Actual.</i>	<i>Sittings.</i>
<i>Build.</i> Thin.	Thin (11).
Fairly broad.	Broad (8). Not very broad (4).
Square shoulders.	Square shoulders (5).
<i>Height.</i> 5 ft. 10 in. to 5 ft. 11 in.	Tall (11). Fairly tall (8).
<i>Face.</i> Nearly oval.	Oval or nearly oval (10).
Long.	Squarish (3). Fairly broad (3).
<i>Complexion.</i> Fair.	Longish (2).
Clear skin.	Fair (4).
<i>Eyebrows.</i> Well marked.	Clear or very good (6).
<i>Forehead.</i> Medium height.	Well marked (15). Not very well marked (2).
Broad.	Fairly high (13). High (6).
Sloping back a little.	Broad (9). Not very broad (1).
<i>Nose.</i> Fairly big.	Slopes back a little (2).
Thin.	Not very big (11). Ineorreet (2).
<i>Mouth.</i> Not very wide.	Thin (2). Small (2). Ineorreet (2).
Rather full lips.	Not very wide (4). Wide (2).
<i>Chin.</i> Rather square jaw.	Full lips (4).
Longish.	Square (4). Rather wide (1).
	Round (1).
	Long (4).

<i>Checks.</i> Rather high check bones.	High cheek bones (7).
Checks little sunken.	Little sunken (2).
<i>Hair.</i> Fair, brown.	Fair (13). Brown (2). Between colours (2).
Thick.	Thick (3).
Darkened with grease or some- thing on it.	Correct (5).
<i>Moustache.</i> Small.	Correct (9).
Fair.	Correct (3).
<i>Age.</i> 21. Twenty-one.	Young (6). Twenty—twenty-five (3). Twenty-five—thirty (3). Thirty—thirty-five (1).
<i>Eyes.</i> Indeterminate colour, grey with brown flecks.	Blue or blue-grey (4). Blue- grey (6). Grey (3). Grey or blue-grey (1). Grey or blue (2). Blue (2).

A's character and disposition.

A is described by his family and friends as being of a gay and happy temperament, but with a vein of seriousness, and liable on occasions to fits of abstraction when he would think about serious matters.

He was very affectionate, and perhaps unusually gentle for a boy of his age, yet this was combined with extreme daring.

He was very musical and artistic, with a keen appreciation of the beauties of nature. Fond of outdoor games rather of the lighter sort, such as lawn tennis, than of the heavier sorts, such as football.

He enjoyed life to the full, and was popular wherever he went.

He was particularly devoted to his mother, and the relationship between them was even closer than is usual.

The following are extracts from the sittings illustrating his character as shown therein.

A.S. 5. Very jolly and clever.

S.P. 5. Used to be very happy, you know, laughing

boy, yet he was sort of reserved, didn't much like to talk about serious things when anyone there.

S.P. 11. Rather quick in movements, rather happy sort of disposition.

S.P. 17. He has rather a nice happy smile.

S.P. 26. Got a nice smiling impression.

S.P. 15. Sometimes he was all right, sometimes he felt very shy, he was rather a clever boy and used to think an awful lot.

A.S. 36. Very happy spirit.

A.S. 51. Him very happy boy, He was sort of happy disposition.

S.P. 14. Very affectionate and kind. Couldnt show things.

S.P. 45. Loves you very much. . . . Affectionate expression.

S.P. 14. He had lots of courage though he was very quiet.

A.S. 89. Although he is so gentle . . . he was frightfully daring.

S.P. 26. He very sweet natured boy, sort of man who got sympathy like a girl. He says he was like a girl to you.

S.P. 5. He used to tease you.

S.P. 45. Doesn't know he may be teasing you. . . . It was teasing, I'm sure. Him teasing you about it.

S.P. 5. Hands long and thin. Fond of music.

S.P. 15. Very fond of music. Had very nice hands.

S.P. 26. Very, very fond of music. . . . Had beautiful hands.

A.S. 55. Very, very fond of music. Had beautiful hands and beautiful nails.

A.S. 70. He is very musical.

S.P. 5. Very fond of games, but not heavy games.

S.P. 14. Very fond of sport.

A.S. 73. Very fond of all sorts of games outside.

The following extracts from sittings with Mrs. Leonard show correspondence with the above.

He's awfully persistent, not exactly obstinate, firm will.

He's got a jolly way with him, so natural, no sense of sorrow.

He would have been very jolly and happy but he sometimes, and it was strange for anyone young, he sometimes had, I don't like to call them fits of depression, but sort of thoughtful fits, very quiet thoughtful fits in which he would think rather deeply, much more than people would expect him to.

The young man is very persistent.

When he was with you sometimes he would seem to you younger, sometimes older: sometimes he was like a little child, so gay and young and liking to tease you. Sometimes he would be like a wise old gentleman (Oh, not so bad as that he says) but he had an insight into deeper things.

He'd got a quiet way with him sometimes, he can be quite quiet and considering and then he wakes up and talks in such an interested way.

Gentle as a girl though he was manly and had courage.

As mentioned before, his aeroplane was shot down by the Austrians and both the occupants were killed. It is not known exactly what his injuries were, but in several sittings with Mrs. Leonard and elsewhere communications purporting to come from him speak of his feeling of breathlessness at the time of his passing.

A's military service and death.

The following are extracts bearing on A's occupation as a flying man and his death.

In eight sittings he is referred to as a soldier.

S.P. 11. Soldier, shows a wing, got wings stuck in himself, breast.

S.P. 14. Used to fly in air.

S.P. 26. Something to do between them in air (referring to A. and a friend), or one of them, Topsy gets taken up in air.

S.P. 14. What a smash! Did something fall from somewhere? (pointing up in air).

A.S. 34. Everything seems to have slipped away from him. (*Note.* This may be a symbol of his airerash, but it is not certain.)

A.S. 70. Symbol of things tumbling in around him.

A.S. 51. It looks like him hurt and him had a big fall when he crossed.

S.P. 15. Gone away a long time and just about coming back and then he crossed over. He calls it hard luck because he was so looking forward to something.

(*Note.* He was killed just before his leave was due.)

A.S. 70. I don't know whether he was shot through the heart. . . . I seem to get difficulty through the heart and breathing.

A.S. 89. He passed over some time ago, it is some sudden passing. I do not think it is an illness, it may be an accident. . . . probably crossed over in an explosion. I hear a lot of noise.

A.S. 87. I feel sure that he didn't finish his education, because I feel almost taken from school to the Army; he was probably under age when he enlisted. I think he enlisted with several more from school or university.

(*Note.* He went into the Army from his Oxford O.T.C., he was seventeen when he first tried to enlist in 1915.)

I should say that he passed over as a result of an injury, if not actually killed outright in the war.

He probably had something to do with aviation. . . . Perhaps there was a crash. . . . I mean an aeroplane crash.

The following extracts from sittings with Mrs. Leonard show some correspondence with the above.

Passed over suddenly, not expected. Fortunately he met a friend who had passed over a little while before.

Just before he passed his thoughts were on a long journey.

He says, "Don't exaggerate suffering in connection with my passing. I hardly felt anything, not conscious, felt faint and dizzy. . . . but it didn't hurt me. I didn't feel any pain or worry or discomfort, only I couldn't breathe. It didn't worry me, it annoyed me. I said, 'Something's happened.'"

He would have passed over more quickly than the lady. He felt terribly out of breath just before he passed. . . . couldn't get his breath at all.

I wonder if you can remember, had he been planning a journey, going somewhere, something he would be thinking of only a little while before he went over.

Representative evidential statements.

The following are selected extracts from the sittings given in order to show the nature of the evidential statements. They are not selected entirely from the definite or higher class of statements, but many of those which are classed as vague are included. It has been attempted to give a fairly representative selection. The notes are quoted *verbatim* from the annotator's remarks.

A.S. 5. Topsy thinks hims mummy had a picture painted of him since he passed over.

Note. Yes, a miniature.

Hims mummy just had dress made, got lots of gold on.

Note. Dress with gold not worn since 1915, when he had known it. About six months ago brought out and made into a tea-gown.

Hims mummy very pretty, she had picture somewhere for people to see.

Note. A drawing of his mother exhibited at the Pastel Society in the lifetime of the alleged communicator in 1912. Lately rehung after being in garret for three years.

S.P. 5. When you comes out of your gate Topsy sees a road not like you sees road here. Topsy sees earth or something, sort of goes down hill. You goes a long way down hill and then you get long stretch of flat and sort of green each side. And there's something little feeding on right when you get to the flat. Not big cows, something little. Going down that road when you get to the flat long way down there's a house with a red roof that shows so plain.

Note. The road from our house in California leads down hill about a mile to flat green marsh land.

Him says him gets lots of music and lots of water, and me thinks there's a joke about water, cos when he says water him laughing; is you afraid water?

Note. I had been nervous the night before on account of a swimming expedition of J——'s—had begged him not to go. J—— says they sang a lot of Wagner after the swim.

S.P. 8. You got name in your family or is your name, something like Mar.

Note. My name is Margaret, also my Mother's.

S.P. 11. Me thinks him sometimes rode n'orses.¹ Shows Topsy n'orses.

Note. Since the sitting his mother has told me that A. before entering the Flying Corps used to ride horses at the Albany Street Barracks.

S.P. 12. You seem to have altered your career or something. Him gives you turned over to something quite different.

Note. Since —— (the ostensible communicator in this sitting) saw me I have turned from writing stories to writing plays. When he knew me I was a civil servant.

S.P. 14. Doesnt know what him means but he says him awfully pleased happiness come to you, old chap. Its about something nice, something happy coming to your life. . . . Looks like a lady connected with it.

Note. This is so. Quite true. (The annotator had recently become engaged.)

The last words him says is "My word, you tooked on some responsibility" and then him laughing.

Note. This obviously means the Editorship of the ——, a —— paper. A.'s mother thinks it is more likely a reference to the girl, "an engagement."

¹ It is one of the peculiarities of Topsy's pronunciation that she sometimes adds an initial 'n' to words beginning with a vowel or aspirate.

S.P. 15. Him says him had long trousers long before he should have done and you remember how proud he was.

Note. Wrong—if unless (*sic*) he was trying to say “long after he should have done.” His school uniform was knickers—he never put on trousers until he was sixteen.

There’s some kind of funny bridge not far away there too; not real bridge. It’s something makeshift, him says. Me thinks he must have been there when he was younger. Takes Topsy baek like few years, and very happy.

Note. A covered passage between the front and the back of the house, the boys used the roof of this passage as a means of getting into the back garden when the gate of the passage was locked.

You used to be cross with him, something to do bad writing. . . . No, not bad writing, no, not enough writing and you used to be cross with him and sort of couldn’t make him say what you wanted to know.

Note. I often reproached him for not writing the particular things that I had asked him about in my own lines. He would not acknowledge parcels or letters for weeks. This applies particularly to the time when he was at different training centres in England. From France and from Italy he wrote often and regularly.

Him sat on your lap and kisses you, and says still you baby. Him says him used to do that sometimes when he wasn’t shy.

Note. Characteristic: has used the phrase when living.

A.S. 31. Him shows me lots of wood, like tree cut up. . . . Shows three distinct blocks, him says.

Note. When my parents and family were all together in 1920, my younger son and I spent a great part of our time cutting down and sawing up trees. In our London garden we cut down a tree and made three blocks of a part of the trunk, which blocks we put on our hearth at different times to hold a kettle.

You have a head of some description in that room.

Note. A head of an old priest modelled by my sister.

A.S. 36. He takes me to a house—not large—seems to stand in its own grounds. Not much of a cultivated garden—seems more like rough bushes and trees and so on. I do not know whether it is a house he has been to, or a house his people are at now.

Note. A photograph of the garden of his mother's house shows that this description is fairly correct.

This boy has a habit of scribbling on all sorts of pieces of paper and backs of letters.

Note. True of A. when living.

S.P. 26. Him says you had three pairs new shoes or boots, doesn't know which, since here last. Him says you are extravagant, but him pleased.

Note. Quite true. I had one pair on for the first time at the sitting, the other pairs just bought and not yet worn at all.

Shows Topsy lots of peas and him laughing.... Me thinks it's a joke against him; doesn't know what it is. Shows lots of peas and laughing, something you and him. Can't understand it, can't get it.

Note. Might this refer to a joke about shelling peas at a Convent at Bruges? He went to school there every morning when he was about six years old, and he was given the whole of the Convent's peas to shell, and never told the family until afterwards. Later when he was grown up he used to refer to the Convent as "where they gave me peas to shell."

Does you know what him means, says that girl with him got no name.

Note. The one who died when two days old had no name. (This refers apparently to an infant sister whom he claims to have met on the "other side.")

A.S. 51. Him says, you birdy still singing in family. Does you understand this joke? he is laughing. The birdy is old now, but still same as young.

Note. In May 1918, in lovely warm weather on A.'s last leave he spoke of the "damned little bird" that was singing Tirritee-tirritee-tirritee in the garden, "singing as gaily as if there wasn't a war on." Ever since, all the rest of the summer when we heard it singing we referred to it as "A.'s little bird," and we have done so every spring since when we have heard it singing.

S.P. 27. Him laughing about you, you buries something and then you digs it up; it some joke. Looks like you buries something, perhaps you buries flowers and then you digs it up. . . . You got bird or animal been ill, or you afraid of it being ill.

Note. This might refer to the tortoise and the difficulty I had in getting a winter bed for it. It wasn't actually buried, but covered over. I put a few dandelions near it in case it hadn't gone to sleep. I thought the tortoise looked ill. (This incident had been mentioned in a previous sitting. The sitter had been disturbed in her mind about a tortoise which she thought had no proper place for hibernation.)

S.P. 27. You had a lot of water in you house, or you been somewhere where there was a lot of water in house.

Note. I didn't know this at the time of the sitting. On reaehing home I found rain had been coming through the roof and had marked the ceiling.

A.S. 55. Gets Bubber or Bubbers, Bubber.

Note. A. saw a great deal of a girl called Bubbles at one time.

Me thinks hims mummy mislaid something wears round neck and there was such an upset and then it was in the natural place.

Note. I thought a valuable neeklaee had been stolen from my trunk on the way from Italy in the summer of

1914. We turned all the luggage upside down and inside out, and then found the necklaee quite safely tuedked away. There was great exeitement over it all, and the evening of my arrival home from Italy was rather spoiled.

A.S. 70. I don't know whether his mother has gone to America or is going to America, eonnected with America, and his mother and a journey to America. Also she has either sold a lot of old clothes or else given a lot of old clothes of her own away; they have been paecked up to go away.

Note. We are, of course, eonnected with America. It is true that last spring I paecked a large bundle of clothes to be ealled for; it was in the box-room for three or four weeks, but exaetly when I cannot say.

S.P. 45. You got hole in something on stairs. Tored or something; its something a little dangerous. . . . Doesn't know, shows something like turned upside down. . . . Didn't you have something new on stairs?

Note. The bottom step of the steps leading from the drawing-room into the garden had threatened to break for some time before it finally cracked right through. Before the earpenter came to make the necessary repairs (three new steps) the bottom step for some days was replaeed by a wooden box turned upside down. This was months ago, I can't quite remember when.

A.S. 89. I don't know, he suddenly laughed and said, Mother has made a long stay this time. She had about six moves when he was here. This lady probably lived in America, or was born in America or her ancestors were eonnected with America; I seem to be associated with America with her.

Note. Quite correct.

S.P. 52. You is always breaking some beads, and broke'd some beads; shows Topsy you trying to piek up some beads, and he's laughing. They'se light beads.

Note. About a week after the sitting I was wearing

a string of coral beads. They broke and I lost three. I don't know why I wore the beads, as I haven't worn beads for years.

A few non-veridical statements.

In order that the reader may be given a fair idea of the nature of the evidence contained in the sittings a small selection of non-veridical statements from this sub-case follows. These are either definitely false or are not recognised as true.

A.S. 5. He shows like glass balls and bottles to Topsy, something he was doing when he passed out. His wife takes care of them. He used to speak to lots of people. Like stand up and speak. His wife going to have long earned rest and him very pleased.

S.P. 8. Him says you wears heavy clothes and heavy boots, something to do with that sport, does you understand? You goes somewhere and him goes with you. Looks to Topsy all country from little house, like big patch country, or else you have just been to it. Anyway him shows that to Topsy and him likes it very much.

S.P. 12. Him had a very big dog, you know, high legs, nearly as tall as donkey, and very thin, long sort of brown hair with little white on it, long head with long ears.

Haven't you got picture at home, not big picture, with lots of people: some funny people, some sitting down and some standing up, some not quite dressed.

Seven, doesn't know whether he was there something in 7, or whether he was there 7 years, or whether he was killed 7, something 7 connected with him and that place.

S.P. 15. Him says there has been three marriages in family since him crossed over.

A.S. 31. Shows Topsy very pointed pair of shoes. Me thinks they still in existence though they are very old. No, very long time ago—not old.

Gets "Ongora"—"Angora"—or "Ankora" sounds like.

Him used to play some funny music. Not violin or piano, doesn't know what it is—makes funny noise.

S.P. 17. He shows Topsy like grave being dug and new sort of light brown coffin and him points some one connected with himself, must be himself, as he points to himself. Will you find out?

A.S. 36. Shows me like carpet or druggetting, fairly narrow strips, put down perhaps in the hall or on stairs.

S.P. 20. You know, when him wrote a letter used to write not only of ordinary things, but describe things like writing a book.

S.P. 27. Doesn't know why it is, but that gentleman like shows roses, like, you know, not like stand up on tree, big trees, long things with separate roses.

Doesn't know whether him's people long way away, or a mental distance between him and you's people—there's some kind of distance and it doesn't quite look to Topsy like real ground going over. No me thinks its some of his people don't understand or didn't understand.

A.S. 55. She'm either going to give a party or going to party and she very happy about it.

A.S. 70. Holding up something shiney to me—I can't see what it is, and then I get the lctter M. Wait, there is something that is written, something longish and shiney. M. and C. There's a short letter which I can't get, seems small writing.

S.P. 45. It's very old lady, but very brilliant mentality. . . . Do you know anybody, name like Sidg. Something to do with somebody Sidg.

A.S. 89. I hear the name Martha. I dont know whether it comes from that old lady. That old lady seemed to be mentally very energetic, but I dont think she could walk very well. I think she must have been very old before she passed out.

2. *Extracts from the X. Case.*

The ostensible communicator in this case was the father of the sitter. He died on the 16th December, 1927, at the age of eighty-one. For some years he had been a complete cripple, having lost the use of his legs

owing to accidents in which both his hips were fractured. He passed his life in bed or in an invalid chair. He was sometimes taken out in a wheeled chair. Before his accident he had been a clever and successful professional man, and was referred to in a letter of condolence to his widow as "a kindly humorous Scotsman." Miss X., the sitter, writes:

After a most successful career he had a great amount of bad luck, and after this seemed to lose his nerve and self-confidence and never regained them. This quite ruined his career, and he lost all his connection. This particularly affected his son, who was in the same profession and hoped to succeed him. . . . All this was before he had his accidents and broke his hips.

As a result of his losses and sufferings he became morose, irritable and difficult to live with, he brooded on his misfortunes and lost his grip on life. This was the cause of a great deal of unhappiness to his wife and family. In Topsy's words, which are said by the annotator to be quite true, "He had funny moods," "His mentality seemed queer and crochety," "Very often at your happiest times he would suddenly say something and make you all unhappy and spoil your pleasure." "Seems like he turned against everybody." He caused many "upsets."

As will be seen from the extracts the general tone of the communications is one of remorse for his unkindness and a desire for forgiveness. He is described as a man of simple tastes and few wants. During the latter part of his life he never handled money. He was reserved and undemonstrative as a rule, but had rare moments when he showed his affection for his family.

He used to read a good deal, latterly mainly short stories and magazines. He was accustomed to sit hunched up in his chair, his chin almost on his chest, he hardly ever raised his head, so much so that some of his visitors remarked that they had rarely seen his face. He was naturally a man of great physical strength and of fine appearance, of great breadth of chest though rather short of stature.

He died, not as a result of the injuries to his hips, but from a stroke; his last illness was of short duration.

(a) *Extracts concerning the ostensible communicator, his health conditions, etc.*

A.S. 47. Some one who couldn't do much, not only ill but couldn't use arms perhaps or legs, Somehow feels difficult to move.

Me feels that gentleman was sort of ill and yet not ill—could have done things with his head.

A.S. 49. Him crossed over like suddenly, him ill long time but him crossed over suddenly—like only short time ill before him crossed. Him crossed over not like from what him had been ill with all the time. Him says him glad to go really 'cause great trial to be tied like that. Him says not being able to use his legs was much more to him than pain. Him couldn't walk, couldn't do things for himself for several years.

S.P. 36. Feel him can't do anything very much, only to read and things like that and grumble.

Topsy me doesn't feel ill, gives the feeling doesn't feel ill and yet him ill for a long time.

Keeps on hearing the word "Stroke."

Me doesnt think that gentleman ill so people expected him to cross over. Him seems to have crossed sudden. Not sudden in half an hour like that, but sort of different kind of illness him suffered from.

A.S. 76. Him still resting and you would think that him didn't need rest after all the rest him was obliged to take.

S.P. 42. (Referring to his invalid chair.) Something that was like legs, like legs to him. . . . It was the only way that he could get to places. Him had legs but sort of couldn't use them.

A.S. 85. He has probably been an invalid, he doesn't seem to sit in an ordinary chair, it seems as if he were propped up in the chair.

That gentleman might have suffered with paralysis or something up to the hip probably.

A.S. 87. I seem to get conditions of an invalid, a cripple of some description. . . . I don't seem ill and yet I feel helpless, as if I am wheeled and lifted about.

I seem to feel that my life is spent either in bed or in a chair, and in each case I seem to be put there and taken out.

I should say that he had a very good brain before this accident or something which deprived him of the movement of his lower limbs. If he has gone over it is not as a result connected with this disability that I have been speaking of. Since that disability he had some kind of sudden illness, but whether he passed over in it I cannot say.

(b) *Other extracts.*

A.S. 47. You had an upset in your house. . . . You is not worried 'cause it is a relief.

Says him appreciates Mummy's courage and all you's courage. Says you miss responsibility more than him, cause him feels him wasn't much use to any of you.

Him says hims passing really a lucky kind.

A.S. 49. Anyway it (his crossing) was a shock to you all and surprise to you all.

Him was the cause of lots of upsets in house and now it's going to be much more peaceful for you all. You's mummy's nerves was beginning to get worn down.

You used to be able to comfort him when him used to get in those funny moods. You used to try to be firm with him and yet you used to comfort him. Sometimes he used to feel hurt about it, like you was too firm, and thought you was rather unkind, but him understands now. Sometimes you used to come home and find Mummy upset and you used to settle it. Him understands now and him laughing about it.

Him says one thing him always thanks God about, that though his mentality seemed queer and erotehetty, yet him remained sane. Towards the end him always imagining things going wrong and people doing wrong. Him sorry about it and realises how kind and tolerant you all were. Him worried Mummy so much, him often upset and she waited for people to come home.

Him so worried about this and him anxious to tell you's mummy that you Daddy is sorry.

Him wants you all to be happy and to realise that you did lots for him. Mummy gets worried and upset and feels sometimes she wasn't always kind to him and sometimes cross. Wants to send a special message that she always did her best and was very good to him and very few people would have been so patient as she was with him all through.

S.P. 36. Him seems to regret him done some waste in hims life—especially in the rest (*sic*) part of hims life. You know, him seems to have muddled things. Him very sorry and all through that muddle, family sort of affected now.

Him very anxious to send message to hims wife that though all outward things pointed to the contrary, him did love her and did love hims children—though him does realise that him pushed them away from him somehow.

If he had been here much longer he would have lost all peoples love and respect for him.

Since him crossed you had much more domestic peace. . . . Him caused indirectly uproars in the domestic life.

It was misery to him some years before him crossed over and him very, very happy to be out of that misery. And after hims fits of disagreeableness him used to suffer a great deal of remorse and him had a great deal of remorse since him crossed over.

Him very anxious to make clear to hims wife—yes, its hims wife—that she did more than her duty and him glad that she hasn't got to have regrets. And now she been free from upsets and worries and anxieties and she

feels that now it was not any trouble and she would go through it again if only him was there.

Though it helps him very much that she thinks of the good things and not of the bad ones, yet the bad ones were very bad. But him could not help himself, it was a very bad life not to be out in the sunshine without depending on someone.

Him wants to give him love to you's mummy and tell her how grateful him is for all she done for him and him really was—in him better moments when him was here—grateful. Him very anxious that she be happy and enjoy life after such long years of strain.

Him was not a rich man and him considers him was not a lucky man. Every thing him had him had to work very hard to get. Whatever him done, him seemed sort of to give nearly all him life to, and then when him could not do it any longer, him sort of lost. That something to do with the disturbances him made and caused you lot of pain, and you got not quite to dislike him but sort of to feel cross.

Thing him seems most anxious about is to know that him loves you and always did love you and him wants you to forgive him. You has only all tried to think of the best, but him feels apology due to you all. Him knows you doesn't try to think of the bad, but it sort of worries him and him sees things clear now where him couldnt before. Him realises it is very difficult for the active people to understand what the inactive people have to go through.

A.S. 76. Him happy him more loved now and more understood than for some time before. More sort of understood.

(After referring to the sitter's work.) Says all that been unnecessary if him hadn't mismanaged things.

Him says you laugh when him tells you, but him still resting and you would think that him didn't need rest after all the rest him been obliged to take. Except for sort of journeys to you, him resting with hims people. Him says him needed very much mental rest, him men-

tally sort of under strain long time, and it made him be unkind to other peoples. But him didn't mean to be unkind. And that strain him feels him must have rest and get over it.

S.P. 42. Him so glad that message delighted her (Sitter's Mother) so much. Some message him sent her some time. . . . She thought it was so like him.

Him says him getting used to hims mind once more as if him was young. It was something here in life that sort of robbed him of something and now him sort of learning. And that makes him anxious to come back, him resting and learning to use hims mind. Last part of hims life seemed as if him was child again and him couldnt develop—growth ceased—and now him got all that to make up. But everybody very kind to him. And him constantly coming to your house to try to make up for irritability that hims child mind caused—Not himself.

Very often at your happiest times he would suddenly say something and make you all unhappy and spoil your pleasure and can't do enough to remedy that. But it was not him—the man.

A.S. 86. That gentleman didn't have anything to do with money for a long time. Says him didn't have any thing to do with money for long time and hims needs were very small indeed. Him didn't fancy anything specially.

Topsy me thinks him was clever man sometimes and then something happened and him was not clever any more. And him so pleased to be able to be sane now but him still learning.

Him got great affection for hims family now and affection that was not suspected for some time. Seemed like him turned against everybody then for a time.

A.S. 87. I should say that he had a very good alert brain before this accident or something happened which deprived him of the movement of the lower limbs.

3. *Other Extracts.*

The following are a few extracts taken more or less indiscriminately from other cases.

S.P. 28. There's picture with family and him holding baby on his knee, but him much younger then.

Note. There is a portrait of my Uncle when much younger holding baby against his knee, and family grouped round.

S.P. 46. And gets a word like *Moon* or *Moons*.

Note. The sitter's name was Miss *Moon* (pseudonym—the correct name was actually given).

She brought you lots of those little lilies.

It means something, either you very fond or she very fond of them. Its something particular.

SITTER. Well, can you tell me if when I smell these lilies round me, she has brought them?

Yes, particularly one night when you was in front of glass somewhere, does you remember?

SITTER.. In front of glass.

You know, what you does your hair in front of.

SITTER. Yes, that's quite right.

It was very strong and you looked round.

Note. This is very interesting, as sometimes I have a very strong scent of lilies near me, and I have never been able to account for it.

(Miss Newton's note.) This impressed Miss *Moon* very much. The fact of her getting impressions of a strong scent of lilies was known to several people before the sitting.

S.P. 16. Something wrong with your shoes or with your stockings, doesn't know, him laughs, and then him shows pile of things, something for feet.

SITTER. Is that stockings I have got to darn?

Doesn't know, shows heap, and something funny with your foot back there (points to heel).

SITTER. Yes, that may be very good.

Well, says you leave it all till you haven't got one pair.

SITTER. That's absolutely splendid.

And you been going to start on to it and bring it out and leaves it.

SITTER. Yes, that is quite true.

All colours, all mixed.

SITTER. Yes.

Little while ago somebody did them for you.

SITTER. Yes, that's right.

Not going to have such luek this time.

Note. This seems very good to me. I hardly ever darn my own stoekings, but my usual darter was too busy at the time, so a dreadful heap of vari-coloured stoekings which I had tried in vain to tackle accuumulated till I had suddenly no holeless pair left, and had to dash out and buy some new ones. I don't think this has ever happened before. After washing the stoekings and getting them all ready in a heap, workbox and all complete, I even failed to do them.

S.P. 40. (Same sitter as above.) You had to go out not long ago and had to buy stoekings quickly, him says.

Note. This is interesting, for another incident exactly similar to this one was described in my first sitting. I vowed I wouldn't let my undarned stoekings accumulate again. I felt really ashamed of myself, but had to hurry out and buy some more. I remember thinking of Topsy as I ruefully looked at the second undarned heap, and hoped she wouldn't know of my shortcomings.

A.S. 20. I dont know whether he had an accident but he shows me the right heel of shoe, something has been done or either torn.

Note. My husband's left leg which he broke was slightly shorter after being set, and when the right rubber heel came off his shoe one day, we left it off, as it gave his left leg the added height.

A.S. 2. That house that Topsy describe, there was an old dark box, sort of chest, me thinks you close it. Belonging to that gentleman there and it's a little broke.

Note. There was in our house a dark old Japanese cabinet with a broken door.

A.S. 10. Don't know why Topsy gets lots of nuts; you know, those little nuts.

Note. She was specially fond of nuts, and sometimes kept a bag of them by the desk she wrote at.

It would, of course, be possible to multiply the number of these extracts, but there does not seem to be any useful purpose to be served in doing so. Those given are fairly representative of the whole.

II.

STATISTICS RELATING TO THE WHOLE SERIES OF SITTINGS.

The following are some statistics concerning the whole series of sittings and the various classifications which have been made with selected sub-cases.

There were in all 142 sittings; of these 53 were Sitter Present. These are classified as 46 successes, 5 failures, 1 sitting rejected because the sitter gave away too much, and 1 special experimental sitting which yielded no result.

There were 89 Absent Sitter sittings—29 were ordinary successes, 9 were intrusion sittings, 3 were special experimental sittings, 42 had to be counted as failures, 1 was rejected on account of the unsatisfactory nature of the annotations, and 5 were pseudo-intrusions, that is to say, the communicator connected with the notetaker intruded. These latter really count as Sitter Present sittings, and in the following figures their scores have been included in that category.

Of the 42 failures in the A.S. type there was often some small amount of matter which could, at a stretch, have been considered as veridical; in some the failure almost certainly arose from the side of the annotator, in others there is some suggestion of an intrusion, but not sufficiently definite to enable it to be classed as such. It is also possible that some cases of intrusion have been overlooked.

For the whole series of sittings the scores for successful sittings are approximately as follows :

	<i>Total possible.</i>	<i>Gross score.</i>	<i>Net score.</i>
Absent sitter -	2645	1247	1116 = 42.2%
Sitter present -	5782	4204	4002 = 69.2%
	<hr/>	<hr/>	<hr/>
	8427	5451	5118 = 60.7%

From what has just been said concerning the nature of the failures in the A.S. sittings it will be readily appreciated that it was not always possible to score these in the same way as the ordinary sittings. One can, therefore, only form an estimate of what the percentage score for absent sittings would be had all the failures been included. It is estimated that it would have been somewhere between 20% and 30%. If the failures among the S.P. sittings had been included in the percentage score it would have been reduced to about 65%.

Sometimes at an A.S. sitting two relics were used belonging to different contributors: such a sitting should really be counted as two separate sittings. Again, there is one case where a double intrusion occurs, and another where a sitting which starts as a complete failure develops at the very end into a particularly good pseudo-intrusion. All these peculiarities render it impossible to do more than give approximate figures.

Eight separate sub-cases were selected as being suitable for special treatment, as described on page 7; these comprised 79 sittings in all, of which 39 were S.P. and 40 A.S.

The following are figures showing the numbers of the various classes of statements as before described.

	<i>Ante-mortem.</i>				<i>Post-mortem.</i>			
	Physical.		Non-physical.		Physical.		Non-physical.	
	Veridical.	Non-veridical.	Veridical.	Non-veridical.	Veridical.	Non-veridical.	Veridical.	Non-veridical.
S.P. -	565	231	254	30	718	250	282	45
A.S. -	180	252	57	23	164	110	33	17
	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>
	745	483	311	53	882	360	315	62

TOTALS.

<i>Ante-mortem</i>	-	Veridical	1056.	Non-veridical	536
<i>Post-mortem</i>	-	„	1197.	„	422
<i>Sitter Present</i>	-	„	1819.	„	566
<i>Absent Sitter</i>	-	„	434.	„	402
<i>Physical</i>	-	„	1627.	„	843
<i>Non-Physical</i>	-	„	626.	„	115

Percentages for Physical and Non-physical Statements.

		Ver.	Non-ver.	Ver.	Non-ver.				
Physical	A.M.	S.P.	71%	29%	60%	40%	Ver. Non-ver.		
		A.S.	42%	58%					
	P.M.	S.P.	74%	26%	72%	28%		66%	34%
		A.S.	60%	40%					
Non-physical	A.M.	S.P.	89%	11%	85%	15%	84%	16%	
		A.S.	70%	30%					
	P.M.	S.P.	86%	14%	84%	16%			
		A.S.	66%	34%					

For sixty sittings the following are the figures, showing how the non-physical statements are divided into those which concern the communicator and those referring to the sitter.

	<i>Ante-mortem.</i>				<i>Post-mortem.</i>			
	<i>Communicator.</i>		<i>Sitter.</i>		<i>Communicator.</i>		<i>Sitter.</i>	
	Ver.	Non-ver.	Ver.	Non-ver.	Ver.	Non-ver.	Ver.	Non-ver.
S.P.	-	202	27	32	7	63	219	15
A.S.	-	31	16	—	—	15	17	6
		—	—	—	—	—	—	—
		233	43	32	7	78	236	21

For the post-mortem statements concerning the communicator, it is, of course, impossible to say whether they are veridical or not; they are therefore lumped together.

It must be remembered that these figures are only approximate, and they must not be taken as giving anything more than rough indications. We may say, however, that the percentage of veridicality in S.P.

sittings is double or more than double that in A.S. That the ante-mortem and post-mortem statements are about equal in number, the veridicality of the latter being, if anything, slightly higher. That the physical statements exceed in number the non-physical, but have a lower percentage of veridicality. That the statements concerning the communicator are more frequent in the ante-mortem, and those concerning the sitter in the post-mortem.

Beyond this we cannot go; any deductions based on the actual figures and depending on their accuracy would be quite untrustworthy.

III.

SOME GENERAL THEORETICAL CONSIDERATIONS.

At an early stage of this case the writer drew up a kind of programme of points to be inquired into and tentative hypotheses, suggested by the evidence, so far as it had gone, to be tested by further investigation.

From the point of view of theory the investigation is divided into two parts: 1st, psychological; 2nd, psychical research proper. In the first part it is sought to throw some light upon and suggest tentative hypotheses to account for the psychological mechanism of trance mediumship, leaving out of the question the source of the information given; the second part deals with that source, and is approached in the form of a discussion of the various hypotheses which have from time to time been put forward.

The division is not, of course, absolute, the two parts having an intimate relation to each other and their respective provinces overlapping to some extent.

The psychological side will be treated first.

The programme above referred to is quoted from, and the tentative answers to the questions raised are given with, supporting evidence from the sittings where possible.

A. PSYCHOLOGICAL.

SECTION I.

The first point to be enquired into is how far there is evidence for the prevalence of the following:

(a) Deliberate invention.

It is, of course, impossible to pronounce with certainty on this matter, but it may be said that there is no apparent evidence of deliberate invention on the part of the medium. The only thing that might suggest it is the occurrence in some sittings of certain rather hackneyed statements, that is to say, remarks of a kind common in trance-mediumship and of such a nature as to be generally acceptable to any sitter. Some of these have been classed as clichés, and this has been done when the type is of a definite and particular as distinguished from a general nature. Instances of clichés will be given and discussed later. There are also a number of remarks of a more general type, such as references to the alleged spirit helping the sitter, being with the sitter at various times, to the sitter going over to "the other side" during sleep, to the sitter being worried, sad, tired, ill and so on. They are statements which either are not susceptible of verification or are of so commonplace a nature that they stand a good chance of being a lucky shot if the medium were inventing.

There are also a number of statements which might be considered as obvious inferences from what has gone before, *e.g.* when the ostensible communicator is a young man it is fairly safe to say that he was fond of sport.

Now it may be that those and similar statements are deliberate inventions, and if the communications consisted solely of them we should undoubtedly be justified in dismissing the case as valueless; but when they are combined with evidential matter of a superior kind it seems scarcely reasonable to discount them altogether just because they are commonplace. It is not fair to condemn the medium for giving these commonplace communications, especially if they happen to be veridical, for, just

because they are commonplace, they are veridical if applied to most people, and in consequence tend to be frequently given.

To be perfectly just, the complete absence or relative scarcity of any such commonplace remarks from a sitting would be more open to suspicion than the presence of a normal amount, for then one might suspect the medium of deliberately selecting evidence which would sound impressive.

When so much that is commonplace is true of most of us it would be strange if a great deal of it did not turn up in the sittings.

The point is not of first importance, as the evidential value of the communications of this type is never very high, and their total disqualification would not greatly affect the result.

In view of the above, and in the absence of any direct evidence of deliberate invention, it must be held not to exist.

In this connection the following incident should be mentioned. Miss X.,¹ who had acted as notetaker at a number of sittings, had been introduced to the medium under a pseudonym. She lives in the same neighbourhood as the medium, or rather did do so—the latter has since moved. On one occasion they met at the Bank.

Miss X.'s account of the matter is as follows:

It must be mentioned that about 5½ months ago Mrs. Elliott met me and my dog Billy in Barclays Bank, Wellington Road. It was only for about a minute, as I hastily said "How do you do?" and beat a retreat, as I was sorry she had met me and would know I lived in the neighbourhood. She, of course, saw my dog, and I am practically certain she heard me call him by name when I called him to go out with me. She might, therefore, easily have remembered his appearance and his name, which gives a normal explanation to her description here of my dog, though I must say she could only have had a hasty glance at him in so short a time.

¹ See also p. 80, the X case.

The sitting to which this note is attached was dated 3rd June, 1927, so that the incident must have happened in January of the same year. The sitting in question was an A.S. one, in which Miss X.'s communicator intruded; much information was given about the dog.

It is, of course, possible that Mrs. Elliott seized the opportunity of making enquiries as to Miss X.'s identity, and acquired by normal means some information concerning her family affairs.

There is, however, absolutely no evidence that this was the case; moreover, some of the information given is such that it could hardly have been acquired except by persistent enquiry from persons intimate with the family. Had such enquiries been made it could hardly have failed to come to Miss X.'s knowledge.

In any case it would not apply to the A.S. sitting given with a relic supplied by Miss X., in which very good results were obtained. There was, of course, nothing to lead the medium to connect this particular relic with her.

Even if this somewhat unfortunate occurrence be held to discount the value of this particular set of sittings it cannot be deemed to affect any other set; and since the evidence given in this set is not different in character or quality from many of the other sittings one can say that there is no evidence of any illegitimate methods having been used.

The incident had to be mentioned, but it is not thought that any great importance attaches to it.

It may be as well to repeat that there is not the slightest suggestion of suspicion that Mrs. Elliott has ever in any way diverged from the path of strictest rectitude.

(b) *Fishing.*

By this term is meant any deliberate attempt on the part of the medium or control to obtain information from the sitter by means of questions, suggestions, skilful twisting of meanings, and so on.

In S.P. sittings, while it is possible that Topsy does sometimes resort to this practice, the occasions on which

it might be suspected are relatively few. On the whole, it may be said that Topsy does not fish, although she gropes, a term explained in the next sub-section. It often happens that when some subject is being mentioned by her, and it seems to the reader of the record as though a little judicious fishing would yield fruitful results, Topsy will suddenly drop the matter and jump abruptly to another topic.

In A.S. sittings fishing is obviously impossible, there being no one present from whom to fish.

(c) *Groping.*

Undoubtedly there is a great deal of groping. It is quite obvious, and no attempt is made to conceal it. Topsy frequently admits that she does not understand what is "being given," and she supplies alternative interpretations. For example, "Me doesnt know whether him is first son or only son or only one left." There is a wide difference between groping and fishing; the first is what might naturally be expected where so much symbolism is employed: the second implies a deliberate attempt to obtain results by illegitimate means.

It does not appear that Topsy always knows when she has hit on the correct interpretation of a symbol apart from any information given her by the siter in S.P. sittings; it follows, therefore, that in many cases the statements are left in the form of alternatives.

The question of groping is of importance as bearing on the matter of symbolism, and will be referred to again when dealing with that point.

(d) *Name, number and similar habits.*

An analysis of the names and initials mentioned in the sittings seems to indicate very clearly that some such habits exist in the medium. Taken as a whole the names and initials have very little evidential value, though there are a few instances where this is not the case. If they are regarded as being of purely fortuitous origin, that is to say, fragments thrown up by the dream stratum, one would expect to find that the relative frequency of the commoner names corresponded with their actual frequency

in real life. Even if the names were all veridical this correspondence should still hold good, for the number of persons mentioned, either as ostensible communicators or connected with them, is sufficiently large to ensure an average distribution of common names.

Now the relative frequency of the common names given does not correspond with their normal frequency of occurrence; it appears, therefore, that there is some disturbing factor at work. This factor seems most likely to be in the nature of a habit; the medium has an unconscious predilection for certain names and initials, which causes them to be thrown up more frequently than they would be by pure chance; this habit would be of the same kind as those habits which have been discovered in card guessing.

In order to arrive at an estimate of the relative frequency of common Christian names, a list was made of about forty-five or so of the male names which occur in the sittings, and the number of times which these names were found among the pupils of a large public school over a period of thirty years was counted, being some 3500 names in all.

The following is the result:

	<i>Sittings.</i>	<i>Public School.</i>		<i>Sittings.</i>	<i>Public School.</i>
Arnold - -	3	11	Frank - -	6	182
Alec or Alexander	2	45	Frederic - -	5	171
Albert - -	2	36	George - -	9	229
Arthur - -	10	232	Geoffrey - -	4	16
Alfred - -	Nil	90	Henry - -	2	252
Andrew - -	1	1	Herbert - -	Nil	150
Benjamin - -	1	12	Joseph - -	4	36
Charles - -	1	237	John - -	33	251
David - -	3	22	James - -	15	110
Douglas - -	1	34	Mortimer - -	1	1
Edward - -	3	207	Maurice - -	7	15
Edgar - -	1	27	Michael - -	9	9
Eric - -	3	17	Nicolas - -	1	1
Ernest - -	Nil	113	Oliver - -	1	9
Edwin - -	1	21	Philip - -	Nil	31

			<i>Sittings.</i>	<i>Public School.</i>			<i>Sittings.</i>	<i>Public School.</i>
Peter	-	-	4	5	Raymond	-	1	4
Percy	-	-	2	79	Stanley	-	3	51
Roger	-	-	12	4	Simon	-	1	Nil
Rufus	-	-	2	Nil	Stephen	-	1	19
Richard	-	-	1	62	Samuel	-	Nil	25
Reginald	-	-	4	68	Thomas	-	2	101
Roderick	-	-	1	2	Walter	-	Nil	69
Robert	-	-	1	103	William	-	30	298

It will be seen that there is a very great discrepancy in some cases.

Taking William as standard, John and Joseph come about right; George, Arthur, Edward, Frank, Frederic, Percy, Richard, Robert and Thomas are all far short; while for Henry and Charles, which should have about 24 or 25 mentions, we get only one each. There are no Alfreds, Herberts, Ernests or Walters, all of which are very common names. James is rather in excess, Maurice, Michael and Peter very much so; while Roger gets 12 mentions in the sittings, whereas he should have only 4.

The girls' names have not been analysed in the same way, but it is pretty clear from inspection that we should find the same thing among them. Names commencing with A. M. and R. seem to be her favourites; these letters, together with J. and W., are the most common among the initials given in the sittings.

The medium has a curious tendency to give names beginning with the sound Rod or Roj, *e.g.* Roger, Rodney, Roderam, Roddin, Rodville or Rodvalley, Roderick; also Robinson, Rosalind, Robert and Rosamund, which have a somewhat similar sound. Fifteen out of the twenty names beginning with S. commence with St.

There are many extraordinary names given, *e.g.* Bwavo or Bravo as the name of a house, Borra Cue or Borrel cue, Capachon, Conweiller, Ebda, Gentlog, Jut, Mordicot, Narronger or Oronga, Orndewa, Orloch, Ormerod, Ouplough, Oshon, Pongee, Pergelos, Roderam, Rodvalley, Ruthleben. It will be observed that these oddities are concentrated around the "O." No normal associations

can be found to account for these names, except perhaps Ruthleben may mean Ruhleben and Roderam Rotterdam, though there is nothing in the context to suggest association with these places. They look to be merely fantastic nonsense names thrown up by the dream consciousness. A parallel may be found in the nonsense words of children. The fact that so many begin with "O" may arise from an infantile association in the mind of the medium with her own maiden surname, viz. Ortner.

(e) *Dream.*

While there are few instances of S.P. sittings being complete failures there are a good number of A.S. sittings where the contributor of the relic was unable to recognise anything as relevant. Some of these have proved to be intrusions, and one or two appear to consist largely of fragments of reminiscences from previous sittings. There is, however, a large amount of matter which cannot be assigned to either of these sources.

A few experiments were tried by sending as relics absolutely new articles which had no connection with anybody, except in so far as they were manufactured articles, sold at a shop, and handled and packed by the contributor. In all these cases a considerable amount of matter was given, none of which could be recognised as in any way relevant by the contributor of the pseudo-relic. This matter was precisely similar in style and character with that given in other sittings. In most cases Topsy recognised that the purporting communicator was not connected with the relic.

The most probable explanation in these cases, and of the unrecognisable matter in the ordinary sittings, is that it is a kind of dream stuff, a spontaneous production of some stratum of the medium's sub-consciousness. In many respects it is more analogous to hypnagogic visions than to dream proper; for example, it lacks the coherence and pseudo-logical sequence of the dream; Topsy jumps abruptly from one subject to another with no discoverable associative linkage. There is reason to think that a large part of the impressions are, for her, in the form of

visual hallucinations. In both these respects the analogy is closer to hypnagogic vision than to dream proper.

It seems, then, a plausible hypothesis that the substratum of the trance material, the matrix in which we occasionally find embedded veridical communications, is a sequence of internally generated hallucinations analogous to hypnagogic visions. These are probably partly built up of fragmentary reminiscences of previous sittings and partly from the normal experiences of the medium.

(f) *Reminiscence.*

As already noted when discussing dream stuff there is a considerable amount of this. It is impossible to draw a definite line between reminiscence and intrusion, although in some cases, where fresh facts are given, reminiscence, properly speaking, seems to be excluded.

It is not suggested that reminiscence is a conscious process: there is no ground whatsoever for supposing that Mrs. Elliott or Topsy deliberately serves up a rehash of old matter; it seems rather as though fragmentary memories of past sittings lingered in some stratum of her sub-sconsciousness, and became interwoven with the dream stuff which constitutes the non-veridical portions of the communications.

As a general rule it has been possible to recognise definitely as reminiscences only those instances which refer to communicators of this series, but there is one case where Mrs. Elliott herself supplied the clue which led to the recognition of the source in a lady who had had sittings several years previously, and with whom she had been on friendly terms. In this case, however, fresh matter was given, and it should, therefore, be classed as an intrusion, though it is possible that some reminiscence is mingled with the new facts (S.P. 48).

Still, the incident suggests strongly that reminiscences from sittings not of this series may form a considerable part of the non-veridical matter. It is, of course, impossible to determine how much is attributable to this cause, but the question is not of great importance, seeing that it is of fragments of memories that dream stuff is largely made up.

(g) *Deferred impression.*

There is a further possibility which has to be taken into account in dealing with the non-veridical matter. It may be that the medium receives impressions at a sitting which do not rise to the surface during that sitting, but remain latent in her subconsciousness until some subsequent occasion. Where the matter is recognisable as referring to some known person, such as a previous communicator, the explanation of deferred impression as against that of intrusion is, as a rule, possible, and but for one fortunate incident it might have appeared to be more in consonance with scientific method to ascribe all these cases to deferred impression. We know from other sources that deferred impressions do occur, and rather than postulate an entirely fresh type of phenomenon for which we have little or no independent evidence, that is to say, intrusion, we should be bound to prefer them as explanation.

It fortunately happens that an incident occurred in one sitting which prevents us from falling into this error, and although in the rest of the cases it may be impossible to decide definitely between the two hypotheses, we are bound to admit intrusion as a *vera causa*.

The incident was as follows: A.S. 73 was a sitting held with an article contributed by Sir Lawrence Jones; the record contained nothing that he could recognise. One of the physical descriptions, however, suggested A., the communicator in the A case (see p. 66), and the record was sent to his mother for annotation. A considerable amount of veridical matter was found.

Many of the statements appear to refer to Miss H—— (see p. 68): for example, the name S——, which is her professional name, the initials H. and P., the first corresponding with her surname, the latter standing for a version of her Christian name, by which she is commonly known; other references confirm this. The important statement is as follows: "That lady that him so fond of, she been very worried and then kind of cold came and made her ill. She been ill and been away. Gets a name like S——."

His mother writes: "P—— was worried, caught a chill and fainted in my drawing-room. This was before she went to Paris, where she was on the day of the sitting. She was there for only a few days. S—— is her professional name." Enquiries were made from Miss H—— as to the approximate date of the fainting. She replied: "I think it would be two or three weeks before I left for Paris when I fainted in Mrs. F——'s house. (April 4th, 1927, I left London for Paris.)" The date of the sitting is 8th April, 1927.

Now there had been no sitting with any member of this group since 16th February, 1927 (S.P. 30). It would seem, therefore, that the reference is to an event which happened after the date of the last sitting, in which case it cannot be ascribed either to reminiscence or to deferred impression.

It may be objected that the evidence is slender, and to some extent the objection is well founded. There are, however, five separate items in the statement, all of which are veridical.

1st. "The lady hims so fond of." Now, although Miss H—— and A. had never met in the flesh, one of the most striking features of this set of sittings is the growth of a posthumous friendship between the two. Whether this is really what it purports to be, viz. a friendship grown up between a living person and the surviving spirit of a deceased boy, or whether it is only a romance conceived in the brain of some living person, is not in point here; the fact remains that the sittings unmistakably display the idea. If it be nothing else it is a correspondence with other sittings with the same and other mediums.

2nd. The lady has been worried. This is in itself not at all convincing: it has been said before in connection with Miss H—— several times.

3rd. A cold came.

4th. She had been ill.

5th. She had been away.

All the last three are extremely commonplace, but when combined with the rest have some evidential value.

However it is only by a very rare lucky chance that an incident of this kind can occur, and such as it is it should be given its full weight. The balance between the two hypotheses, deferred impression and intrusion, hangs evenly; there is nothing, apart from the methodological considerations mentioned above, to tip the scale on either side; it follows, therefore, that any piece of positive evidence will serve to establish as provisionally acceptable that hypothesis which it favours.

It must be emphasised that all hypotheses suggested are only tentative, the evidence in no case is sufficiently strong to enable us to go any further.

SECTION II. THE EFFECT OF VARIOUS INFLUENCES.

The next set of topics to which attention must now be turned is the following.

What is the influence of the following factors?

(a) *The sitter according to type.*

There are certain well-defined types of sitter in this case.

1st. We have the emotionally bereaved, that is to say, those who have recently experienced the loss, through death, of a dearly loved relative or friend, and whose consciousness is largely occupied with the sorrowful emotions arising from that loss.

2nd. The ordinarily bereaved. Those who, having suffered such a loss, have either partially recovered from the first poignancy of their grief by the passage of time, or who, from their temperament or the circumstances of the case, have never had their consciousness completely filled with sorrowful emotion.

3rd. The non-bereaved. Those who have suffered no recent loss, or having lost a friend or relative have not suffered intense grief.

4th. The convinced. Those who already accept the hypothesis of survival of the personality and the possibility of communication.

5th. The unconvinced. Those who preserve an open mind on the subject, not having been able, on the

evidence which has come their way, to arrive at a definite decision. This class would include many different grades, ranging from the nearly convinced to those whose scepticism falls just short of categorical denial. The will to believe and the will to disbelieve are both liable to be very strong in this matter.

It should be noted that, with the exception of the most advanced sceptics, the attitude towards the survival and communication hypothesis has little to do with any belief in a future life held on religious grounds.

6th. To complete the list, though it is doubtful whether this series can exhibit an example of this type, the confirmed disbeliever must be included. This would be one who definitely denies survival in any form, and, of course, as a necessary corollary, the possibility of communication.

(*Note.*—It is difficult to choose a name for the disbeliever which has not a kind of reproachful flavour due to the influence of religious thought over the popular mind. No such connotation is, of course, intended here. Disbelief is equally to be respected as belief: in fact, for the *Psychical Researcher* a certain amount of scepticism might be regarded as desirable.)

These various classes of sitters, of which any of the first three may be combined with any of the last three, can be classified again under three other headings. 1st. Good sitters, *i.e.* those whose sittings as a rule yield a good percentage of veridical matter. 2nd. Moderate sitters, those whose average results are moderate, either consistently in every sitting or by the combination of some good with some poor. 3rd. Bad sitters, *i.e.* those whose sittings are all either complete failures or of poor quality.

The point to be determined is whether there is any relation between the classes in the first classification and those in the second.

It is obvious in the first place that the emotionally bereaved sitter will probably have a strong will to believe in survival, and hence will tend to come into the class 4, *i.e.* the convinced. This fact will almost certainly show its influence in the annotations. Those who are con-

vinced, or are strongly desirous of being convinced, will more readily see and seize upon correspondences between statements in the sittings and their own experience. They will treat as an evidential point that which would be passed over by the sceptic or disbeliever as a trivial chance-coincidence unworthy of notice. This tendency is plainly shown in the annotations. Some of the annotators who are known to belong to class 4, *i.e.* the convinced, exercise, quite unconsciously, very great ingenuity in the interpretation of the evidence, much of what they appear to consider as veridical would seem to the sceptic as farfetched and strained.

On the other hand, it is not unlikely that a similar degree of ingenuity in evading the conclusion of veridicality may be exercised by the sceptic; but, seeing that, as a general rule, they either maintain silence or pass over the statement in question with a plain negative, it is impossible to obtain definite evidence on this point.

There is a further point in this connection. In S.P. sittings the convinced sitter is apt to give more encouragement to Topsy than the sceptic, and there can be no doubt that this factor influences the quality of the sitting to a large degree. Topsy is very susceptible to the attitude of the sitter, whether sympathetic or the reverse, she is easily discouraged by repeated failure to recognise her statements as veridical. She sometimes says that she cannot go on if the sitter does not recognise any of the persons she describes.

It is possible that this absence of encouragement which must necessarily occur in A.S. sittings is responsible to some extent for the consistently lower percentage of veridicality which is found in them as compared with the S.P., although it cannot be held to be the sole cause of the phenomenon.

Again, a convinced sitter is more liable to give away information than is the sceptic; an example of this may be found in one S.P. sitting, which had to be rejected as entirely valueless owing to the fact that the sitter appeared to give away to Topsy more information than he received from her.

Apart from these considerations there seems little ground for thinking that the class of sitter has any relation to the quality of the evidence. No doubt there are certain characteristics which constitute a good sitter, but they are not those on which the present classification is founded.

From the data available it has not been found possible to make any suggestions as to what these characteristics may be.

Out of seven good sitters, selected as having obtained the most consistently high score, three are convinced, three must be regarded as being more or less sceptical, and one is doubtful. Of the three convinced one is emotionally bereaved. Two of the best sitters are rather markedly of a critical frame of mind, having considerable knowledge of psychical research, and being thus able to estimate the value of the evidence as it is given. This capability would tend to check any great emotional excitement during the sitting, such as might be aroused in a sitter of less critical disposition.

Out of six moderate sitters, two are convinced, one sceptical, three doubtful. Out of five poor sitters, one is convinced and emotionally bereaved, two sceptical, two doubtful.

Thus there seems to be no constant relation between the quality of the sitter as good, moderate and poor and the characteristics under which they have been classed, viz. convinced or sceptical, emotionally bereaved, and so on.

Amongst the sitters were seven who were known to possess some psychic faculties. Of these seven, two were definitely good, two were moderate but nearly good, one was poor but nearly moderate, one was definitely poor, and one a complete failure. Here, again, there seems no constant relation.

The next point to be considered is the relation between the quality of the A.S. sittings and the S.P. in regard to the sitter. Here, again, we can find no constant relation.

There is only one instance where the sitter is consistently good in both A.S. and S.P. sittings, but unfortunately the number of A.S. sittings was small. With

the good S.P. sitters we find fairly numerous instances of complete failure in A.S.

In no case can it be said that the A.S. sittings with any sitter (except that just mentioned where the number of A.S. sittings is too small to judge from) are consistently good, though in some cases they are consistently bad.

It may be concluded then that the conditions which determine the success or otherwise of the A.S. sittings do not relate to the characteristics of the contributor of the relic, considered as an absent sitter.

(b) *The notetaker.*

There were only four different notetakers employed in this series of sittings; it is, therefore, impossible to draw any definite conclusions as to any influence exerted by this factor. There is, however, some slight suggestion that such influence does occur. The fact that Topsy fairly frequently refers to the affairs of the notetaker seems to show that she is not entirely indifferent.

In the case of one notetaker Mrs. Elliott expressed the opinion that she was too "positive" in character to be ideal for the purpose, and it is noteworthy that we find considerable evidence in some of the sittings at which she took notes of the intrusion of her own ostensible communicator. She proved also to be one of the best sitters.

There are numerous instances of references made to current events connected with the notetaker, and this seems to occur quite irrespective of whoever may be acting as such at the time. They look like examples of sporadic telepathy or clairvoyance (to use two indeterminate names for a faculty for acquiring supernormal knowledge), and are probably analogous in character to the numerous references to Miss Newton's recent thoughts or actions obtained through Mrs. Elliott and Mrs. Leonard. (Some of these have been published in *Proc. S.P.R.* Vol. XXXVI., p. 190 ff.)

Of the four notetakers the bulk of the work was done by two of them, the others acted on six and seven occasions respectively.

The following are some figures concerning the various notetakers in A.S. sittings.

<i>Successful sittings.</i>		<i>Failures.</i>				
Miss H. -	- 23	Average score	46%	19	Total	42
Miss W. -	- 6	„	„	35%	13	„ 19
Mrs. D. -	- 3	„	„	36%	3	„ 6
Dr. W. -	- 3	„	„	37%	4	„ 7
Dictated by						
Mrs. Elliott	3	„	„	64%	3	„ 6

(c) *The nature of the relic.*

There are very few instances in which the nature of the relic appears to exert any influence over the statements made at the sittings. In one case, where the relic was a dog-collar, Topsy talks a great deal about dogs, but gives nothing which can be recognised as veridical. Before going into trance on this occasion Mrs. Elliott remarked the object felt like a slave bangle.

A large number of the objects used as relics are of such a nature that they give no indication of the type of person to whom they had belonged, even whether male or female, *e.g.* such things as purses, eyeglasses, letters, pens, pencils, and so on. Some are definitely masculine, such as cuff-links, ties, etc.; some definitely feminine, as pendants and bracelets. While there are many instances where the sex of the ostensible communicator corresponds with that of the original owner of the relic there are others where this is not the case, for example, in A.S. 40, where the article was a pair of gold cuff-links, and the communications purported to come from an elderly lady.

It may be concluded with a fair degree of certainty that the nature of the relic has little if any bearing on the matter given at the sitting. Mrs. Elliott has some definite objections to certain types of article, for example, she does not like knives, though the reason for this is not obvious. She also does not care for written matter, probably because there might be a suggestion that she obtained normal information by surreptitiously reading it. It may be remarked here that she is most scrupulous to

avoid any possibility of obtaining normal information. Good results have, however, been obtained from such articles as letters, but in no case is there any reason to think that the statements made at the sitting bear any relation to the contents of the letters. She also objects to any article which has been often washed or handled by a great number of people. She probably holds what is called the psychometric theory, that is to say, the theory that physical objects retain traces from which a sensitive can derive information concerning their past history, which would account for her prejudice in this respect.

In some cases the package in which the relic was enclosed was not opened by the medium, and good results have been obtained. It is not always possible to say whether the nature of the article could have been judged by feel alone in these cases, but in one case at least the article was such that the sense of touch could not have given any definite knowledge of its nature. The sitting in question, A.S. 16, gave a high percentage of veridicality, the relic being a knitted necktie. This is most probably a masculine article, but it is hard to believe that the mere feel of it through the packing would be sufficient to enable anyone to say more than that it was some comparatively small soft article, possibly knitted. For example, it might have been a necktie, which is masculine, or the waist-belt of a knitted silk jumper, which is feminine.

Sometimes Topsy starts with the package unopened, and then, finding that she does not get along very well, opens it. The instances in which this happens are not sufficiently numerous to enable it to be determined whether the opening of the package makes any difference.

(d) The use of the same article at two different sittings.

There were two instances where the same article was used twice at different sittings. It happens, unfortunately, that in one of these, while the first use of the relic gave a good amount of veridical matter, on the second occasion an intrusion occurred which occupied the entire sitting.

In the other case both sittings were entire failures, nothing being recognised by the contributor of the relic, nor with the exception of a possible reminiscence in the second sitting can any intrusion be detected.

The correspondence between the two sittings was very slight, in fact it may be considered as stretching a point to say that there was any correspondence at all.

The following are the only points in which there is the slightest resemblance :

A.S. 32.	A.S. 60.
Old man (and old lady)	Old man
fairly tall	just below average height
very broad, heavily built	inclined to be stout
face fairly long and broad	fairly big face
fairly big nose, little bulbous	long nose
blue or blue-grey eyes, sparkling	blue eyes
eyebrows fairly well marked	well marked
forehead high, not wide, lined	forehead high
grey hair, bald on top	grey hair, going little bald
mentally very alert, very clear	keen intellect
difficulty walking and getting about, used stick	lame
William	W.

The above descriptions are very vague, and might apply to thousands of old men: they do not correspond in several details, such as height.

(*Note.*—Mrs. Elliott herself admits that she cannot always judge heights; this may be due to the fact that she appears to derive the descriptions from some kind of visual hallucinatory image, which varies in apparent distance from her. Thus an image which appears to be some distance away might be judged to be of a shorter person than one which came close.)

Even the more definite detail of lameness does not quite tally—the man in A.S. 32 is said to have difficulty in walking, and used a stick, which may have been due to infirmity of age, whereas in A.S. 60 he is said definitely to have been lame.

None of the other details given or incidents referred to in either sitting correspond in the slightest.

It may be said that there was no real correspondence between the two sittings.

It may, however, be noticed that the relic, a gold chain bracelet, was obviously of a feminine nature, yet in both sittings the principal ostensible communicator was an old man. At the first sitting the package was not opened by the medium, in the second it was opened and she remarked that it obviously referred to a lady. Both sittings are normal. The evidence, then, on this point is far too slender for any opinion to be formed; what little there is is mainly negative in character.

(e) *The condition of the medium as regards health, external conditions, etc.*

As might be expected these factors appear to have some influence on the quality of the sitting, though the amount of that influence is rather surprisingly small, in fact some of the best sittings have been given when the medium has been tired and disturbed by noise.

External circumstances such as noise may disturb her, but there is no instance of the phenomena being entirely inhibited by such causes; probably the occurrence of noise was not very frequent, so that opportunity of judging on this point is small.

As a matter mainly of guesswork, but based on faint indications in the available data, it seems likely that the effect of external disturbances is more marked at the commencement of a sitting, for example, it might hinder or even prevent trance, but when the trance state is fairly established the effect is less.

In three cases where the medium is stated to have been depressed or nervous complete failures have resulted.

Topsy sometimes makes passing remarks on the health conditions of the medium, speaking of her as of a third person.

Probably of greater importance than either of the above factors is the attitude of the medium towards the sitter in S.P. sittings.

Mrs. Elliott is sensitive to psychological conditions, and readily detects an unsympathetic atmosphere. If she likes a sitter the chances of a good sitting are increased, and the reverse holds true. Her liking of the sitter, however, does not guarantee a good sitting.

As before remarked, Topsy becomes discouraged if the sitter does not recognise anything she gives; a little judicious praise spurs her to increased efforts, and she appears to appreciate any expression of friendship towards her. See the "Stockings" incident quoted on page 87.

It would be of great value if it could be determined whether Topsy's moods bear any relation to those of the medium, but—unfortunately evidence is almost entirely lacking on this point. Topsy (speaking, without prejudice, of her as a separate person) is a cheerful, good-natured little person, very friendly and sympathetic, vivacious and intelligent though not possessing perhaps so much humour as Feda. She is remarkably equable in temperament, and exhibits little variation in mood; she gets a bit depressed by failure, and says so quite openly; she is liable to resent any failure to greet her when she appears. She sometimes admits that she is guessing: this is paralleled in normal sittings when Mrs. Elliott says she is romancing.

As a rule questioning her is of no use, she replies that she can only say "what they gives"; sometimes, however, she does make an attempt to answer a question.

Taken on the whole there is nothing to show that Topsy is much affected by the physical or psychological condition of the medium, although, as already stated, the evidence in the matter is extremely small.

It might prove a fruitful line of research in future cases if the medium could be induced to furnish information concerning her state.

(f) Trance or normal condition in sittings.

Equally good results have been obtained under either of these two conditions. The decision which shall be employed at any sitting rests with Mrs. Elliott herself and, it would appear, is based on some subjective feeling. On one

occasion (A.S. 7) she remarks to the notetaker, "I am not going to try a normal sitting again with you," her reason for this being, apparently, that she felt the influence of the notetaker disturbing, and possibly preventing her from receiving impressions connected with the relic.

The following are figures concerning this point:

Trance sittings, 19 successes, average score 39%, 22 failures.

Normal sittings, 19 " " " 47%, 20 "

(These are for A.S. sittings, all S.P. are trance.)

There is clearly little to choose between trance and normal.

SECTION III. ASSOCIATION OF TOPICS.

The next point to be considered is the part played by normal association. It is characteristic of Topsy that she jumps from one topic to another with a somewhat disconcerting abruptness. In very few cases has it been found possible to detect any associative link which might determine the sequence of the topics. The same may be said, in perhaps a somewhat less degree, of Mrs. Elliott in her normal sittings.

The impression which one receives—and it is doubtless the impression which one is intended to receive—in reading the records is that Topsy (or Mrs. Elliott) is simply handing on matter received by her from elsewhere, or describing what is to her an objective appearance. How far this impression is probably correct will be discussed later. The word "probably" is used advisedly—in psychological research we cannot hope for certainties, whatever hopes we may cherish in other departments of knowledge.

The point of the absence of any apparent associative linkage between the various topics is of importance in any psychological hypothesis intended to cover the phenomena of origin and transmission of impressions by mediums. Were the linkage of the ordinary associative kind it seems hardly likely that no trace of it should be observable.

As an implement of research association might prove to be of the greatest value could its presence be detected in any quantity.

About the most individual parts of the entire mental furnishing of any person are his habitual associations, seeing that they arise from his unique experience. If it could be shown that the associative links in a series of sittings with one medium and various sitters all belong to the same set it would point to the mind of the medium as being the source of the impressions; if, on the other hand, any correspondence could be discovered with individual sitters not shared among them all it would point to an external origin.

Further, if it could be shown that the associations which governed the sequence of the topics in any case were the same as those known, from a knowledge of his history, to have been possessed by the ostensible communicator, it would be evidence, not conclusive perhaps but still good evidence, of the identity of that communicator and of his mind being the source of the information.

Unfortunately, in this case no such associative linkage has been discoverable; whether this is always the case in all trance communications is a matter for research. The matter is mentioned here solely as a suggestion for future use.

There are a few isolated cases where normal associations may be detected, for example, in A.S. 35 the name Joseph is followed shortly by the name Mary. In A.S. 21 we get "Something to do with William. They'se conquered something there." The association is obvious.

Some other cases will be quoted when symbolism is dealt with. In the matter of symbolism association plays an important part.

SECTION IV. FUNCTION OF THE RELIC.

The function of the relic in A.S. sittings must now be considered, at least in so far as it bears on the purely psychological issue. For the larger part this question comes under psychical research proper, but it is one of the places where the two territories overlap. The facts are briefly this: (1) The medium handles an inanimate object. (2) She makes a series of statements purporting to be derived from spirits, *i.e.* the surviving consciousnesses of

deceased human beings. (3) In a number of cases these statements are true, or have a relevance for someone who is or has been in some way connected with the object.

As regards fact (2) we can leave the discussion of the authenticity of the statements until psychical research matters are being dealt with; the question now to be decided is: Is there any causal relation between fact (1) and fact (3)? *i.e.* Does the handling of the relic *enable* the medium to make these statements? The precise manner in which this effect is produced is not yet the point.

If there be no causal relation, then the relevance of the statements must be accounted for in some way. The only explanation, other than some unspecified causal nexus, seems to be chance.

The hypothesis of chance has been dealt with elsewhere, and the reader must form his own conclusions on what has been there said. If he is satisfied that chance alone is responsible for the phenomena, then there is nothing more to be said: all the phenomena are normal and require no special psychological treatment. If, on the other hand, he decides that chance alone could not produce these results, then he must allow that there is some causal nexus between the two facts. Beyond this the evidence does not enable us to go without trespassing on the territory of psychical research.

From the point of view of psychology it is sufficient to say that the handling of an inanimate object sometimes enables the medium in some way to make statements relevant to some person or persons who are or have been connected with that object, either directly or indirectly through the intervention of another person.

SECTION V. CAUSE AND MEANING OF INTRUSIONS.

Here, again, the main interest falls under psychical research. Evidence that intrusion proper does occur has been cited when discussing reminiscences (*q.v.*).

For the purposes of psychology it seems a matter of indifference whether the matter is veridical for a person connected with the relic or for an intruder, the psychological mechanism being probably the same in both cases;

for psychical research it may, however, be of the highest importance.

An intrusion sitting must be regarded as a complete failure considered as an ordinary A.S. sitting, the causal nexus above referred to having failed to be established.

It may be that the intrusion itself spoils the sitting and, to use language which must be taken as figurative and not as implying any hypothesis, the intruder butts in and monopolises the medium, thus destroying any chance for impressions from the relic coming through.

This is the view suggested by Topsy herself if we accept her words at their face value. She frequently complains of "spirits" refusing to go and taking all the "power." Mrs. Elliott in one normal sitting remarks that it must be someone who has "strayed in."

It is frequently recognised by both Mrs. Elliott and Topsy that the purporting communicator is not connected with the relic. There is, apparently, a symbol for this, viz. the spirit, *i.e.* the hallucinatory appearance of the purporting communicator, makes the gesture of waving it away.

Acceptance of this view would appear to entail acceptance of some form of spiritistic hypothesis for which many will not be prepared; the facts, however, can be described in such a way as to avoid any such implication. It might be said, for example, that there is a conflict between impressions arising from various sources, and that the success of one set in getting through prevents the rest from emerging.

The principal intruders are the communicators called A. and Mrs. Irving: the intrusions of Miss X.'s communicator take place as a rule at sittings where she acts as note-taker, and are therefore analogous to S.P. sittings: there is only one intrusion proper in her case. The other cases are one for Mr. S—, and one for a former sitter of Mrs. Elliott's not connected with this experiment at all. It is from Mrs. Elliott herself that we derived the knowledge of this last intruder.

It is perhaps suggestive that the intruders are all "good communicators." This may be interpreted as

lending support to some form of spiritistic theory, but against this it must be borne in mind that an intrusion by a communicator who was not "good" might very probably not be recognised.

Both A. and Mrs. Irving are very persistent; the former is represented in a Leonard sitting as insisting on talking himself, and not giving the other "spirits" present a chance to say much. He excuses himself for this on the ground that he is the nearest to the sitter, so that the sitting was really his sitting. Both he and Mrs. Irving are quite experienced communicators, the latter particularly so.

SECTION VI. CAUSE AND MEANING OF CLICHÉS.

These phenomena are very obscure, and in the absence of any definite detailed information concerning the psychology, history and habits of thought of the medium it is difficult to form any hypothesis to account for them.

Several suggestions may be made, and it seems probable that clichés are not all of one kind, but spring from a number of different causes. Some, for instance, look like symbols of which the meaning is not clear; as an example of this the numerous references to boots and shoes may be cited, for in one case Mrs. Elliott says, "He is wearing heavy boots, perhaps it means that he lived in the country." Others may be fragmentary reminiscences from other sittings, *e.g.* lameness and injury to the foot or leg, one of the communicators having been a man who had lost a leg. Probably some arise from normal associations in the mind of the medium: of these "rice" is perhaps an instance, the apparent association being with India, and thence with any hot country. These would belong to the larger class of recurrent imagery in the dream stuff, or rather the hypnagogic stuff which, as has been suggested, forms the background of the whole thing.

In the study of hypnagogic phenomena made by Mrs. Leaning, and published in *Proceedings*, May 1925, it is clear that some people tend to have a certain well-defined type of image, as, for example, faces or geometrical figures. It may be that this tendency extends beyond

the reproduction of a particular type, and in certain cases includes individual images.

One hears many stories of recurrent dreams, and possibly some of these stories may be true; anyhow it is not inconsistent with what is known of the origin and mechanism of dreaming to suppose that certain topics will tend to be reproduced with greater frequency than normal. It seems probable that if it had been practicable to submit the medium to a course of psycho-analysis, in particular, to obtain a series of free association tests, some light might be thrown on the source of these clichés.

There is one particular cliché which exhibits the type very well. On five occasions Topsy (or Mrs. Elliott) mentions a ring with black and an animal standing up on it; this is presumably a signet ring of some sort with a dark stone with a figure of some animal engraved thereon. It is mentioned in connection with five different sitters, and is veridical for none. It may be a symbol—if so its meaning has not been discovered, even by Topsy herself; but more likely it arises from a memory in the mind of the medium which has somehow or other acquired a set of associations, possessing numerous links with the type of matter which is given in the sittings.

The following is a list of the more important of the topics which were indexed for the discovery of clichés.

The numbers are the number of the references in the whole series.

Anniversaries, 22.	Broken or worn-out boots, 8.
Breathlessness, 21.	Carpet or cloth with hole in it, 6.
Chains, 17.	Corn, 12.
Cough, 40.	Old-fashioned dress, 6.
Music, 48.	Desert country, 8.
Pictures, 59.	Digging, 10.
Rings, 28.	Graves, 8.
Shoes or boots, 29.	Someone achieved something, 13.
Writing, 26.	Beads, 12.
Going to or across water, 32.	Embroidery, 9.
Leg or foot hurt, 47.	Paralysed, 20.

Some of these, such as anniversaries and pictures, are commonplace references which would naturally tend to occur with considerable frequency; some, as before remarked, are probably symbols; for example, the references to corn are almost certainly symbolic either of a connection with the country rather than town (Mrs. Elliott is essentially a town-dweller) or of a season of the year.

SECTION VII. GENERAL CONCLUSIONS.

An attempt must now be made to gather up the scattered threads and form some sort of provisional theory of the mechanism of these phenomena. This will fall under three headings. 1st. The mode of reception of the impressions. 2nd. The mode of internal transmission, *i.e.* the process which the impressions undergo between their reception and their delivery by Topsy or Mrs. Elliott. 3rd. Some theory to account for the source and nature of the non-veridical matter.

Concerning the first, if it be accepted that some part of the communications consist of veridical impressions having relevance to the sitter, the contributor of the relic or deceased persons connected therewith, and that the amount of this is beyond what could be attributed to pure chance, the matter given in the sittings must be distinguished as being of two kinds, *viz.* internally generated and received from outside.

It is with the latter kind that we are now concerned.

The question of its source is the subject of psychical research, and will be dealt with in the second part of this report—for the present it may be ignored. The point now under discussion is the bare fact that the medium receives impressions from some external source, and we want to form some theory of the manner in which this takes place.

It is pretty clear from the evidence contained in the records that Topsy and Mrs. Elliott obtain much of their information from a sort of visual hallucinations; for example, the physical descriptions are given as though taken from a picture or actual figure, the movements of the ostensible communicator, rooms, articles of furniture,

and so on, described. There are also some impressions which are obviously auditory, such as names and initials, and, very occasionally, words which purport to be spoken by the ostensible communicator. There is a marked difference in this respect between Topsy and Fedra. The latter gives the impression that she is listening to someone talking to her, for she sometimes exchanges remarks with an invisible person. Topsy, on the other hand, seems to be watching an invisible actor who is trying to convey his message by pantomime. In neither case, of course, is this invariable: Fedra does get information conveyed pictorially and in symbols, and Topsy does hear sounds, that is to say, they purport to do so.

It is probable, moreover, that some of the impressions are neither visual nor auditory, but rather, to use a word much employed by Fedra, inspirational.

The first question, therefore, that arises is whether these hallucinatory images, whether visual or auditory, are the external source from which the information is derived? It would appear not, although it is clear that mediums in general hold the opposite opinion, an opinion which tends to favour the spiritistic hypothesis.

The hypothesis advocated in this paper is that the impressions, other than those internally generated, are received by a stratum of the subconscious mind of the medium, and exteriorised as visual or auditory hallucinations in a manner similar to that employed in the generation of phantasms. If this be accounted as provisionally acceptable it implies the existence of some process of internal transmission, and any evidence for such a process which might be found would be evidence in favour of the hypothesis. This brings in the second of the theoretical questions.

The fact bearing most nearly on this matter is the extensive employment of symbolism. The following are representative examples:

S.P. 26. "Me doesn't think the second one related, or else you didn't know him or something, him doesn't come near you," (Symbol for relationship, etc., is "coming near.")

S.P. 11. "You nearly married and then not married. Shows Topsy like wedding dress and then sort of drops it." (Symbol is obvious here.)

S.P. 15. "Shows Topsy like symbol like all black there."

A.S. 29. "Points to him and waves his hand." (Symbol for not being connected with the other "spirit" who is there at the time.)

A.S. 29. "Shows Topsy like something being dug up." (Meaning unknown.)

A.S. 29. "Doesn't know whether that gentleman was going to water and didn't go. Points to water and shakes his head." (Symbol obvious.)

S.P. 52. "Well, Topsy me doesn't know what him means, him shows symbol like you coming out above it, it's going to drop away, that condition." (Refers to some trouble the sitter had been having.)

S.P. 17. "Doesn't know why he keeps on giving Topsy such a lot of black. Doesn't know whether it's worry or somebody crossed over." (Obvious.)

The following incidents show how the true impression gradually emerges from the symbols. In A.S. 90 there is a non-veridical reference to India or "some hot place across water where there's lots of dark people," then comes some irrelevant matter, followed by the words, "Remember palms." Topsy then says, "Doesn't know what palm looks like. And then him laughing and says, where bananas grow." More irrelevant matter is interposed. Then comes, "Him showing Topsy lot of rice and him laughing. There must be joke about lot of rice; 'more rice, never tired.'"

The annotator says:

He was very fond of rice puddings and often said he would willingly have one at both meals every day. He would often say when we asked what pudding he would like, "You can give more rice, I never get tired of it," or words to that effect.

There would appear to be a connection in Mrs. Elliott's

mind between rice and India. It is easy to trace the gradual emergence of the true impression concerning the rice pudding: first, the symbol calling up India, then, by association, Palms, Bananas, and finally Rice; eventually the correct meaning, that is to say rice as a pudding and not as connected with the tropical country where it is grown.

A somewhat similar incident occurs in S.P. 3 and S.P. 16, both being with the same sitter. In S.P. 3 Topsy says, "Somebody gone to hot country connected with you," and "Me thinks it's a lady," "Him shows me lots of white, do you understand?" then "Is it sister to you?" (this was correct), "But doesn't know why it's happy condition there, for me thinks she murmur sick, some lady sick," "She getting married?" Then a little later, "Me thinks she took lot of white things with her, that is what it is." The last few words seem to give the clue to the whole thing. The white things are a symbol for a hot country or for getting married, and, possibly through association with the white uniform usually worn by nurses, with sickness.

This is confirmed by S.P. 16. Reference is made to a lady who had gone a long way away. Then "She been ill, or something to do with ill people." Then Topsy asks if it is the sitter's sister or sister-in-law. After some irrelevant matter, she says, "Does she always dress white?" "Shows white," then:

Him (referring to ostensible communicator from whom all this purports to come) been to India? Well, what's he say India for? Him said India. Doesn't know whether your sister been to wedding, or what it is, gives something mix up over wedding.

The only veridical impression behind all this is the fact of the sitter's sister having gone to a hot climate, viz. Kenya. The symbol seems to be the white clothes; Topsy recognises this when she says, "That is what it is."

One more instance will be given. It is interesting as showing a mixture of veridical impressions. In S.P. 46

sitter's mother is the ostensible communicator. Topsy says :

Doesn't know whether that lady got two boys or you got two boys. Keeps speaking of two boys.

SITTER. There were two boys ; she had two boys.

Well, they was related to you, must be.

SITTER. Yes.

Did one cross over before her and one after ?

SITTER. No.

Somebody crossed over just before her, a man.

SITTER. Yes.

And then after, but not very soon after her.

SITTER. That's right.

The facts were that the sitter's mother had twin sons who died in infancy, and a brother who died just before her, and a nephew who died after her. The confusion appears to be caused by the two impressions each concerning two male persons connected with the ostensible communicator who had died. It looks as though the impression concerning the two infant sons who had died called up by similarity the two other relatives whose deaths were connected with her own by proximity in date.

It will be observed that the symbols are all of a certain type. They are what might be called natural symbols, and are based on habitual analogies, either verbal, as for example when the hallucinatory figure coming near to the sitter is taken to mean nearness of relationship, or common forms of speech, as when all black is used as a symbol for worry or sorrow ; or else they may be natural pantomime, as when the gesture of waving away is interpreted as meaning that the ostensible communicator was not connected with the relic.

This feature should be contrasted with the symbolism employed by Feda. In her case the evidence available is much more scanty ; if symbols are largely used by her the fact is not so apparent as with Topsy. This may be due to the more developed condition of Mrs.

Leonard's mediumship, and a study of the records of her earlier sittings might reveal a larger utilisation of symbols than shows at the present time.

However this may be, the symbols in Fedra's case, in so far as they can be discovered, appear to be of a different nature from Topsy's; they are purely arbitrary and artificial. For example relationship is sometimes symbolised by a pink line between the appearances of the two persons; a circle of light means that the person in connection with whom it appears is helping somebody.

That Fedra does receive a good deal more of her information by means of symbols than appears on the surface may be suspected from an incident happening in a sitting with Mr. Irving (not of this series) reported in *Proceedings*, Vol. XXXVI., page 218. Fedra says "the test is mackintoshes and umbrellas, test is." Sitter. "Is what?" Fedra. "Means that. No she says water tight." This looks like a case of a mistranslated symbol. Probably a search would discover more instances. In this connection Mr. Drayton Thomas's article in *Proceedings*, Vol. XXXVIII., is important.

Now there are two alternatives: the impressions may be received by the medium in a symbolic form, in which case the process of symbolisation is an external one; or else the translation into symbols is effected internally in the receptor stratum of the medium's consciousness, or at some point between that and Topsy (or, when there is no trance, Mrs. Elliott).

If the facts are taken at their face value the first of these alternatives is suggested. Topsy always speaks as though the communicator were using symbols; thus she will say, "Doesn't know what him's giving but him's showing Topsy so and so."

This view, if adopted, lends considerable weight to the spiritistic hypothesis, for if the process of symbolisation is effected externally to the medium, it appears as if there must be an active, intelligent independent source from which the information is derived. This, combined with the memory necessary for the supply of the information, is a sufficiently good description of a consciousness of the

same type as the human, so that the source might well be the surviving spirit of a deceased person. Personal identity is, of course, not proved, but a long step in that direction is taken.

Doubtless it is some dim appreciation of this reasoning that leads mediums to use language which suggests the view that symbolism is of external origin. It is not suggested, in Mrs. Elliott's case at any rate, that this is consciously or deliberately done, but it seems reasonable to suppose that the personal convictions and mental attitude of the "normal" medium will be, in some measure, reflected in her trance utterances.

But the evidence, on closer scrutiny, seems to show that the symbolism is not of external origin. It is found as a fact that Topsy and Mrs. Elliott have one set of symbols which are in use for all sitters and communicators. Feda has another different set, though she sometimes uses the same symbol for relationship as employed by Topsy, viz. the figure of the ostensible communicator coming near to the sitter. If the symbolism were externally generated we should expect to find a communicator using the same symbols with both Topsy and Feda; moreover, it is extremely unlikely that all communicators should hit upon the same symbols in communicating their information—individual differences would be bound to occur. In the case of Feda in particular it is highly improbable that different communicators should all hit upon the same arbitrary symbols.

In view of these considerations the more tenable hypothesis seems to be that the translation into symbols is effected by the medium herself, that is to say by some stratum of her subconscious mind. That analogous processes of symbolisation are of frequent occurrence the researches of Freud and Jung have abundantly proved. Whatever attitude may be adopted towards their special theories, there can be no question that they have shown that symbolisation plays a large part in the providing the material of dreams. We have already seen reason for allying the phenomena of trance communications with the normal dream, or rather with hypnagogic visions; it

is, therefore, only a natural corollary that symbolisation should play its part in both.

That that stratum of the medium's consciousness which receives the impressions, called the receptor stratum, is not the same as that stratum which is personified as Topsy, or, if Topsy is considered to be an independent entity, is functioning when she is in control, is proved by the fact that symbols are frequently not understood. She sometimes says definitely that she "does not understand the symbol he is giving." The groping which has already been noticed takes place on these occasions. Topsy frequently suggests alternative meanings. Some instances of these were given on p. 50, particularly the one quoted from S.P. 17, where the symbol of black means either worry or the death of someone.

It is a common feature of trance communications that there seems to be great difficulty in getting through proper names. This fact fits in with the theory here suggested. Proper names are, as a rule, not amenable to symbolisation: they have denotation, but no connotation. It seems probable also that this feature renders them difficult to transmit by telepathy. Judging from Topsy's attempts to give names, and the mistakes she makes when doing so, there are two methods used to convey them to her; the more common is the auditory, but she also sometimes appears to get the letters as a visual appearance. In the former case she will make mistakes in pronunciation, *e.g.* in A.S. 40 she gives Rubecca, then correctly Rebecca, or she will give alternative letters having the same sound, such as B. and P., and remark that she does not know which is right.

It seems that the readiest method of conveying impressions from the receptor stratum is by means of pictorial imagery, and this constitutes a good reason for the extensive employment of symbols. Where this method fails, or for some reason is not practicable, auditory impressions may be used. The article already referred to in *Proc. S.P.R.*, Vol. XXXVIII., July 1928, by Mr. Drayton Thomas should be consulted on this point.

In view of the above it is submitted that the following conclusions may be accepted as provisionally established.

(1) The impressions are received by a stratum of the subconscious mind of the medium other than that functioning as Topsy or with Topsy, *i.e.* the trance consciousness.

(2) They are transmitted from this receptor stratum largely by means of symbolic visual imagery. A smaller part is transmitted as auditory impressions.

The question now arises whether the impressions are already in the form of symbols when received by the receptor stratum.

If this were the case there would have to be a translation from the symbols in which they were received into their meaning, and then retranslation into another set of symbols.

While it is not impossible that this double translation may occur, it seems rather complicated and round-about. If a simpler hypothesis can be found it should be preferred.

The hypothesis here suggested is that the impressions are received as "meanings," that is to say, not expressed in words, pictures or any other form of symbols.

A point in support of this theory is that the medium frequently reproduces in herself the conditions of the ostensible communicator; for example, in S.P. 20, where the communications purported to come from a man who had died from a tumour on the brain, she complained of severe headache on waking up from the trance; in many cases she speaks as though she actually felt the conditions she describes. These conditions, or rather the impressions concerning them, would be of the type which is not habitually symbolised, nor is it easy to see how symbols of the sort usually used could be found. These instances look like impressions received and transmitted direct.

In some of the experimental cases of telepathy there is reason to think that information is sometimes transmitted direct, without translation into words or other symbols, for example, the telepathic transfer of tastes, slight local pains, etc. In so far as there is evidence for this it

supports the theory now under discussion. The matter, however, is as yet by no means definitely settled, and further research might advantageously be undertaken. It seems not impossible to devise experiments which should, if successful, provide crucial proof on this point. If telepathic transfer of an idea not translatable into visual symbols could be obtained between two persons who had no language in common, we should have to adopt a very strained and round-about explanation to avoid the conclusion that a "meaning" had been sent direct.

We may then add a third clause to our hypothesis, viz. That the impression received by the receptor stratum is received as a meaning, that is to say the process is a purely psychological one—not expressed in language or other sensory impressions.

It may further be suggested—though this may be accounted as a somewhat fanciful extension which goes beyond the evidence—that the receptor stratum of the medium's consciousness lies below or beyond the point at which differentiation into the various senses occurs, that is to say a meaning would not be visual, auditory, olfactory, sapid or tactual, but just plain meaning. The mode of consciousness of this stratum would be analogous to that attached to the primitive unspecialised irritability of protoplasm, though it might be so far evolved as to be definitely human.

That some such vestigial remains of earlier forms of consciousness may be found in human minds seems not unlikely when one considers the analogy with the human body in this respect. Also, it cannot be denied that we do retain vestiges of instincts which have long ceased to have any value to us. However, the matter is so speculative that further discussion would be out of place.

There is one further point in connection with the reception and transmission of externally generated impressions. One of the most striking features of communications received through trance mediums is their disjointedness. This is particularly apparent with Topsy—she jumps abruptly from one subject to another with no observable associative links. Now if the information were received as

definite messages from an external source one would expect to find more continuity in the communications, and to be able sometimes to detect the associative links which lead from one topic to another.

Suggested Analogy with certain Physical Processes.

To account for this phenomenon the following conception of the mode of transfer of the impressions to the receptor stratum is suggested. The process is likened to the physical phenomenon called osmosis. If a tube containing a solution of sugar, having the end closed by a porous membrane, is immersed in water, it will be found that the water will penetrate the membrane, causing a rise of pressure in the tube. This is called osmotic pressure. The phenomenon occurs with a large number of different liquids besides that named.

It is suggested that the mind from which the information is derived somehow gets into contact with that of the medium, the place of contact in each case being a stratum of the subconscious mind. The precise nature, or to continue to use the spatial metaphor, the locality of this stratum in the case of the medium has already (see pp. 126-128) been suggested. Some parts of the contents of the source mind passes by infiltration into the mind of the medium and there is an intermingling of contents; the process, however, so far as we know, takes place in one direction only.

There is probably some amount of selectivity exercised by the source mind which tends to cause certain parts of its contents to pass more readily, so that we find occasionally something that looks like a definite message, but the bulk of the stuff which gets through appears to be fortuitously determined. This process may be imagined as consisting in the endowment of certain ideas in the source mind with a higher degree of conative energy, which gives them greater mobility than the rest, thus tending to bring them up against the dividing membrane more frequently and with greater velocity, thereby increasing their chances of getting through. This is, of course, going beyond the parallel of osmosis.

It is fully recognised that the language used in making this suggestion has been unblushingly spatial and material, ideas have been spoken of as independent things which move about in space, and the two minds concerned as though they were closed vessels. No doubt this would be considered as scandalous by most psychologists, except perhaps the more ardent of the behaviourist school, but it is intended as a highly allegorical picture and not as an even partially exact description.

Moreover, it is, after all, only an extreme instance of what inevitably is the case in psychology: our ideas of psychological processes are borrowed from the physical world, and psychological hypotheses are no more than analogies with those of physics.

Bearing this disclaimer in mind, and reserving the right to change the metaphor as often as seems desirable, we may proceed.

The infiltration of ideas from the source mind into that of the medium causes a rise of pressure in the latter, analogous to osmotic pressure. We may imagine the receptor stratum of the medium's mind as a vessel in which there is surging about a crowd of ideas striving to find a way out. Some of these are ideas belonging to the medium and internally generated; this would be the dream stuff or rather the hypnagogic stuff which, as has been suggested, forms the background of the whole thing; mixed up with these is a certain amount of externally generated matter, *i.e.* ideas derived from the source mind which have entered by the process of psychical osmosis. Seeing that the membrane dividing the latter from the former permits passage in only one direction, the only means of escape is therefore upwards towards the trance stratum, in this particular case towards Topsy.

Now the channels leading in this direction are not free and unhindered. We have seen reason for thinking that impressions of a certain type are most readily transmitted in the form of symbols, and it is suggested that the condition necessary for their emergence from the receptor stratum is the availability of suitable symbols.

Another physical phenomenon may be employed as an

allegorical picture of what may be supposed to occur. It is possible to super-saturate the air in a closed vessel with water vapour. This vapour remains in a state of suspension provided there are no nuclei available for its condensation. If nuclei are formed within the vessel, as, for example, by ionisation by ultra-violet light or similar means, the water vapour will condense on them and fall as a shower of drops. We may imagine the receptor stratum of the medium's mind as being in a state of psychical super-saturation, the whole crowd of ideas and impressions would remain in a state of suspension until suitable nuclei for condensation were found; such a nucleus would be an internally generated idea capable of acting as a symbol. When such appears, the externally generated impression is carried to the surface, externalised by Topsy, and retranslated from the symbols into her own words.

Or the suggestion may be put another way. The ideas in the receptor stratum are prevented from rising to the trance stratum by the normally high synaptic resistance of the neural channels. Some of these channels, however, will have a somewhat lower synaptic resistance than the rest, viz. those which have already been the path of nervous discharge.

Any idea associated with that of which the original discharge was the neural concomitant¹ will tend to pass along these paths. If, therefore, any impression received from the outside is able to seize upon a suitable symbol which has available through association an already existing path of low synaptic resistance, it will stand a chance of escaping along that path to the trance stratum.

Those impressions or ideas which are highly charged with conative energy may occasionally be able, in virtue of that energy, to force their way up against the high resistance of the synapses. This would be the case where a direct message is successfully communicated.

With regard to the type of impression which appears

¹This word is used "without prejudice," and must not be taken to imply adherence to any form of psycho-physical theory, whether of the parallelist or interactionist type.

to be capable of being internally transmitted without recourse to symbols, it will be observed that they are mostly of a relatively simple and uncomplicated nature. It is the complex physical events which require symbolism. The mind is habituated to deal with the former ideas in their pure form; it seems reasonable, therefore, to suppose that they would be likely to find suitable paths of low resistance available for them owing to the former passage of similar ideas, more likely, that is to say, than would any idea of a complex physical event.

Possible explanation of some of the phenomena observed.

It must be repeated that this is only a kind of allegorical picture of the way in which it may be imagined that the impressions are received and transmitted. By means of it, however, we can suggest an explanation of many of the phenomena observed.

1st. The disjointedness and lack of continuity in the communications. If success in getting through from the receptor stratum is dependent on the finding of a suitable path of low resistance we should expect to find this disjointedness. There would be no logical order in the impressions which came up, but a perfectly haphazard stream, as one here and one there succeeded in finding its path of escape.

2nd. The "spirits" do not tell us what we should expect from them. The salient facts of their history are frequently omitted and trivial details given.

If the process of reception is analogous to osmosis, and the receptor stratum receives its impressions from the outside by a kind of infiltration, then only a fraction of the contents of the source mind would pass into that of the medium, the salient facts might be left behind. Moreover, there is the further possibility that they might be unable to emerge from the receptor stratum even when they had succeeded in getting there.

3rd. The errors and mistatements which occur so frequently.

If the impressions which are received from the outside are carried up to the trance consciousness along with a

stream of internally generated hypnogogic stuff, the result is bound to be a mixture of false with the true.

The question of the source and nature of the non-veridical matter will be fully discussed presently.

4th. Deferred impressions.

These can be simply explained as impressions received from the outside which have failed during the course of the sitting to escape from the receptor stratum, and have remained latent therein until a favourable opportunity at a subsequent sitting has enabled them to arise to the trance stratum.

5th. Reminiscence.

If it be true reminiscence, it has its origin above the level of the receptor stratum, but it is possible that an impression in the receptor stratum may give rise to a communication on two different occasions. Here it becomes apparent how misleading may be the parallels drawn between physical and psychical phenomena. In what has gone before ideas or impressions were spoken of as individual things existing in their own skins, being in one place at one time and capable of moving to another place, in fact, as behaving as if they were particles of matter.

It is sometimes difficult to bear in mind that this is not really the case: an idea is not a thing, it is rather a process, nor must the word process be here understood in the physical sense; ideas and all psychical entities are entirely *sui generis*, and obey laws and are subject to conditions of their own.

It was probably tacitly assumed by all who have read the foregoing allegorical description that an idea having escaped from the receptor stratum is no longer there. This is not necessarily the case, it may have arisen to the trance stratum in the sense that it has given rise to a veridical communication, and yet it may still remain in the receptor stratum in the sense that it may at a future time give rise to a further veridical statement.

Perhaps the nearest analogy is to a flame; the flame of a candle may give rise to an unlimited number of other flames and yet remain where it was, unaltered by

having done so; only, in using this parallel, we must suppose that the flame of the candle has its own peculiar characteristics, and that it communicates these characteristics to all the other flames to which it gives rise. Or the analogy of mirror images might be employed. An object can give rise to any number of images if the mirrors are available.

From this it may be seen that similar communications may be made at different sittings arising from the same impression in the receptor stratum; these would be indistinguishable from true reminiscences, which are the reproduction by memory of communications already given.

6th. Clichés.

A theory of the origin of clichés may be founded on this speculation. If we accept the principle of specific substances at the synapses as the causes of the specific character of the neural discharge, we may suppose that some of the paths of low resistance leading from the receptor stratum to the trance stratum pass through arcs, the synapses of which have specific substances of such a nature that any discharge through these arcs gives rise in consciousness to a certain idea. As an example, take the cliché cited before, viz. the black ring with an animal standing up. We must suppose that there is a neural arc with synapses having the specific substance which gives rise to this idea, and that the resistance of this arc being relatively low it forms an easy path for the escape of ideas in the receptor stratum, and is therefore likely to be frequently used by ideas having the requisite similarity.

If the principle of specific synaptic substances be not accepted, or if this be considered an undue extension of the principle, we can simply say that clichés arise from peculiarly strong associations in the mind of the medium such that they tend to be frequently excited, leaving the *modus operandi* of the excitation unexplained.

In suggesting this hypothesis to account for the reception and transmission of impressions from external sources the writer is fully aware of its fanciful and speculative nature. It is crude and undigested. It must be left

to the readers to judge whether it be provisionally acceptable or not; if the former, further research is indicated as desirable in order that the hypothesis may be modified, refined and rendered more definite; its details will doubtless require much amendment, and probably more suitable language and more apt parallels may be found.

If it be rejected an alternative is still to seek.

Probable origin of Non-veridical matter.

There now remains only to give some account of the probable origin and nature of the non-veridical matter.

It arises from several sources, most of which have already been discussed in connection with other matters, such as reminiscence, etc. It will suffice, therefore, if they be simply enumerated in this place.

First in order of magnitude must be placed Dream or Hypnagogic vision. Probably the largest part of the non-veridical matter is of this nature. Of those which follow it is impossible to say which is of greater importance. They are reminiscence, deferred impression when unrecognised, intrusion of unrecognisable communicators, mis-translation of symbols. Concerning this last it may be advisable to say a few words.

It is not only that Topsy does not always know the correct interpretation to put upon a symbol, but symbols are sometimes mistaken for non-symbolic messages. This is probably the case, for example, with the frequent references to digging. The visual hallucination of the ostensible communicator digging is almost certainly symbolic, but we do not know of what. As Topsy evidently shares our ignorance she simply gives it as a statement of fact.

It is, of course, all more or less a matter of surmise, as there is no certain indication of what is symbolic and what not. However, it seems probable that this constitutes an occasional cause of error in the communications.

Sometimes it is obvious that Topsy does not understand, in fact she says so herself; this has been discussed under "Groping."

In addition to the above sources of error it may well be that the receptor stratum of the medium's mind picks up stray impressions unconnected with the sitter or contributor of the relic; that this does sometimes occur is shown by the fairly frequent references to the notetaker and her affairs; probably also the references to Miss Newton are of this nature, though here the matter is complicated by the distinction which must be drawn between information received telepathically and that obtained by clairvoyance. This, however, comes under the heading of Psychological Research. Whether there is any truth in the suggestion of broadcast telepathy this is not the place for any attempt to determine, but should it be a *vera causa*, that is to say, should there be loose telepathic messages wandering about and liable to be picked up by any suitably tuned receiver, this would constitute a further cause of error, though were all telepathy broadcast the confusion arising would probably be so great that the percentage of veridical matter in A.S. sittings would be lower than it is.

If the suggestion of the nature of the mode of reception of the impressions be correct, that is to say, the theory called psychical osmosis, it seems probable that a good deal of error may arise from the mixture of the stuff in the receptor stratum. We might expect to get composite images, and as a fact these do sometimes appear to occur; a symbol might serve for two or more impressions, items might be ascribed to the wrong communicator, gaps in a description either of a place or person or event might be filled up with material drawn from other sources, such as the hypnagogic stuff contained in the receptor stratum, non-relevant externally generated impressions or Topsy's own exuberant fancy.

And last but not least there are the errors arising from the personal idiosyncrasy of the annotators. This cuts both ways. Some annotators are so ingenious in finding correspondences that their results require a heavy discount, others are so refractory that they will not see anything but the most direct hits.

This, of all sources of error, is the most difficult to

deal with, and it is a difficulty that constantly occurs in all forms of psychical research. It would seem that the only possible method of eliminating this error is to multiply the number of cases, sitters and annotators, to such a degree that the individual variations average out. It is obvious that the number dealt with in this experiment falls far short of that required. A certain amount of compensation can be applied by the judgment of the scorer, but this is, of necessity, very imperfect, and introduces a further source of variation, viz. his own personal idiosyncrasy.

It may be argued that the error introduced by the personal factor in the annotation will tend rather to reduce the score than to increase it, and this independently of the proportion which the credulous annotators bear to the sceptical. The scorer is bound to criticise the hits, and in a good many cases to apply a severe discount to the enthusiasm of the too ingenious annotator. As a matter of fact, in this case quite a number of sittings have had to be rejected altogether or treated as valueless because the annotations appeared to be untrustworthy, far-fetched or fanciful; in some of these sittings there was unquestionably some amount of veridical matter, but it was impossible from the data available to determine how much.

Except in the case of a simple statement a plain "Yes" is not deemed sufficient. Where an incident is annotated as veridical some account of the actual event is required, and the scorer has to judge how far the annotation can be accepted. Any alteration is always a deduction.

On the other hand, with those annotators who err in the opposite direction the data for the scorer by means of which he could criticise the annotations are entirely lacking. As a rule the annotator simply passes by without comment those statements which are rejected as non-veridical, or is contented with the simple assertion of their irrelevance. To criticise this negative evidence the scorer would require an intimate and highly detailed knowledge of the past history of the annotator, which, of course, he does not possess.

In a few cases it has happened that statements have been rejected in one sitting which, taken in the light of the facts given by the same annotator in another sitting, should have been counted as veridical.

It also happens sometimes that the annotator, on thinking over again the statements made by the medium, perceives the relevance of some which were previously rejected. We have no guarantee that these second thoughts are always given, or that in the event of their failure, third or fourth thoughts might not succeed. What this amounts to is that lapse of memory on the part of the annotator is liable to act in the direction of reducing the score.

Probably false memory does sometimes operate in the opposite direction, but the magnitude of this factor would be less than that of plain forgetting.

A suggestion is made in an appendix to this report of a possible means by which this and similar errors might be to some extent counteracted.

Before concluding this part of the report two interesting but extremely puzzling incidents must be mentioned. They illustrate very well the perplexities which assail the psychical researcher, and the difficulty of finding hypotheses to cover all the facts.

In A.S. 90 Miss X.'s communicator intrudes. The physical description is good in parts, but contains a fair number of inaccuracies: by itself it would be insufficient to establish identity. His Christian name and that of his daughter is correctly given; the first is so common a name that it has no evidential value; the second, though a usual name, is not what would be called common, it occurs only on two other occasions in the whole series. A few commonplace veridical remarks are made, then, after a good deal of irrelevant matter, there comes the "More riece, never tired" incident. Some more veridical statements are made, and a good deal of non-veridical matter concludes the sitting.

This may seem a slender basis on which to assume an intrusion, although two or three of the statements are of the definite class. If it had stood alone it would have been rejected, but seeing that there are other instances

of intrusion with this communicator it was provisionally accepted as such.

The following non-veridical correspondences, however, confirm this conclusion, and it is the nature and cause of these that constitutes the puzzling problem now in question.

Before the "rice" incident there was a non-veridical reference to India, etc. The communicator was then said to have been in the habit of reading in a "funny language, funny writing, looks like no letters at all." Later he is said to have been interested in the stars; the name Geraldine is then given in connection with music.

All these references are incorrect for this communicator.

The date of the sitting was June 17, 1927. The note-taker was Miss H.

On November 17, 1927, Miss X. had an anonymous sitting with Mrs. Dowdall at Cardiff, at which her father purported to communicate. Some amount of evidential matter was given, and then the medium said, "Could your father speak languages?" SITTER. "No." MEDIUM. "What are those funny letters then? I see him writing like little crooks, funny letters, twists and turns. Almost like shorthand but it isn't that." In the same sitting it was also said that he was interested in the stars; and at a previous sitting, held the day before, with the same medium, the name Gerald was given in connection with music.

There is thus a good correspondence between the sittings, but the matter is all non-veridical.

So far as can be ascertained Miss X. had not seen the record of the first sitting when she attended the second set. This point is, unfortunately, not quite certain.

It is difficult to suggest any explanation for these facts—the chance that two independent mediums should both have hit on the same set of fictitious statements is practically nil. The only hypotheses which can be put forward to explain the facts are all so complicated, and involve such an amount of cross-references, that they are practically untenable. For example, we might suppose

that Miss H——, the notetaker in the Elliott sitting, unconsciously transmits telepathically the information to Miss X., although she was ignorant that the sitting had any reference to her. Miss X. preserves this information in her subconsciousness, it never having come to the surface, and Mrs. Dowdall draws her information therefrom.

We can hardly put forward the explanation of mistranslation of symbols, because the two mediums are not likely to have the same set of symbols, and the chance that these particular ones should be mistranslated in both cases is very small. Moreover, the name Gerald or Geraldine would not be conveyed symbolically.

Those who hold the spiritistic hypothesis can, of course, explain it quite easily; they may say that the statements refer to some companion of the dead man who is with him on the spirit plane, the only mistake being in attributing them to him, though why the mistake should have been twice made is a little curious. Possibly other more plausible modes of explanation might be given on this hypothesis, but it must be remembered that the spiritistic hypothesis is so wide that it can be made to cover almost anything.

The second case is equally, if not more, puzzling.

In a series of sittings with a Mrs. T. as sitter, or contributor of the relics, there occurs a complicated set of correspondences of a non-veridical nature.

In A.S. 14, 24, 53, 72, and S.P. 39, reference is made to a tumble-down or ruined building; in A.S. 11, 24, and S.P. 39 to Egypt; in A.S. 33 and 42 to a deserted country; in A.S. 66 and 72 to Algeria; in A.S. 42 and 74 to mining operations; in A.S. 24 and 42 to the Navy; in A.S. 24 and 72 to an airman; in A.S. 42 and 72 to an organ. The name Arthur occurs in A.S. 33 and 42, Michael in A.S. 11 and 83, and Mitchell in A.S. 72, Roger in A.S. 14 and 24.

In six out of the twelve sittings reference was made to Miss Newton.

Some of these correspondences may be explained as being clichés; all the names are fairly common through-

out the whole series of sittings, references to "deserted country" are fairly common, to airmen quite common. But the others are not so explicable. They may be mistranslated symbols, seeing all the sittings are with the same medium, but the suggestion is not very satisfactory.

The wisest course seems to be to admit defeat and leave the matter as an unsolved puzzle.

The rather high number of references to Miss Newton is curious. With this sitter they occur in 50 per cent. of the sittings, in the entire series they occur in about 20 per cent.

There is a suggestion of a cause for this peculiarity in some of the sittings which Mrs. T. had with Mrs. Leonard (not included in this case). The ostensible communicator, Mr. T., states that he is working with Mr. Irving's communicator to give good evidence, and also that he will try to give it through the Society. In an early sitting, before Mrs. T. had joined the S.P.R., he purported to say, "Go to I. N., she will help." Neither the sitter nor Feda appeared to understand the reference.

Many of the references to Miss Newton are veridical, and show what seems to be supernormal knowledge of her affairs and incidents happening at the offices of the Society; there is also some correspondence with references given in a sitting which Mr. Irving had with Mrs. Leonard. The following are some of the references:

A.S. 11.

Miss Newton's Notes.

TOPSY. You are having more shelves or eupboards put up in Miss Isabel's room.

No, I suggested a few days before the sitting that we should have a long shelf put in the office.

NOTETAKER. I don't think so, I will ask her.

Possible correspondence with Irving-Leonard sitting (*Proc. S.P.R.*, Vol. XXXVI, pp. 203-5).

T. Shows eupboards or shelves put in. Looks like . . . lightish coloured wood.

T. Shows Miss Isabel turning up dress at bottom.

I turned up and hemmed a dress at the bottom the evening before the sitting.

There are also some references in this sitting which are veridical, but of a private nature.

S.P. 9.

T. Doesn't know what him means, him says Miss Isabel can't eat, lost her appetite.

I do not remember losing my appetite except on the cruise (her holiday), when I got very tired of the food toward the end (*i.e.* between August 23-30). I missed several meals and stayed on deck.

A.S. 24.

MEDIUM. Has Miss Newton had rheumatism in her leg or hurt her leg?

Irving-Leonard sitting, 20th September, 1926.

FEDA. You know Mrs. Isabel. . . . I am not sure if its knees or legs . . . about hurts to the knee.

Note. Not veridical.

The explanation suggested in the T.-Leonard sitting above referred to seems to point to a spiritistic hypothesis, but it is quite possible that the idea of co-operation originated with Feda—it is quite in accord with her usual trend of thought. Mrs. T. would have preserved a memory of the suggestion, and this may have operated to direct the clairvoyant faculty of Mrs. Elliott or Topsy towards Miss Newton.

The difference between the frequency of these references in this particular sub-case and the whole series is 5 to 2; this is not so great as definitely to exclude chance, and it may be simply a coincidence that they are relatively more numerous with this sitter than with others. However, that it should be coupled with the suggestion that this communicator should attempt this form of co-operation is at least curious.

This concludes the discussion on the psychological aspect of the case.

B. PSYCHICAL RESEARCH.

The principal point at issue for psychical research is that of the source of the communications; at the back of all subsidiary questions lies that of survival.

It is not suggested that the evidence contained herein is in the smallest degree conclusive in this matter, in fact it must be admitted that, so far as evidence for the continuance of conscious existence after death is concerned, the case is disappointing.

After much consideration it was decided that the best way to deal with this part of the subject was to enumerate the various hypotheses which have at any time been put forward, and to discuss how they fit the facts of the present case.

By this method it seemed possible to extract the fullest value from the material available; it might prove that some of the hypothesis might be almost eliminated, others rendered doubtful, and others partially confirmed.

The utmost that can be looked for is the arrangement of the current hypotheses in an order of probability.

The method adopted here is the application of the facts to the hypothesis, the alternative being the application of the hypotheses to the facts.

Now it may be taken for granted that all hypotheses which have seriously been suggested have some basis of fact behind them; the object in view being to test their validity, it seems more economical of effort to select from our heterogeneous mass of facts those which appear to confirm or refute, than to take the mass as it stands and attempt an hypothesis to cover it.

In one case we start with an orderly framework and try to fit in the pieces, in the other we take a jumble of pieces and try to arrange them like a jigsaw puzzle.

If it should appear that this method of dealing with the facts of this case is, even in a small degree, fruitful of results, it can be employed as a skeleton technique in other cases where the material is richer.

The hypotheses with which we have to deal may be arranged into four groups, one of which, viz. the last,

contains so many varieties, all of which are so extremely nebulous in character that it will be possible to do very little with theories which come under this heading.

- The groups are (1) Normal.
 (2) Supernormal faculty.
 (3) Survivalist.
 (4) Mystical.

Under Group (1) come all explanations by chance, fraud, fishing, etc.

Under Group (2) come all hypotheses which rely on the operation of a supernormal faculty. It includes (a) Telepathy, (b) Clairvoyance, (c) Object reading (old term, Psychometry).

It also covers all such theories as Osty's. The writer of this report evolved independently a somewhat similar theory. On reading Dr. Osty's book, *La Connaissance Supranormale*, he perceived the substantial identity of the two theories. It will be called, then, Osty's theory, and his account of it will be quoted; possible modifications and extensions may be suggested.

Group (3). Survivalist theories. (a) We have, first, theories of the type suggested by Dr. Walter Leaf and Dr. Broad; these postulate a sort of partial survival or persistence of memory elements.

(b) Survivalist theories proper. These are the spiritistic theories of all degrees of refinement or crudity.

Group (4). Mystical theories. This class is the most troublesome to deal with, the theories being—as has been said—nebulous and difficult to grasp. There is no guiding principle by means of which they may be classified, so that there is no possibility of satisfying oneself that the whole ground has been covered. Entirely fresh forms may at any time be suggested. The only two which will be even briefly mentioned here are the theories of Cosmic Consciousness and the Theosophic Theory of Akashic Records.

Criticism of theories of this type is bound to be unsatisfactory. The essence of mysticism is that it is mystical, which implies that it cannot be described in ordinary

language, but can only be apprehended by a special faculty, the possession of which constitutes a person a mystic. Those who, like the writer, lay no claim to possessing this faculty in the very smallest degree cannot hope to understand these theories, and to criticise that which one does not understand, though not an altogether unknown procedure, is hardly one to commend itself to anyone who aspires to be scientific.

The theories will be discussed in the order given, although a certain amount of overlapping is unavoidable.

GROUP I. NORMAL.

As regards the first group, the most important of these, viz. chance, has already been referred to in the preface. What has been there said need not be repeated, but a few amplifying details of the experiments may be given.

There were nineteen A.S. sittings selected for the second experiment (see p. 51), all of which had given fairly good results with the original contributors or as intrusion sittings.

There was no instance of any one of these consistently giving scores for chance.¹ The highest average score was 20%, being made up as follows: 0, 0, 41, 17, 41, real score was 55%; the next highest was 0, 14, 61, 0, 12=17%, real score 71%. From this it may be argued that the high chance scores were not due to any peculiarity in the matter of the sittings.

It is held by some that a fraudulent medium can produce a spurious appearance of the possession of super-normal faculty by the judicious selection of commonplace remarks, combined with an acute observation of the sitter and a quickness to seize upon and utilise hints and slight indications. This may well be true.

The particular series before us, however, being all Absent Sitter sittings, observation of the sitter is ruled out, and it seems probable that the artificial nature of a sitting made up entirely of commonplaces would be apparent on the surface.

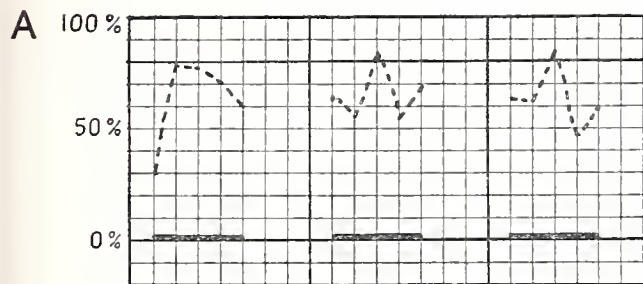
¹ For system of scoring see p. 59.

Not only is it the fact that these communications were not composed entirely of commonplace remarks, but the figures of this chance experiment show that no one particular sitting was better adapted than the rest for giving high chance scores. With the exception of one, which happened to be used once only for pseudo-annotation, there was no case of any of these nineteen sittings escaping one or more zero scores.

The following diagrams have been drawn to exhibit graphically the results of this second chance experiment. They are given in the form of the usual graph, although the subject-matter is not strictly speaking susceptible to that form of treatment. The graph properly shows the locus of a point governed by two variables which are proportioned to the co-ordinates. In this case there is only one variable, but for the purpose of showing the results graphically the horizontal co-ordinate has been replaced by a purely arbitrary arrangement of the sittings; they are, in fact, arranged in order of date, thus each horizontal division represents a single sitting or set of sittings. The diagram should properly have been a series of vertical lines only, but it was thought that a curve would be more conspicuous and show the desired comparison more clearly.

The diagrams marked A. are for each separate pseudo-annotator; that marked B. is for all the annotators, but in this case each horizontal division stands for the set of sittings which he annotated, and the scores are the average. The dotted line is for real scores, the black line for chance scores. There is a certain superficial appearance of parallelism between the real and chance curves in certain of these diagrams, notably A. 5, 6, 7, 9, 11 and 12, but a closer inspection shows that it is more apparent than real. It would be unsafe to draw any conclusions from it.

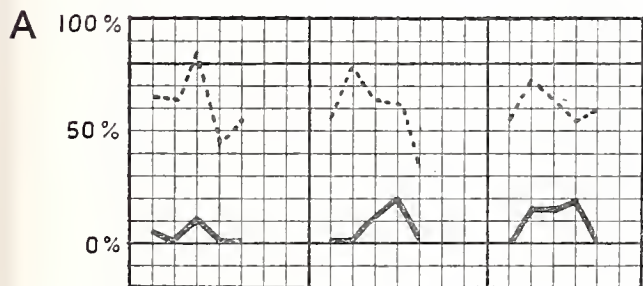
I II III



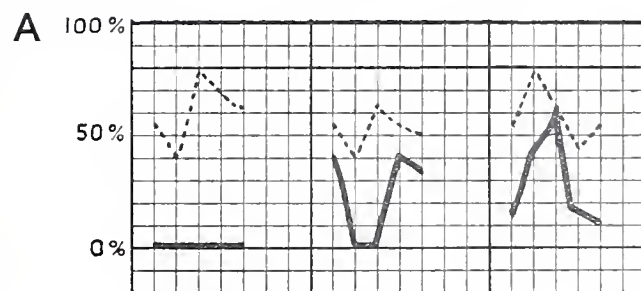
IV V VI



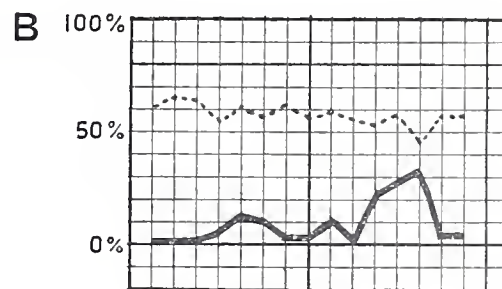
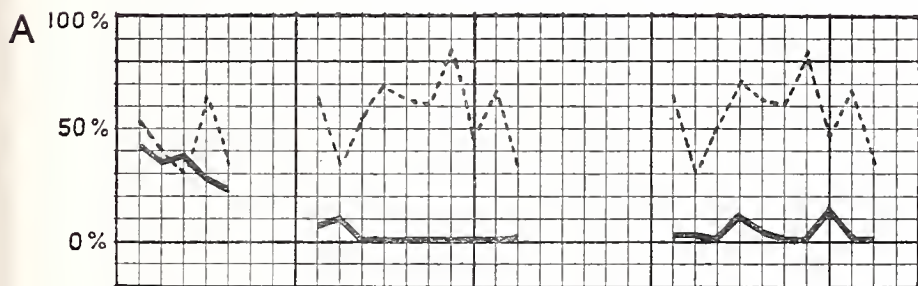
VII VIII IX



X XI XII



XIII XIV XV



The following table shows the various chance and real scores for each sitting:¹

<i>Sitting.</i>	<i>Real score.</i>	<i>Chance scores.</i>	<i>Averages.</i>
A.S. 5	55%	0, 0, 41, 17, 41	=20%
A.S. 10	40%	0, 0, 0, 36	= 9%
A.S. 26	79%	0, 0, 46	=15%
A.S. 31	64%	0, 3, 9, 1	= 4%
A.S. 35	32%	0, 0, 38, 10, 1	=10%
A.S. 40	79%	0, 38, 0	=13%
A.S. 44	53%	0, 3, 0, 0, 0	= 1%
A.S. 51	67%	0, 0, 0, 22	= 5%
A.S. 54	71%	0, 14, 61, 0, 12	=17%
A.S. 55	65%	0, 6, 28, 0, 3	= 7%
A.S. 61	62%	0, 0, 0, 11, 14, 0, 0, 0, 0	= 3%
A.S. 66	85%	0, 0, 18, 6, 0, 0	= 4%
A.S. 70	54%	19, 0	= 9%
A.S. 73	46%	0, 0, 10, 0, 14, 15	= 6%
A.S. 79	55%	10	=10%
A.S. 85	68%	0, 0, 10, 0, 0	= 2%
A.S. 87	58%	0, 0, 14, 0	= 4%
A.S. 89	55%	12, 0, 35	=16%
A.S. 90	34%	10, 0, 24, 0, 0	= 7%

In addition to the two control experiments described there was the further attempt to obtain a comparison with chance in the physical descriptions of ostensible communicators. The following are two typical results.

<i>Man described</i> 20 times.	<i>Man described</i> 4 times.	18 chance sittings.	
General congruity	51.1%	56%	30.95%
Partial „	12.1%	15%	11.9 %
Not inconsistent	2.2%	6%	3.95%
Inconsistent	5.4%	3%	25.0 %
Not stated	29.2%	20%	28.2%

¹The figures in the first column are the scores for the annotations made by the actual sitter or contributor of the relic. Those in the second are the scores for the annotations made by the persons who participated in the Control experiment. The third column gives the average of the figures in the second. All scores are calculated in the manner described under the heading "Method of scoring," page 59.

It will be observed that while the percentage of general congruity is not very much different in the chance sittings, that of the inconsistent is considerably higher. However, it cannot be said that the superiority to chance is so overwhelming as to afford proof that chance alone could not have been responsible for the phenomena.

It must be admitted, therefore, that the experiments undertaken to test the hypothesis of chance are not conclusive.

By this it must be clearly understood that although chance is not definitely excluded it is by no means proved that supernormal knowledge of some sort is absent.

Apart from the objective proof which experiments such as these are capable of providing, one may attain subjective certainty, or at any rate conviction of high probability, from a study of the details of the case. The grounds on which this conviction is based are not, however, capable of being presented in a succinct and easily assimilable form; the judgment is an individual one, and is only attainable by those who have studied the records of the sittings in detail.

It is only possible to put on record the fact that a prolonged study of the case has convinced the writer that chance alone is not responsible for the veridical statements contained therein, and to indicate briefly the type of evidence on which he has formed his opinion.

1st. The fact that the average scores of the S.P. sittings is more than double that of the A.S. The only condition present in the former which is absent in the latter is the actual presence of the sitter. It is, therefore, a logical inference that that factor is responsible for the increase in veridical matter.

After making full allowance for normal causes, such as information obtained by the medium from direct observation of the sitter, information given away by the sitter's remarks, fishing, groping, and so on, there remains a substantial residuum which cannot be so accounted for. The inference is that it is of supernormal origin.

2nd. A comparison of the quality of the evidence in

real sittings and pseudo-sittings. In the former, where we have a number of sittings with the same sitter or group of sitters, there is a considerable amount of continuity running through the series, the same ostensible communicator appears again and again, there is psychological consistency between the various sittings and correspondences between statements made in different sittings.

This is notably absent from the pseudo-sittings. The chance coincidences are more scrappy, we find different ostensible communicators in different sittings or even in the same sitting. The general impression derived from a study of the evidence is that it presents the peculiar characteristics of chance coincidence to a greater degree than is apparent in the real sittings.

In scoring the pseudo-sittings one becomes aware of the necessity of stretching coincidence to allow "hits."

3rd. In the real sittings we occasionally get statements concerning events which are so accurate, and descriptions of the characteristics of ostensible communicators, which are so apt as practically to enforce the conviction that chance alone cannot account for them; for example, see the character-sketch of Miss X.'s father given in the extracts on page 81 *et seq.*

With nothing to guide her it seems impossible that the medium should have been able by pure chance to describe with such accuracy so unusual a character and state of family relationships as is there portrayed.

An attempt has been made to arrive at a more accurate method of estimating the value of evidence.

Acknowledgment must be made to Mr. S. G. Soal, and our thanks are due to him for the trouble he has taken in giving the benefit of his mathematical knowledge and also of his great experience in psychical research. As, however, the formula which he has devised and the reasoning on which it is based are of rather a technical nature the matter will be dealt with separately.

The other normal sources of information, such as deliberate invention, fishing, etc., have already been discussed.

GROUP II. SUPERNORMAL FACULTY.

The next set of hypotheses are those classed under the heading of Supernormal Faculties, and of these telepathy is the first to be discussed. It is assumed that telepathy will be admitted as a *vera causa*; there are still a few die-hards who deny it, but the majority of those who have given any attention to the evidence agree that even if it be not rigorously proved, the case for it is very strong; the opinion of those who have not studied the evidence is, of course, worthless.

By telepathy is meant the transmission of information or impressions from one mind to another by means other than the normal senses. The nature of the process is obscure, but it would seem that two minds at least are requisite, that is to say, the agent from whom the information is derived and the percipient who receives it.

It appears at first sight logical to divide the phenomena into two classes: first, those where the agent is active and the percipient relatively passive—this would be telepathy proper; the second, where the percipient is active and the agent relatively passive—this should, strictly speaking, be called mind-reading. If, however, there be any truth in the theory propounded earlier in this report and called psychical osmosis, this classification may be inapplicable; both agent and percipient are passive, except in so far as one or other of them establishes the necessary *rapport*.

From the evidence derived from experiments in telepathy there is, nevertheless, some ground for holding that intention to transmit a definite message has some efficacy in getting that message through. Whether this is never more than the endowment of the idea with a higher degree of conative energy and in consequence greater mobility, as was suggested in the osmosis theory, or whether there is any definite act of sending a message, seems impossible to determine. It might be said, however, in either case that the agent is in some way active. It is possible that all mental activity depends on relative conative potential. At the present stage of our knowledge the safest position appears to be to allow provision-

ally both types of activity to exist. The actual processes employed in sending and receiving the messages undoubtedly belong to the subliminal part of the mind, and are thus not open to inspection. It is doubtful whether our most powerful instruments of research, viz. hypnosis and psycho-analysis, can penetrate sufficiently deeply to arrive at that stratum of consciousness which is the theatre of the telepathic process, but it would be extremely interesting if the attempt could be made.

As regards mind-reading we are on still more debatable ground—the number of those who are prepared to accept it as a *vera causa* is less than for telepathy. There is, to begin with, an extremely strong instinctive repugnance to the idea that the privacy of one's thoughts can be invaded by an inquisitive intruder, and that one's memories, both conscious and subconscious, can be ransacked.

From the point of view of the value for psychical research of the evidence in communications purporting to come from the surviving spirits of deceased human beings, this question of the possibility and limitations of mind-reading is of the highest importance, and one cannot get very far without adopting a definite attitude of acceptance or rejection towards it.

Pushed to its logical conclusion, the faculty of mind-reading means that the contents of the minds and memories, both conscious and subconscious, of all living beings are open to inspection by the possessor of that faculty. This is little short of potential omniscience, and although there have been some who, after mature consideration, have accepted this position, in the opinion of the writer almost any hypothesis should be preferred to one which ascribes to the human mind powers so utterly out of parallel with anything else we know about it and limitations so wide as to be almost non-existent.

Not only must all other possible hypotheses be tested, but even in the event of their failing to cover the phenomena, positive evidence of the clearest and strongest description would have to be produced before such an extension could be allowed to the normal powers of the mind.

An hypothesis such as this cannot be established by default of the other candidates until we are certain that *all* possible alternatives have been eliminated, and to attain this certainty we should require to know a very great deal more than we do. In our present state of knowledge it is always possible that some new hypothesis may be suggested.

Very different from the unrestricted power of mind-reading, for which it is submitted there is no direct evidence, is the very limited kind which is suggested by some of the facts of the sittings. This power, if it exists at all, extends only to the reading of some parts of the contents of the mind of a person actually present or with whom the percipient is somehow *en rapport*.

It is submitted here, however, that none of the facts which may be adduced as evidence for the possession of this limited power of mind-reading are not equally well explained by the hypothesis of psychical osmosis. The activity of the percipient would then be limited to the actual making of the contact between the two minds; what then passed as a result of that contact is a matter which is not determined by any action on his or her part.

This view is held to be more in parallel with what we know of the normal operations of the mind in acquiring information. The eye does not go out to seek that which it sees nor the ear the sounds it hears; the most that the percipient can do is so to adjust his organs of sense that they may be in the best position for receiving the impressions impinging on them from the outside. Of course, arguments from analogy and parallelism are not conclusive, these supernormal phenomena may be entirely *sui generis*, but, as has been pointed out, we can only draft our psychological and psychical principles on lines borrowed from the physical world; if, then, analogy plays so large a part in normal psychology we may as well be consistent and stick to it throughout the whole piece.

There is one further point which tells against the

theory of unlimited mind-reading. For a being endowed with potential omniscience, and having at her disposal all the contents of the mind she is reading, the medium exhibits an extraordinary lack of intelligence in selecting the items of information which she retails. The ordinary person would do far better: surely potential omniscience should not fall behind our ordinary everyday intelligence.

The attitude, therefore, of this report towards mind-reading is that of denial of its existence, except in a very limited and restricted form.

We may now see how the facts of this case are explicable by the hypothesis of telepathy in the sense above indicated.

The first salient fact is the marked superiority of the scores in the S.P. sittings over the A.S. This can only be attributed to the presence of the sitter. It is generally assumed that proximity is favourable to telepathy, although the evidence for this assumption is not so clear as might be wished. It seems, however, fairly safe to conclude that telepathy between the sitter and the medium plays a considerable part in S.P. sittings, and is one of the causes—and probably a large one—of their superiority of score.

With very few exceptions the information given in the S.P. sittings was such that it might have been derived from the sitter's mind, and in the few cases where it was demonstrably not so the facts were of such a nature as to be accessible to supernormal observation or clairvoyance. For example, in S.P. 11, the medium referring to the relic, a watch, states that there is some writing inside—this was correct, though unknown to the sitter; in S.P. 27 it is stated that water had come into the sitter's house—also correct and unknown.

A power of observation which could transcend the ordinary limitations of space could have acquired these facts.

There is, so far as can be discovered, no single instance of facts unknown to the sitter being given which could have been in the possession of no one but the ostensible communicator.

Taking the S.P. sittings by themselves it may be said that the supernormal knowledge contained therein can be completely covered by the hypothesis of telepathy from the sitter, together with a small amount of supernormal observation or clairvoyance.

As regards the A.S. sittings the case is different. Though facts unknown to the contributor of the relic are no more in evidence than in S.P. sittings, the absence of any means of *rapport* (apart from that furnished by the relic) stands in the way of explanation by telepathy alone. If we take as an analogy for telepathy proper calling up the percipient on the telephone by the agent, it may be said that while the agent knows the percipient's number he does not know the appropriate occasion on which to put through a call. An intrusion might be considered as a call put through on an inappropriate occasion.

The fact that has to be accounted for is that so many of the A.S. sittings exhibit a relevance between the communications and the owner or contributor of the relic.

If it be taken for granted that in successful A.S. sittings the medium becomes in some way aware of the identity of the contributor of the relic and gets into *rapport* with him, then we might suppose that telepathic exchange of information might take place between them. If this is what we have called telepathy proper, that is to say an activity on the part of the agent, then we must suppose that in the first place the medium calls up, as it were, the contributor, and informs him that the sitting is being held, and he, in response, conveys to the medium the requisite information.

It is scarcely necessary to state that none of this would be in the normal supraliminal consciousness of either party.

If instead of telepathy proper some theory such as that of psychical osmosis be accepted the position seems easier. Once the medium gets the necessary direction from the relic it only remains for her to get her mind into contact with that of the contributor for the process of infiltration to commence.

The crux of the whole thing is, therefore, the part played by the relic. This matter must be dealt with

before any further progress can be made, and as any discussion must bear on the hypothesis of object-reading, or psychometry, that hypothesis will be discussed here although out of its proper place.

The conclusion arrived at in the discussion on the function of the relic, in the psychological section of this report, was that some causal nexus exists between the handling of the relic by the medium and the statements which she makes at the sitting.

We have now to press the enquiry further, and endeavour to determine the nature of the process which takes place.

The hypothesis of object-reading is that the medium in some supernormal fashion obtains from the relic itself information relating to its past history, events in which it has taken a part or of which it has been, so to speak, a spectator.

We must assume that every event leaves its trace on the material objects present, and that from these traces a suitably sensitive person can recover a history of the event.

It is extremely difficult to imagine, even in outline, what these traces may be, or how a material object can preserve them. It is true that we can sometimes obtain from traces left on a material object some knowledge of its past history, as, for example, when we see a stone bearing longitudinal scratches of a certain kind we say that that stone has undergone glacial action. In these cases, however, the information is really arrived at by inference—we need past experience from which to draw it.

The only kind of traces which by themselves could convey the entire history of an event seems to be written words or a pictorial record like a cinematograph film: it is obvious that nothing analogous is the case here. However, our inability to imagine how, for example, a gold cuff link can bear the record of a long series of events, both physical and mental, in the history of its owner, is not necessarily an insuperable obstacle to the hypothesis of object-reading, although it is a grave defect. We cannot erect our incapacities into absolute impossibilities, nor arrogate for our limitations a cosmic status.

Fortunately in this case we have positive evidence which settles the question for us by rendering the object-reading hypothesis untenable.

A very large part of the veridical information given concerns events in which the relic played no part whatsoever, for example, the events which have been classed as Post-mortem. Reference to the table on page 29 shows that in some 39 A.S. sittings the Ante-mortem statements number 237 veridical, 275 non-veridical, while the Post-mortem number 197 veridical and 127 non-veridical.

It is, of course, possible that in some of the post-mortem events the relic might have been actually present, but there could have been few such cases; moreover, there is no reason for thinking that it was present in all or indeed any of the Ante-mortem events. In fact, when the relic is such a thing as a letter written by the ostensible communicator, as was sometimes the case, it is clear that it could have been present in very few of the events recorded. We should have to amplify the object-reading hypothesis and suppose that simple contact with an object impregnates it with a condensed history of the person who handles it. This seems to be going beyond the most fantastic limits of supposition.

It is submitted that this fact is sufficient to settle the matter quite definitely.

We may say, then, that the hypothesis of object-reading, whatever its validity may be in other cases, will not cover the facts here in question. This conclusion agrees with the result of Dr. Osty's investigation; he lays it down that the relic does not exercise a "fonction enrégistrateur," but only a "fonction directrice." His theories will be discussed later.

It seems, therefore, that an hypothesis of telepathy on the lines of what has been here called psychical osmosis will cover the facts, provided we can account for the directive function of the relic in some way not inconsistent with that hypothesis. When we come to discuss the last of the theories in this class we shall see how far this condition can be complied with.

The next hypothesis to be discussed is that of Clairvoyance.

Here, again, it is necessary, as a preliminary, to lay down some limitations to the meaning of the term. If by the word "clairvoyance" we mean the power to acquire knowledge of any fact, past, present or future, physical or mental, by the simple exercise of a supernatural faculty, we can, of course, say that the hypothesis covers the facts; it is, in fact, so wide that it will cover any conceivable instance of supernatural knowledge. But unless we can give any details of the *modus operandi* of the process this is surely a most blatant example of explaining *ignotum per ignotius* and *obscurum per obscurius*. It cannot be held to be a satisfactory solution of a mystery to postulate a still more mysterious faculty as explanation.

Clairvoyance such as this would be an even nearer approximation to omniscience than unlimited mind-reading.

It is necessary, therefore, to lay down some limitations to the meaning of the term before the hypothesis can be used for practical purposes. It is suggested here that the supernatural factor in clairvoyance is the partial transcendence of the limitations which ordinary spatial and temporal conditions impose on observation. Only those events which could have been observed had the clairvoyant been present at the time and place of their occurrence are accessible to the faculty.

It still remains, no doubt, sufficiently mysterious, but at least it can be said that some colour is lent to it by modern mathematical and physical theories of time and space.

There is a considerable amount of evidence that some such faculty does actually exist, and there are a few incidents in the present experiment which seem best explicable along these lines. Two have already been cited when discussing the hypothesis of telepathy, although in one of these particular cases the knowledge was almost certainly in the mind of some other person and might have been drawn therefrom. The incidents referred to are those of the writing inside the watch and the water

coming into the house. There are, however, two other incidents of a rather peculiar nature, which present better evidence of the exercise of some sort of clairvoyant faculty and also throw some light on its nature.

The first is as follows. In A.S. 39 Topsy says :

Doesn't know whether its where that lady buried or whether connected with that lady, but, you know, where they buries people, something connected with her, she points to herself; she says, the whole row is now filled except one; looks like whole row of graves, does you understand? There has been some one buried quite recently, you know, quite soon and it was in rain and just brightened up a little for actual service and then rain.

The note is :

This is very characteristic of her, she was much interested in the Golders Green Crematorium where her ashes are deposited and frequently spoke of it. Her urn is in a row accommodating eight, there is one vacant space.

The most recent date in the row was Sept. 1926. The sitting was dated Nov. 26, 1926. Further investigation revealed the fact that although an inscription had been placed on the tablet of the niche in question the niche itself was not filled; the cremation took place in San Francisco. No information concerning the weather at the time of the cremation was obtainable.

The second ease is from an extra A.S. sitting with Mr. Irving. It was a normal sitting, *i.e.* the medium was not in trance. The relevant passage is as follows. After referring to a church, the medium says :

Woman in a shawl, buff with red check, wearing a hat very like a man's tall hat, on the right side less than half way. About 3rd row and in corner seat.

Mr. Irving's note is :

I went on ahead with a missionary, counted the seats from the front and sat down nearest the aisle in the 3rd seat. My sister-in-law came in later, passed by me and sat next, nearest the wall. As no one else was in the seat she put

her mackintosh in the corner against the wall. I should describe my sister's coat as being buff in colour with a very faint tinge of pink in the buff. It had a large check pattern outlined in red and, I think, also yellow in some lines. My sister's hat was black, something like a man's bowler, but rather taller and turned up a little way with pink in one place.

His sister-in-law writes :

I wore a buff-coloured coat, of a large check pattern, the line of the check being red. I had on a black hard hat very like a man's bowler, but turned up with pink. Looking from the chancel, I sat in the 3rd row from the front, right-hand side nearest the wall.

Now the peculiarity common to both these cases is that the medium makes a mistake. In the first she states that the grave or niche had been filled, whereas it was still empty though an inscription had been placed on the tablet. In the second she misdescribes the garment worn by the woman in the church as a shawl instead of a coat. Both these errors are of the same character, that is to say, the erroneous statements are such as might have been made by a person who had had a fleeting superficial glance at the actual physical situation and jumped to the wrong conclusion.

In the first case anyone who observed an inscription giving the name and date of death on the tablet of a niche in the crematorium would naturally conclude that the niche had been used, and contained the ashes of the deceased. Had the information been derived from the mind of any person connected with the actual occurrence such an incorrect inference could hardly have been made.

In the second case all the details of the scene are correctly given, such as the position of the pew in which the woman was seated, the shape of her hat, the colour and pattern of her clothes. The error lies in describing the coat as a shawl. Now this error is one which the wearer of the garment would never have made, nor indeed anyone connected with her. The social status of the family to which all the actors belonged was such

that the wearing of a plaid shawl for church-going would be an almost inconceivable idea.

It is plausible to suggest, therefore, that the information in these cases was not derived from the mind of another person, and the peculiar nature of the mistakes made is consistent with the hypothesis that the source was some sort of dim fleeting glance at the actual scene.

It may be suggested, therefore, as a tentative hypothesis, that clairvoyance consists of a temporary and partial removal or relaxation of the limitations imposed on normal observation by the conditions of space and time under which they ordinarily function.

There are many other incidents in the sittings which can be explained on this hypothesis, such as the descriptions of places, many of the veridical references to facts occurring at the offices of the S.P.R., references to Miss Newton and her affairs, and so on. However, in these instances the hypothesis of telepathy is not definitely excluded, and there is no ground on which to base a choice between the two.

The conclusion arrived at, therefore, is that there is evidence of the existence and exercise of a clairvoyant faculty as hereinbefore described. It remains to be determined whether the whole amount of the supernormal phenomena contained in the sittings can be ascribed to this cause. If the relaxation of the spatial and temporal limitations on normal observation implies that the glance of the medium can travel back into the past and observe events which happened long ago, then it may be said that all the information contained in the statements which have been classified as Physical might have been obtained from this source; but we can hardly make the explanation cover the non-physical facts, that is to say, statements concerning events or conditions which could not have been observed by a person suddenly arriving on the spot. To do so we should have to grant such an extension to clairvoyant faculty as to render it almost equivalent to omniscience. As in the case of unlimited mind-reading, very definite and positive evidence of the existence of such a faculty would be required before it

could be accepted as a legitimate hypothesis: to postulate it as an explanation of these very commonplace mediumistic phenomena is to invoke a greater mystery to solve a less, and amounts, when all is said and done, to an acknowledgment of our complete ignorance.

We may, therefore, add to the conclusion above arrived at that this faculty of clairvoyance is not sufficient by itself to account for all the phenomena.

The next matter for discussion is the work of Dr. Osty as given in his book *La Connaissance Supranormale*. I quote from a translation by Mr. Stanley de Brath, *Supernormal Faculties in Man*.

As regards the major part of the book it is not necessary to speak; it will suffice to say that the conclusions arrived at by Dr. Osty as regards many points, notably symbolism, etc., are almost identical with the views taken in this report. (It should, perhaps, be said here that these conclusions were reached entirely independently.)

On page 136 he says:

The first phase is the subconscious elaboration of the supernormal cognitions.

In a second phase, these elaborated cognitions are manifest under the form of mental images of different sensorial kinds, more or less accurately representative, and often extending to hallucinatory objectification.

Finally, the intellectual function called "conscious" comes into play, interprets the images that have arisen from the depths of thought and translates them into words.

On page 137 he says:

During this phase the elaboration of the supernormal cognitions takes place: and in it the mental images most fitted to transfer cognition from the unconscious to the conscious are sought, found, organized and developed into representations.

And a little lower down on the same page:

The work of supernormal cognition is fully accomplished outside the consciousness of the percipient.

And, finally, on page 140, he says :

It is by using the stock of mental imagery—concrete images, abstract images, symbols and allegories—and by utilizing the whole constructive power of imagination and all the resources of interior language, that the transcendental mode of thought conveys information to the mode that we call conscious.

It is in Part IV. of the book that the theories here to be discussed are set forth. Dr. Osty approaches the matter in the form of three questions, namely: What is the source of the supernormal information obtained by the "*sujet métagnome*" (Dr. Osty's term for his mediums) concerning the states of life of a human being (1st) present at the sitting, (2nd) living, but distant in space, (3rd) distant in time?

The answer to the first of these questions is given on page 183 as follows :

When a metagnomic subject reveals the states of life in a person placed in his presence, his paranormal knowledge comes from that person.

As regards the second question, on page 192 he says :

Experiment therefore leads us to consider the case of metagnomy on a distant person as analogous to that when that person is in presence. The two cases differ only by distance in space.

On page 194 we find :

Among all the hypotheses which we can conceive and accept, that of interpsychic communication at a distance is the only one suggested by practical experiment, and the only one which when applied to the genesis of the phenomena, makes them generally intelligible.

And lower down :

Beyond the human individualities that our senses detach from the continuity of Nature, we glimpse an immense mental world subjacent to appearances in which particular identities seem to be linked in an inconceivable collective psychic life. It is from this vast latent life of thoughts, of thought, that

sensitives unconsciously draw the substance of their revelations, being endowed with a sense which enables them to distinguish the evolving schemes of individual lives.

As regards the third question, that is to say, information concerning a person distant in time, the case of the greatest importance is obviously where that person is dead.

On page 199 he says :

Accumulated experiment shows that sensitives deal with a dead personality in the same way as with a living one.

On page 203, after having discussed at some length a definition of the word personality and the bearing of such abnormalities as cleavages, multiple personalities and spirit guides, he says :

This human personality, so unstable that it is incessantly modified, so little homogeneous that strong emotion, intoxication or suggestion can dissociate it, so little constructed to endure that it may even die before the death of the body, so precarious and so obviously perishable, is not, as we have seen in the preceding pages, the informing centre whence metagnomic subjects draw their knowledge when they work on a life in progress. *A fortiori* it cannot be the informing centre when death has extinguished it.

He goes on to suggest two alternative explanations of the source from which a knowledge of the history of a particular life is drawn.

This story, however, subsists somewhere, since metagnomic subjects are able to reconstruct it; and how can it be imagined to subsist if it is not in a thought or a memory?

In what thought? In what memory? This is to be sought for.

Is there behind individual seeming a collective consciousness, a thought outside time and space, where every reality is represented, of which all phenomena known to us are reflected by our senses; a mysterious well-spring whose knowledge flows through the particular psychisms within the limits that pertain to each?

Is there behind the apparent human personality a real personality, a transcendental individuality clothed in matter playing a part in the life of the world, whose persistence would be the memory containing a life story, from whom the metagnomic subject draws after death that which he drew before death?

Or is the source of supernormal knowledge so unreachable by the very constitution of our intelligence that our actual concepts must necessarily be foreign to it?

Dr. Osty does not deny survival. In a footnote to page 133 he says:

I do not deny the survival of human personality. I neither deny nor assert it.

But it would seem from the passage quoted from page 203 that the personality of which he is speaking is not the manifest supraliminal personality, that which is subject to cleavages, modifications, etc., for he says of it, "*Quand la mort l'a éteint.*" It is, presumably, a transcendental self, which is "*Affranchie des contraintes intellectuelles du temps et de l'espace.*"

It will be seen that one of the above suggestions should, under our classification, be considered as mystical, that is to say, where he suggests a collective consciousness from which the information is drawn. The second suggestion is a modified form of the survival hypothesis.

As regards the function of the relic he contents himself with showing reason why this must be held to be a "*fonction directrice*" and not a "*fonction enrégistrateur.*" The exact nature of this function and the means whereby it is effected remain still "*à chercher.*"

In so far as what he says as regards symbolism and the psychological mechanism of the transference of the impressions is concerned his conclusions coincide with those arrived at in this report, though some further steps in hypothesis have been herein taken, e.g. what has been called psychical osmosis.

What he says of the function of the relic is also in accord with the view herein adopted, but on this point also it may be possible to advance a little.

There was an instructive incident which seemed to throw some light on the matter. A contributor sent to the Society six relics, one of which referred to his deceased brother, the other five to members of his wife's family. The first A.S. sitting given with one of these relics appeared to contain information concerning his brother, the Christian name, Arthur, and some veridical statements concerning him and his widow's recent doings being given; these latter were not within the knowledge of the contributor. The relic used, however, turned out to be one of those which referred to the other side of the family, and not the single one which referred to the brother. With that relic a sitting was given in which information referring to the contributor's mother-in-law and her family was communicated. Now the brother when living had only a slight acquaintance with the other ostensible communicators. The only connecting link between them was the contributor.

It seems reasonable to conclude from the above that the directive function of the relic is exercised, sometimes at any rate, in regard to the person who contributed it rather than to the original owner.

This is also illustrated in other cases: for example, a relic referring to A. brings veridical communications concerning an uncle of his; a relic of —, Miss H——'s father, brings A. as ostensible communicator.

If one accepts the naïve spiritistic hypothesis it can always be said that the reasonable actions of the spirits are sufficient to account for these facts; for example, A. after his death would meet his uncle, and they might very likely arrange that the latter should do the communicating on that particular occasion. Again, Miss H——'s father and A. started their acquaintanceship on the "other side," having been brought together by the sittings of a private circle where both purported to come. The fact that in the first case mentioned information was given concerning the deceased brother and his widow which was normally unknown to the contributor tends to confirm this, but it must be admitted that this particular piece of evidence was not of the highest quality.

On the other hand, if the spiritistic hypothesis be not accepted, we must suppose that the directive function of the relic, sometimes at any rate, leads the mind of the medium to that of the contributor, from whence information is derived or a further direction is obtained.

In view of the extreme rarity in this series of sittings of information normally unknown to the contributor being given, it seems that this direction to the contributor covers almost all the phenomena, the few exceptions being perhaps susceptible of some other explanation such as clairvoyance.

It may be relevant to note that, with the exception of the handling by the various officers of the Society, which is the same for all relics, the fact of being selected, packed up and sent off for this particular purpose is the most recent event in which the relics were concerned.

This *may* be relevant, but the whole matter is so obscure that it is impossible to say; anyhow we have, in the case of every relic, a recent incident in its history which, if known to the medium, would afford the required direction to a source whence the information might be drawn. The fact that in about 20% of the A.S. sittings there are references to Miss Newton or the offices of the S.P.R. is suggestive in this connection.

There is a certain amount of negative evidence to weigh against this. In a few cases experiments were tried with entirely new articles, things bought at a shop and packed up straight away. For these, as for the ordinary relics, the most recent event in their history would give a direction to the sender, but in none of these experimental cases did this occur.

The medium apparently recognised in every instance that the ostensible communicator who appeared with these pseudo-relics had no connection with them.

The sender would, of course, have in his mind when despatching them the fact that the relics were false, and this may have been sufficient to deflect the directive function. We could suppose that the mind of the medium was in all these cases directed as usual to that of the sender, and that the first fact derived therefrom

was that of the falsity of the relic. This might effectually put a stop to the transfer of any further impressions, and thus leave the mind of the medium open to an intrusion, or else to occupy itself with fantasy formation and the construction of purely imaginary incidents and characters.

Anyhow, negative evidence based on so small a number of cases is of little value, and cannot weigh against any positive indications.

The salient feature of the A.S. sittings is the absence of any *conscious* link between the medium and the person from whose mind the information is derived, whether that person be the sender of the relic or the deceased owner. The only connecting link is an inanimate object and, as we have already seen, the hypothesis that this object bears on itself some record of the events described cannot be considered tenable.

What we require is some tentative explanation of the manner in which the object can exercise its directive function. It acts as a finger-post pointing in a certain direction, and we want to know how it does it.

Let us consider first the case of an S.P. sitting. At first glance there appears no special difficulty in the fact of the medium's faculties, whatever they may be, being directed to the sitter. However, when we consider that these faculties are apparently in no way dependent on the normal senses the phenomenon does not seem so simple.

So far as we can see, all the presence of the sitter effects, in so far as the transmission of supernormal knowledge is concerned, is to bring him into conscious contact with the medium. All knowledge which the medium's normal sensory observation of the sitter might afford must be ruled out. All that is left, therefore, is the direction of the medium's mind towards the sitter. Some mediums require actual bodily contact, others do not; it is a case of individual habit, and probably has only a purely subjective significance.

Dr. Osty writes on page 168 (Mr. de Brath's translation):

The reader has learned that most metagnomic subjects, in order to set their paranormal faculty to work, are accustomed

to touch the hand of the personality to be delineated; *i.e.* to put the two organisms—the two nervous systems as it would seem—into contact.

And, again, on page 171 :

A modality of energy of an unknown kind emanates from every human being revealed by its power to set in motion the special faculty of metagnomic subjects.

Now this idea of an emanation has been current since the days of Mesmer. It has recently been investigated, and received the support of the late Dr. Alritz, but the experiments of Professor Thouless and his associates, undertaken for the purpose of confirming his conclusions, having yielded nothing but negative results, tend to discredit the idea.

Negative evidence cannot, of course, establish a positive conclusion; but it seems reasonable, in the face of experiments so carefully devised as those of Professor Thouless, at any rate to suspend judgment, and to require unexceptionable positive evidence before admitting the existence of any such entity as an emanation. The nature of the suggested emanation is not stated by those who hold the theory, but it would seem as though it must be a physical or pseudo-physical entity, for, if it be purely psychical, what is there to distinguish it from telepathy, and what need is there to give it any other name? On the principle of *entia non multiplicanda practer necessitatem* it seems better to hold provisionally that the relation between the subject and the object is purely psychical.

If this be the case the fact of proximity between the two seems to have its importance reduced to that of indicating to the medium the source to which to apply for her information. It may be, of course, that space as such has some effect on the psychical process, and the evidence of experimental telepathy gives some support to this view, but it seems as though the facts may be equally well explained on the hypothesis of "direction" only.

To deal with the matter, let us consider the analogy

with normal recognition. The case of the person to be recognised being in the presence of the subject is the simplest, the senses of sight, hearing and touch all afford means of recognition, that is to say, they bring the subject into conscious contact with the object, their lines of history intersect.

Where the object person is separated from the subject by a space sufficient to eliminate recognition by the ordinary unaided senses, some indirect means may effect the same thing; the subject may know the name, address or telephone number of the object person, which will give the required "direction" and enable communication to be made, provided the necessary physical means are available. Where the separation is both in space and time no such physical means exists so far as we know, with the exception of permanent material symbolic traces, such as written words.

Now the chief point of difference between the normal phenomena and the supernormal lies in the means of communication; we must substitute for the physical processes of direct speech, writing, telephonic speech, etc., the psychical process of telepathy, whether in the form of what has here been called psychical osmosis or otherwise. Recognition is itself a purely psychical phenomenon, whether it be effected by physical means or not. In the case of recognition in the A.S. sittings, that is to say, when it is effected by contact between the subject and an inanimate object, what we have to explain is how the relation between the latter and the object person can produce the same effect as direct contact.

We may say that at the moment when the medium handles the relic their respective lines of history intersect, the medium makes conscious contact with the relic. Normally she is able only to derive from sensory observation a comparatively small amount of knowledge of the history of the relic. As has been seen on page 109 the fact of the relic remaining wrapped up has no effect on the quality of the information given; it would seem, therefore, that this normal knowledge may be neglected.

Somehow or other the medium's consciousness is brought

into relation with some person connected with the past history of the object.

We have seen reason for thinking that if a relation could be established with the contributor of the relic the entire set of phenomena could be accounted for, given the possibility of telepathy.

Now the conscious contact between the medium and the relic starts normally at the moment of handling it. It may be called a point contact; but if we suppose that the normal conditions of time governing the medium's mind be relaxed this point contact might be extended into a line contact, covering that part of the history of the relic where it was selected, packed up and sent off to the Society. This would give the necessary "direction."

It is very difficult to form any idea of a consciousness governed by different conditions of time or to imagine what it would be like, but a possible mode may conceivably be based on the conception of the "specious present." As is well known, normal psychology teaches that the present moment has a definite duration, it is not a mathematical line dividing the past from the future. The reasons for this need not be gone into here. All events which come into the same specious present are received together in consciousness.¹

¹Dr. C. D. Broad writes in a paper read before the Aristotelian Society at Bristol in July 1928: "The essential point in the doctrine of the specious present may, I think, be stated as follows. There is a certain characteristic which an object may have at certain times. This may be called the characteristic of 'being sensuously presented.' This characteristic has various determinate forms which may be called 'degrees of vividness.'

"The facts about the finite duration of the specious present may now be stated in the two following correlative propositions.

"(a) A momentary event, occurring at a time t , can be sensuously presented throughout the whole course of an experience which begins at t and lasts for a time T . It is presented with steadily decreasing vividness in each successive later slice of this experience.

"(b) In a momentary experience, occurring at a time t , the whole course of an event which ends at t and began at $t - T$ can be sensuously presented. Each successive earlier slice of this event is presented with less vividness in the momentary experience."

Readers are also referred to Wm. James, *Principles of Psychology*, page 609, and C. S. Myers, *Experimental Psychology*, pages 308-9.

Now, if we suppose that the specious present of the medium's subconsciousness be extended sufficiently in length, the two events in the history of the relic, viz. the selection and sending off by the contributor and its being handled by the medium, would be received together as in one moment. It would also cover the event of being received by the Officer of the Society. As a rule Miss Newton did this, and we may see a possible explanation of the numerous references to her in the A. S. sittings.

We have spoken of the normal conscious contact with the relic as a point contact; in actual fact it is really a line contact, but the line is very short, being the length of the specious present; the suggestion is that the subconscious contact is also a line contact, but of greater length. This theory possesses the advantage of enabling us to do without the untenable hypothesis of object-reading; it also offers an explanation of the facts of this case without the necessity of postulating communication with the surviving consciousnesses of deceased human beings. In other cases, if such there be, where the information given by the medium was not in the possession of the contributor of the relic, it would not, of course, be sufficient by itself to cover the phenomena, but it would still afford an explanation of the directive function of the relic.

Whether the possibility of dispensing with the survival hypothesis be a recommendation for this theory or not must be left to the judgment of the individual reader: it can only be said here that the phenomena of this particular case do not, by themselves, warrant us in going beyond it. While it is true that the survival and communication hypothesis does undoubtedly cover the facts, we cannot claim that it is established thereby if an alternative explanation can be found.

Some measure of support for this theory may be derived from other sources. For example, in normal states, such as fatigue, great excitement, intoxication, etc., the length of the specious present appears to fluctuate somewhat. It is possible that experiments by means of hypnosis might throw some light on this matter.

As is well known Kant taught that time and space were no more than *a priori* forms governing the phenomenal consciousness, and that the transcendental ego was not subject to their conditions. It may be that the *receptor stratum* belongs to the transcendental.

More recently the mathematical researches of Einstein and his co-workers have demonstrated that time is not an unidimensional stream common to all, but rather subjective for each individual. The time of observer A. is not the same as that of observer B. if they are in motion relatively to one another.

This is not the place in which to discuss the implication of these discoveries: they are merely mentioned here to show that the notion of a modification of the time condition, as usually conceived, is not entirely fantastic and without collateral support.

Those who hold the spiritistic hypothesis can, as mentioned above, claim to cover the facts, and their explanation of the directive function of the relic would be perfectly simple and plausible. The "spirits" may be supposed to be cognisant of events happening in this life, and would recognise the relic as having belonged to them when they were here. They would thus be led to communicate through the medium. In other words, we must suppose that they know all about this experiment, and have attempted to co-operate in it. Until we know more about the conditions of the "other side" we cannot say definitely whether this explanation is possible or not.

GROUP III. SURVIVALIST.

The next set of theories for discussion are those termed "survivalist." They are of two sorts: first those described by Dr. Walter Leaf and Dr. Broad, which postulate the persistence of certain memory elements and not the survival of the entire personality; the second are those which hold that the personality itself survives, and is the source from which the information is derived. These latter exist in all degrees of crudity and refinement.

The gist of Dr. Leaf's theory may be gathered from

the following quotations taken from *Proc. S.P.R.*, Vol. XVIII., page 53 :

The evidence seems to me to show that after death of the body there remains a more or less coherent complex of memories which is accessible to the subliminal self of certain living persons. What I do not as yet see is that this complex has such coherence as can enable us to consider it a personality. . . . The evidence seems to point rather to an alternative which is at least consistent with analogy . . . that, as the physical body only gradually dissolves into its elements after death, so the spiritual retains for a time a certain coherence which is no proof of life. . . . It may be possible for peculiarly gifted living spirits to behold the gradually disintegrating spirit, and bring us word, with more or less completeness, of what the spiritual man was during life.

Again on page 59 of the same volume :

The evidence proves, I think, that the memories of the dead survive and are under special conditions accessible to us. But I do not see that it proves the survival of what we call the living spirit, the personality—unit of consciousness, limited and self-contained, a centre of will and vital force, carrying on into another world the aspirations and affections of this.

Dr. Broad in his recent work, *The Mind and its Place in Nature*, enunciates a very similar theory in a somewhat more detailed form. He lays it down that before it can be held that the personality as a unity survives death some evidence must be forthcoming of the persistence of other forms of mental activity besides memory, for example, conation.

Where the evidence consists of the communication through a medium of information known only to the deceased Dr. Broad holds that to claim this as a proof of survival of the personality is to go beyond the evidence. All that is proved is the persistence of some sort of memory.

These theories are open to criticism on purely physiological grounds, for it might be argued that pure memory,

that is to say memory without any tincture of conation, is non-existent, it is a psychologist's abstraction, and ceased to be believed in when the faculty psychology perished. If this be so in life why should it be different in death?

This, however, is not the place in which to elaborate any such arguments; we must remain content to see how far the facts of this case fit in with these theories.

If, as has been suggested, the source of the information is the subconscious mind of the sitter or contributor of the relic, then these theories are plainly inapplicable. However, while it is perhaps going beyond the evidence at present before us to postulate another source, yet there is plenty of other evidence available which seems to give some support to such a postulate. We may, therefore, without prejudice, consider how our facts would fit in with theories of which, in themselves, they are insufficient to bear the weight.

One of the most obvious and striking features of these communications is the large percentage of statements referring to events subsequent to the death of the ostensible communicator. Many of these are of a non-physical nature, that is to say, such that an observer arriving suddenly on the spot would not have been able to obtain the knowledge implied by the statement.

If the discussion on the theory of clairvoyance be referred to it will be observed that it is just this class of fact which is not accessible to clairvoyance, so that, although we might possibly account for the physical Post-mortem statements by attributing them to clairvoyant observation on the part of the medium, we cannot so explain the non-physical. In 66 sittings selected for this particular analysis it was found that the number of Post-mortem statements was about equal to the Ante-mortem, and the percentage of veridicality is almost the same in each class. Of these Post-mortem statements the numbers of the physical considerably exceeds the non-physical, but the veridicality of the latter is higher.

It is unnecessary to say that the selection of the sittings was not made with a view to supporting any particular hypothesis: they were chosen simply because

they were the only ones which seemed to warrant the expenditure of labour required for extracting the facts.

Now there is absolutely nothing in the records to suggest that the source of the P.M. statements is any different from that of the A.M.; they are all mixed up together, in fact in some cases so inextricably that it has been difficult to dissect them.

It seems to follow, therefore, that although persisting memory elements might be the source of the A.M. statements they cannot account for the P.M. Clairvoyance might account for the physical P.M. statements but not for the non-physical. The theory thus fails to cover all the facts, and seeing there is no ground for attributing different sources for the different classes of statement, for the sake of simplicity of hypothesis, at any rate, it would seem that if any other explanation, which is otherwise not more open to objection, can be put forward to cover all the facts it should be preferred.

Apart from these considerations, if we take the facts at their face value, that is to say, as emanating from some surviving remnant of the deceased personality, there is considerable evidence of both conation and emotion. In the X. case in particular this is very evident, as will be seen if the extracts from that case given in an earlier section are referred to. One can there observe the emotion of remorse, watch its gradual alleviation as the deceased becomes more assured that his family has forgiven him for his unkindness. The desire for forgiveness and the pleas which he puts forward are all evidence of conation. In the case of A. also the same phenomena are exhibited, as, for example, when he congratulates his brother on his engagement.

If this be accepted at its face value it supplies the evidence which Dr. Broad requires for proof of the survival of a personality; if it be not so accepted the only alternative seems to be to hold that the mind of the sitter or contributor is the source of the information, and that the emotions and conations depicted are simply unconscious fantasy constructs of that mind, reflections of a romance woven by the subliminal.

In that case no persisting memory elements are required, so that the theory either fails as a complete explanation or is unnecessary.

The other type of survivalist theory, that is to say those which postulate the continuance of the personality after its dissociation from the body by death, is that most usually held by the mediums themselves and is assumed in almost all mediumistic communications. The spirits claim to be the source of the information, and many details of the methods of communication are forthcoming. (See Mr. Drayton Thomas's article in *Proc.*, XXXVIII. While it is held by many in its crudest and most naïve form, that is to say, that the personality which survives is the ordinary normal manifest personality which was known during earth life, in some cases at least, as is shown by Mr. Drayton Thomas's extracts, this is refined upon to the extent that the spirits claim that after death the sub- and supra-liminal consciousnesses are unified.

Now it must be frankly and fully admitted that some such theory as this completely covers the facts observed in this series of sittings.

Not only can it account for all the phenomena, but there is nothing which is definitely inconsistent with it.

There is much that is perhaps puzzling at first sight, such as, for example, the mistakes made, the gaps and omissions in the information given and, if one may say so without giving offence to those who hold that the communications actually do come from the surviving spirits of those dear to them, the surprising lack of intelligence in the selection of the matter chosen for communication. It would seem, from our point of view, an easy matter to furnish evidence of identity far more convincing than the trivialities which are so plentiful in the sittings, and if a normally intelligent person found himself in the position of wishing to establish his identity under approximately parallel circumstances, say by dictating messages to someone who was speaking through a telephone to those who were expecting to hear from him, he would not waste time in talking about the number of

pairs of shoes they had recently bought or such trivial details of recent happenings.

But the spirits offer a plausible explanation for this when they say that they become confused, and suffer loss of memory when they enter the condition necessary for communication. It may be so—we have no knowledge of the conditions of discarnate existence, and so are not in a position to contradict.

Moreover, apart from this, if the hypothesis of psychical osmosis be accepted we have a full explanation of all these and similar aberrations. We may suppose that *rapport* is established between the mind of the medium and the surviving mind, and that the passage of impressions from the latter to the former takes place in the fortuitous manner which was described when that hypothesis was formulated (see page 129).

That the survivalist hypothesis should adequately cover the facts is not surprising, because, firstly, it is nearly the widest hypothesis which we can make, and, secondly, it is assumed by the mediums themselves. Granted all the saving clauses and means of explaining away errors, it would not be difficult for a medium to avoid making serious blunders.

But a hypothesis so wide as that of survival requires a wide basis on which to rest, and it must be admitted that the evidence afforded by this case is not, by itself, sufficient to support it. If from other sources similar or better evidence is forthcoming then this case will be corroborative. This, in the judgment of the writer of this report, is all that can be claimed in favour of the survivalist theory.

There are, however, one or two facts which, perhaps, bear a little more weight on the side of the survivalist theory than on any alternative, though they are not by any means conclusive.

What may be called the dramatic personification exhibited in some sittings is one such fact. It is a fairly common experience for the sitter to get a feeling of intimacy at a sitting. They feel it hard at the time not to believe that the ostensible communicator is actually

present. With some this feeling fades after the sitting, with others, who are convinced of survival, it may persist.

We know nothing of Mrs. Elliott's or Topsy's dramatic ability, but it would seem that even the most talented actress would be hard put to it to give, on the spur of the moment and on the very meagre information available concerning the character to be portrayed, so convincing a representation as is sometimes shown. It is possible, of course, that the feeling of actual presence and intimacy is due to suggestion, and the fact of its subsequent fading in some cases rather points that way; still, so far as it can be judged by one not actually experiencing it, there is in the sittings some real ground for it.

Taken as a whole the dramatic personification is consistent for each individual ostensible communicator.

Another fact which may be interpreted as leaning towards the survivalist theory is that of intrusion. In all these cases, as has been remarked, the intrusion is by one who has shown himself elsewhere to be a good communicator. A., the communicator with whom this phenomenon most often occurs is extremely persistent in communicating.

This may be explained by the fact that the medium, having had experience of a number of successful cases of *rapport* with the source-mind from which she has drawn information concerning this particular ostensible communicator, tends spontaneously to turn in that direction and to establish *rapport* without the connecting link afforded by the relic. On the other hand, we may take the facts at their face value and hold that the surviving spirit, being anxious to communicate, and having done so successfully on previous occasions where a connecting link was present, seizes any opportunity that presents itself to intrude.

Before passing on there is one point which must be briefly touched upon. If, as Osty says, the transcendental self is free from the restraints of time and space, and physical death is simply the severance of the tie between the body and the self, then the question of survival cannot arise. It is a question which is not unanswerable,

but unaskable because meaningless. For a timeless entity, such as the transcendental self has been held to be, not only by Osty but also by Kánt and his many followers, there can be no before and after. The word survival implies the conception of before and after death.

It may be remarked that the more modest hypothesis suggested herein, viz. an extension of the length of the specious present only requires to be applied "with a heavy brush" in order to bring about very much the same result. For if the specious present be lengthened sufficiently we get a sort of timelessness sufficient for all practical purposes though not absolute. It is the same difference as lies between the conceptions of a future life and immortality.

Now, seeing that time and space are inextricably interwoven into all our mental machinery, our language, concepts, etc., we cannot reason about or even speak intelligibly concerning a timeless entity, but that, in itself, is no reason why such a hypothesis should be rejected. It simply means that we have reached the boundary of the intellectually knowable and pushed the matter into the province of the intellectually unknowable. If we are to deal with it we must employ some mental faculty other than the intellect.

Such faculties there are, but from their very nature as non-intellectual they are not amenable to the rules of logic, and in consequence the whole subject is lifted out of the sphere of science.

Psychical research, therefore, if it aspires to be scientific, must content itself with abstractions from reality in exactly the same manner as do all other sciences, that is to say, it must be satisfied with relativities and leave the absolute severely alone.

Doubtless it can accomplish useful work within these limits, and its efforts are the more likely to be successful if it recognises its limitations. It may be that the manifold perplexities which beset the Psychological Researcher all arise from the attempt to apply spatial and temporal concepts to that which is non-spatial and non-temporal,

to measure the immeasurable, and to force the absolute into the framework of the relative.

GROUP IV. MYSTICAL.

There now remains only the type of theory called Mystical, of which two only will be mentioned. As already indicated very little can usefully be said in criticism concerning this type of theory.

The first of those selected for mention is the Cosmic Consciousness theory.

This postulates some sort of universal consciousness, which embraces all particular consciousnesses and includes their contents in its own.

The relation of the particular to the universal consciousness may be thought of as being analogous to a wave on the surface of the sea. The wave possesses its own individuality, but at the same time is only a part of the sea. All that is in it is also in the sea.

The suggestion is that the source of the information given by the medium is this universal consciousness, in fact the mind of the medium and all other minds are in reality one mind, separated only by the temporary differentiation which we call personality. All knowledge, therefore, which is in any mind is also in this universal mind, and is accessible to anyone who can transcend the temporary differentiation.

This theory is based on a conception which is substantially the same as that of the Vedantic philosophy. In the *Upanishads* we are told that when the self receives enlightenment it perceives that all that differentiates it from the Absolute (Brahman, neuter) is Maya, illusion. As soon as this is grasped Maya vanishes and the self becomes Brahman.

The idea may be found in many other philosophies and religions, both Oriental and Western.

Now from the nature of the hypothesis it seems impossible that any evidence bearing on it can be found. If the survival hypothesis be wide it is also pretty definite. This of cosmic consciousness is certainly as wide, but it is so indefinite as to be nebulous.

Until one can form some conception of what a cosmic consciousness is like, and can give a clear description of what is meant by those words, it seems useless to discuss the matter.

That any such clear statement should be forthcoming seems unlikely: we should have to imagine a thought without a thinker, and transcend the fundamental category of subject and object, self and not self.

Owing to its width it can cover all the phenomena, owing to its nebulosity it cannot be broken. Criticism passes through it like an arrow through a cloud.

It has been put forward as an explanation by several thinkers, including Dr. Osty, who suggests it as an alternative, but does it get us anywhere? Might we not just as well say that the source of the information is a mystery as to postulate an entity which cannot be conceived?

The last theory to be discussed is the Theosophical theory of "Akashic records." By this theory there is supposed to be somewhere a permanent record of all the events which have ever happened. If the mind of the medium can obtain access to these records the required knowledge can be derived therefrom. It is not clear whether theosophists would admit that the ordinary medium could obtain this access, or whether such is available only to adepts and Mahatmas.

The theory is almost too fantastic to merit serious consideration, and the only thing to be said of it is that if these records do exist in a material form there is absolutely no evidence of it, while if they be of a psychical nature there is little to distinguish the theory from that of Cosmic Consciousness.

This concludes what must be admitted to be a most unsatisfactory and incomplete discussion of mystical theories.

However, until those who hold these theories can formulate them in definite language so that they can be understood by the ordinary non-mystic, when, of course, they would cease to be mystical, no satisfactory or complete discussion is possible.

The final conclusion, then, of this report may be stated as follows :

Telepathy from the sitter in S.P. sittings is probably responsible for a large part of the supernormal knowledge shown; in A.S. sittings telepathy from the contributor may be held to be a possible explanation provided the necessary *rapport* can be accounted for. The type of telepathy employed is probably that called here psychical osmosis or something similar.

Clairvoyance as herein delimited is probably exercised by the medium to some extent.

Explanation by "object-reading" is rejected.

Dr. Walter Leaf's and Dr. Broad's theories fail to cover all the phenomena.

Dr. Osty, though he leaves many points open, accounts for the phenomena of supernormal knowledge of living persons on lines substantially the same as those here suggested under the heading of telepathy; as regards knowledge of dead persons he suggests two alternatives, one survivalist, the other mystical, viz. cosmic consciousness. He suggests no theory to account for the power of the rielie to establish *rapport*.

A theory based on an extension of the specious present of the medium's subconsciousness has been suggested. This, if taken in conjunction with telepathy of the psychical osmosis type, it is submitted, will cover all the phenomena of the sittings under discussion.

It must be noted, however, that the quality of the phenomena here dealt with is not very high. There may be other cases where information given was not in the possession of any living person. In such cases the theory above stated would fail.

The survivalist theory covers all the facts if taken in conjunction with the osmosis theory, that is to say, if the transfer of information from the surviving consciousness is of the type described as psychical osmosis. It is not, however, necessitated by the evidence here available.

The mystical theories are too nebulous for anyone not a mystic to understand or discuss.

The final choice, then, lies between telepathy from the

sitter or contributor, together with a small amount of clairvoyance, and communication from the surviving consciousnesses of deceased human beings.

The evidence is not sufficiently definite to allow that choice to be made with any degree of confidence.

APPENDIX.

A suggestion for a technique to be employed in future cases is appended. The methods of collection and use of the relics should be the same as in this experiment.

The notetakers should note the following :

- (1) Whether the relic was opened or not by the medium.
- (2) Where not opened, whether the relic was handled by the medium as though she endeavoured to obtain knowledge of its nature by her sense of feeling.
- (3) Whether the wrapping was such that this knowledge could have been thus obtained.
- (4) The state of the medium as regards (a) physical health, (b) mood, in so far as these can be ascertained.
- (5) Any differences in the apparent mood of the control.

After the sitting the records should *immediately* be sent to the contributor for annotation. Only those persons who are able and willing to undertake to annotate records of sittings *immediately* on receipt should be invited to contribute relics.

A small committee of investigators should be formed.

Annotated copies of records should be sent *immediately* after reception from the annotators to each member of the committee.

The committee should meet at short intervals, say once a week or once a fortnight as found necessary, to consider the sittings recently given.

Each item should be discussed separately and its value assigned as follows :

- (1) Probability of its being true by chance.

This must be stated in the form of a fraction.

If agreement cannot be reached after discussion, the mean of the various assessments to be taken. The probability, where not capable of exact estimation, may be stated in the form of a lower limit. Suppose, for example, it be a ques-

tion of a man having only one leg, one could start thus: "Not one man in every hundred has lost one leg." Agreed. "Not one in every two hundred," and so on until a limit is reached where the committee begin to feel uncertain. This will constitute the minimum value for the statement. An upper limit could be fixed. Statements which were of so unusual a nature as practically to exclude chance would be considered of limit probability.

(2) The committee would decide on the merits of the annotations, *i.e.* whether a statement should be considered veridical or not. Further information could be sought and questions addressed to the annotator, if necessary the latter should be interviewed by some member or members of the committee in order to clear up obscure points.

(3) The committee should also mark the items for the various classifications adopted, such as A.M. and P.M., physical and non-physical, and so on.

(4) The committee should note and discuss any instances of symbolism, intrusion, etc.

The records thus scored can then be dealt with by a single person, who will calculate the value of each sitting¹ making the necessary indexes and note correspondences. He would refer back to the committee any points of interest arising from his work.

The final stage will be when the combined results of the whole series of sittings are laid before the committee, who will then discuss them and draw up a report.

This technique would entail the expenditure of much time and labour, but it would eliminate to a large extent the personal factor, and would also provide a keener scrutiny and more searching criticism of the annotations than could be afforded by any single investigator.

The results, therefore, and the conclusions arrived at will possess a higher value than can be assigned to those of individual effort.

¹ Discussion on the correct formula to be employed is still proceeding. It is hoped that an agreed result may be published shortly

I. A REPLY TO M. SUDRE'S ARTICLE, "AN EXPERIMENT IN CARD GUESSING."

BY INA JEPHSON.

In the February number of *Psychic Research* M. Sudre has written an article, "An Experiment in Card Guessing," the experiment being that reported on by myself in *Proc. S.P.R.* Part 109, December, 1928, Vol. XXXVIII. As M. Sudre in his article raises some important points in regard to different methods of research into supernormal phenomena, I am glad of an opportunity to reply to his criticism, and to try and make clear why his arguments, interesting as they are, have only served to strengthen me in my adherence to the form of research chosen.

To be strengthened in one's opinions and preferences, is, I suppose, the normal effect of controversy, and M. Sudre and myself seem to be following the normal course very faithfully. In February 1928, M. Sudre, spurred to action by my appeal to collect data for my experiment, wrote in *Psychic Research* begging investigators, including myself, not to do collective experiments based on a mathematical comparison with chance. In my report on the experiment I replied shortly, giving my reasons for continuing in my obstinate path, and trying to persuade M. Sudre of the error of his views. In February 1929 M. Sudre, horrified by the method and conclusions of my card-guessing experiment, writes again (digging himself more firmly into his trench), and amplifying and elaborating his criticisms. And now (crawling further into my dug-out) I reply once more, using precisely the same arguments that I used before, and feeling more warmly than ever the illogicality of M. Sudre's position, and the strength and good sense of mine.

My reply falls into two parts. First I would like to meet those criticisms and theories which do not seem to me to be

very good ones, and by these, of course, I mean those with which I disagree; then I will discuss the criticism which strikes me as helpful and worth discussion, and by that, of course, I mean one with which I am in agreement.

One of M. Sudre's main objections to the method used in collective experiments based on mathematical calculations is that the very method itself stands in the way of the demonstration of the faculty under observation, for the existence of the faculty may be so slight that it has no chance to appear in a relatively small number of trials, and that therefore the whole experiment must inevitably be quite inconclusive. I admit this may be a genuine drawback, but to abstain from undertaking an experiment for fear it may fail seems a curious, or I should say *ineurious* frame of mind, and to declare an experiment valueless because not enough data have been collected, is less helpful than to collect more.

Another criticism made by M. Sudre is that where the method of experiment combines all results, the results which are successful due to supernormal faculty, may become masked by those unsuccessful results from normal people. This must be admitted, but until we try we cannot tell who may have the faculty and who may not, and again, why. abandon an experiment because we are not absolutely sure it will succeed? Even if the experiment is not, and cannot in its nature be conclusive, a suggestive experiment can be of great value, indicating a line of research which may finally lead to definite conclusions.

With the next objection raised by M. Sudre I cannot agree in any way. He considers that only complete success should be scored; that is to say that no approximation to success should be allowed to influence the sum of results. He will not allow, for instance, that to guess or divine a nine of hearts for a ten of hearts, is any nearer to success than to say nine of hearts for a black court card. This seems to me quite strangely arbitrary. It might be possible that a clairvoyant divining cards should get colour right every time without exception, and yet, in M. Sudre's view, this must not be counted, being disqualified as only a "partial" success. We should be obliged to ignore, because of this self-imposed rule, what might be a most important and interesting charac-

teristic of lucidity. It is exactly as if M. Sudre, not content with the difficulties already inherent in the quest, decided to invent a few more barriers between the investigator and his goal. One can only hope that M. Sudre's methods of research will not spread to other scientific activities, or we shall find when we visit the oculist that we are considered totally blind if we fail to read quite all the letters on the test card; and at the aurist deaf and dumb, because we can neither hear very clearly nor understand some of the more enterprising words in the Welsh language.

But M. Sudre's postulate provokes an interesting train of thought, raising as it does the question of the mechanism of perception of recognition. It is arguable, I suppose, that it may be an unwise analogy to compare clairvoyance with normal sight, but on the other hand, if we insist on being so extremely cautious, with what else *can* we compare it? After all, the object of the experiment is to find out whether supernormal sight can furnish us with the same information that we are accustomed to receiving from normal vision. It seems reasonable to assume, even if lucidity and normal sight are quite different in their nature, that the objective facts perceived (by whichever means) are synthesised by the same process. We recognise, for instance, that a four of hearts is a four of hearts, because we have learned by three different acts of perception that the group of impressions, four units, redness, and heart-shaped pips is called the four of hearts. If we subtract one of these perceptions, or if one of them fails to be translated into conscious expression, there is no reason why we should assume that the other two perceptions were wrong or non-existent. The possibility that the three acts of perception may not all, or may not simultaneously reach consciousness, cannot possibly be taken as proof that no one correct perception has taken place.

Dr. Fisher's method was devised on the perfectly natural assumption that complete success is the synthesis of several possible acts of perception, and that however arbitrary a system of scoring may be it must supply a direct measure of the subject's power of scoring on that system. But I need not defend Dr. Fisher's system, and he is writing a reply to M. Sudre in this article.

If M. Sudre's method of scoring only complete success would give but a barren and unprofitable experiment, his desire to narrow the experiment still further, and to concentrate on achieving one enormously above chance experiment, would reduce the interest of psychic research almost to zero point.

M. Sudre's desire is to confront the scientific world with an experiment so undeniably above chance that the most sceptical and antagonistic would pale unanimously at its unavoidable implications. This I agree would be delightful, and it is one of the results of research to which I am looking forward. But I do not think the result will be attained by M. Sudre's method.

As an example of an experiment which would be unanimously considered as above chance, he gives a picturesque illustration, imagining the case of a monkey, playing on a typewriter, reproducing his article in *Psychic Research*. But however undeniably above chance this event might be considered, I am afraid that M. Sudre would be disappointed in its effect on scientific opinion. I feel certain that indifference would remain till M. Sudre could exhibit his gifted monkey writing articles regularly for *Psychic Research*, and could show at least some traces, among other monkey tribes, of an ability to contribute articles to magazines dealing with the supernatural. Scientific conviction, after all, is only the name we give to the effect of the prolonged persuasion due to repeatedly observed facts or events.

In my report I used the phrase "fatigue curve" to describe the diminution in success from the early guesses to the later ones. M. Sudre objects that to call the curve a fatigue curve assumes that the faculty is a continuous one, "whereas we may more rationally suppose that it is exercised in flashes under a determinism of which we remain totally ignorant." This is a perfectly just criticism, and I myself do not altogether like the use of the word fatigue, and agree it may be misleading. Whether the faculty is continuous remains to be seen, but in any case I imagine the deterioration to which my experiments drew attention is due more to loss of spontaneity after the initial guess, than to fatigue, unless we use that word in its very widest sense, meaning only the deterioration of some quality or condition unknown.

I agree with M. Sudre in thinking that interference from objective or subjective causes, rather than weariness, is more likely to be responsible for the diminution of success. After the initial guess or divination the normal mind is almost sure to interfere, giving suggestions that a card of a different colour should be tried, or that it is most unlikely that a card will be the same twice running; a preference for red may bias the guessing, and a number will be suppressed for conscious or unconscious reasons. The phrase "censorship-curve" has been suggested, but perhaps it is assuming more than the experiment warrants to do more than to note the deterioration, and call the curve X.

I have discovered one more point on which it is possible to agree with M. Sudre, which is, that in making experiments in clairvoyance it is best to try with people who have the gift. I agree. But even in discovering this amount of agreement I must make a small protest. I have not neglected the golden opportunity of studying the clairvoyant gifts of Mr. Saltmarsh. Mr. Saltmarsh very kindly was one of the first to make some trials, but has not, so far, divined five series of five cards in the way attributed to him by M. Sudre, and I can find no record of such a remarkable success in my report.

I can only deduce, rather sadly, that my paper itself produced a fatigue curve in M. Sudre, but I hope that, given time, he will be sufficiently recovered to attack afresh any further experiments should more of the same nature be devised.

II. THE STATISTICAL METHOD IN PSYCHICAL RESEARCH.

BY R. A. FISHER, Sc.D., F.R.S.

I HAVE been invited to make a few comments on M. Sudre's interesting criticism of Miss Jephson's card-guessing experiment, but after reading Miss Jephson's own reply, it is clear that she is so well able to justify the experimental technique she has adopted that there is very little for me to add on her behalf. The little that I should like to say is rather on behalf of, and in explanation of, the statistical method in general, in relation, not specially with psychical research, but with all phenomena the experimental demonstration of which is not so simple that the facts are universally accepted.

M. Sudre goes to the heart of the matter with the paradoxical statement that the report of a single case in which five cards in succession were correctly guessed on five separate occasions "would have been vastly more convincing to the incredulous" than (presumably) the demonstration that a large number of persons working independently should, while making many mistakes, consistently make better guesses than could be ascribed to chance. How paradoxical this statement is must be apparent to anyone who has watched the reactions of an audience to a good conjurer. All, with their own eyes, have seen him produce a living rabbit from a hat which they have also seen to have been empty; most of them are confessedly unaware of the "normal" means by which the trick is performed. Yet they do not feel that their knowledge of the nature of things has been enlarged, or that new potentialities in their environment have been revealed to them. The unexplained phenomenon is discounted because it is only performed by a special person, the conjurer, presumably after special but unknown preparations. If Mr. Saltmarsh had, really performed the feat, which M. Sudre ascribes to him, before our eyes, his performance would still have carried little conviction to the incredulous, who would, however, be rationally impressed if they were to discover a much slighter power of clairvoyance in repeated trials with ordinary people like themselves.

It is important that the incredulous should react in this way, but it is more important that they are right so to react. If any one of the conjurer's audience, after witnessing phenomena beyond his immediate comprehension, were to face the problem whether they were produced by "normal" or by "supernormal" means, he would see at once that he had not obtained the data necessary for a decision. What is needed is for him to frequent the company of amateur conjurers, and of learners in all stages of acquiring the art. The exhibition phenomena will then no longer be isolated and inexplicable; they will be brought into relation with special apparatus and special adroitness, which he can understand and accept, without necessarily being able to imitate it. In the opposite possibility that they were actually produced by the possession of powers hitherto unknown to science or common experience,

he would receive, what the isolated performance could never give him, adequate and convincing evidence of their reality.

In the investigation of living beings by biological methods statistical tests of significance are essential. Their function is to prevent us being deceived by accidental occurrences, due not to the causes we wish to study, or are trying to detect, but to a combination of the many other circumstances which we cannot control. An observation is judged significant, if it would rarely have been produced, in the absence of a real cause of the kind we are seeking. It is a common practice to judge a result significant, if it is of such a magnitude that it would have been produced by chance not more frequently than once in twenty trials. This is an arbitrary, but convenient, level of significance for the practical investigator, but it does not mean that he allows himself to be deceived once in every twenty experiments. The test of significance only tells him what to ignore, namely all experiments in which significant results are not obtained. He should only claim that a phenomenon is experimentally demonstrable when he knows how to design an experiment so that it will rarely fail to give a significant result. Consequently, isolated significant results which he does not know how to reproduce are left in suspense pending further investigation.

If I have rightly expressed the principles upon which scientific truth is established, it will be obvious why Miss Jephson did not confine her attention to one subject, but rightly extended her enquiry to many. She was trying out an experimental method which might (and may) prove capable of raising clairvoyance to the level of a demonstrable scientific fact. I believe that few will doubt the importance of such a fact, if true, or the need, in a sceptical world, of giving it a critically sound demonstration.

The value of such an experimental method depends on its chance of success if the phenomena is there to be revealed. It depends, in fact, on being sensitive to whatever powers of clairvoyance, however slight, might happen to be possessed by her subjects. It is this that M. Sudre has overlooked in suggesting that only complete success, an exact reproduction of the Suit and Value of the card drawn, should be taken into account. A subject who could always perceive the colour

of the card correctly, but could see no further, would possess very remarkable clairvoyant powers. Such a subject would score complete success only once in 26 trials, whereas one with no clairvoyant powers whatever would score complete success once in 52 trials, or just half as often. With M. Sudre's system of scoring, even so remarkable a clairvoyant could only count on scoring a significant result after several hundred trials; and since data could not be collected on this scale, all such cases would be missed. On Miss Jephson's system the same subject would score an average of 4.01 above chance expectation, and after 25 trials, with a standard error of 2, this is significant; the score, in fact, would serve to call attention to the astonishing special powers possessed by the subject.

This example, though conclusive in showing that Miss Jephson's procedure, in taking account of all the chief points to be perceived by a clairvoyant, is much more sensitive than the crude method of ignoring all but complete success, suggested by M. Sudre, does not really do full justice to her system of combining all powers of perception in one composite test; for the subject might well perceive, sometimes colour, and sometimes form the more clearly, and all tendencies to be right rather than wrong are allowed to reinforce one another in proportion to their power of excelling the results of random guessing. In this matter, as M. Sudre severely says, I have done nothing more than translate into that language of figures the instructions which I have been given; I take, however, full responsibility for judging this to have been worth while, as a contribution to experimental science.

In testing significance, a lower standard should not be taken than twice the standard deviation, and in M. Sudre's sentence: "Hence the score of a series of 25 tests may be attributed to chance if it falls between 9.18 and 13.18," one should read 7.18 and 15.18 as the more reasonable limits. M. Sudre's attempt to demonstrate by example the "univerisimilitude" of the scoring system therefore fails; as indeed any attempt to show that significant results could frequently be obtained without the action of a real cause, is necessarily foredoomed to failure.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 113

LIBRARY CATALOGUE

(SUPPLEMENT 1928-1929)

COMPILED BY

THEODORE BESTERMAN

LIBRARIAN AND EDITOR

PREFACE

THE present second supplementary Library Catalogue covers the additions made to the Library from July 1928 to September 1929. The period from July 1927 to June 1928 was covered by the first supplement, published as Part 108 of *Proceedings* and separately. The main catalogue, complete to June 1927, forms Part 104, volume xxxvii, of *Proceedings*, and is also published as a separate volume.

The organisation of the Library is briefly explained in the Preface to the first supplement, and the Library Rules are reprinted on the next page.

The larger part of the books described in the following pages has been bought out of the grant made to the Society's Library by the Carnegie United Kingdom Trust.

TH. B.

LIBRARY RULES

I. The Library is open every weekday from 10.30 a.m. to 5.30 p.m., except Saturdays, when it closes at 1 p.m. The Library is closed from Christmas Eve to New Year's Day, inclusive; from the Thursday before Good Friday to the following Thursday, inclusive; on Bank Holidays; and during the summer vacation.

II. The Library is open to all Members and Associates of the Society, but books may be borrowed by Members only. Associates may borrow books on payment of 5s. for One Month or 10s. for Three Months.

III. Readers have direct access to all the books in the Library, and are particularly requested not to replace volumes on the shelves.

IV. Borrowers are required to fill up and sign a voucher for each book taken out. The vouchers are cancelled and returned to the borrower when the books are returned, and borrowers are held responsible for the books so long as their vouchers remain uncanceled. Books may be requisitioned by post, in which case vouchers will be sent and should be returned completed.

V. The usual number of volumes allowed to each borrower is Three, and the usual time One Month, except in the case of books much in request, which are allowed for shorter periods. Books may be allowed for a longer period, on application being made, if they are not required by another borrower. Special arrangements may also be made for borrowers requiring a larger number of volumes.

VI. Any volume may be called in at any time with seven days' notice from the Librarian.

VII. Old books and books of special value or such as it would be difficult to replace if lost can only be lent out by special permission, on application being made in advance.

VIII. Borrowers are required to pay the carriage of all books sent to them. All books sent to the Library must be well packed and prepaid. Borrowers are held responsible for any delay, loss or damage to books in transit to the Library.

IX. Marking any book, whether by writing, turning down the leaves or otherwise, is strictly prohibited.

PART I

PERIODICAL, SERIAL, SOCIETY AND OFFICIAL PUBLICATIONS

- Bombay, Government of**, Report on the Work of the Water Diviner for the Period October 1925 to January 1927. 8vo, pp. 12. Bombay 1927. [RDo 3 (4)]
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- British Museum**, Jacob Boehme. An Extract from the Catalogue of Printed Books. Folio, pp. ii. 13. London [1922]. [25 i 18]
- Columbia University : The Library**, A Contribution to a Bibliography of Henri Bergson. Preface by W. D. Johnston. Introduction by J. Dewey. 8vo, pp. xiii. 56. New York 1913. [RBi 4 (1)]
- Institut Métapsychique International**, Les Conférences en 1926. 8vo, pp. 134, 18 diagrams and ill. Paris [1927]. [6 i]
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- International Psycho-Analytical Library, The**. Edited by Ernest Jones. London. Vienna, New York [nos. 1-2, 7 ; 3, 8, etc. : London ; 4-6 : London, Vienna]. 8vo. [10 h]
1. J. J. Putnam, *Addresses on Psycho-analysis*. pp. ix. 470. 1921.
 2. *Psycho-analysis and the War Neuroses*. pp. v. 59. 1921.
 3. J. C. Flügel, *The Psycho-analytic Study of the Family*. pp. xii. 259. Third edition. 1929.
 4. S. Freud, *Beyond the Pleasure Principle*. pp. vii. 90. 1922.
 5. E. Jones, *Essays in Applied Psycho-analysis*. pp. vii. 454. 1923.
 6. S. Freud, *Group Psychology and the Analysis of the Ego*. pp. vii. 134. 1922.

- 7-10. S. Freud, *Collected Papers*. 4 vols. 1924-1925.
11. S. Ferenczi, *Further Contributions to the Theory and Technique of Psycho-analysis*. pp. 473. 1926.
12. S. Freud, *The Ego and the Id*. pp. 88. 1927.
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PROCEEDINGS

OF THE

Society for Psychical Research

PART 114

MARCH, 1930

SOME THOUGHTS ON D. D. HOME.¹

By COUNT PEROVSKY-PETROVO-SOLOVOVO.

It is not by any means the aim of the present paper to prove either that the so-called physical phenomena of spiritualism do not exist at all or that D. D. Home had no genuine mediumistic power. Such an aim would be far too ambitious ; besides, such is not precisely my view. My object is different. It has often been alleged that there is no evidence that Home—that bulwark, that alpha and omega of physical spiritualism with whom modern spiritualism must stand or fall, to use Mr. Podmore's words—was ever detected in fraud. I will attempt to show that this supposed immunity from detection is a fiction and that evidence to the contrary undoubtedly exists ; I will also insist on some elements of weakness—very serious elements—inherent in my opinion in the Home evidence. My conclusion had better be stated at the outset : the great bulk of D. D. Home's phenomena must be thrown overboard. A certain residuum, not easily explicable, will remain. But though I have no ready explanation to offer with regard to it, it seems to me, I confess, somewhat vitiated by its association with so many other " manifestations " which can be accounted for with so little difficulty, and whose explanation lies, as a matter of fact, on the surface.

I will begin by referring the reader to my paper in the *Journal* for July 1912 (xv. 274-288) entitled : " On the alleged Exposure of D. D. Home in France," in which I reproduced two letters by a Dr. Barthez, physician to Napoleon III's son—the Prince Imperial—dated respectively Biarritz, 5th September and 25th September, 1857, and printed in the *Revue de Paris* (1912, pp. 80-84). Dr. Barthez's

¹ This paper was read at a *Conversazione* (for Members and Associates only) on 6th November, 1929.

letters have since appeared in book form.¹ In the second of these two letters the following passage occurs: "It will amuse you to learn that we have at last grasped one of the methods by which Mr. Hume evokes spirits. As a result the Empress is reduced to saying that the Hume of to-day is no longer the Hume that he was, that he has lost his power, and that he tries to replace it by trickery. The thing is very simple. Mr. Hume wears thin shoes, easy to take off and put on; he also has, I believe, cut socks which leave the toes free. At the appropriate moment he takes off one of his shoes and with his foot pulls a dress here, a dress there, rings a bell, knocks one way and another, and, the thing done, quickly puts his shoe on again. This has been seen by M. Morio,² who has made of it a fine record, written and signed, with all the details necessary to establish the authenticity of his discovery. Hume saw that his secret had been guessed and I assure you that he cut a pitiful figure . . . The sittings for the evocation of spirits at once ceased at the castle and we hope that this undeserving character is put out of currency. Nevertheless Her Majesty is unable to accept the fact that anybody should have had the impertinence to make a mockery to such a point, and during a whole year, of herself and of the Emperor."³

In Dr. Barthez's letter we had at last in a concrete and authentic form an extremely vivacious legend connected with D. D. Home's name which had hitherto obstinately refused to "materialise," though cropping up again and again. Hence the very great importance of this document.

In my above-named paper I arrived *inter alia* at the following conclusions:

"(1) The famous legend as to the French exposure of Home has at last been 'hunted down.' It resolves itself into a real incident which happened in the course of Home's sittings at Biarritz about 20th September, 1857, and which, there is good reason to believe, consisted of an attempt at trickery on Home's part. . . ."

"(2) The sort of trickery used was precisely of a kind suggested to an attentive reader by a certain category of Home's performances. . . ."

"On the whole, a fairly good instance, I think, of the partial truth of the saying: '*Il n'y a pas de fumée sans feu.*'"⁴

It has been objected⁵ *à propos* of a few words I said about Home

¹ *La famille Impériale à St Cloud et à Biarritz* (Paris, no date).

² Baron Morio de l'Isle, "Prefect of the Palace," whose name also appears in the same connection in Comte Fleury and L. Sonolet's *La Société du Second Empire*, 1851-1858, pp. 185-186. This coincidence is very significant since the latter work has nothing to do with the Barthez letter.

³ [The French text is printed in *Journal*, xv. 281, and in the Appendix to the present paper. *Hon. Ed.*]

⁴ *Journal*, xv. 288. ⁵ Miss Dallas in *Journal* (January 1929), xxv. 17.

and the Biarritz exposure in the *Journal* for November 1928 (xxiv. 370 n.) that according to a statement of the late Sir William Crookes's he never detected any trickery or deceit whatever on Home's part, "nor heard any first hand evidence of such from other persons."¹ Well, the last few words would rather imply in my opinion that the great scientist probably was acquainted with *second-hand* evidence bearing on the subject; at any rate, there is in the sentence quoted absolutely nothing disproving such a supposition. And few will, I think, be disposed to dispute, that cumulative second-hand evidence may in the end become irresistible.

I have also been reminded of Professor Charles Richet's words: "Sometimes indeed, as in the case of Dunglas Home, it has had to be acknowledged that the accusation of fraud was unjustified."² I answer: if M. Richet has in his possession evidence refuting Dr. Barthez's testimony, it is to be earnestly hoped that he will communicate it to our Society at once. If he has not, such a sentence has about it nothing decisive.

Again, the late Sir W. F. Barrett (and obviously F. W. H. Myers) failed, I am told, to obtain adequate testimony as to the detection of Home. Now what does this prove? How can such a circumstance affect the Barthez evidence which obviously remained unknown to these two gentleman? It affects it just as little as the fact of A, B and C *not* having seen a certain act performed by E proves—or may prove—nothing against the evidence of D who *did* see it.

It has also been pointed out that the undoubted fact of D. D. Home having entertained relations with the Empress of the French several years after the exposure, gives us reason to think that the story of the detection may have been a calumny. To all those who know how easily—especially in the domain of spiritualism—persons anxious to believe are apt to forget such unpleasant incidents and indeed all evidence unpalatable to them, this circumstance proves little or nothing.

The following two letters bear on the opinion which the late Empress Eugénie apparently entertained of the Biarritz episode at the end of her life. Both are addressed to myself and are now printed for the first time:

"Dear Sir,

"9th March, 1929.

"I have only to-day had your letter of the 9th of February, which has been following me. Here is the information you ask for: . . .

"This passage³ was published by me in the *Revue de France* for January 1924. Some time afterwards Dickson, who was giving lectures against spiritualism in which he told, *en passant*, the story

¹ *Proceedings*, vi. 99.

² Quoted by Miss Dallas, *loc. cit.*

³ An extract from Dr. Barthez's letter of 25th September, 1857, (*Journal*, xv. 287; see also above).

of Home at Biarritz, told me that he had been approached, in the Salle de Géographie on the conclusion of one of his lectures, by M. Morio de l'Isle *fils*, who confirmed that his father had often told him this anecdote.

"For my part I called, in February, on Prince Roland Bonaparte and took him the passage from the letters of Dr. Barthez, begging him, since he was soon to see H.M. the Empress Eugénie at Cap Martin, to ask her if these things had really happened as described.

"Some weeks after I again saw the Prince, who told me textually : 'The Empress remembers the things very well and confirms all the details. She no longer remembers who turned up the gas ; but Home was undoubtedly in the scandalous attitude described by Barthez.' I therefore published this new testimony in my book *Où en est la Métapsychique ?* . . .

"(Signed) PAUL HEUZÉ." ¹

"Sir,

"12. III. 29.

" . . . I certify that :

"(1) On the 1st of May, 1919, Monsieur G., French Consul, called on me at the Sociétés Savantes with his friend M. Morio, son of the one who exposed Home on the 5th of September, 1857, at Biarritz. M. Morio described to me how his father had unmasked Home.

"(2) On the 7th of November, 1920, Prince Roland Bonaparte came himself to the Société de Géographie, of which he was President, to confirm the authenticity of the Biarritz sitting, which had been several times described to him by the Empress Eugénie.

"The Prince is dead, but his daughter, Princess George of Greece, who was *au courant*, is here to certify the words of her father . . .

"(Signed) Prof. DICKSONN, Cte. de St. Genois."

We are not bound to regard either M. Paul Heuzé as infallible or "Professor" Dicksonn—a conjurer—as absolutely impartial. But the statements by the late Prince Roland Bonaparte they refer to are precise and categorical. We have no right to attribute to them deliberate lying, nor do I see any reason why we should attach to their assertions less importance than to such a passage as appears in my paper of 1912 (*Journal*, xv. 283) : "Mr Feilding having made inquiries, heard from a friend of his in the Empress's *entourage* that Her Majesty had lately expressed, in conversation on the subject, her firm belief in the genuineness of Home's performances, and had

¹ M. Heuzé is a well-known adversary and exposé of fakirs and mediums. He is not however irrevocably hostile (a circumstance to be borne in mind) to *all* supernormal phenomena. [The French text of this letter and of the one following is printed in the Appendix. *Hon. Ed.*]

said that, in her opinion, the stories of a gloved foot under the table were false." I note the apparent discrepancy and pass on.

Here is another testimony on the Biarritz incident. The writer is the late Professor Cyon of the Imperial Medical Academy in Saint Petersburg. The sitting (Home's) to which he refers at the beginning, took place in the presence of some Russian *savants* (also of Aksakov) and was a perfect blank :

"Immediately after the publication of my article on the sitting of 10th March, 1871, General Comte Fleury, one time Ambassador at Petersburg, communicated to me through Dr. Pélikan the details of a sitting at Compiègne where he had succeeded in catching Home in the act. At this sitting were present the Emperor, the Empress, Princess Metternich, and a few other members of the Court. Round the table Home had on his left the Empress Eugénie, on whose left sat Napoleon III. Count Fleury, seated facing Home, was struck by the persistence with which the latter directed the conversation in such a way that the Empress was obliged to turn continually towards the Emperor to put questions to him.

"Suspecting some conjuring trick, General Fleury asked permission to withdraw ; he left by the door at the right of the table, but returned unobserved by another door behind Home. He then saw the latter open the sole of his right shoe, leave his naked foot some time on the marble floor, then suddenly with a rapid and extraordinarily agile movement, touch with his toes the hand of the Empress, who started, crying, 'The hand of a dead child has touched me !' General Fleury came forward and described what he had seen. The following day Home was embarked at Calais, conducted by two *agents* ; the order was to keep the incident secret." ¹

Again, I do not by any means regard Professor Cyon as either infallible or impartial. On the contrary. And the inaccuracies are numerous and obvious. But the main facts are the same : (a) the sitting takes place at Napoleon III's Court ; (b) the process of the alleged fraud is more or less identical. That, writing in 1910 or 1909 Professor Cyon should have forgotten many details, written "Compiègne" for "Biarritz" (both imperial residences), etc., the facts having been communicated to him at second-hand nearly forty years before, is surely only natural. To me, however, Dr. Barthez's evidence suffices. I only wish to draw attention to the fact that it is confirmed from several other sources which are absolutely and entirely independent.

Was it at first hand ? We do not know. I incline at first to think not, though his daughter, Mme. Pouquet says (*Journal*, xv. 282) : "He had witnessed it himself." But let us here make what the

¹ Dr. E. Cyon, *Dieu et Science* (Paris 1910), p. 255. [The French text of this extract is printed in the Appendix. *Hon. Ed.*]

Germans call a *Seitensprung* and reason somewhat as follows : X, let us suppose, has great luck at cards, and many people suspect him of cheating. There is a rumour that he was once caught re-handed ; this rumour varies so far as dates and places are concerned, but it is a very persistent one. It does *not* vary—or varies little—as to the *method* of cheating. Investigators try to find its origin but fail. Then suddenly a letter turns up, written by a gentleman, Y, who may or may not have been present himself at the detection, but who certainly had every possibility of learning exactly what had happened. The letter is written at the time of the exposure, in the town where the incident took place, and the fraudulent method therein mentioned is the one which had been rumoured about for so many years. What are we to think ?

The answer is obvious. We may not regard the letter as absolute proof, but to deny that it justifies critics in declaring it very probable that X *was* detected in cheating and in using precisely such and such a method, would, in my opinion, be absurd.

The more so, as—I have often urged this point before—the *modus operandi* alleged to have been used by D. D. Home in September 1857 at Biarritz is over and over again suggested by accounts of his phenomena which we possess. To maintain that the action of D. D. Home's feet could explain everything we read about him, would be absurd. But that it can account for a good deal is an undoubted fact, a fact on which the Biarritz exposure throws an additional if somewhat lurid light.

Let the reader consider, for instance, the following passage from one of Aksakov's works : “ Sometimes at Home's sittings (Home's hands being upon the table and a candle burning) I would hold under the table some object ; a pencil, a bell or a handkerchief, and would feel how something began to touch, to take and to pull it ; if I let it go, it would not fall down ; I once put my hand under the table with a ring on one finger in order that it should be taken away, as I hoped thus to know the operating agency better ; all at once tender but firm fingers began to work, trying to take off the ring, in so doing they naturally and inevitably touched my hand and I was fully convinced these were living, warm, thin, human fingers.”¹ Professor Boutlerov, who witnessed similar incidents mentions, it is true, in his account first published in the Moscow review *Russky Vestnik*, that Home's feet “ were dressed in boots, were controlled and did not move ” ; but we are not told whether this “ control ” was uninterrupted, what it consisted of and whether the boots were not of such a kind that they could be slipped off and put on again with impunity and without difficulty. I may add that I once asked M. Aksakov, who, if I mistake not, was a relation of the second Mme.

¹ *Pradvestniki Spiritisma* [*The Precursors of Spiritism*], p. 485 n.

Home's,¹ "Did you control Home's feet?" To this he replied in the affirmative, but at once added: "And then you surely cannot tie a knot in a handkerchief with your toes." From which I concluded that it was chiefly in the *nature* of the supposed phenomena, not in the *character* of the control that M. Aksakov saw a guarantee of authenticity. Now, that an experienced conjurer—especially one who has chosen to specialise in that way—is able to tie a knot thus, I have no doubt.

To turn to the chief scientific witness to Home's "manifestations," Sir W. Crookes describes (*Proceedings*, vi. 123) a case of "direct" writing (also under the table) which he calls "as striking a manifestation as I have ever seen." In this case again not a word is said about the position of Home's feet: an omission which is certainly unfortunate and strange. Perhaps it will appear less strange to us when we reflect that Sir W. Crookes seems to have been much impressed once by a sitting in which three mediums took part, Home being one of them and Herne and Williams the other two; an episode which surely justifies us in assuming that his canons of evidence in the domain of the physical phenomena may not always have been identical with those of the average psychical researcher of this year of grace 1929—nay, of some years earlier.

A propos of the letter, dated 12th April, 1871, mentioning the sitting in question, which will be found in Dr. Fournier d'Albe's *Life of Sir William Crookes* (pp. 191-3),² I may be permitted to recall very briefly in passing: (a) that the greatest scientific attainments and achievements are no guarantee that an investigator will prove a competent observer in dealing with phenomena such as those of Home and other mediums; (b) that in our researches the personality and individual capacities and qualifications of such investigators are of far more vital importance than the personality of the medium. For against the latter's supposed attempts at cheating—supposed or indeed very real—we may, in theory at least, adopt precautions which will completely nullify such attempts. As for the investigator, if his name is one of scientific eminence, we are sometimes, in retrospect at least, almost at his mercy. I shall never forget Aksakov's attitude towards the Crookes and Zöllner investigations; it irresistibly reminded me to some extent of a believing Christian's

¹ It should be noted by the way that nothing or hardly anything is said in either of her two books (*D. D. Home, his Life and Mission* and *The Gift of D. D. Home*) about the Biarritz scandal, a circumstance the more worthy of notice as much space is devoted by her to recounting and ridiculing the many often grotesque and absurd rumours and *racontars* circulated about her husband, the very grotesqueness or exaggerated character of which made refutation the more easy. This omission seems, to me, to be of a certain weight in estimating the general character and value of the two books.

² The writer's comments on the letter are suggestive and, I think, justified.

attitude towards the Gospel. No doubt our position now is more favourable than that of the generation of fifty or sixty years ago ; yet there is still some room for improvement.

To return to D. D. Home : the Biarritz episode is not the only one in which he may be reasonably suspected to have used fraudulent methods. In our *Journal* for May 1903 (xi. 76-80) a narrative is printed by a Mr. Merrifield (if I mistake not, Mrs. Salter's grandfather) which is unfavourable to the genuineness of Home's "spirit hands." And—a very eharacteristic detail surely—from a passage of this narrative we are allowed to infer further that at a certain moment the medium's hands were under the table and apparently free : a eircumstance which, eoupled with not a few statements appearing in other aecounts (I have in view partieularly Lord Dunraven's very eandid and instructive *Experiences in Spiritualism* ¹), throw an ominous light on the eonditions of eontrol which seem to have prevailed often enough, very possibly as a general rule, at D. D. Home's sittings.

These *Experiences* in partieular prove to us quite eonclusively, it seems to me, that it was virtually Home who eontrolled and directed the sittings ; and that preeautions against fraud were either praetieally non-existent or obviously inadequate. Omissions appear in the aecounts which prove that the observers had no idea of the eonditions required to make an experiment even relatively eonvineing. Home was praetieally at liberty to aet as he liked, moving freely about the room and even leaving the room². To absolutely non-evidential (if not absurd ³) trance addresses the eye-witnesses obviously attached—judging by the eare with which they reproduced these addresses almost verbatim—the same importance as to eonelusive phenomena. And in some few eases we may indeed infer quite legitimately that Home eould have introduced a eonfederate into the *séance*-room without much difficulty. In saying this I have partieularly in view the sitting of 9th February, 1869 (*Experiences*, p. 173) ; and surely it is signifieant that in Lord Adare's (Dunraven's) aecount, whilst not a word is said as to how the supposed materialised form disappeared, we have plenty of details as to absolutely non-evidential talks held at the sitting.

It may not unreasonably be asked whether the medium's eloquence did not play here, so to say, the part of a smoke-screen to eover the materialised spirit's retreat.

Here are a few extraets which enable us, I think, to form an

¹ My references are to the S.P.R. reprint in *Proceedings* (1925) xxxv.

² See in partieular *Experiences*, p. 166 and p. 167, footnote, from which it follows that on a certain oecasion Home may have left the room twice in eonnection with a single "experiment" ! Here a slight doubt seems to have entered even Lord Dunraven's mind.

³ *Experiences*, pp. 144, 158, 245.

adequate idea of the conditions of control prevailing as a general rule at least at the Dunraven sittings. In the account of the 62nd sitting of 12th March, 1869, we read (*Experiences*, pp. 221-222) :

“He [Home] got up, and acting under an uncontrollable impulse, walked about the room, his hands and arms being strangely waved about and agitated ; he made mesmeric passes over us all . . . He made passes for some time over my father’s forehead, the back of his head, and behind his ears, occasionally going to the table at which we had previously been seated, and extending his fingers over it as though withdrawing some influence from it. While walking about he suddenly stopped in the middle of a sentence with a violent gasp, and sinking on his knees went into a trance. He got up, walked about, apparently conversing with someone, and then, taking each of us in turn by the hand, led us to the other table, placed chairs for us, and signed to us to sit down. My father requested me to bring paper and a pencil ; but Home shook his head, and afterwards brought them himself. He then commenced arranging the furniture in the most minute detail, consulting apparently all the time with someone. He placed the small round table near us and behind my father, and moved a chair up to it ; he altered the position of several of the chairs in different parts of the room, placed the miniature portrait of his wife on the small round table, and the case containing little Dannie Cox’s photograph on the large table behind me, then going to the bookcase he took out several books, looked into them and replaced them ; at length he appeared to find what he wanted, for he took out a volume, folded his hands across it on his breast, and after standing for a few seconds in a most reverential attitude, sank down upon his knees and appeared to pray earnestly ; then rising to his full height he held the book as high as he could above his head and placed it upon our table.”

Here is an extract from the account of the 68th sitting, 10th April, 1869 (*Experiences*, pp. 249-250) :

“Home got up, took a striped rug off the sofa, and covering his shoulders, head, and face with it, began walking about the room in a stealthy manner, hiding behind the furniture, and crawling about flat upon the ground, apparently lying in wait for some one. Suddenly he put his hand upon the candle, and left us in almost total darkness. I could just distinguish him gliding about the room, and crawling on the floor.”

Imagine the results a conjurer could achieve when operating under such conditions ! Now it is needless to say that from the critical and scientific standpoint we are bound to regard D. D. Home as having very possibly been a tolerably good conjurer ; indeed we have no right to regard him in a different light.

The following extract from Lord Adare’s *Experiences* bears not

so much on the conditions of control as on the degree of critical acumen displayed by the sitters. At the sitting of 6th March, 1869, already mentioned (*Experiences*, p. 209) :

“He [Home] placed chairs for us all, drew over a little table, and sat down. It was pitch dark. Immediately a hand was laid on the back of my head. He said ‘They will touch you with flowers.’ Both Mrs. Wynne and I felt them. He said to Mrs Wynne, ‘It was John who touched you.’ We heard at a little distance the sound of flowers being stirred and broken, and immediately some were placed in our hands. Mr. Home said, ‘Take Daniel’s hands, dear Emily, we want you to be able to tell others that you held his hands and felt his feet.’ While Mr. Home’s hands and feet were thus in contact with Mrs. Wynne’s we all simultaneously felt flowers waved across our faces, heads, and hands.”

From which it seems to follow that the writer (Lord Adare’s father) was inclined to regard as supernormal phenomena occurrences taking place in the dark and when the medium’s hands were free. Needless to say, the mere fact of the latter having been later in contact with Mrs. Wynne’s hands affords us but an inadequate guarantee of genuineness.

At another moment in the same sitting, a flower pot with cyclamens having been put under the table, and raps having been heard, Lord Adare’s father reports the phenomena thus: (*Experiences*, pp. 207-208) : “I said to [Major] Blackburn : ‘Get under the table and hold Mr Home’s feet.’ He did so, and we heard the raps distinctly over his head. Mr. Home suddenly said ‘Oh, look at the hand near me holding a flower!’ Twice he said that he saw the hand. I, somehow instinctively, put my hand under the table, and immediately felt a flower placed very gently in it. The following was then given : ‘The flower is from Augusta, with fond love.’”

Now surely the crux of the whole matter is, in the present instance, whether Home’s feet were held *at the moment* the flower was placed in the hand of Lord Adare’s father. But as to this all-important circumstance we are told nothing. Let us however be fair and note that there is some evidence of a critical attitude at times. Thus Lord Dunraven tells us in his account of Sitting No. 76, 25th June, 1869, that having had the opportunity a few days before of seeing Miss C— R— write “under supposed spirit influence,” he “obtained permission to put a few questions. Among them I asked, ‘What do you think of Mr. Home?’ Miss R— wrote instantly, ‘He has a certain degree of power, but a vast amount of trickery.’ In answer to another question was written, ‘He [Home] deceives people by pretending that he can call up the spirits of their friends, etc.’ These and other answers made me think that this was probably a deceitful spirit” (*Experiences*, p. 270).

So Lord Dunraven *did* show some scepticism at times. But this scepticism appears to have been chiefly directed against those who doubted Home's powers, even when such suggestions of doubt came from what was to him the dark Beyond.

The following extract is, I think, particularly significant. At Sitting No. 27 (no date, but clearly in October 1868) we are told, (*Experiences*, p. 128) :

"Home then fetched the lamp back. We heard a knocking at the door, he opened it and appeared to invite some one to come in, but did not succeed ; he shut the door, when the knocking recommenced he opened it again, but was unsuccessful ; this was repeated three or four times, at last he went and gathering some ferns and flowers from off the coffin opened the door and held them out ; still it was in vain, the knocking again occurred at the door, and this time he took little Ada and led her to the door, when he appeared to succeed in inducing the person to come in. He said, ' It is — ' (a little servant girl who had died two days previously)."

Why have I called this extract particularly significant ? Because it seems to show that, at times at least, Home would have been able to admit an accomplice into the room if he had chosen to do so. The number of sittings where we have to suppose that such an introduction *must* have taken place, if we want to explain the phenomena in a natural way, is indeed exceedingly small, but, as I have pointed out already, there *are* such sittings. In the account I have just quoted no particularly sensational incident occurs after Home's movements, openings of the door, etc. But it is, I think, an essential part of a fraudulent medium's equipment that his sitters should get accustomed to view without suspicion certain gestures which may ultimately play an important part in producing the phenomena. In order to allay suspicions nothing of any importance will follow such gestures or movements at first or indeed in most cases. It is therefore conceivable that the sitters will attach to them no significance and therefore will end by omitting them from their accounts ; and indeed this may have happened in connection with the sitting of 9th February, 1869, though another explanation is also possible : that the accomplice may have been hidden in the room.

Now there is every reason to suppose that the 78 Dunraven sittings¹ were not different in quality from Home's other sittings. We may therefore legitimately infer that in the accounts we possess of some of the latter (not excepting the Crookes evidence) there are

¹ Among them we have the famous sitting of 13th December, 1868 (Home being levitated out of one window of Ashley House, Victoria Street, and carried into another). This occurrence, says the then Master of Lindsay, later Earl of Crawford and Balcarres, one of the sitters, took place with the moon " shining full into the room." As a matter of fact the new moon could not then light the room, however faintly (*Experiences*, p. 152, footnote).

omissions, perhaps not unimportant ones, likely to afford us a natural clue to episodes apparently inexplicable. In the numerous involuntary indications Lord Dunraven's candid account gives us in this respect lies in my opinion the chief value of the *Experiences in Spiritualism with D. D. Home*.

To return to Sir William Crookes's experiences: when he published for the first time his accounts of investigations in the phenomena of Spiritualism (I refer chiefly, but not exclusively, to his first papers in the *Quarterly Journal of Science* for 1871) the reader was certainly entitled to conclude from his descriptions that these investigations were scientific experiments occurring in fairly good light and under conditions reminding us more or less of those which prevail in a laboratory. This was, so to say, a first phase.

But in 1889 the "Notes on séances with D. D. Home" published in *Proceedings* presented us with a somewhat different picture. The supposedly carefully planned experiments proved chiefly—though perhaps not exclusively—to have been incidents in spiritualistic sittings selected out of a more or less long series. From various passages it was obvious that these sittings were not very different in character—except perhaps for the personality of some of the sitters and at times for the intensity of the phenomena—from many ordinary ones. This was, so to say, phase number two.

Already at this stage it was becoming obvious that in order to attach a decisive importance to observations occurring under such conditions psychical researchers must needs throw overboard a canon of evidence imposed on them but a little time previously with much insistence and against not a little opposition. Indeed if—as some of our most eminent members were inclined to conclude from such experiments as those of S. J. Davey—no paramount importance can be attached to observations made in full light and under apparently very simple conditions (I have in view Eglinton's slate-writing) but conditions not excluding the necessity for "continuous observation," what were we to think of the evidential cogency of supposedly supernormal occurrences sometimes of a rather complicated character taking place in poor light and often unexpectedly, such as Sir William Crookes briefly described? The conditions for continuous observations seemed here still more unfavourable than at the Eglinton sittings.¹

It seems to me that the Adarc *Experiences in Spiritualism* bring us one step further. We seem to have entered on a third stage.

¹ Personally, it is true, I have never attached decisive importance to the negative argument derived from continuous observation. Why? Because to me it is the personality of the witness or observer which is the paramount factor, all the rest being of subsidiary importance. But I am speaking here from the standpoint of the psychical researcher of the Hodgsonian type—in the physical phenomena, not in the Piper sphere.

They present us with regard to the Home sittings and to Home's behaviour at these sittings with a picture to which Sir W. Crookes's accounts had not accustomed us. Various actions of the medium's are described as to which the great scientist's notes seem to be much more reticent. What are we to conclude? Seeing as I have already said that there is not the slightest reason to think that the Adare sittings were different in quality and in character from other Home sittings, we are justified in thinking that if Sir William Crookes's notes had been as full as Lord Adare's we should have found in them, now and then, indications suggesting a possible natural explanation of occurrences which as described seem to exclude it. This is only a hypothesis, but a hypothesis based, it seems to me, on data not to be lightly set aside.

Here is another negative testimony bearing on Home, and a particularly significant one, because coming from Baron du Potet, the celebrated French mesmerist. He writes (I quote from the *Revue Métapsychique* of September-October 1927, p. 390):

"There is something mysterious to investigate, and it is only with circumspection that we must admit the avowed and advocated cause of all the strange phenomena produced by Mr. Home. In any case our feeling is that there is a mixture, that all is not always independent of the medium himself and that one day we shall be astonished to learn that the psychic force was not alone at the service of Mr. Home. But is it I who dare to-day write these words of distrust, I who have praised and extolled this extraordinary medium, I the spiritualist? Why not, if doubt has entered my mind?"¹

Du Potet had therefore doubts—very possibly very grave ones—as to the genuineness of some of Home's performances. He expresses them in somewhat Delphian language, and gives no details. How many of the great medium's clients and friends, it may be asked, may have felt the same suspicions and under strong pressure have been prevented from uttering them? *Chi lo sa?*

These two instances—Mr Merrifield's and Baron du Potet's—show us that, even apart from the Barthez evidenee, we have testimony tending to disprove the legend as to Home's immunity from detection. In any case there are in his career several suspicious incidents. And there is besides this most sinister similarity between the substance of the Biarritz detection and the character of a great many phenomena which have been recorded of him.

I pass on to another aspect of the subject, and I think, a very important one: the late Mr. Podmore has been accused in our publications—and possibly elsewhere—of making insinuations against the honesty of Stainton Moses and also of Home (*Journal*, xxi. 140). Against such accusations I feel bound to protest emphati-

¹ [The French text of this quotation is printed in the Appendix. *Hon. Ed.*]

cally. D. D. Home seems to have led, on the whole, a very pleasant and attractive life, moving in the "best" society (not precisely a very competent judge in such matters as psychical research as a general rule, I am afraid) sitting to-day with Napoleon III of France, to-morrow with Alexander II of Russia,¹ having the satisfaction of his phenomena being endorsed by one of the first scientists of the century. He is supposed never to have been *paid* in the proper sense of the word, but he most undoubtedly derived from his sittings and from the glamour emanating from them very substantial and useful material advantages. As a general rule the conditions under which these sittings took place were such that, judging at least by what Lord Adare tells us, in nineteen cases out of twenty, it was a question not of Home being prevented from cheating, but of his being unwilling to deceive. The "manifestations," if of no fraudulent origin, were most obviously inexplicable by known natural laws. It does not by any means follow necessarily from this circumstance alone that they were of a spurious character; and I for my part do not propose to reject any really well attested evidence in, I think, almost any domain on purely *a priori* grounds. But—and this is with me a crucial point—so long as the authenticity of the alleged phenomena has not been proved beyond doubt, we are perfectly and absolutely justified in regarding the good faith of the supposed medium or clairvoyant, or whatever else, as open to suspicion. This, quite independently from the "social position" (a rather elusive conception) of this clairvoyant or medium, and whether his name be D. D. Home, Rev. Stainton Moses, Chamberlain X, or any other. To me this is, I confess, an axiom so self evident that I am almost formulating it with a certain *gêne*.

This self-evident truth amply justifies us in casting a shadow—the shadow of a doubt—upon Home's memory and career, without taking much into account in what society he moved, whose friendship he enjoyed and in what drawing-rooms he was lionised.

Two more remarks. *Primo*: we are often told that the physical phenomena of Spiritualism are produced, on the spirit hypothesis, by an inferior class of entities, to whom we cannot look either for high moral teaching or for proofs of identity.² But with Home, on the contrary, the phenomena have a distinctly spiritualistic character; they are originated by the "spirits" of the sitters' deceased relations, if not invariably at least often; identity tests seem also to be given at times. Surely it is a significant fact that this deviation

¹ One of whose A.D.C. Generals was my maternal grandfather. He believed Home to have authentic power, but to cheat occasionally. Mrs. Salter has seen a translation of part of a letter received lately by me from my grandfather's daughter and bearing on the subject.

² I know of course that there are exceptions to this generalisation. But broadly speaking such a statement has become almost a truism.

from what we were entitled to consider a "law" occurred in the case of a privileged medium, a *rarissima avis*—who enjoyed an unique opportunity in having access to various sources of information concerning his clients' private affairs and family matters. For it is obvious that many of these clients looked on Home as *un des leurs*, as one of their number, and had every confidence in him. By calling attention to this curious fact, I do not necessarily imply that its explanation *must* be of a sinister character; but if not so, it seems to be one more instance of that diversity of "laws"—real or alleged—in the domain of spiritualism, and in particular of physical spiritualism, which we are well entitled to regard as somewhat disconcerting.

Secundo: Were I asked which of Home's phenomena seem to me nevertheless particularly unlikely to have been due to trickery and cheating, I would answer, I think, that some of his performances with burning coals, especially such a case as Mr. S. C. Hall's (*Experiences*, pp. 280-282), would appear to me to exceed the limits of conjuring (but not of a peculiar form of suggestion perhaps); and in this connection I would add that the most conclusive evidence collected by the S.P.R. as to physical phenomena is possibly that which deals with alleged facts of a similar character (such as the Fire-walk). It is not inconceivable, some will think, that in this similarity there may be something more than a mere chance coincidence.

Before finishing I wish to remark that, so far as I have ascertained, some people are even now somewhat "touchy" when doubts are expressed as to the moral character of D. D. Home. Such people will permit me respectfully to remind them that the celebrated medium belongs to history. And that to matters historical that Latin saying (a saying, it seems to me, of somewhat doubtful wisdom): *De mortuis nil nisi bene* cannot, by any means apply. Otherwise there would be no history!

I also wish to say in conclusion a few words explanatory of a passage of my paper which is likely perhaps to be misunderstood. I said above that if the investigator's "name is one of scientific eminence we are sometimes, in retrospect at least, almost at his mercy." This sentence must not be interpreted as implying hostility towards *savants* on my part. Believing as I do Science to be the greatest benefactress of mankind, how could I view those who represent it otherwise than with feelings of the greatest respect, and often of the greatest gratitude? But from this to the infallibility of *savants* there is a long way. From this to asserting that a scientist cannot be, in the sphere with which we are here dealing, an inadequate observer¹ there is also a long way. Nor am I, I confess, altogether

¹ The late Professor N. Wagner of the University of St. Petersburg, one of the pioneers of spiritism in Russia and a Corresponding Member of the S.P.R.,

convinced that even a prince of Science will always be ready publicly to admit that he made a mistake—that he has been in fact deceived—however easy it has always seemed to me to recognise one's mistakes. Nor do I think a man of science, whatever name he bears, is justified in omitting to describe in detail what precautions he took against fraud in his researches; and by the way, in so speaking I am merely repeating what Mrs. Sidgwick said in 1886 in a paper on the physical phenomena of spiritualism in our *Proceedings* (iv. 65), specially mentioning by name in this connection Sir William Crookes and Professor Zöllner. I do not think it improbable that the explanation of part of the halo which still surrounds D. D. Home's name in the domain of so-called *scientific* investigation of his phenomena is to be sought somewhere in this direction; and coupled with the undoubtedly suspicious incidents and circumstances spoken of above, the whole seems to me to detract a good deal from the reputation of the greatest of known physical mediums. I do not feel justified in asserting more; but so much can, I think, be asserted safely.

Note which Sir Oliver Lodge wishes appended to the foregoing Paper, as an additional caution to new members:

Although I had no first-hand experience of Home's phenomena, other evidence has convinced me that psycho-physical phenomena are possible, and I would remind readers that there is abundant testimony in favour of the production of such phenomena by Home. Count Perovsky's Paper aims at being a fair presentation of the testimony in a contrary direction. It does not claim to settle the matter, as by a judicial decision.

APPENDIX.

THE FRENCH TEXTS OF EXTRACTS TRANSLATED IN THE FOREGOING PAPER.

Extract from Dr Barthez's letter dated 25th September, 1857.

... Je te dirai pour t'amuser qu'on a fini par saisir l'un des procédés au moyen desquels M. Hume évoque les esprits. L'Impératrice en est réduite à dire que le Hume d'aujourd'hui n'est plus

is a case to the point. His credulity was bewildering. Aksakov was not a hypercritical observer or writer, yet I shall never forget with what indignation he spoke to me once of a report of Professor Wagner's (a very distinguished entomologist by the way) on some "experiments" with a Russian physical medium called Nikolaev.

le Hume d'autrefois, qu'il a perdu son pouvoir, et qu'il cherche à le remplacer par des subterfuges. La chose est fort simple. M. Hume a des souliers fins, facile à ôter et à remettre ; il a aussi, je erois, des bas coupés qui laissent les doigts libres. Au moment voulu il ôte un de ses souliers, et avec son pied tire une robe par-ei, une robe par-là, fait tinter une sonnette, eogne d'un côté ou d'un autre, et la chose une fois faite remet prestement sa chaussure. Cela a été vu par M. Morio qui en a fait une belle relation écrite et signée avec tous les détails nécessaires pour établir l'authenticité de sa découverte. Hume a vu qu'on devinait son affaire et il faisait, je t'assure, piteuse figure . . . Du coup les séances d'évocation des esprits ont cessé au château et nous espérons eet indigne charlatan est démonétisé. Cependant Sa Majesté ne peut pas digérer qu'un homme ait eu le front de se moquer à ee point d'elle et de l'Empereur pendant une année.

Extract from M. Paul Heuzé's letter dated 9th March, 1929.

Je lis aujourd'hui seulement votre lettre du 9 février, qui m'a eouru après. Voici les renseignements que vous me demandez : . . . Ce passage fut publié par mes soins dans la *Revue de France* de janvier 1924. Quelque temps après Dicksonn, qui faisait des conférences eontre le spiritisme et qui racontait, en passant, l'histoire de Home à Biarritz, me dit avoir reçu la visite, à la Salle de Géographie, à l'issue d'une de ses conférences, de M. Morio de l'Isle *fi*ls, qui lui confirma que son père lui avait souvent raconté cette anecdote.

De mon côté, en février, j'allai trouver le Prince Roland Bonaparte et lui portai le passage des lettres du Dr Barthez, en le priant, puisqu'il devait voir bientôt S.M. l'Impératrice Eugénie, au Cap Martin, de lui demander si décidément les choses s'étaient bien passées ainsi. Quelques semaines après, je revis le prince, qui me dit textuellement : "L'Impératrice se rappelle fort bien l'aventure et m'en a confirmé tous les détails. Elle ne se rappelle plus *qui* avait tourné le robinet du bee de gaz ; mais Home était bien dans l'attitude scandaleuse que déerit Barthez." J'ai donc apporté ee nouveau témoignage dans mon livre *Où en est la Métapsychique . . . ?*

(Signed) PAUL HEUZÉ.

Extract from M. Dicksonn's letter dated 12th March, 1929.

Paul Heuzé est mon disciple depuis 1921, époque à laquelle il est entré dans la lutte, et je lui ai fourni tous les éléments pour se documenter.

Les spirites sont, eomme toujours, de mauvaise foi, heureusement qu'il y a des preuves.

Je certifie que :

1°. Le 1er mai 1919 Monsieur G., Consul de France, m'a rendu visite aux Sociétés Savantes avec son ami M. Morio, fils de celui qui a surpris Home le 5 7re 1857 à Biarritz, et celui-ci m'a raconté comment son père a démasqué Home.

2°. Le 7 9re 1920 le Prince Roland Bonaparte tint à venir lui-même à la Société de Géographie dont il était Président, pour me confirmer l'authenticité de la séance de Biarritz que lui avait raconté plusieurs fois l'Impératrice Eugénie.

Le Prince est mort, mais sa fille la Princesse Georges de Grèce qui était au courant est là pour certifier les paroles de son père.

Ces deux témoignages réduisent à néant les démentis intéressés des spirites.

Voilà, Monsieur, ce que je peux vous déclarer et vous autoriser à crier bien haut en mon nom.

Depuis 17 ans je mène une campagne acharnée contre les exploiters de la crédulité publique et suis heureux d'avoir fait des adeptes.

(Signed) Professor DICKSONN, Comte de St. Genois.

Statement by Baron du Potet printed in the Revue Métapsychique (September-October 1927), p. 390.

Il y a quelque chose de mystérieux à approfondir et ce n'est qu'avec circonspection que l'on doit admettre la cause avouée et soutenue de tous les phénomènes étranges produits par M. Home. Dans tous les cas, notre sentiment est qu'il y a mélange, que tout n'est pas toujours indépendant de lui-même, et qu'un jour on sera étonné d'apprendre que la force psychique n'était pas seule au service de M. Home. Mais est-ce moi qui ose aujourd'hui écrire ces paroles de méfiance, moi qui ai loué et prôné ce médium extraordinaire, moi le spiritualiste ? *Pourquoi pas, si le doute est entré dans mon esprit ?*

Extract from Dr E. Cyon, Dieu et Science (Paris 1910), p. 255.

Aussitôt après la publication de mon article sur la séance du 10 mars 1871, le Général Comte Fleury, ancien ambassadeur à Pétersbourg, me fit communiquer par le Dr Pélikan les détails d'une séance à Compiègne où il avait réussi à prendre Home sur le fait. A cette séance assistaient l'Empereur, l'Impératrice, la Princesse Metternich et quelques autres intimes de la cour. Autour de la table Home avait à sa gauche l'Impératrice Eugénie et à gauche de celle-ci se trouvait Napoléon III. Le Comte Fleury assis en face de Home fut frappé de l'insistance avec laquelle ce dernier dirigeait la conversation de manière à ce que l'Impératrice fut obligée de rester continuellement tournée vers l'Empereur, pour lui poser des questions.

Souçonnant quelque tour de passe-passe, le Général Fleury demanda permission de se retirer ; il sortit par la porte située à droite de la table, mais il rentra inaperçu par une autre porte qui se trouvait derrière Home. Il vit alors celui-ci entr'ouvrir la semelle de sa bottine droite, laisser quelque temps son pied nu sur le marbre du sol, puis subitement par un mouvement rapide et d'une agilité extraordinaire, toucher avec ses doigts de pied la main de l'Impératrice qui sursauta en criant : " La main d'un enfant mort vient de me toucher ! " Le Général Fleury s'avançant alors, dévoila ce qu'il avait vu. Le lendemain Home fut embarqué à Calais, sous la conduite de deux agents : la consigne était de tenir l'incident secret.

A METHOD OF ESTIMATING THE SUPERNORMAL CONTENT OF MEDIUMISTIC COMMUNICATIONS.

IN the general report on the Warren Elliott sittings (*Proceedings*, xxxix. 117-184) mention was made of an attempt to devise a method of estimating the value of the evidence for supernormal knowledge as compared with chance.

The problem was placed before Mr. S. G. Soal and after a considerable amount of discussion he evolved a formula. Mr. Soal felt that an authoritative confirmation of this formula was desirable, seeing that, while a professional mathematician, he had not specialised in the statistical branch. The problem was then submitted to Dr. R. A. Fisher, F.R.S., who most kindly gave the benefit of his wide knowledge of these matters. He suggested a slight modification of the formula evolved by Mr. Soal and in view of his great authority, this final formula may confidently be accepted as correct.¹

I. THE METHOD.

By H. F. SALTMARSH and S. G. SOAL.

The problem is as follows. The medium makes a series of statements, some of which are veridical and some not. The intrinsic probability of each individual statement is taken as known. The method suggested for arriving at this knowledge has been discussed in the appendix to the report above referred to. It should be noted that this method does not profess to give the exact value of the individual probability but only an estimated maximum. The result arrived at by the application of any formula will, therefore, be a minimum value for the sitting as compared with pure chance: the actual value will always be higher. This, from the nebulous and indefinite nature of the subject-matter, is all that can be obtained, but from the point of view of psychical research, a minimum value is all that is needed. It cannot be denied that it would be an in-

¹ In this paper the mathematical details, subject to Dr. Fisher's emendation, are due to Mr. Soal, but Mr. Saltmarsh is responsible for the application of the formula and the estimation of the probabilities of the various statements.

estimable benefit in the work if it could be said with some confidence that the veridicality of the statements made in any particular case was *at least* so many times in excess of chance. For the purposes of the mathematical evaluation of a sitting, however, the maximum individual probabilities are taken to be the real actual values.

There are three possible conditions which may govern a speaker when making a statement : (a) He may have knowledge and be speaking the truth ; (b) He may have no knowledge and be guessing ; (c) He may have knowledge and be speaking untruth.

In the case of a sitting with a medium it is first of all necessary to eliminate the possibility of normal knowledge, and this can only be done by a consideration of the conditions of the sitting and the nature of the statements made. We will suppose that this has been done and that all possibility of normal knowledge, fraud, etc., has been excluded or allowed for, also that other disturbing factors such as clichés have been eliminated. Condition (a) would then be when the medium possessed supernormal knowledge of some sort and obtained the information given by means of telepathy, clairvoyance or some other supernormal faculty. Condition (b) would be where any such faculty was absent. Condition (c) would be where the supernormal faculty was what may be called negative. It may be doubted whether condition (c) ever actually occurs, but for the sake of logical completeness it must be envisaged as a possibility. If the evaluation of a sitting should yield an amount of veridicality far below that attributable to chance, that is, a negative score, we should have to suppose, in the absence of normal causes, that some sort of negative supernormal faculty was in operation and had inhibited successful guessing.

It must also be borne in mind that, even where conditions (a) and (c) are in operation, they will not always be successful ; thus, though the medium may possess supernormal faculty, she will sometimes make mis-statements. Many conditions may be suggested which would bring this about, for example, vagueness of the supernormal knowledge, fluctuations of the faculty, confusions between sources of information and so on. Moreover all three conditions may be mixed up together in the same sitting.

The figure given as the result of the application of the formula to a sitting will be called the " real score " for that sitting ; a further figure is calculated which represents the " standard error " ; the ratio of the real score to the standard error is called the " value " of the sitting.

There is one further point which must be mentioned. The evidential value of a veridical statement depends entirely on itself and cannot be held to be altered in any way by any subsequent mistakes which the medium may make, except in the limiting case

where the total amount of veridicality of the sitting comes within the range of chance. If I score a couple of bull's eyes, the fact that I may then miss the target altogether a dozen times, does not alter the fact that I have made two good shots.

The thesis is that when normal knowledge can be excluded and the real score is positive and considerably exceeds the standard error, condition (a) has governed the medium to some extent, that is to say, he possesses some sort of supernormal knowledge; where it is negative and considerably exceeds the standard error, condition (c) is inferred.

To turn now to the formula itself, let the sitting consist of n statements, of which n_1 have each an intrinsic probability of p_1 , r_1 of these being correct or veridical and $n_1 - r_1$ incorrect, n_2 with a probability p_2 and r_2 correct, n_3 with probability p_3 and r_3 correct and so on. The p 's are fractions less than unity, so that if any statement has a chance of being true say once in every four times, then for this statement $p = \frac{1}{4}$.

Mr. Soal argued as follows. If the production of the statements was governed by chance alone, then such scores should be allocated to the individual statements as would make the resultant score equal to zero. Now with a group of N statements each of the same probability p , the most likely number of correct statements due to chance will be Np and the most likely number of incorrect statements will be $N - Np$. It is natural to assume the score of a single correct statement to be $-\log p$, and on this assumption the score for the Np correct statements will be $-Np \log p$. Hence the score for the $N - Np$ incorrect statements will be $+Np \log p$, and it follows that the score for a single incorrect statement is

$$Np \log p / N - Np = p \log p / 1 - p.$$

In this way the total "probable chance score" of the group of N statements is clearly zero.

Dr. Fisher makes a slight modification in this formula. He lays it down that the *difference* in value between the scores of a correct and an incorrect statement should be $-\log p$. This makes the score of a correct statement $-(1-p) \log p$, and of an incorrect $= +p \log p$. As before it is evident that the resultant chance score of the Np correct and $N - Np$ incorrect statements is zero.

Hence on this system the resultant score for a series of statements of which n_1 have the probability p_1 with r_1 correct, n_2 have the probability p_2 with r_2 correct and so on is

$$\begin{aligned} & -r_1(1-p) \log p_1 - r_2(1-p_2) \log p_2 - r_3(1-p_3) \log p_3, \text{ etc.} \\ & + (n_1 - r_1) p_1 \log p_1 + (n_2 - r_2) p_2 \log p_2 + (n_3 - r_3) p_3 \log p_3, \text{ etc.} \\ & = (n_1 p_1 - r_1) \log p_1 + (n_2 p_2 - r_2) \log p_2 +, \text{ etc.} \end{aligned}$$

It now remains to calculate the Standard Error of this expression.

In the case of a group of N statements each of probability p the total score has been seen to be $(Np - r) \log p$, where r is the number of correct statements in the group and the standard error for this score is, by Bernoulli's theorem, calculated to be

$$-\sqrt{Np(1-p)} \log p.$$

Hence by Gauss's law of squares the Standard Error of the whole series of $n_1 + n_2 + n_3 + \dots = n$ statements will be

$$\sqrt{n_1 p_1 (1-p_1) [\log p_1]^2 + n_2 p_2 (1-p_2) [\log p_2]^2 + \text{etc.}}$$

In accordance with the usual practice of statisticians we shall not consider any series to be significant as regards the existence of conditions (a) or (c) unless the total score, positive or negative as the case may be, is at least equal to twice this Standard Error in numerical value. In the case of a positive score greater than $2 \times \text{S.E.}$ there will be grounds for suspecting the existence of (a) and of (c) in the case of a similar negative score. If the real score is positive or negative but less than twice the Standard Error we cannot assume either conditions (a) or (c) to have been present but we cannot definitely infer their absence, for it is clearly possible that a combination of (a) and (c), that is for positive and negative super-normal knowledge might produce a result that would simulate a chance score.

II. A SPECIMEN APPLICATION OF THE METHOD.

By H. F. SALTMARSH.

As an example I will apply the formula to an actual sitting taken from the Warren Elliott series. It is a particularly good Sitter Present sitting, but was not chosen for that reason but rather for its shortness. For the sake of brevity I give here only a summary of the statements.

(1) "You had an upset at home." Certainly not more than one person out of every four has an upset at home at any given time. I estimate the maximum probability at $1/4$.

(2) "You are not worried because it is a relief." Very few "upsets" are "reliefs"; say $p=1/10$.

(3) "It was hard on your Mummy." This really contains two statements, first that the sitter had a mother living, second that the upset was hard on her. The age of the sitter renders the first pretty probable, say three to four, $p=3/4$; the second is also inherently probable, most upsets in a home are liable to be hard on the mother, say again $3/4$, or combined say $1/2$.

(4) The ostensible communicator "Couldn't do much, not only

ill but couldn't use arms or perhaps legs." This again combines two statements : first that the ostensible communicator was an invalid, second that he couldn't use arms or legs. It is about seven chances that any communicator, that is to say a person who has passed over, had been an invalid in life, say $p=1/2$. That any invalid should besides his illness not be able to use arms or legs, may be fairly estimated at one in three, combined $p=1/6$.

(5) "Upset made you jolly busy." About even chance, $p=1/2$.

(6) "You got young girl in house, when trouble comes she collapses." Two statements are again combined, viz. the presence of the young girl in the house and her alleged collapse. The modern girl does not as a rule collapse under trouble, it therefore seems fair to put this pretty high, say one in ten, for the combined statements, $p=1/10$.

(7) "He appreciates Mummy's courage and all your courage. Says you miss responsibility more than him, cause him feels him wasn't much use to any of you." Most people are courageous under difficulties, say three in four, $p=3/4$. It is not every invalid who is not much use to his family, say even chances, $p=1/2$. Combined say $p=1/3$.

(8) "Him quite happy." Unverifiable.

(9) "He was broad." There are roughly three possible descriptions, that is, broad, medium and slight, $p=1/3$.

(10) "He was not tall." There are four possible descriptions for height with Mrs. Elliott, very tall, tall, medium and short : say $1/4$.

(11) "Him says hims passing was really lucky kind." The communicator actually passed as the result of a stroke and not from the paralysis from which he had suffered for many years. His final illness was a short one. I estimate the maximum probability of this statement about the passing being lucky, being true of any person who has died at $1/4$.

(12) "He is glad you did not go to a lot of expense for things." People do not spend so much on mourning as they used. I estimate this at $p=1/3$.

(13) "Sends his love to three people in your house." The significant part of this statement is the number three. What proportion of families consist of three persons? I should say not more than one in four, $p=1/4$.

(14) "He was sort of ill, yet not ill, could have done things with his head." The communicator had for years been paralysed in both legs as the result of accidental injury to his hips, otherwise he was well. In the above statement Topsy describes his condition very well in her own peculiar manner. Such circumstances are unusual and the statement would not be true of many invalids, certainly not one in ten. Call $p=1/10$.

(15) "Used to read a lot." About even chance, $p=1/2$.

(16) "Somebody closely connected with you." Probably true for any communicator, say $p=3/4$.

Of the above statements (8) is unverifiable and is therefore neglected. All the rest are correct with the exception of (6). We have therefore :

1	correct	of the value of	$p=3/4$
3	"	"	$p=1/2$
3	"	"	$p=1/3$
4	"	"	$p=1/4$
1	"	"	$p=1/6$
2	"	"	$p=1/10$
and 1	incorrect	"	$p=1/10$

Real score then is

$$\begin{aligned}
 & -(1-3/4) \log 3/4 - 3(1-1/2) \log 1/2 - 3(1-1/3) \log 1/3 \\
 & - 4(1-1/4) \log 1/4 - (1-1/6) \log 1/6 - 2(1-1/10) \log 1/10 \\
 & + 1/10 \log 1/10 \\
 & = 5.59166
 \end{aligned}$$

The Standard Error is

$$\begin{aligned}
 & \sqrt{\frac{3/4(1-3/4)[\log 3/4]^2 + 3/2(1-1/2)[\log 1/2]^2}{+ 3/3(1-1/3)[\log 1/3]^2}} \\
 & \sqrt{\frac{+ 4/4(1-1/4)[\log 1/4]^2 + 1/6(1-1/6)[\log 1/6]^2}{+ 3/10(1-1/10)[\log 1/10]^2}} \\
 & = \sqrt{.84861} = .9212.
 \end{aligned}$$

$$\begin{aligned}
 \text{Hence Value of Sitting} &= \frac{\text{Real Score}}{\text{Standard Error}} = \frac{5.59166}{.9212} \\
 &= 6.2 \text{ (approx).}
 \end{aligned}$$

A reference to the Probability Integral tables shows that for normal distributions positive deviations of magnitude equal to or greater than the above ($6.2 \times \text{S.D.}$) would be expected to occur rather less than once in a thousand million trials. I submit that this result is such that the hypothesis that chance alone could have produced this amount of veridicality is definitely excluded and that the margin is sufficiently large to allow a substantial modification in the estimation of the individual probabilities if it should be considered that they have been put at unduly favourable figures.



PROCEEDINGS

OF THE

Society for Psychical Research

PART 115

PRESIDENTIAL ADDRESS.¹

BY DR WALTER FRANKLIN PRINCE.

ON other occasions of this character, the subject of Psychical Research has been approached from the standpoint of eminent men who were primarily philosophers, psychologists or physicists. That rare combination, the poet and scientist, has been heard. There has been the light but incisive touch of the litterateur. The statesman, the physiologist, the logician, the theologian, the classicist, the physician, and the astronomer have spoken, likewise others who have attained convictions of greater or less scope through experimentation. I, the humble follower of this illustrious line, have one chief passion, the essaying the solution of puzzles, and one chief qualification, a lifetime of indefatigable practice.

Evolution of a Psychical Researcher.

I speak, then, from the viewpoint of one who likes to tackle puzzles, and has from earliest recollection. All sorts of puzzles in boyhood, mechanical, mathematical, verbal. Later puzzles as to literary genuineness, the identity of handwriting, mooted points in history, etc. Then puzzles in psychology. And finally puzzles in psychical research.

The first piece of work of mine which furnished a slight addition to human knowledge was to correct the statement in books on American history that the so-called Blue Laws of Colonial New England were forgeries by one Samuel Peters. I am one of those who, in relation to psychical research, are suspected of a will-to-believe. Since the view I came to take on that historical question

¹ Delivered at a Private Meeting of the Society, 14 July 1930.

was as contrary to the prevailing one as any view regarding telepathy, etc., is now to prevailing views, or prejudices of the scientific world, and since it was held as passionately or dispassionately, I have sometimes looked back on that early puzzle and asked if the will-to-believe led the way to its solution. Really that question is irrelevant to the actual issue, which was decided by the facts and the argument and is so accepted. But it is exactly as relevant as it is to facts and arguments with which this Society is concerned. Perhaps a previous entire conscious acquiescence in the then opinion of historians that the "Blue Laws" were forgeries by Peters only covered a subconscious opposition, but that opinion can be satisfactory only to those psychoanalysts to whom advocacy and opposition argue the same inward disposition, either openly in operation or latent and masked by a defensive mechanism. Perhaps the first and unsought discovery, embedded in the voluminous Colonial records, of an actual statute whose existence had been denied, was guided by a desire to find it, but that would seem to imply seership. Since the prevailing view in respect to the "Blue Laws" and Peters had never satisfactorily been shown valid, one might think it would have been gratifying to *amour-propre* to have been the first to demonstrate that this account was true. Further discoveries of obscure statutes substantially corresponding to Peters' abstracts, and also of customs of the courts—the beginnings of an American common law—likewise in correspondence, and finally of a much earlier compilation from which Peters directly derived the most of his examples, would seem sufficiently to account for the conclusion finally arrived at and to make the supposition of an inward bent unnecessary.

This instance is analyzed because it presents an exact analogy to subsequent studies of a different species. Puzzles in psychical research presented themselves; some unusual and striking fact, apparently an anomaly, appeared, awakening curiosity, and curiosity was the stimulus to examination; there was the same initial doubt, the same willingness either to establish or destroy as developments directed, and the same tipping of the scales more and more with accumulating evidence and its inexorable logic. Such, in a nut shell, has been the story of my being drawn into psychical research, and that of the most of us has probably not been far dissimilar.

True, certain investigators, including one of our Presidents, have confessed, nay proclaimed, that a desire to ascertain if there might not be found proofs of survival, lay at the root of their investigations. This desire need not cripple the spirit of rigid analysis and the logical sense, and it emphatically did not in the case of Myers, whose appraisal of cases could hardly be improved upon. Another

President intimated that a desire to receive messages from deceased friends must underlie favourable interest in this field. To this I emphatically demur.

In the first place there is earnest study of phenomena which do not in the least seem to relate to the question of survival. But, it may be urged, one feels that to prove a wider extension of psychical powers and relations renders more likely a still wider extension, reaching even to communication with intelligences once embodied on this earth. Then I fall back upon the assertion that it is quite possible, and a possibility realized by some, to be so dominated by intellectual curiosity as to the solution of puzzles that all other emotions and considerations become insignificant. Not that interest in, and curiosity about the question of survival need or can be quite extinguished, either in ourselves or those who have contempt for psychical research. That would be mental atrophy. But in all the years of my experimentation with mediums, I have never asked for a message from a deceased relative or friend, not because of the adoption of any self-denying ordinance, but because not messages but data from whatever purported source were the desideratum.

Furnished with but scanty information regarding the real puzzles of psychical research, and no actual observation except of a few examples of professional fakery, in the rash confidence of youth I used to deliver lectures, "explaining" them all along the familiar lines of hallucination, delusion, auto-suggestion, mass-suggestion, mal-observation, memory-aberration, chance coincidence, and the rest.

Later, the fact that certain distinguished representatives of the sciences said to be in integral opposition to the validity of any of these puzzles, in the presence of their contemptuous colleagues had announced their conviction that some of them were, nevertheless, valid, that is that they yielded only to what we provisionally call a supernormal explanation, reduced me to a silence maintained for years. If psychical research were indeed a rat-hole, it was a rat-hole, to quote Lincoln, worth looking into. In other words, the puzzles must be very hard, if such men could not find a solution on the old familiar terms, and hard puzzles invite the puzzle-lover. It might be that all the smoke hovering above alleged mental and physical phenomena was merely smudge, but it began to look as if under it all there might possibly be fire.

Years later, in the interval between the avocation of solving certain historical and other puzzles, and that of attacking puzzles of abnormal psychology, my curiosity was again attracted to those of psychical research, and I undertook a course of intensive reading of the literature most suited for the student, the *Proceedings* of the S.P.R. After making all deductions that mental self-respect would

allow, there remained many focal points in the mass of evidence of one kind and another, for which the only escape from perhaps more extreme theories of the supernatural was to adopt throughout the logical technique of Podmore which itself involved a supernatural theory stretched to the uttermost. Indeed, it seemed as if the lines between the evidence and his farthest telepathic assumptions were attenuated to invisibility.

Here active interest again subsided with the coming into consideration of puzzles in abnormal psychology. Then, as late as 1912, it was re-kindled permanently by an incident comparatively trivial, but which acted, as personal experience often does, as a precipitant.

I had lately moved three thousand miles to California. No biographical sketch had preceded or followed my advent in the little city, no *Who's Who* or similar work was then available to give facts regarding my parentage, and to not a soul there had there been any occasion to allude to such. My wife and I were calling for the first time, without previous notice, upon a young woman, wife of a local business man. I saw a planchette on the table and asked, "Do you operate that?" "We have done so a few times," she replied. "Get anything?" "Mainly 'yes' and 'no' to questions. And once we got 'Bill.'" Laughingly I said, "Suppose we try our luck," and she, who was but languidly interested, and I, put our hands upon the instrument. She did not cease talking with Mrs Prince, nor look at the planchette, nor was she, I think, aware that it was moving until the following words were drawing to a close: "W. M. Prince. Your father, W. M. Princ." These, W. M., were my father's initials, and each of them might have been—of course not all with equal chance—any one of twenty-five other letters.

My hand was indeed on the board likewise, but I have never been able to produce a single automatic jiggle with planchette, ouija or pencil, usually my added hand stops or impedes the process of another, and it never has evidenced helping. And, after she became consciously interested, the lady, with or without me, got nothing more of interest.

Comparatively a slight thing, but a puzzle, nevertheless. Not offered as evidence for others, but to me, critically aware of all the surrounding circumstances, it pointed the way for sustained inquiry.

Since then I have experimented with a multitude of persons. All but a comparatively few have yielded nothing beyond possible normal explanation. But these few, separated by a wide gap from the rest, have been capable of producing, not at all times nor with all persons, complex groups of correlated statements, correct or approximately so, under conditions and to a degree

not admitting, in my judgment, of any of the usual sceptical explanations.

William James, in his Presidential Address, said that Mrs Piper was the white crow of his experience. Mrs Soule ("Chenoweth"), Sra de Z., Mrs King and the singular happenings in my own residence during many successive months, are the particular "white crows" of my experience.

Mr Piddington gave his opinion, in a Presidential Address, that it is impossible for a person to be as much impressed by evidence in this field furnished by investigators who are, relatively to himself, foreigners, as by the evidence of compatriots. I go farther, and assert that it is impossible as yet for anyone to be so much impressed by evidence furnished by any other man as by evidence of similar cogency arising in the course of his own experience. This emphasis upon personal experience may be entirely without, or it may have some justification. With persons of untrained or uncritical mentality, it may mean nothing more than a deeper tinge of feeling, but a person of aptitude for and training and experience in critical investigation, no matter who he is, can never feel quite certain that another of his own rank has not committed an inadvertence or neglected a minute precaution, as he is quite confident that in a particular case, he himself has not done.

Two comparatively brief (and I select them because they are brief) concrete groups of mediumistic affirmations, occurring in my own experimentation, would alone be enough to bring me to approximate certainty that information may be acquired by other than the means universally recognized. Upon no other critical person could they exercise an equally compelling force, since no one else could be as certain as I that there were not inadvertently omitted from the narrative circumstances which ought to have been taken into account.

I refer (1) to Mrs King's 34 testable statements while an unseen object was held between her palms, 33 of which were correct and 1 partially so, ("Studies in Psychometry," *Proc. A.S.P.R.* xviii. 204-18), and (2) Mrs Soule's "Story of Stephen," resurrecting almost in its entirety and with wealth of detail, an incident of my boyhood (*Leonard and Soule Experiments*, B.S.P.R. pp. 334-52). These two incidents, united with the others contained in two reports alone of the work of Mrs Soule and Mrs King, done under my eye, are such that, being satisfied that no normal sources of information were open, my mind revolts at the supposition that there could be contained such a quantity, a proportion and a quality of coincidence by chance.

And backed by experimental results in other personal quarters, and by the ever-growing phalanx of results reported by other

investigators whose astuteness and special experience are well-attested, I have slowly, very slowly, been forced to accept types of supernormal mental phenomena, to the number, in their external description, of half a dozen. I will not say they are, to me, as certain as that parallel lines cannot meet, as it appears that doubt has been cast upon that proposition. But I will say, carefully measuring my words, that they are to me at least as well established as is the existence and career, in all its relations, of Socrates. Mythical characters have been invented and believed in for centuries, as likewise long narratives of purportedly historical but really imaginary facts. Thus the hypothesis, wild though it be, is yet conceivable, that Plato and Xenophon conspired to invent Socrates, biography, personal description, conversations, addresses and all, sheerly out of imagination, in order to play a huge practical joke upon the gullible centuries to come, and that later copyists of the classics, say in the first centuries of our era, aware of the shaky foundations of the Socrates story, but interested in occultism and hence in the "voice," altered other classics in copying them, introducing the casual references to Socrates which we find, and obliterating denials. This would only be doing on a gigantic scale and with more extraordinary skill what was actually being done, likewise with an ethical motive, as witness the forged Gospels and the interpolations in Josephus. A hypothesis so preposterous as this I nevertheless find it easier to entertain regarding those facts of which we have no original manuscript, than I do to apply any suggested explanation or combination of explanations, and thus deprive of their supernormal significance, certain complexes of facts of several of the types to which we have given so long and painstaking study.

Some mechanical puzzles I can execute, and know just the mechanical principles on which the at first apparently impossible is really possible. But there are others which I have equally solved as facts, but I yet do not understand the mechanical principles involved, as a skilled mechanician might. We have an analogy to this in psychical research, wherein we have established certain categories of facts, concerning two or three of which we theorize with more or less confidence, whereas we can as yet only vaguely conjecture regarding the others.

The Will-to-Believe.

On the American side of the ocean, at least, there has been much parroting of William James's expression, the will-to-believe, with the intent of disparaging serious studies in psychical research whose facts the critics knew not how to meet.

There should be made a distinction between the will-to-believe and the will-to-prove. Of course so far as a man has arrived at any conclusion he has the latter and should have. For him it is important that the facts rather than the desire have produced the will-to-prove, lest this be turbulent and unruly. But if it is not, if its current is proportional to the cogency of the facts, then it is quite irrelevant to the reader whether the author at any stage was possessed by the will-to-believe or not. If the original material is guaranteed (stenographically reported, *et al.*) then all the damage a will-to-believe is capable of is in vitiating the interpretation of and argument from the facts, so far as these are ventured. But the basic material being guaranteed, to the extent that its interpretation and the argument thence emerging are valid, they are their own vindication. All peeping behind the scenes to discover a will-to-believe is silly.

Darwin while for many years gathering and examining facts gradually saw his grand hypothesis rise and shape itself as the procession of facts directed. More and more conviction grew in the essential validity of his hypothetical scheme. At length he had the will-to-prove which found expression in *The Origin of Species*. But to the reader all that is important is whether his record of facts is true and, granting that, whether his interpretation of the facts and his general synthetic conclusion are convincing. Probably no reader in the seventy years since that book was issued ever troubled himself with the question whether Darwin had the will-to-believe. One sees in a moment, should the question be put, that it is irrelevant and inane. Such an imputation is flung only at the authors of studies in psychological research, and it has the value solely of an epithet or a sneer.

But of course there is such a thing as a mischievous will-to-believe, which leads to careless credulity, as there is a fanatical will-to-prove which leads to intellectual dishonesty. How shall one protect himself from the taint of either? Every intellectual operation is accompanied by emotion, and if to be devoid of emotion is a prerequisite, no scientific work is possible. The only way by which the emotion may be made innocuous, is by accustoming oneself not to associate it with the outcome of a process but with the process itself. It is the difference between a conscious or sub-conscious "I *hope* that this (or that) is coming out" and "I wonder *what* is coming out." Exactly as an assayer presumably tests a mineral specimen brought in by a client, or a chemist analyzes a blood stain in connection with a criminal trial. I do not declare that anyone can perfectly achieve this detachment, but think that many are so constituted that by practice they can approximate the mental habit of expending their emotion in the mere determination

of the facts and their relations, and of not caring before a particular case is in the last stages of the process of investigation whether a particular possible hypothesis will be confirmed or negatived.

Psychical Research Demands Intellectual Rectitude.

All scientific investigation demands aloofness from every extraneous consideration. But the psychical researcher above all others needs aloofness, devotion to truth, and moral courage. He is subjected to temptations as no other type of investigator is to any such degree, to inner urges, to outer pushes and pulls. The chemist, the biologist, the physicist is subject to no appeal to regard his subject matter emotionally, but there have been psychical researchers who undoubtedly have been influenced by liking, and possibly some by disliking, for the human subject. The desire to please a clientele inevitably spells a greater or less scientific damnation. The psychical researcher must, in this relation, disregard motives of fame, fortune, and favour, and put by his very hopes of Heaven, that his intellect may function undisturbed. Perhaps it is not needful for all investigators to go quite so far, but there is need of those who can and will do it.

When a particular field or class of phenomena is under investigation, and until its facts are independently determined, they should be dealt with exclusively, and no considerations brought in from another field of facts or claims which are themselves debatable or which show no immediate connection with the facts under inspection.

This is plainly seen when I refer to a religious sect in America which holds that the Bible teaches that all men at death are virtually annihilated until the Day of Resurrection. Viewed through the medium of such a dogma no evidence of survival could make an impression upon a man who holds it and will not for the time being lay it aside. Such has been the effect in past generations of making the Bible a text-book of astronomy, geology, and biology. Every book of a past period which has professed to discuss a science, and instead of viewing it in the light of its own facts has done so through the medium of extraneous dogmas or sacred texts perhaps egregiously misinterpreted is amusing reading to-day, to most religious as well as to secular minds.

But so must religion, ethics, every extraneous consideration, be left aside, when one is engaged in an assay of current phenomena. This sounds cold-blooded, and a religious person might think it a counsel of evil. It is not; whoever doubts that any and every field of phenomena, when viewed solely in the light of its own evidence, will eventually yield truth in harmony with truths derived from

other fields—he it is who is the infidel, who doubts the unity of the universe.

The vitiating effects of bias, of a desire to reach a foreseen end, is illustrated perhaps nowhere more clearly than in biographies. Formerly the most of them were panegyrics that were silent about Cromwell's wart, and corrected Washington's occasional bad spelling and too vigorous language. Of late many of them specialize in warts and peccadilloes, and the last state is worse than the first. It was your Boswell who first in the history of mankind, so far as I know, enunciated clearly the true norm of biography, and magnificently illustrated it in his *Life of Johnson*. He told the truth about the man he admired, the truth in all its parts and varieties and proportions. He was almost the discoverer of scientific biography, and for this he deserves honour more than his detractor Macaulay, for all the splendour of that political poem, the *History of England*. And what was the result of Boswell's discovery that it was best to tell the truth throughout? That Johnson is one of the best known and best loved characters in English history, and that a whole group of figures grouped about Johnson stand out as in a spot light.

It seems elementary to say that it is always best to tell the truth. But this is not always the sole goal even in scientific investigation. As Jared Sparks, old-time president of Harvard, thought it would not do to let the public know how human Washington was (and so made him an unloveable graven image), as some pious folk would balk the freedom of investigation lest it injure religion, so some investigators pass by, cover up or cast on the ash heap the residual scraps which annoy them. It is said, as you know, that when a certain European announced his beautifully rounded theory and was told that some facts were adverse to it, he replied, "So much the worse for the facts." But do you remember that following the death of a well-known psychical researcher, not of your land, one of his friends remarked in print, in substance, that he did not allow such a thing as a little contrary fact to disturb him? There were printed an epochal report and discussion of a case of dissociation, but certain facts which puzzled and nauseated the psychologist whom we all honour were entirely omitted. Probably he thought he was doing right, that printing these unpleasant facts would foster superstition. But it was not right nor was it truly scientific—the facts should be told though the heavens fall. Perhaps one of the keys to the interpretation was thrown away. Perhaps something which harmonized with a factor of the Sanders Case, the Fancher Case, the Doris Case, and the Mary Barnes Case, or with a puzzling factor in some other group of cases, has been lost, whereas such data are obtained all too rarely. No man of science any more than any biographer, may rightfully quarrel with his facts or select certain

classes for exhibition and other classes for concealment, according to his taste.

Dr Sadler is deserving of honour when in his book *The Mind at Mischief*, in the midst of his warnings relating to fraud, delusion, hallucination, illusion, etc., and his recital of cases which he found it possible to explain by the principle of psychology, we find him repeatedly saying, "But see Appendix." And we find that there he reveals the fact that out of a hundred cases he has found one which he has long studied and which does not yield to such a solution. I parenthetically remark that I doubt if, had I not exceptional opportunities for hearing of promising human subjects, more than one in a hundred which I have met would have seemed compellingly to demand some supernormal solution. But the point here is that by telling the whole truth, Dr Sadler made his book a well-balanced one; not only did it warn against falling into the one ditch of credulity, but it did not drive into the other of unreasoning incredulity.

Psychical Researches Ranked with the Historical Sciences.

By at least two Presidents you have been told that so far the methods of psychical research are those of historical research plus those of the courts, rather than those of physics and chemistry.

As one who has built two or three small bricks into the edifice of history, and is aware what painstaking and critical processes may be applied to the task of determining historical facts, I am not sure that this leaves psychical research in an entirely lamentable state.

The facts, for example, of Nelson's life were not of a nature to be established in a laboratory, but that he was an admiral of the English navy, that he was in command at the naval battle of Trafalgar, that he won the battle, that thus Napoleon's plans were shattered, that Nelson was killed in the battle, etc., are, I take it, as little subject to doubt as the facts of chemistry and physics.

There is bad history as there is bad science. For a long time it was believed that the first criminal code of Virginia was an importation by one of its governors of the code governing his army when he was in the service of Holland. This was as erroneous as the history of phlogiston, which was likewise long believed. Science had to wait until certain methods had been improved and certain intermediate discoveries made before the true nature of combustion could be determined. History had to wait, perhaps, until certain critical methods of approach had come to command before the theory of the origin of the Virginia Code could be shown false. At length the martial code of the English army in Holland was translated and found utterly incongruous with the claim. Then there

was made a laborious examination of various proclamations posted in Jamestown in consequence of emergencies which arose in the community life of the colony, and time sequences and verbal identities were so constant and consistent that there remained no doubt how many articles of the code did in fact originate. Technically the law of inverse squares may have a better standing, or various uniformities which exist in the phenomena of electricity and can be exhibited by experiment anew any day, but practically a great range of historical as well as of scientific facts are certain. Perhaps mathematically one of the fixed stars is more inaccessible to our bodily presence than is the planet Mars, but we are entirely convinced that we shall spend a vacation on neither.

In a very valid sense, too, we can repeat experiments in historical investigation. The original experiment was to apply the various critical tests, say, to the original and validated documents and by that process to reach certain conclusions. The documents are materials as fully as are substances in a crucible or retort. And thereafter, any qualified person may go to those documents and repeat the experiment and, if the first conclusion was correct, he will probably reach the same conclusion.

It may be said that what I choose to call historical science as contrasted with mere amateur scribbling about history, while it can, where its materials are adequate, fix the outstanding or surface facts, when it tries to trace their ultimate reasons which are to be found in the souls and half-concealed machinations of men, becomes uncertain and utters conflicting opinions. But are not the natural sciences in a similar situation? Do any of their experts expect, any more than Newton expected, to reach the very bottom of the riddles of nature and in their ultimate speculations do they not also express opinions at variance with each other? Of course, even when the extant materials have been abundant, historical scholars have erred even in their major conclusions. Then it is the business of other investigators to repeat the process by more careful methods and correct the mistake. Exactly this is a frequent episode in the history of the natural sciences. Dr Bastian, for example, instituted a series of experiments in heating certain infusions and sealing them as he supposed from the air, and he announced that life had appeared and that he had demonstrated abiogenesis. Then Huxley repeated the experiments, taking more pains to exclude the air, and no life appeared.

But more than this. Certain of the natural sciences escape in part or entirely from the laboratory, do not repeat their phenomena before a class, have to be studied by what may be called field work, in the spots where their exhibits happen to be found. This is true, particularly of geology, which is what may be called an historical

science. There are in geology, and, for that matter, in anthropology, all sorts of possibilities of getting temporarily astray in by-paths, and doubtless never can these sciences become as technically exact as chemistry and physics, but does any intelligent and cultured man doubt that we are making solid progress in tracing the history of the planet and of the human race?

By this time the analogy to psychical research has become noticeable. We also have to go to places, that is in human experience, where the phenomena are found, and geologists and anthropologists have to argue from their discovered facts much as we do from ours.

Of course the exhibits of geology and anthropology, being material, are visible, and many of them may be seen in museums, whereas most psychical exhibits, being mental, are not tangible, and continue to exist only as records. This is a disadvantage, but taking into consideration the centuries to come psychical research has its advantage. The available exhibits or visible evidences of say geology, although immense, are yet limited, whereas in the centuries to come the exhibits of psychical research, so far as they are genuine, will repeat themselves in all their species indefinitely, here and there, even though we may never be able to tell in advance just when and where they may come, and the evidence will continue to pile up until the facts become as incontestable as the major facts of history. Volcanic eruptions are also generally temporary, and the evidence of volcanology is largely in the form of personal testimonies and records. And if volcanic eruptions can sometimes be foretold, this is not true of the fall of aerolites. Very few if any scientific men have ever seen one fall, and the where and when of their falling cannot be controlled.

Dr Schiller remarked, in his Presidential Address, that the modern mind tends to hold that "Historical evidence never suffices to establish scientific truth." Very well, and two pages earlier he truly told us that "the visions of Jeanne d'Arc, for example, decisively checked the English conquest of France, and force us to choose between a recognition of their supernormality and the alternative that 'delusions' may be the really important and dynamic forces in human life." There is no lack of certainty that Jeanne d'Arc did have these subjective experiences and that they did influence Jeanne to certain acts whereby the tide of English conquest was turned back. This evidence is purely historical, it is not a part of established *scientific* truth, it is only truth, as well as established in the convictions of mankind as any truth of the laboratory can possibly be. It should be added that Dr Schiller stated that he did not entirely agree with the dictum which he quoted.

Facts and Hypotheses.

A friend of mine and of psychical research remarked that the mere collection of facts is not science, although a preliminary step to it, and that science begins with the formation of a hypothesis. And he said that it would not be science to set down, "Last night I saw the moon," and morning after morning to repeat the record, "I saw the moon last night."

We can all agree that to go no further than that would not be science. And practically that, with few exceptions, is all which had been done in relation to our field of phenomena, up to 1882. There had been the telling of wonder-tales, little more. Examine any of the periodicals devoted to psychical phenomena and issued prior to the date specified, and see how many of the reports of incidents and experiments carry with them any satisfactory guarantee of validity. But the collection of facts coupled with testing of the facts, the evidence of their validity, the analysis of the facts, the classification of the facts, and the determination of the relations of the facts and the conditions under which the facts occur, are a very large and essential part of the scientific process, and that is what we have been chiefly engaged in for many years.

What, is not the discovery of a new planet science? Is not the determination that it is a planet and not a mere comet science? Was not the marshalling of myriads of facts by Darwin to evidence his theory a part of his scientific work not to be set apart from the theory itself? Were not the laborious and ingenious analysis of pitchblende by the Curies, the discovery of radium, the determination of its properties and its relations to other elements science? Yet all these are facts, and the work done in bringing them to light is analogous to that which we are doing in a very different field.

There is a very great difference between the facts which still, after nearly half a century of collecting, psychical research continues to collect and repeating over and over, "Last night I saw a moon." No one, scientist or yokel, denies or doubts that there is a moon. But our facts are still doubted and still denied. Who will have patience to consider our hypotheses, so long as they discredit the facts for which the hypotheses are framed? There is no way to convince the scientific world of the facts except by multiplying them and increasing the cogency of the proof and the number of competent attestants.

Furthermore we do not simply reiterate facts of precisely the same description. From time to time we note in some new case of the same kind as often reported before a peculiarity which we had not before observed or which had not been so strikingly exhibited,

some indication which, taken with others previously noted, has its bearings upon the hypotheses which we are seeking.

For ages men said "I saw the moon last night" before they came to know anything valid regarding it. The time came with the invention of the telescope when they really began to see it. And seeing it then a dozen or a hundred times did not exhaust the possibilities of the extension of knowledge by seeing, for seeing it in various aspects, and the continued study of what was seen, with the aid of wondrous scientific processes, still continued productive. With better and better telescopes, more is seen; by the aid of photography still more fruitful is the seeing.

The sun has been seen by all men day after day from the dawn of time, but still astronomers travel thousands of miles to see it just before, during, and just after eclipse, and still they are unsatisfied, for they continue to see and discover facts which are new or to strengthen facts which were uncertain.

So it is in psychical research. From time to time there is recorded an instance of a type of phenomena, which is unusually suggestive of the process which lies behind so that, as in other species of inquiry, there is always the zest of the unexpected addition of new light on often-repeated facts of the same species.

I note a tendency on the part of some who have pursued psychical research and some in other fields of investigation to think it requisite to form a "working hypothesis," symmetrically outlined as a lay figure, very early in the process of the collection of facts. The danger of that, as it seems to me, is that the investigator may get so attached to his hypothesis as, unconsciously, to cultivate a special mental vision which will colour the facts. An analogous case is that of Professor Berringer who, shown forged fossils differing but slightly from types with which he was familiar, did not take into consideration any hypothesis except that the story of their being dug up *in situ* was correct and was thereafter led along by slow degrees to credit wider and wider departures until it reached the shores of absurdity.

I have known of more than one sudden awakening from a hypothesis dream, like the rude awakening of Berringer, in the field of psychical research.

Especially where the facts are elusive, obscure, and problematical almost beyond comparison, it appears to me better to put off for a long time the confident espousal of any hypothesis, but to keep all possible hypotheses constantly in mind, and to pursue the ledger method of crediting and debiting to each as the facts seem to warrant. Many items may have to be put down as equally favouring two or more hypotheses at the same time.

I by no means intimate that there is no category of facts in

psychical research in relation to which it is not now expedient to frame and urge a hypothesis. I should say that the time had not come for this in what is called psychometry, but that it is not too early for the telepathic and spiritistic theories to be urged against each other in relation to mediumistic "messages." The very fact that there are these two opposing hypotheses, that psychical researchers are not agreed in their theories of causation, has been pointed to as a reproach.

Psychology is hardly situated so as to reproach us with dignity. The psychological concepts of a Bergson or a McDougall are in sharp conflict with those of a Watson, to whom psychology is little more than dynamic physiology, a system of conditioned physical reflexes. And the methods run all the way from that which emphasizes the value of analytical introspection to that which denies that there is any value whatever to introspection. Physical science might indeed cry "A plague o' both your houses," but on second thoughts blush to do so, since its history contains many a chapter of conflict, and in some quarters the smoke of battle has hardly yet lifted.

It should remember that the two-fluid theory, the one-fluid theory, and other theories of electricity were fought over for a century and a quarter, until Maxwell turned them all out of doors. It should remember that for ages "the doctrine that nature abhors a vacuum" was a part of the scientific gospel, until the discovery with its momentous consequence that the "abhorrence" ceases at a certain height. It would do well not to forget its old theory of "phlogiston" which reigned from Becker in 1680 until Romford announced his molecular theory of heat in 1798, and did not receive its final quietus until Joule's researches forty years later. Even in relation to evolution Dr Nabours has lately said: "We are still in a morass, it may as well be admitted, with regard to the ultimate problems." All we have is "a rough path, a sort of crude track in the jungle."

With or without this becoming example, psychical research can afford to be very modest. It is true that it has but obscurely begun to trace some paths in the perplexing region which it is trying to explore, and that not all its searchers are agreed that these are the right ones. What else could be expected, when another "historical" science, after more years and the labours of a hundred times the number of investigators, must speak with such restraint of its attainments.

A Plea for Sweetness.

Where persons differ in regard to facts or interpretation of facts it is a pity that their exchange of views is not always carried on

with the calmness and good temper which befits academic discussion. I do not propose to give any instances nor do I propose to construct any diagrams of "rows" and "friction" between particular persons, as was attempted by one of my friends. In the first place, I do not think this a measure calculated to lessen the evil, and in the second place his effort would not encourage me to think I could make an accurate survey of two continents. To my surprise I found by his graphic chart that I was supposed to be in conflict with my friend himself and that I was also set down as being in a "row" relation to a man with whom I had never conversed or corresponded, and of whom I never had made mention in any of my writings, in my life.

But it is a matter of history for the last eighty years, that investigators in this field, particularly those of a cautious and conservative type, have been frequently assailed in most bitter fashion by a criticism which professes to penetrate their very souls, and discover therein various malignancies and moral delinquencies. Such was the treatment of Agassiz sixty years ago, and how many other instances could be given! In fact, it would be hard to find cases of scientific men of standing who, having consented to take part in a psychical investigation, and having failed to reach conviction, have not been attacked with ridicule and personal imputation. The impolicy of such a course should have been evident all along. Without doubt, it is eminently desirable that such men should be induced to pay attention to psychical facts, so far as their other duties permit, not for an hour or a week, but for years. But when one is charged with moral obliquity and intellectual crime of which he does not feel guilty, and held up as an object of scorn, is it any wonder if there is added to his initial doubts a personal distaste, so that he can never again be induced to interest himself in the subject? I doubt not that progress has been greatly deterred by such reactions which, if they were not otherwise objectionable, would at least be short-sighted and impolitic.

I have always admired the degree to which the officers and investigators of the S.P.R. have kept discussion upon a calm, judicial, intellectual plane. Not only have all its Presidents, when they had occasion to refer to species of phenomena which had not yielded to the Society's investigations very satisfactory results, urged that the investigation of these species should go on; not only, in reporting such a case, has there seldom been lacking great care to give due weight to all supernormal possibilities in connection with doubtful phenomena; but when representatives of the Society have been fallen upon as though they constituted the Guy Fawkes gang of Psychical Research, caught plotting to blow up the whole edifice, they have still kept their tempers, controlled

their pens, and replied in the terms of reasoning and not those of emotion.

And since I deem it a duty to set forth my views as to the comparative evidential status to-day of physical and mental phenomena, and some of the measures which seem to me desirable to adopt in the future survey of both fields, I trust that all who hear or read what is to come will at least credit me with sincerity, and that if any discover errors of statement, they will convince me by the only effective means, logic and evidence. It ought to be quite possible for one honest investigator, whichever side of a question he espouses, to listen to the differing views of another honest investigator, with perfect equanimity.

Physical Phenomena.

In view of the facts that (*a*) between persons generically known as psychical researchers difference of opinion exists on a wider scale in relation to physical than to mental phenomena, some going to the extreme of disbelieving that any genuine examples of any type of the former exist, while at the other extreme some maintain that there have been many genuine examples of several types; (*b*) this disparity of opinion leads to unpleasant reflections, belief being explained as due to insufficient experience of a special kind, sensory deception, the beguilements of the art of magic, etc., and disbelief imputed to obstinate prejudice, the baleful effects upon the phenomena of inward opposition, etc.; (*c*) although organized attention of one sort or another has been paid to physical phenomena for eighty years, and although some varieties of manifestation have almost disappeared, others have arisen to take their places, and settlement of the original question is as far off as ever; and, most to be emphasized (*d*), the disgust engendered in the minds of a host of scientific and intelligent people by the numerous scandals of spurious physical phenomena, which have been frequent for eighty years and continue to this day, illogically but naturally extends also to mental phenomena and goes largely to explain inattention to and inability to appreciate at its true value the evidence for the supernormality of certain mental phenomena, therefore: (1) It is supremely desirable that there should be a speedy settlement of the question whether there are genuine physical phenomena, and, if so, of what types and descriptions.

In view of the facts that, (*a*) in general, the phenomena referred to are concerned with tangible objects and their movements, either the purported production of supernormal substances, variously characterized, and their supernormal movements, or the supernormal movements of normal objects, and (*b*) these phenomena are

capable of being repeated indefinitely, if not in all experiments, at least in a large proportion of them: (2) theoretically the determination whether a given type is genuine should be a matter of comparative ease, not requiring a very long period of experimentation. There must be some factor in this which makes it differ from every other sort of physical experimentation.

In view of the facts that in other fields of physical investigation the experimenter (*a*) has the full use of his hands, here not, (*b*) has the benefit of the full use of his eyesight, is here wholly or partly deprived of it, (*c*) has his sense of hearing unblocked, here frequently more or less blocked, (*d*) the exceptions to the above rules being wholly subject to the will, not of the experimenter but of the subject or the subject's alleged spirit control, and (*e*) is free to employ all suitable instruments and processes when and as seems best, but here is utterly denied them, limited in the scope of using them or dictated to as to the moment when they may be employed: (3) it is fair to conclude that the inability to make visible progress toward a *rapprochement* where theoretically this should be easiest, is due to the senses being more or less blocked, and the means which science and common-sense suggest more or less denied, as is the case in no other department whatever of physical investigation, and to the doubts inevitably arising relative to the possibilities of sensory deception and the arts of conjuring. And it is well known that these disabilities and inhibitions are imposed on the ground of a set of alleged psychic laws. It would be rash to deny, antecedently, that any of these asserted laws is not a valid one.

But, in view of the facts that (*a*) to a marked degree in general and particular, the alleged psychic laws of physical phenomena correspond with the precautions which would be necessary to protect imposition, (*b*) many of them were first promulgated in connection with old-time mediums who afterwards became notorious through exposure, (*c*) certain once confidently asserted psychic laws have been discarded (*e.g.* the one declared by Thomas R. Hazard of America and adopted by Alfred R. Wallace and others in England, according to which, when a spirit is grasped, the medium is drawn "swift as lightning" through bonds and bars, to "coalesce" with the spirit, thus accounting for the medium's being found in the hands of the grabber); (*d*) the alleged laws show an occasional singular mutability (as where in general the phenomena bear the human eye but not white light, whereas in certain cases the phenomena can bear white light but not the human eye); (*e*) some alleged psychic laws affront common-sense (as that a mysterious substance capable of overturning furniture and administering blows to experimenters may not be touched, apart from warning and directions as to time, place, and manner, with the tip of an investigator's finger

lest it suffer damage); (f) yet some particular friend of the subject may violate a psychic law (as by turning on a light without permission) and no disaster such as others are warned against follow; (g) although for eighty years obedience to the psychic laws has been enjoined on the ground that infractions would endanger the subject's health or even life, there are records of hundreds of infractions, even to the extent of purported spirits being violently seized by suspicious Spiritualists and a fight ensuing, and not one case of death is recorded, nor is there in any case of indubitable evidence of damage, given by a calm observer, amounting to more than a nose-bleed or a bruise inflicted in a fray: (1) it is very doubtful if some of the alleged laws relating to physical phenomena are valid, and there is as yet no certainty that any of them are.

(5) If there are genuine physical phenomena, ectoplasm, materialization of full or partial human forms and the like, it is of course very much to be desired that they should be proved as physical entities, to the satisfaction of the most cautious. But, it is to be feared, this cannot be until investigation is freed from its bondage to mere dogmatic assertions which give no proof of their validity. That freedom, if achieved, must be won by the efforts of persons already convinced, for to no others will the subjects give heed. Let me suggest, in no unkindly spirit, to advocates that chiding those unconvinced by physical phenomena or certain types thereof, and ascribing to them a malevolent disposition, have never yet convinced any of them, and that it were far better, if they have missed the truth, to clear away some of the obstructions on the path to it. By way of examples, I further suggest:

(a) That they undertake to test the alleged psychic laws, as is done in every other department of physical inquiry, rather than to take them for granted. For instance, it might well be, as is often alleged, that psychic structures cannot, any more than photographic plates, bear white light. But the photographer will demonstrate this to your satisfaction, so here the parallel breaks. And the law in the case of photography is consistent and unvarying, whereas the alleged law on physical phenomena apparently breaks down in presence of the fact that photographs, when the word is given, may be taken of alleged materializations and ectoplasmic structures by a blinding flashlight, without any resultant disaster. It is time to test whether a gentle beam of light directed by the experimenter at moments when it would be of most advantage, would really do harm to anything besides imposition.

Study should be given to the question why certain alleged psychic laws do not act with any uniformity, as a scientist would certainly give if he found that in certain places the magnet does not attract iron, and that in the laboratory of chemist A hydrochloric acid shows

no affinity for lime as it has shown in the laboratories of chemists B, C, and D. In short the whole matter of psychic laws supposed to govern physical phenomena should be carefully studied, with a view of the retention of those only which are of demonstrated validity, that investigation in this field may haply be brought nearer to parity with the field of mental phenomena, wherein, with all our best subjects, investigation is carried on absolutely untrammelled by alleged psychic laws.

(b) Advocates should cease to lend their assent to the dogma that inward doubt, or even disbelief on the part of an amiable and acquiescent investigator, has the effect of preventing phenomena. This is the mere remnant of an old dogma, now discarded, that suspicion, although based on observed facts, actually summons bad spirits to bring along with them deceptive paraphernalia. The remnant of the superstitious doctrine should be repudiated, especially as it has been thoroughly disproved. Phenomena are not inhibited by the presence of doubters and disbelievers whose identity is not known or whose doubt or disbelief is not known; only where they or their suspicions are known is there sometimes an inhibitive effect, quite susceptible of a normal explanation.

(c) Advocates should insist that when, in a given case, a phenomenon, whether by happy discovery or inadvertence, occurs in a state of improved conditions, as in the daylight experimentation for that phenomenon, investigation should continue under these conditions which promise to lead to a speedy determination, and not revert to worse ones under excuse of a psychic law already violated with impunity. Exactly this sort of thing has often taken place, experimenters smothering their intelligence and acquiescing.

(d) When there exists some simpler but surer instrument or means of testing a physical phenomenon than that in use, and one which manifestly lays no greater burden upon the subject, advocates should not tolerate refusal to allow its use, simply on the statement that the "Control" does not want it, or at least should not maintain silence in regard to the sinister appearance of such refusal.

(e) Advocates should frown upon the doctrine which has found adherents in their ranks, that it is rather to be expected that physical mediums should *sometimes* practise fraud. No such doctrine attaches to mental mediums on whose results we lay weight, and, although we do not make their *bona fides* a part of our evidence, such a doctrine would seriously handicap the credit of our reports. It should be impressed upon the consciousness of physical mediums, made to sink into their subconscious, and enjoined on their alleged spirit controls, that *any* such instances of imposition must inevitably affect the credit of valid instances, suggesting that in them the imposition has merely been successful.

(f) Advocates should discountenance the all but universal practice of excluding from investigating groups persons whose conduct has been unexceptionable, save that they have given expression to doubt as to the validity of the phenomena. What would be thought of a parallel course pursued by a physicist who claimed to have discovered a revolutionary process or to have invented an apparatus revealing a hitherto unknown property of matter! What a method of convincing an honestly unconvinced man! Is truth afraid, and must it shield itself from the most searching inquisition?

(g) Advocates should resist any further encroachments upon the liberty of thought, as by a rule that statements of facts made by an experimenter and recorded at the moment of observation shall be sacred against after-revision. Innumerable instances have demonstrated that a sinner may honestly make a declaration of fact and afterwards discover that the fact was quite otherwise.

It is to advocates, that is persons already convinced of physical phenomena, whether now present or who shall hereafter read these words, that I venture to make these respectful sample suggestions, since known advocates are in the most favourable position to give the suggestions, or the approved among them, effect. If some investigators have reached the goal of revolutionary truths in this quarter, it is by following the lines of such suggestions that the truths can best be brought home to their less fortunate fellows.

I trust that no one will read into the above rather lengthy passage on the thorny subject of physical phenomena, any spirit or temper which was not present in the writing of it. After a former address before this Society, reporting the unsatisfactory results of a series of experiments, a gentleman referred to what he called my "sarcasm." I was appalled, and protested that I had studied to employ the terms consistent with the facts least calculated to give offence, had never used the word "fraud" in relation to the persons with whom I had had unfortunate results, had employed euphemisms like "normal causation." Here he interrupted me and said, "That is what made it so funny." But what is a poor man to do? And so I hope that nothing I have said has seemed like sarcasm or covert attack, but that I shall be visualized as bearing a very large olive branch, and heard as expressing very real difficulties which I would fain see removed. For, I repeat, it is useless to disguise the fact that physical phenomena can never be established in the minds of thinking men generally, until the shackles are taken off from investigation, and experimenters are permitted, without hostile criticism for so doing, to pursue their inquiries untrammelled either by dicta laid down in advance of an experiment, or prohibitions enjoined in the course of it. It will be said that for the investigators to arrange conditions will prevent phenomena. Very well, ex-

perience will soon teach them what the necessary conditions are, whereas dogmas can only prolong doubt. I quote the true words of Sir Oliver Lodge, in his Presidential Address of 1902: "Full control must be allowed to the observers—a thing which conjurors never really allow. I have never seen a silent and genuinely-controlled conjuror; and in so far as mediums find it necessary to insist on their own conditions, so far they must be content to be treated as conjurors." I regret to add that, whatever the experience of others, I have had the good fortune but once to be present when investigators of mediumistic physical phenomena were untrammelled throughout, and that in this instance the results were entirely disastrous to the claims advanced.

Alleged Phenomena of Nondescript Character.

By "nondescript" is meant that the alleged phenomena do not easily classify as either mental or physical. I refer to such as the following:

1. Astrology, a complicated set of assumptions come down from remote antiquity as to the relations between movements, and locations at given moments, of the heavenly bodies, on the one hand, and the characteristics of a person and the events of his life, on the other. Utterly devoid as it seems to be of any rationale, yet psychical research would be obliged to accept it, if the facts proved compulsive. I myself, at the risk of appearing ridiculous even to my colleagues, have for fourteen years held my archives open for astrological evidence, and have collected many exhibits of what was offered as evidence by supposed experts. In 1895 Mr F. W. H. Myers remarked "I do not know on what evidence this belief is based." In 1930 your speaker knows of no evidence which is not the result either of a forced application of the rules to human careers already known, or of a careful culling of "hits" from preponderating numbers of "misses." I do not think that any psychical researcher in forty-eight years has given attention to the claims of astrology and has not definitely cast the pretended science on the dust heap.

2. Palmistry, another set of irrational assumptions rooted in the hoary past, to the effect that a relation exists between the lines, protuberances, etc., of a person's hand, and the characteristics and experiences of that person. Like astrology, these assumptions are more or less accepted to-day by many thousands in many lands. Mr Myers in 1895 also said, in relation thereto, "I have seen no evidence of any value." Nor is there, I believe, an investigator of standing in the forty-eight years of psychical research, who has been, like myself, quixotically good-natured enough to solicit evidence for

claims so absurd, and who has been more successful than Mr Myers, or who has failed to assign palmistry to the ash-pile.

3. Numerology, a modern and protean species of Cabala, which holds that the letters of which a person's name is composed exert a profound influence over his character and his course of life. Like astrology, this has a department devoted to it in a number of occult periodicals, and is advertised in many Spiritualist ones, hence must be credited by a large number of credulous people. I do not remember that any psychical researcher of whatever grade has ever said a word in its favour, nor do I believe that there is one who could not, in one hour of application of its pretended principles, blast its claims.

A query is in order. If, as some insinuate, there is no real evidence for the validity of phenomena which have convinced a great many of our investigators, and that what seems such is transfigured by the will-to-believe, then why has none of them accepted astrology, palmistry, or numerology, also?

And a remark. Possibly psychical research has not done its full duty toward an indiscriminating section of the public, by the way of showing not only that these and other occult beliefs lack rational basis, but also by the exhibition of tests proving that they do not work out in practice.

Mental Phenomena.

For nearly a half a century this Society (to confine our attention to it for the moment) has been trying out claims to the acquisition of knowledge by supernormal means, *i.e.* apart from the recognized senses, more generally though apparently not always when the human subject is in a more or less dissociated state of mentality (abstraction, trance, dream), and with or without physical instrumentalities, as a writing implement, a scrying ball, an unknown object held in the hands, or a dowsing rod. There were not wanting at any time investigators who were ready to smash these claims as those of astrology or palmistry. It was no doubt confidently expected by many onlookers that when such minds as became enlisted got to work upon these problems there would be a speedy smashing of them all. That none of the types of claims which I have in mind has been smashed is in itself a momentous fact. It is profoundly impressive considering that neither darkness nor any other embarrassing condition is demanded by these mental phenomena, and inquirers are at liberty to employ every means of investigation that common-sense and science suggest.

More than this, there have been no scandals in connection with any of the cases of this description which the Society has published

as evidence, or with any of the persons upon whose work they were built, unless possibly once or twice in the first stage of telepathic experiment. Not one of the dozen or more chief psychics whose names are best remembered from your reports has ever been "exposed," and, so far as I know, not one has ever been assailed by any attempted evidence of intentional deception. The same can be said of every medium for mental phenomena in America whose work Dr Hyslop or his successor presented as evidence.

Still further, so far are we from explaining away those asserted types of phenomena that we have brought some of them far on the way to factual establishment. Probably all of our workers are and have for years been convinced that telepathy is a fact, although one only rarely capable of brilliant demonstration. Some of these workers have not personally dealt with certain types, as for instance, psychometry, and might be hesitant in regard to them, while those who have had much experience with the same are generally agreed that they present unassailable evidence of the supernatural. We are not all agreed in theory as to mediumistic deliverances, some holding that in them is found sufficient evidence for communication from the dead, while others think it better to explain by a very comprehensive and agile telepathy. But it is hard indeed to find a person who has really given long and patient attention to the study of mediumistic work of the highest class and who thinks that the facts can be explained by normal knowledge or chance coincidence. The claims that some coincidences of thinking between two or more persons, some mediumistic "communications," some dreams, some apparitions and other hallucinatory impressions, some psychometry, some previsions and predictions, and even some peculiar mental phenomena associated with particular houses, constitute supernatural facts or involve supernatural factors, are not after nearly half a century of organized research put to confusion like astrology, which takes its place with divination by entrails or the flight of birds, and do not remain evidentially just where they were in the beginning, a matter of dispute, like most physical phenomena, but are to-day on a far more solid evidential basis than they were found, by the well-nigh unanimous verdict of experienced psychical researchers, who have dealt at length with them.

It is also profoundly interesting to note that at least some of the types just mentioned have never been met by any theory which can explain them in normal terms. Storms have indeed assailed them, but they have been windstorms. Cases such as those reported by this Society have not been met squarely and discussed fairly. I have declared this again and again in print and named many hostile writers and shown their methods of quibbling and evasion in detail. It would be cruelty even to name again the learned American lady

whose ambitious attempt the late Dr G. Stanley Hall hoped would be "the turning of the tide." She touched nothing that she did not mangle, and seemed incapable of making a correct abstract of any incident as long as half a page. She was demolished by the deadly parallels of Dr Hyslop, the calm reconstructions of the facts by Mrs Sidgwick, and the irony of Andrew Lang. A friend of mine, a psychologist, was for years visible to the public standing with drawn sword before the threatened "laws of nature," which he apparently thought were all finally identified and labelled, but the only attention he paid to any report of actual facts was to dance scornfully on some of its *disjecta membra*. For years I vainly endeavoured to induce some learned sceptic to attempt the explanation of one of our real cases, in its entirety, even if by a reasoned theory that the investigators had been conspirators and romancers. The only open opponent to promise such an attempt was furnished with all the documents of the not lengthy case, and offered all facilities for cross-examination, but unfortunately "has not had time" in the eight years which have followed. The wiser doubters have held their peace rather than betray their insufficient information or to sin against intellectual conscience by malpractice on records.

Mr Podmore stands quite apart from the writers referred to, in that while he opposed several possible supernormals, he did so by recourse of another supernormal, telepathy, attempting to merge them all in this. However some of us may think that he pushed his argument far beyond the warrant of his data, and became highly speculative at the end, he was intimately acquainted with the data, and did really reason. The time has not come when we can afford in our studies to leave out of account the theory that in telepathy we have the solvent of all mental psychic phenomena; Podmore has presented it with the greatest labour and ingenuity, and we owe him grateful recognition for so doing. Misrepresentation of the facts, whether deliberate or blundering, sarcasm (of which even Huxley was guilty) and dogmatic generalization, can do no good, but any writer who conscientiously studies the facts and in terms of logic attempts their appraisal, or works out to the uttermost a theory of the process by which they came into being, no matter what his verdict or his theory, confers a benefit upon psychical research.

Summing up, the species of organized investigation which began in 1882, and which has spread to various parts of the world, stands far in advance of where it was at that date. If a part of the area it was called upon to survey proved to be desert, even to learn that is a gain. If wider areas are still subject to earnest debate, yet still other areas are by nearly all our experienced ex-

plorers therein pronounced fertile land. At least in one respect they have advanced beyond the mere affirmation that certain classes of phenomena demand supernormal explanations. They have reached the point of affirming that instances of immediate sequences of thought as between human minds have been observed in such number and of such quality and under such conditions as to demonstrate that thoughts can pass from one mind to another. This is proved in the same sense that hypnotism was proved a number of decades before the proof was acknowledged as valid by the scientific world in general. Telepathy will ultimately duplicate the later chapter of the history of hypnosis, by which the latter was definitively recognized. Psychical researchers have done much to help bring about those discoveries regarding the inner relations and mechanisms of the mind which have transformed psychology within the memory of the living. To show that a mind can form outer relations, reach out to other minds other than by the mediation of the known senses, is to accomplish another revolution, and to open up to possibilities of still further external relations to which no one is yet entitled to fix a limit. Although it is possible to confront the spiritistic hypothesis in cases where evidential material is received regarding deceased persons, by the telepathic hypothesis, on the other hand to prove telepathy tends better to establish the mind as an entity in itself and strengthens the foundation for the hypothesis of telepathic reception from minds disembodied.

Whatever we have ascertained, as Newton said, a great ocean of undiscovered truth lies yet before us. Ours is a task not for years but for centuries. We do not know, for instance, by what process telepathy is accomplished. For that matter we are no nearer than Newton in understanding the *how* of gravity, unless Einstein has showed it, and even then it is said that only about a dozen men can understand it. We do not in the least know the process by which certain astounding feats of psychometry were accomplished. There are problems on every hand. But physics, a science centuries old, can say the same thing; many of its solutions to-day are not what they were yesterday, and some will doubtless be changed to-morrow. The magic word "cryptesthesia" helps us not at all; we might as well employ the symbol X, or, it may be, x, y and z.

A few persons, popularly called mediums, when their normal consciousness is more or less in abeyance, are able at times, in the form of purported communications from the dead to produce complexes of correct or approximately correct factual statement, under conditions which prohibit normal knowledge of them, and to a number and of a quality which make preposterous the notion that they are the result of guessing or mere chance. The only two theories which scientific procedure can take into account by way of

explanation are the Spiritistic and Telepathic. The concept of a Cosmic Reservoir of all human thought since time began has no scientific standing whatever since it attaches itself to no known fact. As well might we hypothesize that the psychic repeats what God tells her, or that thoughts hang to the walls of the room and thence fly to her brain.

Certainly the Spiristic theory has the simplicity of the Copernican system, while the Telepathic theory has the complexity of the Ptolemaic system. The former does not require us to believe that the subconscious of mediums always plays a false part and the same part, while, granting telepathy between the living and if the dead do survive, mediumistic results might well be telepathic seepings from discarnate consciousnesses, at times almost unmixed, at others more or less mixed and obliterated with and by the medium's subconscious thoughts. On the other hand, the telepathic theory would account for nearly all examples of mediumistic material—for I strongly think that there are a few exceptions—but only by positing that the medium can filch both from the conscious and latent thoughts of the sitter, resurrect long forgotten knowledge and reach out to distant persons then busy and unaware of the experiment in progress, and bring back facts to fill out the story; at the same time presenting the curious problem why the exercise of telepathy should always suggest to the medium's consciousness relations with the dead, when it never does so in our experiments for thought-transference between the living.

Allow me for convenience to employ the term telepath, for human subjects of experimentation for telepathy between the living, and mediums for the human subjects of experimentation who purport to transmit the thoughts of persons who once lived in the flesh. We need facts, facts and yet more facts from and relating to both. Not facts simply as such, but facts with a difference, facts marked by some peculiarity, facts carrying, so to speak, a co-efficient. We need to know much more definitely than we now do whether and to what extent sex, age, health, temperament, etc., affect the liability to telepathic faculty, whether and to what extent such particulars in combination between agent and percipient, as also relationship, friendship, and similarity of tastes tend to affect results. We need to study the internal characteristics of reactions of telepaths to experiment. We need to vary the conditions of experiments and the nature of what we attempt in them. There ought to be a revival of interest in telepathic research, and first of all, necessarily, an earnest search for favourable subjects. A similar course of observational and analytical inquiry should be made in the case of mediums on a wider scale than ever before, and the results on both sides compared.

If the evidential "message" of a medium is based upon thoughts telepathically received from spirits, I think the facts favour the opinion that the spirits are capable of a better control of the process on their side, have at times a better understanding of what is really going on, however much they may be impeded by obstacles in the medium's mind, than is the case with telepaths. If so, there might well be differentiating indicia which if carefully observed and studied could help us to come to theoretical agreement. There seems to be good evidence that thoughts upon which the agent is at the moment concentrating, sometimes pass to another mind. There are striking indications in the Doris Case, for example, that thoughts not at the moment in the agent's upper consciousness, but which he had dwelt upon a few hours previously may also pass over (if it be not merely a case of later emergence in the upper consciousness of the percipient). Whether facts not at the time of experimenting in consciousness, and not thought of for a long time, can pass over to a percipient, we have as yet little knowledge, or, if so, what law governs their selection. If it should sometime appear that in such case facts which in the past we had pondered deeply and emotionally have the right of way, that it is the precipitate of our own reactions in the past to an event, rather than our pale recollection of what were the reactions of another person to that event, which can get or is likely to get expression in telepathy from our mind, we might have at command a gauge of great value. For undoubtedly many evidential purported communications from deceased persons do appear to bear the stamp of their point of view, their original focussing of attention in the past, and their emotional reactions, rather than our own. As I have stated in the book, *Leonard and Soule Experiments*, after a great many true statements had come relating to my wife's last days on earth, it quite suddenly struck me that these to a large degree did not reflect my most vivid and poignant memories of that last period but rather seemed to reflect those particulars which loomed large in a sick mind which was kept ignorant of distressing major facts and exaggerated the importance of trifles. And after, as reported in the same book, the story of Stephen with its host of correct particulars had been finished, it struck me as peculiar, on the theory that the facts had been telepathically filched from my mind, that there was so little regarding that stage which at the time affected me most profoundly and has since remained so vividly in my memory, and that there was such dwelling upon the earlier stage at which I was not present, concerning which I had so little information, but which was certainly for my father charged with emotion at the time and must have remained vividly in *his* memory. When we know more about the laws which govern telepathy between the living we can better estimate whether

such appearances in mediumistic deliverances are, or are not, significant.

When I spoke of the need to collect facts with a difference—marked by some peculiarity, I had in mind such an incident as I discussed on pages 103-5 of the *Proceedings* of the A.S.P.R. for 1923. Here is the appearance as of one intelligence persistently trying to get through a word beginning with B (“Bossy,” a calf), of another intelligence (the subliminal of the medium?) persistently thinking that a rabbit is meant and trying to set down the word “bunny” and being checked, the conflict continuing until the first intelligence scatters the fixed idea of the second by a device precisely like one I employ when over the telephone. Someone persistently understands the name I give him as Pierce, and at last I, in desperation, say “No, no, Prince, son of a king, Prince!” I think that one reading the incident in its entirety could hardly avoid the conviction that it is an authentic subconscious exhibit, and one which does not easily classify with what we know of telepathy between the living.

Elsewhere than in experimental telepathy and in mediumism, incidents which present uncommon features should be studied with special care. One such is where a woman in broad daylight had a hallucination of *seeing the face of an old schoolmate with a bandage around it, as formerly had been the case when she suffered from a toothache, and then the face changed to that of a certain Frank——, likewise bandaged.* Later her husband, a commercial traveller, returned from a trip, reported that he had been unexpectedly called to a certain town, and while there attended the funeral of Frank, and said “I don’t know what was the matter but his face was tied up in a cloth.” Here was a mental mechanism eloquent with meaning, even though it is yet uncertain what that meaning is. Another incident is that of the man who at a moment of peril saw the apparition of his father on the deck beside him, while his sister, four thousand miles away, at about the same time, saw an apparition of him *and likewise the apparition that he saw.* Both incidents contain unusual features, and it is such differentiating features which, when multiplied in number and variety, may prove very posts to guide our way.

Concluding Reflections.

Whether one is a great scientist or only, to employ the language of Professor Sidgwick, one who means to be as scientific as he can, he is irresistibly urged to step across the line which separates science from philosophy and choose between the opposing concepts as to what factors constitute the universe. Some persons naively suppose that materialism is peculiarly scientific, whereas of course

it is a philosophical theory, exactly as is idealism or vitalism. One must choose, else his scientific thinking at last becomes confused. Haeckel chose materialism, and shaped his scientific theories accordingly. Huxley and Tyndall, although I learn with surprise that many regard them as very high-priests of materialism, were unable so to choose. Huxley said that either from the hardness of his head or his heart he could not identify mind with either matter or force, as science understands force. Tyndall speculated on the possibility that all matter has a mental side. It seems a strange moment, now, when science is pursuing matter behind the curtain which is the aspect that it presents to our senses, and finding its essence in the invisible, for mind which has accomplished this feat to deny its own sovereign entity. The mind can direct the very forces of nature. No more work need be performed by the brain in blowing up an ammunition depot and destroying a thousand lives than in setting the match to the task of lighting a cigarette. In every generation myriads of minds leap into fuller vigour and seek new adventures sheerly through thoughts that originated in the brain of a Plato, a Shakespeare, a Darwin, or some other thinker who uttered them yesterday, a century or a millennium ago. Surely the retina is indifferent to what characters lie on the printed page; only in the thoughts themselves do we find the efficient cause. I do not wonder that Wundt in the ripened period of his life looked back upon that earlier one when he pronounced psychology a branch or department of physiology, with a sort of shame, and called it the season when he sowed his wild oats. Probably the most of us here are impelled by the sum-total of our studies to take a stand with Wundt and Huxley and Carpenter and Ladd.

Very well, then: as physicists are studying a multitude of effects which are shaping their views on the nature of matter, from the centre of the so-called atom to the farthest range of the visible stars, so psychical researchers are studying certain effects to determine whether or not they indicate that which is not set down in the text books regarding the inmost powers and the external relations of the mind of man.

It would not surprise me if science should some time be led to regard that instinct, so to speak, by which every race of man from the lowest to the highest stage of development, entertains religious ideas, the one unifying element in which is the apprehension of invisible spiritual energy with which human beings are in some sort of contact, whether by virtue of gods or God, devils or beneficent dæmons, or the spirits of deceased men, as a scientific datum, even as it does gravity now. Anthropologists, as they come better to know the most primitive tribes, are gradually relinquishing the opinion that there is any quite without it. Individuals or groups

may cultivate insensibility to this integral trend, but like circus performers who learn to walk on their hands, they perish and leave no dynastic successors. Science may not take into account the interpretations which constitute the various religions, but it may have to take into account the central fact out of which the interpretations, so various, grow. But that time is not yet; we are now scientifically limited to empirical observation of facts, and to attempt at their proper time to weave together the facts into a connected scheme.

In a recent magazine article, a blind deaf mute has told how she was taught the sign language. Speaking of the beginning of the process she says: "The doings of this teacher were certainly strange to me at this time, especially the way she kept wiggling her fingers in the palm of my hand. . . . The queer, meaningless way she insisted on wiggling her fingers." If we are spiritual entities, it is certainly within the range of theoretical possibility that there are other spiritual entities as viewless to our senses as our own real selves are, and existing in other relations than ours. And it may be that many of the odd occurrences which occasionally take place, which we cannot classify with the mass of our experiences, and which we puzzle over, are, as it were, a wiggling of fingers in the palms of our hands, to make us beings of imperfect senses understand that external intelligence is trying to reach us.

If that view finally prevails and the fact of occasionally demonstrable telepathy from another sphere of being comes to be generally accepted, I think it fortunate that this supreme result of psychical research is likely to be reached only by slow steps and the gradual accumulation of evidence.

Otherwise I should fear another wave of indiscriminating credulity such as swept over parts of the United States in the latter half of the nineteenth century, and carried thousands of bright men and women from their moorings of common-sense, followed by a reaction and the flats of incredulity in which most of their children and grandchildren live to-day. Electricity, one of the most useful servants of mankind, is terribly destructive when uncontrolled. Popular self-government is a good thing, but there are peoples which, if too suddenly it were achieved, would plunge into anarchy and misery. There are truths which are dangerous if received too soon or too suddenly. So there is an ancient text which says: "I have yet many things to say unto you, but ye cannot bear them now." If by some accident the radio could have come into existence a thousand years ago, without the long course of discovery and invention which prepared the way for it, it would have made the world half mad. I think I see which way the finger is pointing in psychical research, but before any such general agreement as I have

mentioned, even in the ranks of this Society, much preparatory work is needed. All superstitions and every species of human imposition which has invaded our field must be banished, discrimination and moderation of expectation must be taught, it must become fully understood that if there are rifts in the separating curtain it does not mean that the curtain is to be torn fully away, and that most people will still have to take the facts on authority like the early Christians who did not themselves see the apparition of Jesus.

So thinking, I quote, quite calmly, the words of William James. "The facts are evidently complicated in the extreme, and we have as yet hardly scratched the surface of them. . . . These earlier observations of ours will surely be interpreted one day in the light of future discoveries which it may well take a century to make. . . . What we need is more and more observations."

Whatever the final issue, I refuse to believe that facts, however some of them may elude the conditions of the laboratory, but which do nevertheless perennially repeat themselves and force themselves upon observation, will forever be denied recognition. I have too much respect for human intellect to credit that there exists any category of observable facts concerning which it will remain perpetually in doubt. Their actuality and something of their workings and their significance—and in no department of inquiry can we say more—will at length be acknowledged.



PHOTO: *Fandyle*

PROCEEDINGS

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ARTHUR JAMES, EARL OF BALFOUR.

LORD BALFOUR was a Member and Vice-President of the Society for forty-eight years, and President in 1893. An appreciation of Lord Balfour's attitude to $\frac{1}{2}$ psychical research was printed in the *Journal* (xxvi. 59). We publish this portrait as a further record of his interest in the Society's work from the time of its foundation to the time of his death.

SOME INCIDENTS OCCURRING AT SITTINGS WITH MRS
LEONARD WHICH MAY THROW LIGHT UPON THEIR
MODUS OPERANDI.¹

By MRS W. H. SALTER.

THIS report consists of a number of incidents from trance sittings with Mrs Leonard which appear likely to be of interest to students of the *modus operandi* of such phenomena. One point upon which some of these incidents have a bearing is as to the form in which the knowledge shown by the entranced medium presents itself to her consciousness, whether the language of the sensory images which the control, Fedá, habitually employs, is really appropriate. Another point to consider is the way in which knowledge derived from various sources, of which direct thought-transference from the sitter may be one, but cannot be the only one, seems to be blended into a consecution of ideas appropriate to the supposed communicator. This phenomenon, which is not uncommon at good sittings, is obviously, and on any hypothesis, of psychological interest and importance. Thirdly, there is the question whether the time at which some piece of supernormal knowledge was acquired can be determined. I have quoted two book tests in which it would seem that the knowledge shown by Fedá was obtained either during, or very shortly before the sitting.

Some of the incidents I am about to relate are taken from my own sittings with Mrs Leonard, of which I have now had a considerable number, and some from sittings taken by other Members of the Society who have sent us their records.

As to my own sittings—the purporting communicators are my parents, Dr and Mrs A. W. Verrall, and during a part of the sitting my father, to whom in his character of communicator I shall refer by his initials, as A. W. V., frequently purports to control Mrs Leonard himself. My father died in 1912, my mother in 1916; my first sitting was in January 1917, and I went to Mrs Leonard anonymously. After my fourth sitting it became impossible to conceal my identity, as the Society was carrying out a series of experimental

¹ A considerable part of this paper was read at a Private Meeting of the Society on 9 January 1929.

sittings with Mrs Leonard, for the organisation of which I was mainly responsible. I do not, however, in my own case attach much importance to this question of anonymity, because nearly all the interesting matter I have received at these sittings has been of such a kind that knowledge of my identity would have availed Mrs Leonard hardly at all towards obtaining the required information from normal sources. It is noticeable that in all the sittings I have had with her she has never once proffered the least scrap of information of the sort easily obtainable in my father's case from books of reference or in other obvious ways.

1. *Concerning knowledge apparently derived from sensory impressions.*

The first point I propose to discuss is the form in which the various impressions registered at a sitting present themselves to whatever entity is purporting to control Mrs Leonard at the time. According to Feda her perceptions are definitely sensory; she uses such expressions as "he's building up," "he's showing me so-and-so"; or, addressing the communicator, "say it again, Feda can't hear," and so forth. But careful observation of Mrs Leonard's phenomena has clearly shown that these statements are not always to be taken at their face value. Lady Troubridge has called attention to this question in a paper on "*The Modus Operandi in Mediumistic Trance*," published in Vol. xxxii of our *Proceedings*. She writes (p. 369): "There are certain aspects of the Feda phenomena which leave me very doubtful as to whether these simple sensory terms convey any accurate analogy with the process really involved." She goes on to report an occasion when Feda described "to Miss Radclyffe-Hall with great accuracy and great detail a portrait of Miss Radclyffe-Hall." But Feda, who according to her own statement "sees this picture and is able to describe it at such length, never apparently grasps the fact that the picture being described by her is a portrait, and a striking resemblance at that, of the very familiar sitter to whom she is speaking."

The same difficulty is dealt with by Mr Drayton Thomas, a regular and long-established sitter with Mrs Leonard, in his paper in our *Proceedings* (xxxviii. 49-100), and it has been clearly stated by his communicators on various occasions that Feda's own opinion as to how impressions reach her cannot always be taken as accurate. For instance (p. 54): "If I then project a thought of some concrete object, Feda may remark, 'I see so-and-so,' but though she may seem to be seeing the object, it is really my thought of it that has reached her." And again (p. 56): "For example, she will say 'I hear the name Lily' when actually a lily has been pictured to her, or the idea of a lily impressed on her mind. She

says 'hear' because she has given herself the suggestion of hearing and fails to realise that she has not actually heard it. Expecting to perceive ideas as sound, she thinks they come that way, even when they are coming by sight."

I will now quote from my own sittings some passages which seem to show clearly that an impression represented by Fedá as visual was reaching her fragmentarily in some other way. At the first sitting I had with Mrs Leonard on 29 January 1917, after some quite appropriate talk which I interpreted as referring to my mother, Fedá said: "There's a gentleman with her with a beard, not a very big beard, shortish at the sides, longer in front. He wants to let you know they're together. He's a nice-looking man, he's smiling all over his face." Almost immediately after this the sitting came to an end.

The important point to note here is that the man said to be with my mother, afterwards identified with my father, is described as having a beard. My father did in fact wear a beard from early manhood to the end of his life.

At my third sitting, on 29 May 1917, after remarks which again seemed to refer to my mother, Fedá said: "She's got a gentleman with her, a bit thin, a bit of a cough. His nose comes out, it's bent down a little, it's a bit thin at the sides. The bone shows a bit. He's been good-looking. The eyebrows are brown, a little arched, the hair brown, the temples bare, have a bony look." Then after some quite appropriate remarks as to the man described having been in bad health for some time before he died "not able to get about," Fedá gave the letter "H or A" and then the name "Arthur," my father's first name.

The above description is, as far as it goes, quite a good description of my father as he was before his hair went grey. Although there is no mention of a beard this time, nothing is said which is inconsistent with his having had one. At my next sitting, on 15 November 1917, after some further references to "Mr Arthur," I asked Fedá if he could "build up." She said: "He's altered a bit, got younger-looking, more towards the middle of life, hardly that. He's got good features. The nose shows the bridge. It's narrow above, round at the tip. It broadens a bit below. The mouth is a bit large, the lips are pink, not red. The chin is more rounded. The face is not quite oval. It's too broad to be oval, but it's not round. The eyebrows are brown, straight, going out a little at the corners. The eyes are not prominent, the hair brownish, shows the temples. The forehead is good, a fine shape, an open, clever look. People would say that was the finest part of the face. The bumps on the temples stand out a little. There's a rather straight line between the nose and forehead. The hair is a bit longer on the top. He brushes

it away from the face. There's a photograph, an old-fashioned one in a low collar, a bit of hair coming forward."

Now this description, in which the communicator's face is apparently viewed sometimes from the front, sometimes in profile (see *e.g.* "there's a rather straight line between the nose and forehead"), is on the whole distinctly good, but it contains details which seem to imply that the man described is beardless, the size and colour of the mouth, the shape of the chin. It is impossible to suppose that any one who was describing in detail a man visibly present could be mistaken as to whether or no he had a beard. Against the interpretation that Feda is seeing my father as a quite young man, clean-shaven, is first her own statement that he is "towards the middle of life, hardly that," and secondly the fact that at the sitting of 29 January 1917 the man afterwards identified with my father is said to be bearded. My own interpretation of what occurred is that Feda was not really "seeing" anything, but that on this, as on other occasions, she was receiving a series of mental impressions which she translated into visual terms. Her statements that my father had a rather large mouth and a face not rounded but too broad to be oval, are in fact correct, as early photographs show.

Two years later, on 8 November 1919, when A. W. V. was purporting to control Mrs Leonard himself without the intervention of Feda, what seems to be an allusion to this mistake of hers was made spontaneously: "A mistake of Feda's, a great mistake. (Touches side of face, a little below the cheekbone.) I can't understand how she managed to make such a mistake without correcting it after; it's a most strange thing. It's a good thing I'm able to correct these little things myself. Yet it has a very good point in a way, it certainly does not suggest that the medium has any knowledge of me, because if the medium had any knowledge of me whatever, such a mistake would have been impossible."

It is of course true that if Mrs Leonard had ever seen a photograph of my father or had heard his appearance described, she would have been most unlikely to describe him herself as beardless. The incident is also an example, of which many others could be found, of the powers of memory shown in these trance-phenomena. Between the original statement and the correction of it, all but two years elapsed, during which Mrs Leonard had sat with a great number of other sitters and had presumably given some hundreds of descriptions of supposed communicators and their friends. One might have thought that all recollection of the incident of 15 November 1917 would have been obliterated from her mind. But something, or somebody, seems to have remembered that a description had been given, which in one particular was wrong, and when opportunity occurred, to have made the necessary correction.

Between November 1917 and November 1919 I had had six sittings with Mrs Leonard myself. A. W. V. took control himself for the first time on 24 October 1919, and again on 25 October 1919; on neither of these occasions did he make any reference to his own personal appearance.

I now quote from my first sitting with Mrs Leonard, 29 January 1917, some statements made concerning a communicator subsequently said to be my mother. Many of these statements, as will be seen from my notes, given below, are true; they contain, however, an admixture of such errors as could hardly have arisen had Feda's knowledge been derived from any clearly apprehended image or series of images. Her words, though for the most part they purport to describe something seen, suggest to my mind a number of rather disjointed mental impressions, patched together and occasionally eked out by a little guesswork.

Extract from sitting of 29 January 1917.

FEDA. There's an elderly lady with you, medium height, lost her figure which made her look broader, rounded cheeks but a little sunk, the chin a little fallen away, the mouth not full, the lips not red, a little drawn down, the nose rather straight, the eyebrows brown, slightly arched, the eyes blue grey. The hair has been brown, getting grey, the hair is done close back, twisted up.

This lady was not well before she passed out: she was not well for two or three years, not an invalid. Feda thinks she had an internal weakness for some years; she didn't know, but it caused her passing on. The doctor would tell you she had it for some time. About two years before she began to feel ill, but she kept it to herself. At the end she got suddenly weak. Towards the end she got tired of her physical body. . . .

My mother was fifty-eight when she died; she was of medium height, her figure a little broad in later life, but not at all stout. The description of her features is correct except that her eyebrows were decidedly arched (though so lightly marked as to be barely visible) and her eyes were distinctly blue, a conspicuous feature in her face. The statements about the hair are true.

All the statements about my mother's last illness are true. She died of abdominal cancer, which according to the doctor must have begun some three or four years before her death.

She was used to doing things herself, she didn't like not doing them. She used to do things with her fingers a good deal, it soothed her. (Motion of sewing or knitting.)

You've got a photograph of her, it shows the top part, almost full face. It's not very nice of her. She looked serious in it, thoughtful, but when she smiles she looks quite different. Her face got a little haggard before she passed on. She got so tired-looking round the mouth. Her cheeks are more filled out now. Her hair was a pretty brown one time, a warm brown. She looks much younger now. That photo doesn't do her justice. She says something about an enlargement, there was an enlargement made.

H. S. Yes.

F. She was pleased about it, thinks it was rather good.

H. S. Yes.

F. She plumped down suddenly. She wanted to show Fedra she was sitting slightly sideways, but you can see both sides of her face. There's a piece of furniture at one side. One arm is stuck out more than the other, on the side of the furniture. She's got a dark dress on, a black, Fedra thinks. She didn't like low necks. She's got a band round the throat, a little white band at the top. She's got a bodice, not a blouse, a made bodice, fastened in front, with little folds, pleats at the side. There are bands round the wrist. She's got an oval brooch

My mother did a good deal of crochet-work during the last year of her life.

I have a photograph of my mother which is an enlargement from a snapshot I took myself about two years before her death. References to enlargements of photographs are, however, so common at sittings with Mrs Leonard and other mediums that little evidential value can be attached to this point. The following statements are correct in relation to this photograph of my mother: she is looking serious and thoughtful, the photograph shows "the top part, almost full face," my mother is sitting slightly sideways, she has a black dress on with a white band at the throat. It is not true that there is a piece of furniture at one side, that one arm "is stuck out more than the other," that my mother is wearing "a made bodice," "not a blouse," that "there are bands round the wrist." My mother is wearing a black coat, not unlike a bodice but open in front, showing a white silk shirt. There are no "bands at the wrists," which are not visible at all in the photograph, nor is there any "oval brooch with a gold rim." My mother possessed such a brooch, but so, I imagine, did most people of her generation. The most distinctive feature of the photograph, that my mother is reading a large book held

with a gold rim, not quite oval, rather old-fashioned. The outside is like a rim. There's something different in the middle. open on her knees, is not noted at all.

The general inference which I should draw from the above extract is that a certain amount of veridical information about my mother was woven by Feda into an imaginary picture of an elderly widow, based on preconceived ideas of the appearance such a picture might be expected to present. The "bands at the wrist" are presumably widow's bands, which my mother never wore. She was, in fact, a widow at the time the photograph was taken.

I now give a group of extracts from my sittings, showing various attempts, finally successful, to give my mother's maiden name, Merrifield. These extracts afford a good example of the way in which a sensory impression, in this case auditory, may be helped out and brought to precision by association of ideas.

Extract from sitting of 15 November 1917.

FEDA. Mar, he's trying to say four letters. He wants to get a name starts Mar (Feda draws in the air with her finger M a r s (?) t e n). And another name beginning with M, some one on the other side. The other name is on the earth plane, and another name in the spirit-world. It's the same name Marston. It starts Merry, but there's more to the name.

H. S. I think I understand.

F. (*sotto voce*.) Deth, deth. (Aloud.) Can't get it, starts Merry and another syllable. About four letters after Merry. (She whispers what is evidently an attempt at Meredith.) Three or four letters above the line. There's a repetition of letters in the name. Two letters are doubled. It's not Meredith. Merivale, more like that. It's

This first attempt is very confused; possibly some confusion was introduced by the fact that my mother's Christian name was Margaret; this name had not at the time been given at my sittings, of which the sitting of 15 November 1917 was the fourth. The name Marston means nothing to me. Having got the first syllable "Merry," Feda proceeds to guess, and tries "Meredith."

Feda then guesses again with

not a name you know, not an ordinary name like Smith or Jones.

“Merivale,” a better approximation to Merryfield in sound and sense.

Extract from sitting of 25 October 1919.

FEDA. He wants to speak about Flora. [In a passage which follows, not quoted here, the name Maria was given.]

A word he's reminded of by talking of Flora, can't get it, Maybank, Maybank. Not quite right, I get bank and M. A word bank at the end of it. . . . This name was not connected with anything in his own house. When he got into touch with the person or place of that name, he had to go away from his own house for it. Marybanks, Merrybank. M and bank is all he can give.

Flora is the Christian name of my mother's only sister, Miss Flora Merrifield. The name had been given quite appropriately at the end of the immediately preceding sitting, 24 October 1919, when A. W. V. was purporting to control the medium himself. The name Maria had also been given at an earlier sitting; it was the name both of my aunt Flora and of my grandmother, Mrs Merrifield.

There follows another attempt at Merryfield, which appears now as “Merrybank.”

Extract from sitting of 25 October 1919.

FEDA. He's talking about a place called M, many years ago, he was fond of a place called M himself, a place with a long funny name, he thinks you know the name.

H. S. I must think it over.

F. He says it isn't a name you'll have to verify. It's a long name, with three sounds or more.

H. S. Can he remember anything about it?

F. He'd lived in it.

H. S. I'm off here altogether!

F. It was not lately, not in the late part of his earth life. You've heard of it, Merryfield, Merryfield.

The name Merrifield is here correctly given, but is incorrectly

H. S. I know that name, but it isn't a place where Mr Arthur lived.

F. Merryfield is a field where people enjoys themselves. That's what it means.

said to be the name of a place in which my father had lived. There is a village called Merrifield in Devonshire (the Merrifields were a Devon family), but my father had no association with the place himself. This mistake Feda explains by reference to the meaning of the name. This name would have associations for my father with the earlier part of his life and with the place in which he then lived, Brighton.

With Feda's attempts at the name Merrifield may be compared the similar attempts made to give the same name by Mrs Thompson's Control, Nelly; the incident is thus reported by my mother (*Proceedings*, xvii. 208):

"Merrifield was said to be the name of a lady in my family. The name was given at first thus: 'Merrifield, Merriman, Merrythought, Merrifield; there is an old lady named one of those who,' etc. Later, Nelly said: 'Mrs Merrythought, that's not quite right; it's like the name of a garden,' and after in vain trying to give me the name exactly, she said: 'I will tell you how names come to us. It's like a picture: I see schoolchildren enjoying themselves. You can't say Merrymans, because that's not a name, nor Merrypeople.' Nelly, later on, spoke of my mother as 'Mrs Happyfield' or 'Mrs Merrifield,' with indifference."

2. *A detailed description of an article which had belonged to the purporting communicator.*

An incident also presenting a curious problem as to the form in which the knowledge shown, which was in some respects remarkably exact and detailed, reached the medium's mind, occurred at a sitting taken by a Member of the Society, Mr Alexander Graham; the notetaker was Miss E. M. Horsell, Assistant Secretary to the Society. Mr Graham [*pseud.*] had sat with Mrs Leonard before, but there had been no previous allusions to the matters here discussed. I have included in my report another incident in the nature of a "picture-test" which is not immediately relevant to the particular point I am now illustrating, because it seemed best that these two incidents, occurring in the same sitting, should not be divided.

Extract from sitting of 6 October 1928.

FEDA. She [the purporting communicator, the sitter's wife] feels you are going to be reminded of her to-morrow. That it will be a nice condition, something rather beautiful, . . . that there will be something happening to-morrow that will make you think of her, bring her back to you. She feels as if to-morrow you are going to be reminded of a place that you knew when she was here with you. You may be shown a picture of it or see a picture of it, of a place that she was very very fond of. Wait a minute. Oh! Rocky. When she was here on earth do you remember going to a very rocky place?

MR GRAHAM. Yes.

F. Because she giving me a feeling of a very rocky place and as if you were happy there. It make me feel a rather happy time, and yet though it was happy there was something occurred just about that time. wait a minute, that was rather worrying. A happy time in a way, but something rather worrying came into it. I don't know if you remember something like a little illness?

A. G. Yes, I think I know what she means.

F. Something began or was worrying just about that time.

A. G. Yes.

F. It's that place that she thinks you are going to have a reminder of it again.

Notes by Mr Graham.

The sitting was on a Saturday. Every Sunday when I am in London I attend a certain special Church Service at which I invariably have the strong impression of being in very close contact with my wife. There is possibly a reference to that here.

The first years of our married life were passed in the foothills of the Rocky Mountains. We had many happy recollections of that time, and it is true that "something rather worrying," "a little illness" occurred, as I had an attack of pulmonary haemorrhage which alarmed us very much at the time, though it did not recur, or lead to any disabling incident.

On the Sunday (the "to-morrow") I had no reminder of the above place, but while reading the *Daily Express* soon after 8 on the Monday morning, I came across an article on a tour in Canada with a picture entitled "The Rocky Mountains." They were not mentioned in the letterpress.

I should further add that 8 October was the anniversary of our wedding at Montreal, from which place we proceeded to our home near the Rocky Mountains. This anniversary is referred to in an earlier sitting of 22 October 1926.

It should also be mentioned that the *Daily Express* for 6 October, the day of the sitting, contained under the heading "New Features for Women" a statement that on the following Monday would appear the first article of a series entitled "The Truth about Life Out There," giving an account of a woman's trip across Canada. There is no mention here of the Rocky Mountains. In reply to an enquiry as to whether he was likely to have read this notice in the *Daily Express* before his sitting, Mr Graham writes (in a letter dated 27 January 1930): "I am sure that I did not *consciously* see in the *Daily Express* of October 6 the allusion to the forthcoming articles on Canada previous to my sitting. At that time I always on the morning before a sitting made a point of never looking at a newspaper, being desirous of concentrating all my attention on the coming sitting. . . . Of course I may have *subconsciously* taken in the allusion, though I do not think so, as my tendency was rather to avoid even a glance in the direction of a newspaper that might be in the room. I searched several newspapers on Oct. 7th (Sunday) for a picture of anything *rocky* that might seem to fit in with the communication, but without success. I was exceedingly surprised to come upon a picture of the Rocky Mountains in the *Daily Express* of Oct. 8th, having given up all expectation of finding anything which might seem to coincide with the communicator's remarks."

Mrs Leonard informs me that the daily papers she was taking in October 1928 were the *Daily Mail* and the *Daily Herald*. There is no reason therefore to suppose that she had seen the announcement in the *Daily Express* at the time of Mr Graham's sitting. Nor, so far as is ascertainable, had she any normal knowledge of his personal association with the Rocky Mountains.

I will now quote a further extract from Mr Graham's sitting of 6 October 1928 in which a scent-bottle was described which had belonged to the communicator. There was another allusion to the matter at a later sitting on 14 January 1929 (see below). Concerning this scent-bottle Mr Graham writes: "I was unable to identify the object described at the time of either sitting. After the first sitting I asked my wife's two sisters if either had a scent-bottle of hers. One of them had none, the other a single bottle, which, however, had never contained scent, and did not in the least correspond to the article described, and finally I gave this latter up as unidentifiable.

"In July 1929 I was inspecting at a furniture storage some things of mine which I had not seen for three years, and there noticed a scent-bottle which struck me as being possibly the object referred to at the sittings. It was forwarded to me from the store in October 1929, and in some important respects it seems to correspond with the description."

Extract from sitting of 6 October 1928.

FEDA. And what was that you had of hers in metal, open at the top, unscrewed at the top; a small thing, not a big thing you drunk out of, a little tiny thing. You unscrewed the top like a little tiny bottle. It still exists.

Notes by Mr Graham.

Inaccuracies are as follows:

The top only of the scent-bottle is of metal, the body is of cut glass. It is not tiny, but large for a scent-bottle, the body of it being about three-fourths the size of an ordinary water-



A. G. Have I got it still?

F. A little tiny bottle. You unscrew two things at the top because when she undid one she seems to be doing something more to the top. It isn't plain—it got like engraving on it, like a little fancy-work stamped on it, and just round the edge of his top got a little beading and on the top part of it, the top part it has got like little sprays of flat open flowers, round, very round, just as round as pennies, only tiny, like forget-me-nots, not

bottle such as one finds on a washstand. Only the metal top unscrews. Beneath it is a glass stopper.

The apex of the top is stamped with initials.

Correct.

This description is remarkably accurate [see illustration].

like a closed rose, you know a full flower. It is something that she was fond of and used here. She must have had it a long time. Perhaps you will be able to find it.

A. G. I don't think I've got it now.

F. It still exists. If you haven't got it, she thinks it could be traced.

A. G. I think her sisters might have it.

F. She would be pleased if you could find it because it was a little thing she was very fond of. Some time, if it's easy, she would like you to remember it and verify it. Oh yes, oh! she smelled him; something she smelled, like that she is giving.

A. G. Yes, I understand, I think I know what it is.

F. Only she doesn't suppose you will remember that particular pattern on it. As well as the flat flowers there's something rather streaky on it, long curving streaks on it. It's very pretty, but no one is using it now. It's just put on one side.

The bottle and contents were a present from a valued friend. My wife, though she only used it seldom, was very fond of this perfume and also of the bottle containing it. She had had it during the last seven or eight years of her life.

I had no recollection of having stored it. When my things were stored I was recovering from a severe illness and my capacity for mental attention was sub-normal.

Some cotton-wool inside the metal top has a perceptible perfume. The metal top with the cotton-wool still in place was sent to the Society for confirmation.

Although I said "I think I know what it is," I had no impression in my mind of this object. I was thinking of another scent-bottle of my wife's with a spraying apparatus, but I am sure this had no metal top at all similar. Anyhow it has disappeared long since.

Quite accurate [see illustration]. I am one of the least observant of mortals as regards ornamentations on silverware, etc.

Extracts from sitting of 14 January 1929.

FEDA (*aside*). I think you told him before about the tiny weeny bottle.

A. G. Yes, I could not get hold of it yet.

[There appears to be some doubt in Feda's mind whether the object here described is the same as in the sitting of 6 October 1928. The description,

F. Not that. She shows me now something partly of glass, very small and round, the top screws on, a metal top with engraving, very small, she thinks you have it. It is not *strictly* speaking a bottle, she says.

(Later in the sitting) :

F. What have you got of hers that is so scented? She is not a lady who would use scent. I like gallons of scent! But she didn't, as a rule, use scent. But you still have something that was strongly scented, and still has the scent in it. It is about the only thing that you have that is scented, aromatic.

however, is very similar, and the small size of the bottle is again emphasised.—H. de G. S.]

Notes by Mr Graham.

Correct.

There was about a quarter of an inch of scent remaining in it.

It is the only thing.

The points as to which Fedra shows a correct apprehension in these two sittings are as follows :

(1) It is true that the communicator had a scent-bottle which she valued during her life and that this bottle is still in existence, but not in use.

(2) The metal top of this bottle is accurately described in considerable detail.

On the other hand, at the sitting of 6 October 1928 no knowledge is shown of the fact that the body of the scent-bottle is of glass, and at both sittings the bottle is incorrectly described as very small. (Fedra's words rather suggest a bottle of smelling-salts.) Fedra does not anywhere definitely state that she is seeing the bottle, but the words "when she undid one she seems to be doing something more to the top" suggest a visual image of the communicator holding the bottle. But when we take into account the complete misapprehension as to the size of the bottle, it seems clear that we have again to do with a series of disjointed impressions (some of them, *e.g.* the impression of pattern on the metal top, perhaps visual) which Fedra pieces together and ekes out with some purely subjective additions.

3. *Impressions derived from some person present at the sitting.*

Various writers on Mrs Leonard's phenomena have called attention to the fact that according to statements made by the communicators a particular line of communication may be facilitated by the fact that a certain word, or a certain train of thought, had been recently in the mind of some person present at the sitting. I

have noticed several incidents in my own sittings which confirm these statements, and I think that this tendency to draw upon the mind of the sitter or of the medium may occasionally lead to confusion or irrelevance, because it may be that Feda herself does not always know the source of her impressions and ideas which have been accidentally gleaned from some living mind, ideas which are "in the air," so to speak, may be wrongly associated with the communicator. I will give from my own sittings two examples of what I mean.

At a sitting on 8 November 1919, when A. W. V. was purporting to control the medium himself, after what I interpreted at the time as a reference to the fact of his having worn woollen cuffs in winter to keep his wrists warm, he went on: "I don't think you'll understand what I am going to say now, but it isn't given to many men to wear two pairs of trousers (said with a laugh). Well, I did once, I think you'll remember. Your mother remembers it well, you knew about it too."

Now, although I cannot of course assert positively that my father never at any time of his life wore two pairs of trousers, I *can* say that I have no recollection of ever hearing such an incident reported. But what interested me in the allusion was the fact that I had spent the evening before the sitting at the house of the friend who was taking notes for me, and in her and my presence another friend had related an incident in her own experience when a man had worn several pairs of trousers. I was at once reminded of this story by what A. W. V. said.

Again, at a sitting on 1 November 1921, just as Feda was relinquishing her hold upon the medium in order that A. W. V. might assume control, she ejaculated without any comment or explanation the name Sylvester. Subsequently A. W. V. referred to the name himself, thus:

A. W. V. Do you remember Feda saying Sylvester?

H. S. Yes.

A. W. V. She got that from me. I suddenly thought of Sylvester. It's the name of some one I knew, some one not on the earth, over here with me. I wonder if you know you've reminded me of this yourself, you've made it easy for me to say the name. Often I can say a name if you've thought about it lately.

Now, in this case as in the preceding one, I ¹cannot assert that my father never knew a man called Sylvester,¹ but I am not myself

¹ Mrs Sidgwick and Dr C. D. Broad have both suggested to me that my father is likely to have had some acquaintance with the well-known mathematician Professor J. J. Sylvester (St. John's College, Cambridge). Sylvester was associated in his work with Cayley, whom my father certainly knew; he died in 1897.

aware of his having any close personal associations with that name. On the other hand I can perfectly well understand the assertion that I have made it easy for him to get the name by having it lately in my thoughts. My husband and I had recently sold our London house to an old acquaintance of my husband's family, Mrs Sylvester Horne, and I had received a letter concerning this sale only a short time before the sitting.

4. *Three book-tests.*

(a)

I propose now to quote, again from my own sittings, a book-test, together with the context in which it was given. The incident is of interest for two reasons: (1) the time at which knowledge was probably obtained of the book seems to be definable within narrow limits; (2) the book-test and the context on either side are closely linked together by a consecution of ideas natural to the ostensible communicator, that is, my father. The sitting in question took place on 4 October 1921, and I acted on this occasion as my own notetaker.

FEDA. He says he's met an old friend lately.

H. S. Yes?

F. But he means a man, not a lady; some one who's passed over recently, and he's quite an elderly man.

Stroke down and a piece on (draws P). It seems connected with him, but as if this gentleman's interests, some important ones, would be right away from here, from London and where you live now. It's as if he had lived a good deal away from here. Now he's giving a B connected with him, it's a place, a rather important place connected with him.

H. S. I think I know who this is, Fedra.

F. He had been what Mr Arthur calls an active man, but he'd not been well for a good while before he passed over. But Mr Arthur says he doesn't think up to a very little while before he passed over that he was expecting to go just then. He said he seemed rather surprised to find himself on the other side, and also he had started, not exactly started, but attached himself to some rather important work that he apparently intended seeing through. This was not connected with his native place, or the place he actually lived in. [There followed an allusion to a hospital, in which the man in question was said to be interested.]

At home, Mrs Nelly, you ought to have things that would connect him with Mr Arthur. Get a feeling of writing about him. You've got a book with his name in.

H. S. Yes ?

F. Mr Arthur seems quite positive of that. One or two things you should have that would connect him with Mr Arthur. Do you know that they were spoken of together, linked up in a book ? Mr Arthur's not sure if you see what he means by that at the minute ; but as if this gentleman had done things that Mr Arthur would be linked up with. He keeps saying a book which links them up. You've got the book.

H. S. I'll look.

F. He knows what the book looks like. It's so dark, the binding, it looks black to Feda. It's a pretty thick book, about that thick (indicates about $1\frac{1}{2}$ inches). And I should think it's a bigger book altogether than the books about dukes and earls she [Mrs Leonard] reads and thinks she'd like to write, a bigger book altogether. The paper is not very smooth shiny paper, rather good quality, a little bit rough. Is it letters in it, like printed letters ? No, extracts from what different people have said. There's some extracts in it of what different people had said. Like somebody writing a letter and then printing pieces of it. That seems to be in this book. [Then followed the book-test which will be discussed presently.]

He wants to say this friend of his and he will be doing some work together, and Mr. Arthur was very pleased to meet him. Do you know a name J that should be linked up with him ?

H. S. Yes.

F. I keep getting a big J. What's that ? Page, not in a book, a name. Sounds like Page. He doesn't mean a page in a book, he means a name. He seems to think that talking about this spirit that's passed over should remind you of Page.

Another name beginning with J, not the one he mentioned before, like Joyce-lin, Joyee and N at the end. Some one Mr Arthur knew well here and who's also on the other side. He would be a link up with the one who's passed recently.

The " old friend " I identified at the sitting as Professor Henry Jackson, who had died in the latter part of September. My father was associated with Henry Jackson during the whole of his life at Cambridge, a period of over forty years. Both men were classical scholars, both were Fellows of Trinity College, Cambridge, and for many years members of the College staff. It is true of Jackson that he was " quite an elderly man " (he was indeed an old man, over eighty when he died). Although his health had been failing for some years, he was at work until a short time before his death.

Of the three letters given by Feda, J is the first letter of his own name ; P might well stand either for philosophy or for Plato (Jackson was for many years famous as a teacher of Greek philo-

sophy, and his lectures on Plato and Aristotle were familiar to many generations of Cambridge scholars); B, "a rather important place connected with him," I should suppose to be Bournemouth, where his wife and family resided for many years and where Jackson himself spent his vacations. But this habit of giving single letters without any clear indication of the matter to which they are intended to refer, I consider an unsatisfactory feature of mediumistic sittings; it leaves too much to conjecture. As to the statement that he had "started" or "attached himself to some rather important work that he had apparently intended seeing through," "not connected with his native place or the place he actually lived in," it was stated in an obituary notice that he had been preparing a collected edition of his papers, and it was hoped to bring this out after his death. His papers would, of course, be connected with his work at Cambridge, as distinct from his native place (somewhere in the Midlands) and Bournemouth, his place of family residence. I do not know of any special connection he had with any hospital.

There are several books in existence in which my father's name and Jackson's are to some extent linked, and two of these are in my possession. One of the two, *Fasciculus J. W. Clark dicatus*, to which both my father and Jackson contributed together with many others, seems to be ruled out by the fact that it is bound in a rather light shade of brownish yellow, whereas the book described by Feda is said to be so dark as to look almost black. But I have in my possession a book in which my father and Jackson are linked more closely and immediately than in the *Fasciculus*. In 1895 my father brought out the first volume of his essays on *Euripides the Rationalist*. The dedication of the book runs as follows: "To Henry Jackson, Litt.D., this book written in response to his suggestion is offered as a small acknowledgement of many debts." The book is bound in a very dark green, which looks almost black in a poor light. Feda has rather overestimated its thickness (which is barely an inch), but in height and width it is distinctly larger than the average novel (9 by 6 inches). It is printed on rather good, heavy paper, but the paper is not rough. There are no letters in the book, but my father's own writing is frequently interrupted by extracts from other writers: e.g. there are one or two quotations from Paley, several from Browning's *Balaustion's Adventure*, and throughout the whole book very numerous quotations from Way's translation of Euripides.

It was immediately after the reference to this book that Feda interpolated the book-test (with which I shall presently deal). She then returned to the subject of the "old friend" with the allusion to the letter J and to the name Page. "He seems to think that talking about this spirit that's passed over should remind you of

Page." This is an interesting point. In the year 1873 my father was bracketed second in the Classical Tripos with Dr T. E. Page, and in the following year he and Page and Henry Butcher were all bracketed together for the Chancellor's Medals. The name Page would therefore be closely associated in my father's mind with his early years at Trinity and consequently with Henry Jackson.

At my next sitting on 1 November 1921 Fedra referred again to Page, and said this was the name of some one my father had known who was "still on the earth." This statement was correct. There have been no other references to Page in my sittings before or since. In the reference to another name beginning with J, "Joyce and N at the end," I think there is some confusion and that this is an attempt at Jackson. I do not know of any name beginning with J specially associated with Dr Jackson except his own.

I now turn to the consideration of the book-test, and I must begin by explaining that on my way to Mrs Leonard's house (she was then living at Oakleigh Park) I bought at Broad Street Station a copy of the *Strand Magazine* for October to read in the train. The magazine was bought at some time between 9.30 and 10 a.m. and the book-test was given shortly after 11 a.m. On my arrival at Mrs Leonard's house I laid the magazine closed on a chair. Mrs Leonard was never alone with it and made no attempt to examine it in my presence. After two or three minutes' conversation on indifferent subjects, she passed as usual into trance. I now quote again from my record :

FEDA. Over on the chair, the book over on the chair. Your book on the chair over there (indicates *Strand Magazine*).

H. S. In this room ?

F. Yes. Will you look about page fourteen ? Something there should remind you of Mr Arthur's old haunts, old pursuits. He's just thought of that and was afraid of losing it.

When I came to examine the *Strand Magazine*, I found, as was to be expected, that it is numbered consecutively from the beginning of the year. It has, however, been clearly laid down that at any rate in the case of those book-tests for which my father is made responsible the test is to be verified by counting the pages from the beginning of the main reading matter of the book, irrespective of the actual numbering of the pages. According to this reckoning the fourteenth page of the October number of the *Strand* was the page numbered 302. I could not find on this page anything to suggest my father's old haunts in the local sense of the word ; the reference to his old pursuits is clear enough. Near the bottom of the left-hand column (the *Strand* is printed in double columns) the following passage occurred : " It is all very well to excite pity and

terror, as Aristotle recommends, but there are limits. In the ancient Greek tragedies it was an ironclad rule that all the real rough stuff should take place off-stage, and I shall follow this admirable principle." To my father's life-long preoccupation with Greek tragedy, his many published works on Aeschylus and Euripides abundantly testify, and it may be noted that this allusion is very aptly interpolated in the midst of references to an old friend with whom my father's classical interests were closely associated, the friend to whom, as I have already mentioned, he dedicated the first of his three volumes of Euripidean studies, *Euripides the Rationalist*. The reference in the *Strand* to Aristotle constitutes a further link with Henry Jackson, whose lectures on Aristotle were a familiar institution at Cambridge for many years.

As to possible sources of the knowledge displayed, I had not myself at the time of the sitting read the story in which the quoted passage occurs, but I cannot of course be certain that my eye had not fallen on it in turning the pages. It seems, however, unlikely that I should have made the mental calculation involved in referring to page 302 as the fourteenth page. Mrs Leonard, when I questioned her after the sitting, without giving a reason for the question, said she had not read the October *Strand*, which on the 4th of October had only been out a few days, and the argument in regard to calculating the page applies to Mrs Leonard as well as to myself. She could, of course, have no motive for deliberately preparing the test beforehand, for she could not possibly know that I should buy the *Strand* that day and bring it with me to the sitting. So far as my recollection goes, I had never done such a thing before. I should like to add that throughout my acquaintance with Mrs Leonard, extending now nearly fourteen years, I have always found her scrupulously honest in revealing any possible source of normal knowledge concerning matters referred to at a sitting.

I should like to point out that the whole consecution of ideas shown in these passages from my sitting of 4 October 1921, passing from allusions to Henry Jackson to the suddenly interpolated book-test with its references to my father's connexion with Greek tragedy, and then back again to Henry Jackson and the name Page (very possibly helped to emerge by the immediately preceding allusions to the page of a book) is a consecution of ideas perfectly natural to the supposed communicator. I think this whole question of association of ideas is interesting and important, because what we seem to get in these trance sittings is not a clear stream of communication from any one source, but evidence of modifications and deflexions of Mrs Leonard's mind having their origin in the influence of other minds, possibly discarnate. Apart from all physical characteristics a man's personality is chiefly expressed in the associations of ideas

which constitute his memory, and his reaction to any new idea or image presented to him will be founded on these pre-existing associations. At the sitting of 4 October 1921 we are given what purport to be a particular communicator's reactions to two new ideas, (1) the recent death of an old friend, (2) the passage from the *Strand Magazine* with its allusion to Greek tragedy, and these two ideas are linked together and combined with other ideas in a way that would be natural to a mind that was built upon that communicator's memories.

(b)

As another example of a book-test which affords evidence of an association of ideas more natural to the supposed communicator than to Mrs Leonard, I will quote a passage from a sitting I had on 21 November 1919. The books on this occasion were chosen not from my own house, but from the house of Lady Troubridge, who was acting as recorder at the sitting. The books were said to be in some shelves which, as Fedra knew, had been specially reserved for test purposes; the position of these shelves in relation to the door of the room was correctly given, and the height of the test-book from the floor, as indicated by Fedra, was found to correspond with the uppermost shelf.

The first half of the test, which is confused and in part a failure, I omit. Fedra continued as follows:

FEDRA. Now he wants to go to the fourth book from the left, the fourth from the end of the shelf, page six, two, sixty-two, also near the top of the page, about a quarter of the way down, suggests Arcadia to him, that'll be nice! And lower down on the same page he got the idea of drinking, something you could drink, or the act of drinking.

The fourth book from the left in the shelf in question, as verified by Lady Troubridge and myself, was *The Bomb* by Frank Harris. I quote the first eight out of twenty-seven lines on the sixty-second page: "Between the acts Elsie told me that she was enjoying it too, but she objected to Rosalind's dress. 'It wasn't decent,' she said, 'no nice woman would wear it.' And she scoffed at the idea that Orlando could take Rosalind for a boy, 'Unless he was a gump; no man would be so silly.' She did not like Jacques particularly and the Court in the forest seemed to her ridiculous."

It is easy to understand how references to a performance of Shakespeare's *As You Like It*, actually naming Rosalind, Orlando, Jacques and the Court in the forest, might remind such a man as my father of Arcadia. Arcadia, as witness amongst others Sir Philip Sidney, is *par excellence* the literary name for the country of

simple, sylvan life, as portrayed in *As You Like It*. Turning the pages, for example, of Mr. J. B. Priestley's essay on the clown Touchstone in his *English Comic Characters* I came on the sentence: "Romance, however, having enticed him [Touchstone] into her own green Arcadia. . . ."

On the other hand, the association of Arcadia with *As You Like It* is not one which seems to me likely to arise in the mind of Mrs Leonard. I questioned her as to her knowledge of the play, and she told me that she had read it "probably many times over" as a small child, but had never read it since and had never seen it acted. She does not now care for Shakespeare.

(c)

I will now give another example of a book-test in which there is some evidence as to the time at which knowledge of the books was obtained. The sitter on this occasion was the Rev. W. S. Irving, and the sitting took place on Thursday, 20 September 1928, the notetaker being Mr N. C. Fonnereau. Mr Irving informs me that the sitting began at 2.25 p.m. and the book-tests were given in the earlier part of it, say, between 2.35 and 3.30 p.m. Two groups of tests were given on this occasion, the first from my own house, The Crown House, Newport, Essex, the second from Mr Irving's house in Gloucestershire. It is with the first group that I am now concerned, and it may conveniently be stated at this point that neither Mr Irving nor Mrs Leonard has ever visited my house, from which on one previous occasion a book-test presenting some features of interest had been obtained at a sitting taken by Mr Irving.

After stating that the books were "in Mrs Nelly's house" (Feda always refers to me as "Mrs Nelly"), she went on to give the following indications for the identifying of the room: "There are several things of Mrs Nelly's father in the room; there is a picture of her father, a very good one. There's also a picture of another man wearing a garment that her father always referred to by a very humorous name. . . . It's rather a funny mixture of things in this room. . . . There's a very funny contrast of things, things very new and very old, close together; and very serious things close to very unserious ones . . . things more like playthings; there *are* playthings. 'I think these things must be recent additions,' she says." The books were said to be "the first lot of books to the left as you come in at the door." There was also said to be "a smaller bookcase" in the room, and "a set of shelves smaller."

The room at The Crown House from which these tests are taken might at first sight be either the drawing-room or the study. On the whole the indications fit the drawing-room best, especially the reference to the "smaller bookcase." The three bookcases in the

study are all rather large and all about the same size. In the drawing-room the largest bookcase—4 feet square—is to the left of the door on entering ; there is a smaller case, $3\frac{1}{2}$ by 3 feet, opposite the door, and to the right a still smaller one, $3\frac{1}{2}$ feet by 1 foot wide. I did, however, examine the books which would seem to be indicated on the assumption that the test had been taken from the study, and found nothing relevant. There are several things in the drawing-room which belonged originally to my parents, *e.g.* a Persian carpet, a Sheraton card-table, a clock, etc. There is not usually a portrait of my father in the drawing-room (nor in the study, except some photographs put away in a cupboard). But on Thursday morning, 20 September 1928, I was looking through some of the notes of my early Leonard sittings with a view to the paper I had already begun to write, and I came on a description of my father which reminded me of a large framed “ portrait-photograph ” I have of him, which was taken many years ago by Mrs Frederic Myers. It is considered by people who knew him one of the best portraits of my father in existence. This photograph was stored at the moment in my dressing-room, and wishing to revive my recollection of it, I fetched it down and stood it beside me on the drawing-room sofa. As near as I can remember it was there from about 11 a.m. till a little before 1 o’clock. After that it was moved into the study.

The reference to the picture of another man is not clear. The allusion to a garment my father called by a humorous name must, I think, refer to the fact that my father used in domestic circles to refer to a very old smoking-coat he wore at home as his “ robe of state.” A reference to this was made spontaneously at one of my own Leonard sittings some years before, and was acknowledged by me at the time as a very good evidential point. But I cannot connect an allusion to this with any “ picture of another man ” either in the drawing-room or the study. In the bureau drawer in the drawing-room there was at the time of Mr Irving’s sitting a snapshot I took of my father in his garden, wearing just the sort of old coat he called a “ robe of state,” but if allusion was intended to this photograph, there is some confusion ; it is not a picture “ of another man,” but of the same man, namely, my father.

The allusion to “ a funny mixture of things ” in the room, “ things serious and unserious, playthings,” is, I think, clear. Immediately next to the case containing the test-books is a large oak “ bible-box.” This bible-box was never in my father’s possession, but it has been in the family for a long time, and came into my possession about a year ago. It now contains some of my little girl’s toys and books. Both the box and its contents are recent additions to the room.

I turn now to the test itself. The first book chosen was the

second from the left in the lowest shelf, p. 95. On this page it was said "there is a description of a place you and I [Mr and Mrs Irving] knew, were in; an old-fashioned place, old-world, a village. There's something given in the description which will point it for you, place it. There's some characteristic there that is a strong characteristic of a place we knew together. . . . At the very beginning of this book there were conditions referred to that would interest Mrs Nelly's father, conditions he was interested in, when on earth." On p. 91 of the same book there is said to be something descriptive of Mrs Irving, a rather personal characteristic.

The second book from the left in the lowest shelf of the bookcase to the left of the door was a volume of Charles Kingsley's *Poems* (Macmillan, 1872); pp. 91 and 95 are part of the text of a play called "A Saint's Tragedy," which deals with the story of Elizabeth of Bohemia. As the tests from this book were personal to Mr Irving I sent the book to him for verification. He writes:

"On the page numbered 95 I found,

Monks . . . we poor religious
Are bound to obey God's ordinance and submit
Unto the powers that be, who have forbidden
All men, alas! to give you food and shelter. . . .

ELIZ. I'll go.

The above scene is described on p. 94 as 'A street . . . at the door of a convent. Monks in the porch.'

"About the year 1902, my wife and I, being very interested in monastic life, and hearing that there was a monastery of secluded monks at Broadway in Worcestershire, cycled there and asked to be allowed to look over the place. A foreign-looking monk who opened the door could not speak English; he tried to send us away. We would not go, as we had come a long way to see the place, so at last he admitted us. Presently angry and threatening voices were heard, and a red-faced Irish priest rushed in shouting, 'Get out, ladies are not allowed in the monastery.' He would not listen to our explanations, so we had to go. Broadway is an old-world village, an old-fashioned rural spot, as described. I still have a photograph of the monastery chapel, obtained about the same time.

"On the page numbered 91, I found:

Your smiles are worth gold pieces.

My wife's gay smile was one of her greatest attractions. In a letter which I still have, written to me by a parishioner just after my wife's death, is the following: 'Her happy face and bright smile always seem to do one good to see.'"¹

¹ I have seen this letter.—H. S.

With regard to the statement that right at the beginning of the book there were conditions referred to that would interest my father, I could not find anything that seemed altogether satisfactory. On the title-page of the book appears the name "Andromeda," the title of one of the principal poems, and the two parts of the "Proem" to "A Saint's Tragedy," the first poem in the book, are called respectively "Epimetheus" and "Prometheus." All these names would have associations interesting to a classical scholar, but the mere occurrence of the names hardly seems to fulfil the requirements of the test.

I now turn again to the record of the sitting :

FEDA. She says, "on the outside of a book close to this one [the Kingsley *Poems*] is a name of her's, but she doesn't think it's Dora [Mrs Irving's Christian name], but another name. It's on the outside. . . . Wait a minute ! She got a name like Watson, Watson, Watson, on the outside of a book close here." Then she says, "It's an ordinary name, but I picked it up rather personally, because it fitted in, was a name of some one I knew rather well here on the earth. . . . Watson."

The book standing immediately next to the test-book and three other books in the same shelf were volumes of poetry by William Watson, and in each case the name Watson appeared on the back of the book. I am informed by Mr Irving that Watson was the name of a married sister of Mrs Irving's who died in the spring of this year. The name has never been given at his sittings before. I did not make Mr Irving's acquaintance until some years after his wife's death, and I know nothing whatever of Mrs Irving's family connexions except the irrelevant fact that her maiden name was Whitehead. It will be seen that the statements made in this part of the test seem to combine knowledge concerning the position and title of books at my house (possibly derivable from my mind) with knowledge of a personal link existing between the name Watson and Mrs Irving, not derivable from me, nor, so far as can be ascertained, normally possessed by the medium.

Upon this point Mr Irving tells me that Mrs Watson had never visited him at his present home, nor were he and Mrs Irving in the habit of discussing family matters with parishioners, so that the name would not have been easily learnt by local enquiries. After the allusion to Watson, the record of Mr Irving's sitting continues thus :

FEDA. "Now," she says, "go along this shelf, the seventh book from the left." When she touched this book she wanted to say 'How are the mighty fallen ! How are the mighty fallen !' That's

what she kept saying. . . . She says that book gave her such a strong feeling on it that she wants to repeat these words. On the book, something on the book. She says, 'Mrs Nelly will probably understand that, will, indeed, understand that before you do.' "

In the early part of this book, which proved to be *The Thoughts of Marcus Aurelius* (The World's Classics), there were said to be "geographical comparisons," "plans and comparisons of distances," and on p. 53 a reference to "rivers or streams, running water" and also to a special anniversary connected with Mr and Mrs Irving. This part of the test is not satisfactory. The only point which seems worth noting here is that on p. 52, which touches p. 53 when the book is closed, are the words "all flow from the same fount." Feda frequently alleges the liability of communicators to confuse two adjacent pages. If we include the two pages adjacent to the test-page in our range, we thereby multiply by three the possibility of chance-coincidence, and a success of this kind has no evidential value unless, as in this case, there is a fairly close correspondence between the communicator's statement and the printed matter. Even so, the point should in my opinion be reckoned doubtful.

The words "How are the mighty fallen," which the communicator emphasises in connexion with this same book, might easily occur to the mind of a scholar or a student of the Roman Empire in allusion to the fact that the death of Marcus Aurelius closes an important and splendid epoch in the world's history; he was the last of the great pagan philosophers and also the last of the great Roman emperors. I will give two quotations from Frederic Myers and Gibbon to illustrate my meaning: "And, finally, the most subtle and attractive of living historians has closed his strange portrait gallery with this majestic figure [M. Aurelius], accounting that the sun of Christianity was not fully risen till it had seen the paling of the old world's last and purest star." ¹ And, "It is the design of this, and of the two succeeding chapters, to describe the prosperous condition of their [the Roman] empire; and afterwards, from the death of Marcus Antoninus, to deduce the most important circumstances of its decline and fall." ²

It is worth noting that throughout this group of tests my father, to whom the circumstances of Marcus Aurelius's life and times were a matter of familiar knowledge, appears to be associated with Mrs Irving, and that in reference to this particular test it is said that "Mrs Nelly will probably understand that, will indeed understand it before you do." The interpretation of the words "How are the mighty fallen," which I have given above, did in fact occur

¹ F. W. H. Myers, *Classical Essays*, "Marcus Aurelius Antoninus," p. 177.

² Gibbon, *Decline and Fall of the Roman Empire*, ch. i.

at once to my mind. But this point, again, is not a strong one; it suffers from the defect, all too common in book-tests, that the meaning is not sufficiently obvious and on the surface; too much interpretation is needed.

Omitting a reference to something on the outside of another book in the same shelf, the interpretation of which is doubtful, I turn to the last test in the series, which is said to have been given by A. W. V. with Mrs Irving's assistance.

FEDA. He went on to the shelf above the one Dora was on; . . . he took the third book from the right and he took page seven of that book; and on page seven was something very characteristic of the Image. [Feda then explains to Mr Irving that "the Image" is my little girl and continues.] There's something on that page that's very much to do with the Image, that refers to the Image, and he wanted it called the Image because of what's on that page.

The third book from the right in the shelf indicated was Gilchrist's *Life and Work of William Blake* (1880), vol. i. Page 7 is part of a chapter concerning Blake's childhood and the word "childhood" appears as a chapter-heading at the top of the page. I will quote from the bottom of page 6: . . . "and then unsophisticate green field and hedge row opened on the [p. 7] child's delighted eyes. A mile or two further through the large and pleasant village of Camberwell . . . the sweet hill and sylvan wilds of rural Dulwich . . . the fertile verdant meads of Walton-upon-Thames; much of the way by lane and footpath. The beauty of those scenes in his youth was a lifelong reminiscence with Blake, and stored his mind with lifelong pastoral images."

My daughter, just three years old,¹ is called Imogen. At the first sitting I had with Mrs Leonard after her birth I was told by Feda that my father intended to refer to her as "the Image," and she has habitually been called by that name at my subsequent sittings. Whether Mrs Leonard has, or had at the time "the Image" was first referred to, any normal knowledge of my daughter's name, I cannot say, nor is the question material here. I take the reference in the test to be to the country scenes and "the child's delighted eyes." We live in a country village with fields and woodland all about us, and the love of flowers, which is characteristic of most small children, is very marked in Imogen. She usually notices any new flower she sees on her walks and asks its name. It will be observed that at the end of the paragraph from Gilchrist quoted above the word "images" occurs, making a definite link with the child's name. This I take to be the meaning of the words "he wanted it called the Image, because of what's on that page."

¹ This section of the paper was written in January 1929.

THOUGHTS ON MR SALTMARSH'S REPORT ON A SERIES OF SITTINGS.

By the REV. W. S. IRVING.

IN venturing to put down a few thoughts that have been suggested to me by Mr Saltmarsh's comprehensive and stimulating "Report on the Investigation of Some Sittings with Mrs Warren Elliott" (*Proceedings*, xxxix: 47 ff.), I am emboldened to do so, not only because he has on several occasions referred to sittings taken by me, but also because it has been possible for me to compare his results with a large number of my own sittings mainly taken with Mrs Osborne Leonard.

On page 91 of his paper, Mr Saltmarsh says with regard to post-mortem statements concerning the communicator, "It is, of course, impossible to say whether they are veridical or not." If, however, we find that statements of a somewhat unusual nature are made by two or more Controls, through different mediums, to independent sitters, such statements may, I think, at least be said to be worthy of consideration, and to have some small amount of evidential value. And there would seem to be a correspondence of this kind (with material given to me at a Leonard sitting) on page 86, as follows: "Him says him getting used to him's mind once more as if him was young. It was something here in life that sort of robbed him of something, and now him sort of learning . . . learning to use him's mind . . . seemed as if him was child again and him couldn't develop—growth ceased—and now him got all that to make up." At a sitting with Mrs Leonard on 30 April 1925, my communicator brought with her to communicate a man who claimed to have been someone I used to know. I was able to identify him quite definitely as a former Vicar of mine who had also been Chaplain to the County Lunatic Asylum. At another sitting on 31 July 1925, my communicator referred to him again in Personal Control, and it is what she said then that makes the correspondence with the extract from the "X Case": ". . . He's working very hard. (What's he doing?) Working for the poor demented. . . . There aren't any on our side—those still on your side. . . . People aren't demented on our side, but there are people who *can't use their minds* because *they've got to*

learn to use them. People who've been, what you call, 'out of their minds' find difficulty at first in using them, but it comes all right in a little while."

On page 124 Mr Saltmarsh suggests that "Feda does receive a good deal more of her information by means of symbols than appears on the surface," and he illustrates this from an incident at one of my sittings in which Feda says "the test is mackintoshes and umbrellas test is." To anyone reading the script I can see that this may appear to be the interpretation of a symbol given by the communicator. This was not, however, how it appeared at the sitting. I took it to be simply a joke of Feda's—promptly squashed by the communicator in the words, "No . . . water-tight." And on turning to my index of those sittings I find the incident classed among "Humorous sayings of Feda." Of course, my interpretation may not be the right one, but there is not much use of symbolism in my Leonard sittings, and when symbols are used care seems to be taken by Feda to let us know. There was such an instance in a recent sitting of mine with this medium, when it was desired to give the name of a lady who had passed over, and it had not been possible to give it. Feda said, "There's a name connected with this lady I can only give symbolically. Very much connected with fires and burning, like as if a gentleman were called Mr Cinder, do you see? . . . Dora thinks of this lady in connection with tea and cakes?" By a process of elimination, I managed at last to identify the person to my own satisfaction, as a Mrs *Baker*. It is seldom, however, that instances of this kind have occurred at my sittings. Humour by Feda is frequent, especially at good sittings, and tends to lighten the strain, and I think improve the material.

On page 126, Mr Saltmarsh mentions the "great difficulty in getting through proper names." Nevertheless, quite a number of names have been given to me on occasion, through Feda, spontaneously, which I have been able to recognise, among them being Jesson, Hadow [notetaker had Haddo], Sharp, Watson, and Miller. And Topsy once described a communicator, said to be Mr Charlie [Colonel Baddeley], as saying something about a Mrs Murbing. "This gentleman says that he's met Mrs, like, Murbing, she's been very good to him, and he likes her very much."

When one remembers Topsy's way of putting M's or N's on to words, it is not so bad an attempt at my wife's surname. The above quotation is from a sitting on 4 February 1925 (Recorder: Dr Woolley), and was taken nearly a year before the date on which Mrs Elliott learned my name normally.

On page 107, Mr Saltmarsh alludes to "the numerous references to Miss Newton's recent thoughts or actions obtained" by me, through Mrs Leonard, and published in *Proceedings*. These he

seems inclined to put down to telepathy or clairvoyance, acting sporadically. It should be noticed, however, that it is by no means the case that all these "Impression" tests were known to Miss Newton. Even in those taken from the S.P.R. Rooms, there are a number concerning things of which Miss Newton knew nothing. Some of the best of these have not, as yet, been published, but a few have, and the "cupid" case on page 197 of volume xxxvi is a good example of what I mean. In this case my communicator said, at a sitting with Mrs Leonard on 23 September 1924, that at the "Psychical 'Searchers'" she got the "word 'cupid' coming to her strongly." She thought "they must have that word written very largely on something that has been discussed, or written there, lately." Miss Newton's note on the above was: "Mrs Ham, the housekeeper at 31 Tavistock Square, showed me to-day (September 29, 1924) a white china cupid which had been given her by our tenant's housekeeper, when leaving, some time during the first half of August. Neither Miss Horsell, Mr Dingwall, nor I knew anything about this until Mrs Ham told me . . . when I enquired on September 29. . . ."

In the same volume, some on pages 191-2, is a short series of these tests that seem to have been found at the Rooms of the London Spiritualist Alliance, and on pages 242-5 a longer series from a house unknown to Miss Newton, and not well known to me. Further tests of this nature, not yet published, come from a house that is not known to me at all—though in this last case I understand that the occupier has herself had sittings with Mrs Leonard. These are from the house of the lady referred to in *Proceedings*, xxxix, on pages 141-2. The telepathy, therefore, if telepathy it be, can hardly be of a simple nature, directly from mind to mind, nor would any direct form of clairvoyance meet the whole of these cases.

Concerning Apparent Discontinuity in Communications.

On page 132, Mr Saltmarsh speaks of the "disjointedness and lack of continuity in the communications. . . . The 'spirits' do not tell us what we should expect from them. The salient facts of their history are frequently omitted, and trivial details given." That this statement would be incorrect as regards Leonard sittings at least, I venture to submit. To begin with, the memory of the communicator, through Fedra, is extraordinary. Dora, for instance, has alluded (practically without repetition) to most of the important events of her life. At my last sitting she went back about 32 years to find her subject for an interesting reminiscence. And there does not seem to be overlapping in material given through different mediums concerning the past.

But perhaps Mr Saltmarsh is thinking of the apparently disconnected way in which incidents are brought forward at single sittings. I mean incidents that seem to have no line of thought connecting them with each other, but to have been given haphazard. As far back as 20 September 1921, my communicator referred to this, spontaneously, as follows: "[Feda]. . . Sometimes when you're tracing a thing through your notes, do you find she speaks of one thing, goes on to another, and links up to earlier one? She says, 'I give Fact no. 1, Fact no. 2, Fact no. 3, and Fact no. 3 is a distinct link up with Fact no. 1. I'm not always able to make Feda understand that.'" Bearing this in mind, I have from time to time been able to trace the line of thought underlying Leonard material and to see the connexion that otherwise might have escaped notice. This can be easily illustrated from recent sittings fresh in my memory. On 23 April 1929, at a Leonard sitting, subject no. 2 was a reminiscence of an expedition taken many years ago, with my wife, to look at some "big lumps of stone—very old," so that we said, "Strange how long these must have been here." Subject no. 3 had no connexion with the above. Subject no. 4 was a careful description of the *design* on a round or oval brass article belonging to my wife, which I still use but in a different way to the way in which she had used it. This I recognised as a good description of her brass afternoon tea-tray. At first sight there is no connection between the subject no. 2 and subject no. 4. Nevertheless, through the giving of the latter, I was able to place beyond doubt the identity of the former. The brass tea-tray I gave to my wife when we went to live at Leckhampton, and the lumps of stone represent "The Devil's Chimney" at Leckhampton, though the visit referred to was made, I think, many years before we went to live there.

Here is another example, from a sitting with Mrs Leonard on Thursday, 19 September 1929. This is an attempt to show knowledge of what I had just been doing on my holiday, and continued interest therein. Subject no. 1 describes my looking in a shop window. Subject no. 3 is a description of a tea-shop to which my wife used to go in Richmond, Yorkshire, with its surroundings carefully described. Subject no. 4 describes a present that I gave my wife for her to use and enjoy, and the statement is made it will be brought into a book-test. This point is not definite enough for certainty, as it does not state what the present is. I take it to be another reference to the brass tea-tray mentioned above. Subjects nos. 5-11 are not connected with the above. Subject no. 12 describes a room in my sister's new house at Richmond, Yorkshire, in detail, and "something circular shape, in brass," is referred to. These things seem disconnected—they are not. In the room described in subject 12 I decided to buy my sister a brass tea-tray like

the one I bought my wife long ago. Subject no. 1 describes my looking in London for trays in shop windows. Subject no. 3 shows knowledge of where the place is where I have been staying—Richmond—and describes a shop, long since forgotten by me, where my wife used to go for tea and cakes. This was identified by my sister. I should add that the shop's description is very unusual, up "a narrow turning" . . . "upstairs" . . . "old-fashioned place" . . . "steps inside dark," etc. That behind the scenes, as it were, there is an acute intelligence, with considerable knowledge of the past lives of myself and my wife, with the power, also, to plan ahead, and arrange beforehand, the programme for the next sitting, is, at least, a simple explanation of the phenomena. For an example of this latter point, the power to plan ahead, I next give another extract from my latest sittings of all.

A few days before leaving home for my summer holiday, on 2 September 1929, I had read in the paper by Mr Saltmarsh the Warren Elliott description, in an Absent Sitter sitting, of my sister in church, wearing a "shawl, buff with red check," and a hat "very like a man's tall hat." I had thus been reminded of a plaid shawl.

On Monday, 16 September 1929, at a sitting with Mrs Leonard, I noticed that she was wearing a lighter coloured shawl than the one that she usually wears. In view of what was said at the sitting, I asked to be allowed to look at the shawl afterwards, and found that, in certain respects, it was rather like the description of the supposed shawl belonging to my sister. It was part plaid, grey and buff, near the edge, though the greater part of the shawl was grey. This is important as showing what my subconscious mind may have grasped, though the light was none too good, and consciously, I noticed nothing of this before the sitting. The effect on my communicator is given below :

Extract from Sitting with Mrs Leonard, 16 September 1929.
Sitter : Rev. W. S. Irving, taking his own notes, as the note-taker left at the end of Feda's Control.

D. G. I. . . Will! I've got a shawl on. [Control had been fidgetting with the shawl, feeling it with her fingers.] Reminds me of something—of a shawl I had. Not this colour—pale grey. (Describe it.) Greyish, got something else on it, too, another shade or colour in it too, I think. One I used while here—right towards the end of my [life]. (You ought to remember more than that.) Will, it's because there are other things. . . . Will! Don't you know, I've a special reason for reminding you of that shawl? (That shawl, or a shawl?) That very same shawl. You've reminded me of it. Not that same shawl—that's impossible—something you

were thinking of lately, quite lately, a few days ago. I didn't see the shawl—that shawl. That shawl was linked with my passing, and something happened about the shawl, when I was here. Oh, dear! Oh! That's the very thing I wanted to say. (Next time.) Rather important. A bit worrying about the shawl. Rather important shawl—bringing it for me. (That's right.) Getting it specially for me. Fetching it for me. Putting it round me. Oh, dear! It was this one [Mrs Leonard's] reminded me, but it wasn't like it. Feeling it reminded me. Connected with a special time, the shawl was. Do you remember about an anniversary connected with it? (No.) But I do,—an anniversary. I look back on that shawl as something rather important. Bringing it to me at an important time. Fetching it, putting. Going away. Will! I'm losing (Try and bring it in next). . . . [Sitting ends with farewells.]

My wife had a beautiful Egyptian scarf or shawl, which she valued greatly and used up to the time of her death. It was bought by her father, in 1911. After the sitting, I met one of my wife's sisters, Mrs Savy, and asked her to describe the shawl for me in writing. This was on the following Wednesday. I also wrote to her mother, Mrs Whitehead, and asked her to tell me anything she could remember about the shawl.

The following is the evidential part of Mrs Whitehead's reply (26 September 1929): "It is interesting to know that Dora has mentioned her Egyptian scarf. . . . It was amongst the things you allowed me to bring away. . . . after the funeral. . . . I remember Dickie [Mrs Savy] had this particular article. . . . Mr W. brought six from Egypt—one each for the five girls and one for me. . . . The colour of the scarf was cream bespangled with gold tinsel in an artistic design. [But see below.] Its width about 24 or 26 inches and length about 54 inches. . . . It was given her on her father's return from Egypt in the month of April 1911. . . . given them unassociated with any anniversary of birthdays, etc.

Mrs Savy wrote (8 October 1929): "The following is a description to the best of my recollection of the Egyptian scarf or shawl which belonged to my sister Dora and which was sent to me amongst other personal things of hers, shortly after her death. A shawl about a yard and a half to two yards long, and about half a yard wide, made of net of a dull white, or more exactly greyish colour, the net being interwoven with a design of silver-coloured tinsel."

My own recollection—and I saw the shawl much more often than the others did—is that it was of pale mauve, or pale grey, heavily weighted with silver. I do not know of any anniversary connected with it, save that it marked Mr Whitehead's return after many

months' absence. The following April, however, was a very important time for us.

Three days later, *i.e.* on 19 September 1929, I had another sitting with Mrs Leonard. It must be noted that I had not been home since leaving there on 2 September 1929, nor had I heard from anyone at my Vicarage.

Extract from a Sitting with Mrs Leonard on 19 September 1929.
Sitter: Rev. W. S. Irving. Recorder: Mr N. C. Fommereau.

FEDA. Now she want to go home to some books. (Right.) Mr Bill! She's going to an old place. (Good.) The shelves that she's been to before. What's that, Dora? . . . Now, Mr Bill! She's taking one—two. She's taking, Mr Bill, the second shelf—second. That's right! Second. And she's going to the third book from the left, and she's looking at the page sixty-three, and on it you'll find a reference to something she was talking about at the end of the last sitting here. Do you remember what I was talking about at my Personal Control right at the end? (Yes.) There's a reference to it there—words that would describe it. Page sixty-three. (About a shawl?) Yes, she says, yes. And, on forty-one, forty-one, of the same book, there's another reference connected with the same thing.

Directly I got home, I asked my housekeeper to go with me to my drawing-room, and in her presence I counted the books and found those from which the tests were taken, *i.e.* the third and fifth books from the left, second shelf up. The titles I wrote on a piece of paper, which I gave to my housekeeper to keep until a friend could come in and question her on the matter, and properly witness the books. The third book from the left, second shelf up, was *The Reproach of Islam*, by W. H. T. Gairdner (London 1910). On page 63 I found, "Whence came it? . . . the Islam of Mohammed." These words were the heading of the page, and on the second line, not counting the heading. On page 41 I found: "Whence came it? . . . the Arabian nation. . . ." Also, between pages 40 and 41 was a full-page picture entitled "Crossing the Desert." This picture shows some Arabs crossing the desert on foot, and these appear to be men. There is also an Arab on a donkey, and two Arabian women riding on a camel. The last is particularly prominent in the picture. Behind the women, on the camel, is a large bundle of rich merchandise,—it looks not unlike a bundle of shawls, or stuff of that kind.

It should be noted that an attempt was made, apparently, to give, through Mrs Mason, a book-test from this book on 30 July 1929, so that this book was among some that I had recently handled. On the other hand, the tests were then said to be on page 13 or 18,

not near the present test-pages, though I may have looked further. And, of course, the subject of shawls had not then become to me a subject of special interest. I take it that my communicator is here trying to tell us that *feeling* Mrs Leonard's new shawl on her shoulders, while controlling, reminded her : (1) Of her own Egyptian shawl ; (2) That this is not the first time that she has recently been reminded of it. I do not recall myself having thought of my wife's shawl for months—if not years.

And then, again, there is another thing that would seem to be evidence of continuity of thought : the occasional giving of additional material said to have been forgotten at a former sitting. Here is an instance. At a Leonard sitting on 16 September 1929, my communicator mentioned a number of minor trivial things that she said she had recently seen or been reminded of, at home. On 19 September she again gave, through Fedá, a few more. Later, in the Personal Control on that day, she said as follows.

D. G. I. . . . Will ! Can you remember, were you thinking of anything in connection with coins and pieces of money ? I got such a strong feeling of coins *before the last sitting*. *I was going to mention it then, and forgot*. In case you've forgotten, the coins should have reminded you of me. Coins I knew when I was here, and touched—handled. It's the only reason they're important. I didn't want you to think I meant some ancient and valuable coins. When I passed over I loved them—left them—behind. . . .

I may just say, in passing, that years ago I used to collect coins, old and foreign. Shortly before leaving home for this holiday, I came across, in a drawer, a chain bangle of my wife's to which she had attached two coins annexed from my collection. They are not of any value, a Jubilee Victoria token, and a Swiss coin dated 1880. The interesting point is, of course, that this little bit of evidence would seem to have been part of the former sitting's programme that was omitted through forgetfulness at the time. It will be noted that I had not been home between the two sittings.

Turning now to my Warren Elliott sittings, I find a similar statement concerning the difficulty of getting things through in connected order, to the one quoted above given through Fedá. It was given on 19 November 1926, and runs as follows :

TOPSY. She'm says that she'm always working hard. After one talk she starts working very hard to collect her power, and lot of sort of test things, to be able to shoot out next talk. Gets quite a collection, and then chooses power according to what she can get through. That's why things sometimes are so scattered, does you understand ?

Nevertheless, the evidence for this, through this particular medium, would not, I think, be strong were it not supplemented by other things such as cross-correspondence with Leonard material. Here, however, are two instances, of varying types, both apparently concerning the same people, Mr and Mrs Y., who at one time figured somewhat largely in tests given to me through Mrs Leonard (see *Proceedings*, xxxvi. 239 ff.).

Extracts from a Sitting with Mrs Warren Elliott on 23 September 1925. Sitter: Rev. W. S. Irving. Recorder: Mrs Dingwall.

TOPSY. Doesn't you like potatoes? Then why does she show Topsy potatoes and laughs at you? [This was item no. 33. Items no. 34-38 concerned matters apparently having no connection with the Y's. Item no. 39, however, runs as follows]:

TOPSY. She keeps on saying she likes it better without tablecloths. Do you know what she means? (No.) You ask at home, see what's meant.

After the sitting I was going to the Y's to dinner. On one of the last occasions that I had dined with them there had been no vegetables but potatoes, and this had been the subject of the "shortage of vegetables" test (Leonard), published in *Proceedings*, xxxvi. 244, and corresponding note on page 249. Further, the Y's are almost the only people I know, just now, who do not use tablecloths at dinner—a custom that my wife, in life, thought good—though we did not adopt it ourselves.

The other instance has, unfortunately, to be given at some length as there is much confusion and extraneous matter, which, if omitted, would make the case seem stronger than it is:

Extracts from a Sitting with Mrs Warren Elliott on 4 May 1927. Sitter: Rev. W. S. Irving. Recorder: Miss Horsell.

TOPSY. [Item 4.]... There's a lady crossed over where you is, fairly young, got young children, doesn't know how long ago, but she looking after lady crossed over rather young. Me thinks you know her. . . .

[Item 13.]... You know that lady you spoke about crossed over? She's like downstairs from the hill, low down. . . .

[Item 16.]... Have you got garden and trees and garden? . . . No! It's not those trees—it's somewhere that way where people buried. She says, There's five new places below her, do you understand? (I've got an idea.) Well! Something sad happened, and one of those places somebody crossed over, and sad circumstances. Me think there's two people buried together. Me doesn't think it

anything important your life, or her life, just to show you she knows what's going on there. Looks like it was somebody young, and somebody fairly old. . . .

[Item 21.] What fashionable crowd you get your Church now. Something she amused about. It's new family, or new one, or two families, look like older people, and some young people. . . .

[Item 30.] You does understand how to comfort and help more when you come face to face with sorrow, and she particularly thinking of people connected with that lady told you about at the beginning. . . .

With regard to the above scattered items :

[4.] About a year before this sitting, Mrs Y. had been taken suddenly ill in London. She was brought to her father-in-law's house about two miles from my Vicarage, and died soon after, aged 34. She left one child, a small boy.

[13.] Mrs Y. was buried in my churchyard at Oxenhall. My Church is on a hill, and the ground slopes down slightly from the main door of the Church to the boundary wall. Mrs Y's grave is nearest the wall.

[16.] Mrs Y's grave is next to that of my wife. She is the only young married woman, according to our Registers, that has been buried in this churchyard in very recent years. Unfortunately this point is weakened in value because on 19 June 1926 a man who had been killed by a bull, and his little boy, were buried together ; and on 2 March 1926 and 2 June 1926 a woman and her granddaughter were buried near together. They had, however, no strong personal touch with me beyond the fact that they were parishioners well known. The Y's are personal friends.

[21.] On Easter day, 17 April 1927, *only 17 days before this sitting*, Mr Y. attended at the 8.30 a.m. service at Oxenhall with a lady, and at 11.0 a.m., on the same day, he brought a small party, including his father, a lady, and, I think, his small boy, to my other Church at Pauntley. As my own parishioners are mainly small farmers and labourers, it is of interest when visitors from the big houses in the neighbourhood come to our church.

REVIEW.

UPTON SINCLAIR, *Mental Radio: Does it Work, and How?* With an Introduction by Professor William McDougall. London: T. Werner Laurie, 1930. 8vo, pp. 207, 281 ill. Price 8s. 6d. net.

This book by the well-known American novelist and writer on social subjects, Mr Upton Sinclair, is an important one. By *Mental Radio* is meant telepathic communication between two minds, and we have here a valuable addition to our evidence concerning experimental telepathy. It contains a careful account of experiments carried out in 1928-1929 in which Mrs Upton Sinclair was the percipient, and other persons, chiefly her husband, the agent. Earlier experiences of Mrs Sinclair, spontaneous and other, are described, but the main object of the book is to describe and discuss experiments in the transference of rough drawings looked at by the agent and which, without seeing them, the percipient attempted to imitate, often with great success. The experiments resemble some that have been described in the *Proceedings* of our Society (especially the earlier volumes) and in *Phantasms of the Living*. Out of 290 such experiments for which 38 drawings were supplied by Mr Sinclair's secretary and 252 by himself he counts 23 per cent. as successful, 53 per cent. as partially successful, and 24 per cent. as failures. No doubt individual opinions will differ somewhat as to where exactly the line should be drawn between successes, partial successes and failures, but we are given in the book a large proportion of the drawings—agent's and percipient's—in the experiments classed as successful, and a considerable proportion of those classed as partially successful, so that readers can judge for themselves of the author's standard; and they will not, I think, after reading the book, or even the first two chapters, feel any doubt that Mrs Sinclair's success is far beyond what chance could produce.

To the experiments in telepathy is added a chapter (Chapter IX) on experiments in clairvoyance rather of the book-test order. These are interesting and will, it is to be hoped, be continued. But they do not appear to have been carried out, or at least are not described, with the same care and minuteness as the telepathic ones.

Mrs Sinclair's experiments were all done in full consciousness, without trance, and she is thus able to remember and give a careful

analysis of the mental process, so far as she is aware of it, through which she obtains the telepathic impressions, and of the technique she has found useful in getting into the right state of mind. This she does in Chapter XXI. An account of this kind by a person so much interested, and so well able to express herself, greatly adds to the value of the book, and will be useful to other experimenters. Mrs Sinclair, however, would not maintain that there is only one possible method, or that the kind of deliberate concentration she finds useful is always necessary. She herself sometimes receives with no deliberate effort both waking and dreaming impressions, some of which are described in the book, of the thoughts and feelings and doings of others, especially of her husband. Moreover, we cannot assume that the same technique would always be the best for all percipients.

Mrs Sinclair does not in this chapter about her method tell us much about different individual impressions and the way they developed in her mind, though she tells us something. Various things may, however, be deduced from experiments illustrated and discussed in the book. It seems clear, for instance, that, as has been previously observed in similar series of experiments (*e.g.* Mr Guthrie's, *Proceedings*, vol. ii), that it is sometimes the form of the agent's drawing that influences the percipient and sometimes the idea behind it. When for a letter *y* in current writing the percipient produced a capital *Y* as in print, it is clear that what was transmitted was the idea or the name of that letter, not its shape (p. 79, no. 33). And again when Mr Sinclair draws (very roughly, for neither agents nor percipient are artists) a volcano with thick black smoke issuing from the crater, and Mrs Sinclair draws an absurdly similar picture, but calls it a black beetle—the black smoke representing the body of an insect and the sides of the mountain its antennae—it is clearly the form of the object, not its name, that is transferred (p. 70, no. 25). Sometimes other telepathic ideas reinforcing memory associations seem to intrude themselves into the percipient's impression. Mr Sinclair calls attention to a case of this sort on p. 42 (no. 15). He drew a football "neatly laced up." This was reproduced as a "belly-band on calf." Except that the outline of the percipient's drawing had a sort of projection at one point representing apparently the calf's head, the two drawings are distinctly like each other. Whence the idea of the animal? While Mrs Sinclair was trying to get the required impression it appears that her husband was reading "a treatise on the feeding of cows," which may have telepathically brought cows to the percipient's mind, and this with the drawing of the football may have revived for her the idea of "a calf sewed up like a football" which, as it happened, had been one familiar to her as a child.

In the particular experiments I have mentioned agent and percipient were no further apart than adjoining rooms, but as Mr Sinclair remarks (p. 11) telepathy "works as well over forty miles as over thirty feet." Mrs Sinclair did five experiments with a brother-in-law living forty miles off, as agent, which were all successes or partial successes (Chapter VII). These experiments were done on five different days at hours agreed on beforehand, the drawings being compared later. They may be called single experiments as distinguished from those in which Mr Sinclair was agent. It is to be regretted that we have not more such single experiments, but Mr Sinclair's usual plan was to prepare batches of six to twelve (on one occasion fourteen) drawings at a time, which were then placed beside Mrs Sinclair, each enclosed in an envelope or otherwise arranged so that she should not see them. She went through the whole batch one by one, holding each successively in her hand and drawing what she mentally "saw." The order in which she took them seems to have been haphazard and not known to Mr Sinclair.

This plan of working through batches just prepared by the agent is doubtless convenient, and a less severe tax on the agent's time than single experiments. But it adds complications in some ways. The agent presumably has the whole batch more or less in his mind while the percipient is trying to get impressions. She holds one of the batch in her hand, saying to herself, "I want the picture which is on this card, or paper, presented to my consciousness" (p. 174). But it sometimes happens that the impression received corresponds not to the drawing she is holding, but to one later in the series. Several examples of such anticipations are given in Chapters XIV and XV. It seems unlikely that they are in any real sense premonitions, for the agent, as already said, has the whole batch in his mind and any one might be transferred. A more difficult question is why when, under these circumstances, a correct impression is received, it is usually of the drawing held by the percipient? Is there clairvoyance supporting telepathy? A very peculiar case of this anticipation is no. 58, p. 101. Two drawings by a stranger to the percipient had been inserted in a batch of drawings by Mr Sinclair's secretary. The percipient failed to "get" the first of these, "seeing" instead a grotesque figure which she described in detail. The description corresponded quite remarkably well with the stranger's second drawing, which came seven drawings further on in the batch. What was the influence in Mrs Sinclair's mind connecting the stranger's two drawings?

The plan of working with batches of drawings has enabled Mrs Sinclair to make an observation agreeing with what Miss Jephson found in her experiments in clairvoyant card guessing. Her percipients, it will be remembered, "guessed" in batches of five, and

she found a distinct tendency to greater success at the beginning than at the end of a batch (*Proceedings*, xxxviii. 230 ff.). Mrs Sinclair in her chapter on technique says (p. 185), "I found that, in doing a series of several drawings, the percentage of successes was higher in the first three attempts. . . . This may have been due to the fact that the memory-pictures of these first three experiments now constituted a difficulty. So much attention had to be given to inhibiting these memory-pictures, and in deciding whether or not they were to be inhibited. Or it may be due to some other cause, such as fatigue or boredom."

I have said enough, I think, to show that these experiments will repay study, and I may add that Mr Sinclair is to be congratulated on making a book on so dry a subject, not only useful to students, but readable for the ordinary reader. For students I think the addition of a chronological list in tabular form of all the telepathic experiments in which drawings were used, with their divisions into batches, might be useful; but we are given so large a selection of them in full, and so much information about them, that it seems ungrateful to ask for more.

I will end with heartily endorsing the final sentence of Professor McDougall's interesting introduction. "Mr Sinclair's book," he says, "will amply justify itself if it shall lead a few of his readers to undertake carefully and critically experiments similar to those which he has so vividly described." And that this effect should be produced among the members of our Society is all the more to be desired, because they have sent in so little work of this sort for so many years.

E. M. S.

I. THE VISIT OF M. PASCAL FORTHUNY TO THE SOCIETY IN 1929.

By V. J. WOOLLEY.

THE mediumship of M. Pascal Forthuny has been a subject of interest to an increasing number of students of clairvoyance for some years. Since, however, most of the accounts of his powers have appeared in French it may be of interest if I give a short summary of his development by way of preface to the description of our London experiments.

On a few isolated occasions prior to 1920 M. Forthuny had spontaneous supernormal experiences. One in particular has been described by him : a vision of a coffin which caused him to return to Paris from a long distance in time to see his mother before her unexpected death.

In June 1919, however, he was overwhelmed by grief at the tragic death of his son as the result of a flying accident, and about a year later he began suddenly to produce automatic writing. These writings purported to be in part messages from his son and in part communications from a spirit guide. None of them, however, showed any knowledge of verifiable facts which were not known to M. Forthuny himself, and on Christmas Day, 1920, the writing abruptly ceased and has never since recurred. He claims, however, that this gift of automatic writing was replaced by a gift of clairvoyance, so that he is now able from time to time to discover facts relating to persons in whose company he is, these facts consisting of details regarding their past history, present surroundings and even their future.

This clairvoyance is said to be to some extent subject to his conscious control so that he can direct it towards assigned individuals, but it is most commonly demonstrated in large gatherings. Under these conditions M. Forthuny says that he is aware of the direction of the individual with whom his impressions have to do, but does not know whether he is one of those near him or further away. A number of descriptions of his successful demonstrations have appeared in the *Revue Métapsychique* and other French journals. Mr Besterman has also described ¹ a private sitting at M. Forthuny's

¹ *Proceedings*, xxxviii. 474.

house, and Mrs Brackenbury and I have had similar private sittings, and have also attended one of the public demonstrations which were held in a hall in Paris. In each case there seemed no doubt that M. Forthuny had a knowledge of circumstances which he could not have acquired normally, and the Council invited him to visit the Society during the course of 1929 in the hope that he would be equally successful in London.

During his visit we were able to arrange three meetings for those Members and Associates who wished to be present, thus reproducing as far as we were able the conditions of the public demonstrations which M. Forthuny is accustomed to give in Paris. On the other days of his visit he kindly agreed to give private sittings to those whom we introduced to him for the purpose. As sitters we selected for the most part those who had provided interesting material from visits to other mediums, our aim being to compare these previous results, of which we possessed the records, with whatever was produced during the present series.

For purposes of classification it is perhaps convenient to divide the records rather arbitrarily into three groups: A. The public gatherings; B. The private sittings; C. Psychometric experiments.

A. THE PUBLIC GATHERINGS.

Our aim in these meetings was to follow as closely as possible the procedure to which M. Forthuny was accustomed in Paris. This consists in his facing the audience, with the light behind him, and relating to the various persons concerned the impressions which he may receive about them and their affairs. All that was said was taken down by a notetaker engaged for the purpose, but we found an unexpected difficulty in securing the services of a French shorthand writer, and the detail and accuracy of the records suffered in consequence. A further difficulty lay in the fact that it is not always quite certain who is the person about whom the impressions are received. M. Forthuny is only able to locate his direction, and this sometimes rather vaguely, so that he is rather dependent on his impressions being claimed by the person whom they really concern. Experience shows that different people vary very greatly in the readiness with which they will claim an impression as relating to themselves, and there is reason to believe that some evidential matter was not recognised owing to the unwillingness of those concerned to claim it.¹

It is not easy under these conditions to form an estimate of the

¹ Thus, at one of the public meetings, M. Forthuny mentioned two Christian names to a member of the audience who failed to recognise them, but I learnt afterwards that these names were the names of the father and uncle of two ladies sitting immediately in front of the person addressed, and that the father had purported to communicate at various sittings with other mediums.

degree of success attained in the demonstration as a whole. The method suggested by Saltmarsh and Soal¹ for estimating the merit of mediumistic communications does not seem wholly applicable to a case like this in which purported communications from the dead are hardly concerned.

On account of these difficulties it seems impossible to do more than to give a few quotations from the records of the sittings, together with the comments thereon of the people to whom the communications were made, and I have deliberately chosen those communications which were admitted to be in part correct. I must leave it to the reader to decide how far these correct statements may be due to chance and how far such an explanation is excluded by the detail involved.

The first meeting took place on 29 May 1929, and communications were addressed to twelve different members of the audience. Some of these were only a few sentences and some were of a kind difficult to confirm or deny with precision, but I propose to quote in full the communications made to three people together with their comments, which were added later to the written record.

1. *F.* Curieusement, Monsieur, j'ai l'impression que vous avez eu, il n'y a pas très longtemps, des relations d'affaires avec le Monde Asiatique.

(Oui)

Quelqu'un que vous connaissez bien en Asie est mort ici blessé ou d'une maladie organique ?

["*Ici*" in this question refers to the upper part of the abdomen and not to a locality.]

(I do not know exactly that he was wounded here, but it was not a natural death)

Ville de Hong Kong.

(I do not know the town)

Je vois écrit ici £12,000. C'est un chiffre qui m'apparaît comme ayant rapport avec vos relations d'affaires avec l'Asie.

Correct psychically in general, The Far East in particular for three years.

My special friend "on the other side" is a Chinaman who was mortally wounded on the Western Front in the War. He was blown to pieces.

A gentleman sitting immediately behind the one addressed writes later that he had spent many years in India, and that towards the end of this time he had remitted home sums amounting to about the £12,000 mentioned.

¹ *Proceedings*, xxxix. 266.

2. *F.* Le nom de Cecily, Ceeilia, revient. C'est une sorte de petite anarhiste de bonne qualité. Ce n'est pas une nature domestique ni obéissante. Elle déclare qu'elle n'aime ni la conversation ni la compagnie des femmes et ne les trouve pas intéressantes.

(Ceci est exact)

3. *F.* Il y a plus d'un an vous vous êtes trouvé devant un jeune homme et vous avez montré votre fermeté de caractère et vous l'avez abaissé dans son orgueil ; est-ce exact ?

(Yes, it is true)

C'est un nom Juif. J'ai deux perceptions. Vous êtes très content de la situation matérielle que vous avez, mais vous vous attendez à une amélioration au point de vue matériel dans cette situation.

(Yes)

Maintenant dans cette situation il a été nécessaire pour que vous réussissiez que l'on déplace un garçon qui vous gênait dont le nom est Richard ?

(No)

Je vois aussi que vous étiez fait pour tenir dans la vie une autre situation plus noble et que vous considérez que vous avez manqué votre vie.

(J'ai fait dans la vie ce que je devais faire)

Je vous vois étudiant des langues étrangères, portant des papiers d'un caractère officiel, diplomatique.

(Oui, j'avais été éduqué pour les Affaires Étrangères et élevé pour cela)

A near relative of the gentleman addressed states that her relative had in fact been intended from childhood for the Diplomatic service and had been accepted as a candidate for it. Only after taking his degree at Oxford did he give up the idea and take up his present profession.

It will be seen that in each of the three communications I have quoted there is a considerable proportion of truth. That can only be attributed to one or more of three possible sources: (a) Chance coincidence; (b) Conscious or unconscious knowledge obtained by normal means; (c) Some supernormal faculty. I think we may fairly argue that the proportion of accuracy in these three cases is too high to be due to chance coincidence, and I am reasonably sure that M. Forthuny neither had nor ever had had any normal knowledge of the facts he detailed. It follows that we are driven to assume that his knowledge comes from some supernormal faculty, and it seems reasonable to suppose that this faculty consists mainly in a supernormal knowledge of what is in the minds of people present with him, whether we call such knowledge telepathic or clairvoyant.

The other two public meetings took place on 4 and 12 June. The results on the whole were inferior to those which I have already quoted, and I do not think that anything would be added to the evidence by giving the notes here.

B. THE PRIVATE SITTINGS.

Nine of these were held, the sitters being invited mainly from those whose records of sittings with spiritualistic mediums were in our possession previously. In two cases the sitters took their own notes, and in the others either we or the sitter provided a notetaker.

The French shorthand writer whom we engaged was unfortunately quite inexperienced in taking notes of sittings, and the records which were made by the sitters' friends were in fact more useful for our purpose.

As regards evidence of a supernormal faculty these sittings are in no way different from the public meetings. There is a certain amount of accurate material mingled with a good deal which is erroneous. Even in those sittings which showed apparently supernormal knowledge there was curiously little in common with the recorded results of sittings with other mediums. This is well instanced by the record of the sitting with the two Misses X, one of whom was the Miss X who had successful sittings with Mrs Warren Elliott in 1928.¹ Besides having sittings with Mrs Warren Elliott this lady had had sittings with several other mediums, and on each occasion had been given veridical details regarding the life and character of her late father, though, as far as could be determined, she had always gone as an anonymous sitter, and the mediums concerned had no normal knowledge of her family affairs. In fact, in all these sittings, the most prominent feature of the record is the personality of her father and his "remorse for his unkindness and a desire for forgiveness".²

¹ *Proceedings*, xxxix. 80.

² *Proceedings*, xxxix. 81.

Now in this Forthuny sitting the only allusions that might apply to her father are of the vaguest sort and might equally apply to a number of other people. On the other hand there are a number of statements which I propose to quote, with annotations, which show in my opinion a degree of accuracy which is collectively too great to attribute to chance. It is particularly noteworthy that the first remark, which opened the sitting, contained a statement of a fact which was unknown to either of the two sitters and was apparently a prediction. I have noticed in several cases that M. Forthuny's opening remark is often the most striking "hit" of the sitting.

The sitting took place on June 5, and the notes were taken down in English by the other Miss X, no other notetaker being present.

I must just tell you one small thing that comes to me in connection with you.

There is a lady whose occupation is sewing or dressmaking, connected with clothes and dresses, who has either made out to you an inexact account or taken or kept back some stuff from you. It is in connection with some business house. Yes, a lady occupied with sewing. I think it is over now but you have been deceived or cheated. It is not important but I am giving it to you. You have been cheated in some question of dressmaking.

P, P, Paterson, Peterson; is there a street like that? Any street in London like Peterson?

[Later in the sitting:]

Is November or December a special month for either of you?

A person, very much attached to you, is coming from afar into your house, and she will tell you worries about her health. I see her coming to you, and in her secret thoughts she is more worried than she lets people think.

We were unaware at the time of the truth of this statement. I was then having a ready-made dress from Peter Robinson's altered at that shop. Three days after this sitting the dress came home and I found that a promised half yard of the material, left over from the alteration, had not been returned with the dress. I do not think I was wittingly cheated as the assistant told me afterwards it had been omitted through the packer's oversight.

Is Peterson an attempt at Peter Robinson?

Our sister is expected from the Argentine with her family in December. She has lately been worried about her health for the first time in her life. She would be anxious, but it would be her nature to keep the worry to herself.

MA—MA—MARGERY ? MAL-
LINS ?

She is more anxious than she tells you, but perhaps you have guessed how anxious she is. She has had an example from a friend who had had this same illness or ill-health. She had a bad accident because of her illness: this has frightened this person. She has either brought flowers to you or brought them from the country.

She has been forbidden many kinds of food.

Has she not had in her own family some one who has had the same trouble and this worries her ? She is much more worried about this trouble than she allows people to think.

The rest of the sitting consists of a fairly accurate character delineation of the sitters and a number of statements, some of which convey nothing while others are partly right and partly wrong.

In Mr Saltmarsh's report on the sittings with Mrs Warren Elliott a good deal of veridical matter is noted in connection with what is there called the A case.¹

On June 8 A's father, mother, brother and sister-in-law had a private sitting with M. Forthuny in order that we might be able to compare his results with the previous ones. No notetaker was present and the notes were taken by A's mother, who has added her annotations. On the whole the results are less good than in the Misses X case that I have described, but, just as in that case, there is hardly any reference to A who came to the front so prominently in the sittings with other mediums. Apart from a reference to a young man whose photograph requires framing, whereas there was at the time a photograph of A in a frame which required glazing, there is very little to suggest him in the whole sitting. There were, however, a certain number of accurate statements made regarding past events and family connections which could hardly have been known normally to M. Forthuny. I quote below some examples from which it may be seen that they are of the same type and degree

No.

She had several sharp attacks of gall-stones. Strange to say, my mother, for the first time in her life had a very bad attack of this same trouble. My mother was so ill that a serious operation was threatened, but mercifully did not have to be performed. It is quite possible my sister may fear the need of an operation in her own case if her trouble occurs again.

This is quite true as she is on the strictest diet.

See above.

¹ *Proceedings*, xxxix. 66.

of accuracy as those I have quoted in connection with the other sitters.

1. There is a little dog who is dead. It had a little coloured cushion. A dog was hurt: there were two different dogs: Pitty . . . Petty . . . A man of the church is standing near. You took it somewhere where there was "une sorte de barrière".

Now on July 15, 1926, A's mother had a sitting with Mrs Warren Elliott. The following is extracted from the notes:

Him was fond of dog with long ears, does you know?

(Yes)

Not very tall, sort of long and not very tall, but long with long ears . . . There was an accident to that dog, does you know?

(Yes)

You got white doggie or had white doggie when him here. Shows Topsy white doggie. Got a little brown on him somewhere. Does you know it?

(Yes)

From the sitter's annotations it appears that A's family had several different dogs in his lifetime. One was a dachshund called Pattikin, in connection with which M. Forthuny's attempt at the name "Pitty or Petty" seems a fairly close approximation, while two other dogs were accidentally killed.

2. There is a lady who limps, she walked with two sticks and is getting out again for the first time. There has been some reconstruction of her house lately. She is elderly, has a very good head and gives help to a great many people.

The sitter (A's mother as before) comments:

I have a friend who broke her hip over a year ago. She is not "out again for the first time" but she is just beginning to walk again without depending on a stick. She used two crutches for a long time. Her house has been reconstructed lately, and a week or two before the sitting she had been showing me over it. She is an unusually gifted and clear-headed woman with a passion for helping people less fortunate than herself. Her age is 65.

3. He shows a room with one window and a something built out. Mauve and red flowers that recall something. WA—WHITE—WATERLEY.

Comment: Our drawing-room has one window opening on to a small balcony. A few days before the sitting there were mauve

irises on the mantelpiece and on the piano a bowl of mauve and rose-coloured sweet peas. When I put them there I remembered having had the same flowers in the room the last afternoon A had been at home in 1918. Waterley meant nothing to me at the time, but afterwards I remembered that a Mrs Whatley had been here to tea that same afternoon. This is written in my diary.

The remaining private sittings were of varying degrees of accuracy. They all contained some "hits" as well as a good deal that was quite inaccurate or unverifiable, and I do not consider that anything is to be gained by quoting from them. The extracts already given will serve as samples of the whole.

C. PSYCHOMETRIC EXPERIMENTS.

There remain only the experiments in what is generally called Psychometry and Object-reading. These were four in number.

The first was made at the second public meeting on 4 June, when M. Forthuny was handed by the Rev. J. W. Hayes a box containing a small Egyptian figure. Unfortunately he took out of the box the small parcel containing the figure before saying much about it, and thus, although he did describe it as Egyptian and very old, it is impossible to be certain that this is more than could be learnt by normal means. He added rightly that the object had been given to Mr Hayes by a lady, but there was nothing accurate in what he said about her.

The second experiment was made in private on 11 June. An article in a closed packet was handed to him by Mrs Brackenbury, who had no knowledge of the contents of the packet. No one else was present. It is not worth while to quote what he said about it because hardly any of it has any relevance to the object. It is however of considerable interest to note that he concluded with the statement, "I get the name Kathleen". No other names were given before this, and Kathleen is the name of the owner of the article, a fact which was known to Mrs Brackenbury.

The third and fourth experiments were carried out at the third public meeting on 12 June.

The first object consisted of an irregularly shaped piece of canvas measuring roughly about 10×5 cms. It was in fact a piece of the envelope of the Zeppelin airship which was shot down at Cuffley on 3 September 1916. It was sealed in an envelope in one side of which had been cut an oblong opening of about 3.5×2 cms. This allowed one side of the canvas to be seen and touched but not the other. The side which was thus visible showed a rough canvas surface. The other side was covered with some waterproofing composition. When I examined it myself before the meeting I thought it was a piece of a bicycle tyre.

On handling this object M. Forthuny said :

“ Je marche dans une route éblouissante de soleil : je suis conduit : je domine les choses au dessus du monde. J’entende les noms ‘ Michael ’ . . . ‘ Hardy ’ . . . un nom qui finit par ‘ man ’. Impression d’un violent tonnerre, tournade épouvantable. Je ne peux pas arriver à définir, je vais aller directement. Je vois un homme combattant dans une bataille, tourmenté. Le 17 Avril. Initiales A—A.”

The second object was a small irregularly shaped piece of aluminium about 2.5 cms. long, contained in a sealed matchbox. In the top of the matchbox was a roughly circular hole about 1 cm. in diameter through which the object could be just touched and rather indistinctly seen. This piece of aluminium had formed part of another Zeppelin which was shot down at a later date.

M. Forthuny’s impressions were as follows :

“ Je vois quelqu’un qui étouffe, qui arrache son col, qui défait des boucles, qui se jette en avant et qui tombe. Théodore. C’est une impression tout-à-fait semblable à celle du premier paquet.”

These experiments seem to me to be the most successful ones of M. Forthuny’s visit, and the reports are worth considering in some detail.

Both objects were provided by Mrs Brackenbury, who was present at the meeting and who of course knew their histories. In the case of the first one, the phrase “ je suis conduit, je domine les choses au dessus du monde ” is very applicable to a member of the Zeppelin’s crew. The names “ Michael ” and “ Hardy ” are the names of two people who were closely connected with Mrs Brackenbury’s life and with one another.

The “ impression d’un violent tonnerre, tournade épouvantable ” is easily applicable to aerial fighting, as is the “ homme combattant dans une bataille ”.

The accuracy of what is said about the second object is even more striking, especially as there is nothing whatever to cause this fragment of aluminium to be associated with any fatality. As I mentioned above it formed part of a Zeppelin which was shot down near London. Mrs Brackenbury was present in the course of her duties as an ambulance driver, and in walking over the field she discovered the body of one of the crew at some distance from the wreckage. His back was broken, but his body was not burnt or otherwise injured, and it seems clear that he threw himself out before the airship came down.

Finally, I should like to call attention to M. Forthuny’s concluding remark that he received just the same impression from the second object as from the first. It seems to me quite impossible to attribute this to any normal observation. He had no knowledge even that

they were contributed by the same person, and there was nothing whatever to suggest a common origin for the two.

CONCLUSION.

I have tried to put forward those cases in which there seem to me to be the best grounds for supposing the existence in M. Forthuny of some supernormal faculty. That the evidence is not stronger and the cases more numerous is due in part to two causes. I have already mentioned one, our difficulty in finding a competent French shorthand writer. The other cause lies, I think, in the difficulty which M. Forthuny felt in entering into the psychology of another nation. Nearly all the sitters, both in public and private, were quite unknown ground to him, and this brought about a very real though largely unconscious inhibition of his powers. Besides this he also felt a certain conscious restraint in a fear lest he might by accident transgress some convention unknown to him.

The nature of that supernormal faculty remains obscure. In these London experiments very little was said by him which had not at some time been known to someone present, and we must assume that his information was derived in some way from that person's mind. What appears to me to be of interest is the fact that his communications are very little concerned with supposed messages from the dead, and in that way afford a marked contrast to the communications we are accustomed to receive from English mediums although the verifiable details are of much the same type in both.

Finally, I must express to M. Forthuny the very warm thanks of the Society and of myself for the willingness with which he lent himself to our experiments and acceded to all our wishes.

II. THE MARGERY MEDIUMSHIP, AND THE LONDON SITTINGS OF DECEMBER 1929.

I. A CRITICISM OF THE METHOD OF CONTROL.

By V. J. WOOLLEY AND E. BRACKENBURY.

IN December 1929 Dr L. R. G. Crandon of Boston gave three demonstrations in the Society's *séance*-room of some of the phenomena which he has described as occurring through the mediumship of Mrs Crandon (*Margery*). The procedure was understood to be the same as that employed in the sittings which usually take place at Boston, and an examination of the method of limb control and the phenomena occurring seems to be worth while in affording data for an estimate of the mediumship as a whole.

The method consists in the attachment of the wrists and ankles of the medium to the arms and front legs of her chair by a number of turns of adhesive tape. The position of the tape is marked on the skin by lines drawn over tape and skin in pencil. We do not think it necessary to describe the method in any greater detail, since we are in full agreement with Dr Crandon that his method does secure the wrists and ankles firmly to the chair so that no movement is permitted to the lower ends of the forearms beyond what is allowed through the skin moving for a short distance over the underlying muscles and bones.

In addition to this attachment of the wrists and ankles a cord is tied at its centre to the back of the chair and the two ends are brought over the shoulders and then under the arms, passed through the back of the chair and tied behind. This arrangement limits the position of the shoulders so that they cannot move forward more than a short distance, and so limits to some extent the movements of the head.

The table and chair used in the sittings were procured in London, the table being specially made to the dimensions asked for by Dr Crandon, while the legs of the chair were cut short in such a way that the knees of the medium were just able to pass under the top of the table, part of the table frame being specially cut away to allow of this. It follows that a very slight raising of the knees is enough to bring them in contact with the table and so to lift it off the floor,

and it is impossible, when such a low chair is used, to adjust the tapes round the ankles in such a way as to prevent this. We have mentioned above that the tapes round the wrists prevent movement of the lower ends of the forearms. It is however clear that the hands, which are not secured at all, have a very considerable latitude of movement, depending on the degree of flexibility of the wrist joints.

We have made experiments under this method of control, and have satisfied ourselves that it is by no means difficult to move the table about and to handle and lift up any objects placed upon it.

In addition to being ineffective the method has the further disadvantage of being exceedingly uncomfortable for the medium. It makes difficult or impossible the small changes of posture which are essential for comfort, and the tightness of the tapes causes the skin between the turns to become swollen and painful. Also, the adhesive used is not water-soluble, and the tapes have to be dragged off the skin by force at the conclusion of the sitting, a process which is painful at the time and which tends to damage the skin if repeated at too short intervals.

We have mentioned already that our own experiments have satisfied us that it is possible for a person fastened in this way with adhesive tapes to the chair used in the Crandon sittings to handle and lift up objects placed on the table, and that on that account we consider the method of control to be ineffective, but it is of importance, in judging the value of the sittings in question, to know if there is any definite evidence that the objects were in fact so handled. It is the chief purpose of this paper to present what we believe to be such evidence.

In order to make intelligible what follows it may be worth while to describe the usual procedure at the Crandon sittings when the personality known as Walter expresses his intention of making a finger impression.

Two dishes are placed on the table in front of the medium, who is secured to her chair in the way we have described. The dish to her right is filled with water which is as hot as can just be endured for a short time by the fingers. The dish to her left contains cold water. These dishes are generally filled in red light. Lying on the bottom of the hot water dish with its ends projecting over the edges is a folded cloth or handkerchief. When directed by Walter, a sitter puts into the hot water and on the cloth a marked cake of Kerr dental wax. The red light is then extinguished. When this wax is sufficiently softened by the hot water it is pulled out by means of the cloth and an impression is made on it. As soon as the impression is made the wax is put into the cold water to harden. It is alleged that all these manipulations of the wax after it is put into the hot water are carried out by a supernatural structure made by

Walter for the purpose of manipulating material objects, and known as his "terminal". The chief argument used to support this contention is the allegation that the medium is so effectually secured in her chair that she cannot herself handle the objects on the table.

During the course of the sitting of 7 December 1929 a piece of dental wax (which we will call A) was, by Walter's direction, broken in two,¹ and for clearness we may call these pieces A1 and A2. One of these two pieces, A1, was placed in hot water to receive the impression of a digit from an alleged "visiting lady," but owing to the water not being hot enough it showed finally only a small surface depression with no skin-markings. This piece A1, which remained in our possession, is only of importance as helping to ascertain the weight of A2. The second half, A2, was then placed in hot water, but this time the water was too hot; the wax became adherent to the handkerchief and finally rolled up in it and no impression was obtained. It is with the fate of this second half, A2, that we are mainly concerned. By Walter's direction it was left as it was on the table, still rolled up in the handkerchief, until the next sitting (on 8 December 1929) and at some time in the course of that sitting it was squeezed up in one of the pieces of wax which were softened during that sitting to receive impressions. This is definitely proved by the weights of the cakes of wax used and of the lump remaining at the end of the sitting of 8 December. To decide this we weighed ten new cakes of Kerr wax, with the following results:

Weight of lightest cake,	24.77 grams
Weight of heaviest cake,	29.70 grams
Average weight of one cake,	27.00 grams.

Now the weight of the first half piece of 7 December, which we call A1, is 16.58 grams, so that the weight of the other half piece A2 was almost certainly between 8 and 13 grams and probably about 11 grams. The weight of the shapeless lump, which we believe to consist of one whole cake and this half, is actually 38.96 grams, a figure which fully confirms our belief. We think then that it may be taken as certain, and we believe that Dr Crandon will agree with us in this, that this lump remaining at the end of the sitting of 8 December 1929 was made by the joining together of the old half piece, A2, from the night before with one of the whole new pieces first softened during this sitting.

At the sitting of 8 December two new pieces of wax, one marked with one notch, and hereafter called B, and one with two notches, hereafter called C, were used. At the beginning of the sitting the half-piece A2 was, as already stated, on the table.

¹ *Psychic Research* (New York 1930), xxiv. 260.

If we now turn to Dr Crandon's dictated notes of the sitting of 8 December we are met by the surprising fact that they contain no mention of the softening of the first piece of wax B, which is only referred to indirectly in Walter's statement that "*the first piece was spoiled owing to the water being too cold*".¹ To rectify this omission, and to make the account of the sitting intelligible, we give below our own notes of what occurred.

After "*Walter says 'if we get a print, mind you, it may be a poor one'*";² our notes continue: "Walter called for red light and directed Lord Charles Hope to put some hot water in the basin and Dr Woolley to put a marked piece of wax therein. Dr Woolley put in the hot water a new cake of Kerr, marked with one notch [that is, B], and the light was then extinguished. After a minute or two Walter announced that it was no good; the water was too cold.

"Lord Charles Hope asked where the spoilt piece of wax was. Walter said that we could not have it now, or words to that effect.

"Then he asked for another piece [that is, C] to be produced and directed Lord Charles Hope to pour more hot water into the dish without the red light being turned on."³

From this point Dr Crandon's notes continue as follows:

"*Walter directs that later when he so orders them, Dr Brown is to stand and put his right hand on the psychic's left hand and similarly when ordered, Dr Woolley is to stand and put his marked wax in a fresh dish of hot water and keep his hand on the psychic's hand, thus keeping practical⁴ control of both hands. . . .*

"*Under orders from Walter, Mrs Brackenbury takes Dr Crandon out of the room. As Mrs Brackenbury had to take Dr Crandon out of the room, Lord Charles Hope attended to the gramophone, when it got to the end of the record. He (Lord Charles) says, 'I moved across, restarted the gramophone and sat next to the notetaker, putting both my hands on her arm.'*

"*Dr Woolley dictates 'At Walter's direction, Dr Crandon and Mrs Brackenbury left the séance-room and Dr Woolley and Dr Brown were instructed to stand one on each side of the medium, Dr Woolley holding the medium's right hand with his left, and Dr Brown holding the medium's left hand with his right, and the two joining their unoccupied hands as far as possible away from the medium.'*

"*Before doing this Dr Woolley had put a piece of wax into the dish.*"⁵

¹ Dr Crandon's notes are printed in *italics* throughout the present paper.

² *Psychic Research*, xxiv. 261.

³ This was the only occasion during the series on which the red light was not allowed while hot water was being poured into the dish.

⁴ In the notes as printed in *Psychic Research* this word has been altered to "tactical".

⁵ This piece of wax was marked with two notches.

“After the expiration of some time Dr Brown was instructed to lift up the wax impression from the table by the side of the cold water dish. He lifted first a lump of wax in a cloth which Walter said was the wrong impression. Dr Brown later found what was said to be the right impression on the table and handed it to Dr Woolley and both resumed their seats.”¹

We then note that Mrs Brackenbury and Dr Crandon returned to the *séance*-room.

Dr Crandon's notes continue, “Walter tells us that the first piece was spoilt owing to the water being too cold, and that the piece which was handed out as the proper result was the second piece of wax.”²

Our notes continue: “A few minutes after Mrs Braekenbury and Dr Crandon returned Walter said, ‘Here you are, Woolley’, and we heard a hard object fall on the floor.” This object was picked up afterwards by one of us (E.B.) and found to be the spoilt wax already described as consisting of one and a half cakes of Kerr squeezed together into a shapeless lump. Now this squeezing together took place during the course of the sitting, and was completed before the medium was released from her bonds. We thought therefore that this lump was deserving of a very careful examination for finger impressions, since any found ought to have been produced by what is known as “Walter's terminal”. We were only able to discover one impression on its surface, and this bore no resemblance to the well-known Walter impression. On comparing it however with the impressions of the medium's fingers which she kindly allowed one of us (V. J. W.) to take while she was in London, we found that it was quite certainly the impression of her right index finger. Figs. 1 and 2 are enlarged photographs of this impression on the squeezed lump and of the impression of the finger in question. This seemed such an important fact that we communicated it to Dr Crandon in a letter of 30 January, and on 17 February he replied as follows:

“The spoilt and squeezed piece of wax was handled by the medium in the presence of everybody after the sitting because, with Scotch thrift, she wanted to retrieve the handkerchief from it. This handkerchief you remember was the one put in hot water by you at Walter's request. The water was too hot and wax and handkerchief got completely stuck together.”

It is clear that the sitting to which Dr Crandon refers in this letter

¹ This “right impression” is an imprint of a thumb which Walter attributed to an elderly lady and is referred to by Dr Crandon as the Old Lady impression. The identity of this piece of wax, *i.e.* whether it was the first piece, B, or the second piece, C, forms the subject of Part 2 of this paper.

² Dr Crandon here adds the word “divided” which does not seem to us to make sense, because neither of the two pieces softened on that evening was divided. Each was a whole new cake.

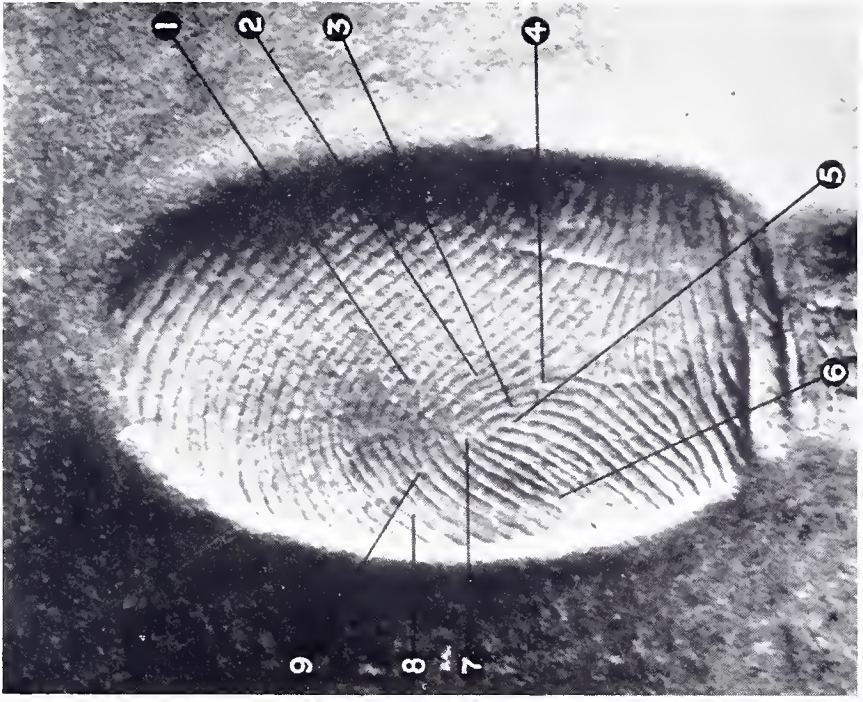


FIG. II.

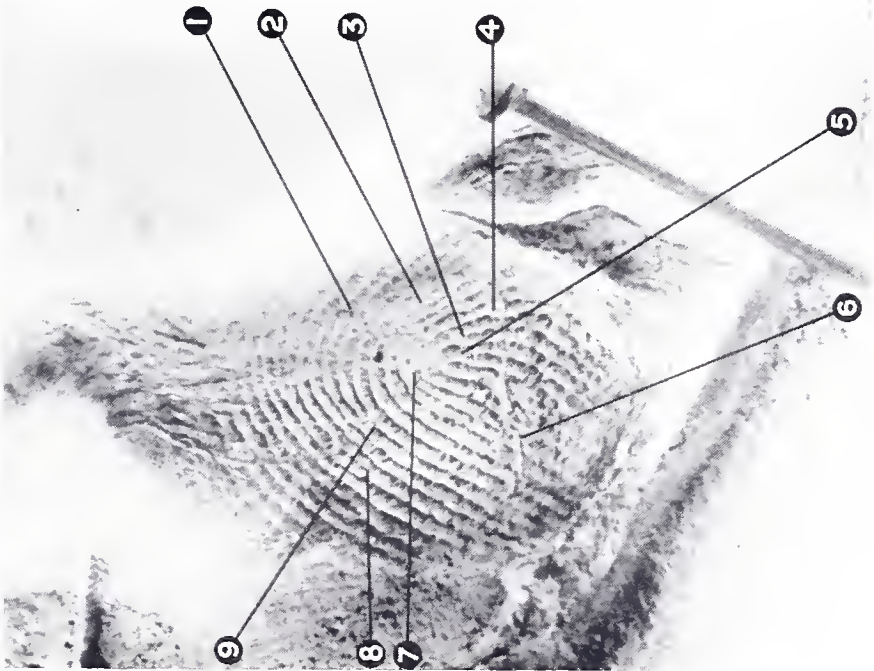


FIG. I.

took place on 7 December 1929, (a) because the squeezed lump found after the sitting of the 8th had no handkerchief attached : it was thrown on the floor as already described, (b) because on 8 December Walter complained that the water was too cold and not too hot.¹

Some further correspondence ensued, and on 28 March Dr Crandon wrote " December 7 Walter instructed us to leave everything on the table as it was. Just before the sitting of December 8, Margery, impulsively and casually, picked up one of the pieces of December 7 wax which had been left on the table. You, Mrs Brackenbury and myself all cried out to her not to touch it. If her finger mark is on it that is the way it happened."

This second suggestion of Dr Crandon's seems to us to be even less admissible than that put forward in his earlier letter. The impression on the wax is in three dimensions, and it could only have been produced while the wax was hot and soft. It could not possibly have been done on the 8th before the sitting began since there was not at that time in the room any hot water or other means of softening the wax.

This was pointed out to Dr Crandon in the course of further correspondence and he has now published the first suggestion,² as the true explanation of the finger impression, and gives four reasons why its presence on this piece of wax is of no importance.

These are : (1) It was not a piece of wax which was a part of any experiment. We submit that this piece A2 was a part of an experiment on the 7th although no impression was obtained on it. It also became part of an experiment on the 8th because it was on the table when the sitting of 8 December began, left over from the previous day at Walter's request, and remained there throughout the sitting of 8 December. Dr Crandon's contention seems to imply that only those objects which show the desired result are to be taken into account, while those that indicate a normal causation of phenomena are to be neglected because their evidence is accidental.

(2) It was warm at the end of the sitting and would receive an impression from anyone who touched it.

We have carried out some experiments on the time of cooling of the wax used, and find that a whole cake of Kerr wax, heated in water to the highest temperature which allows it to be manipulated at all, then removed and made into a ball (in order to cool as slowly as possible), is too cold to receive a recognisable imprint after 15 minutes in a warm room.

On 7 December only half a cake (or less) was used : it was not squeezed into a ball ; and we may safely assume that the cooling time was not more than this.

¹ *Psychic Research*, xxiv. 262.

² *Psychic Research*, xxiv. 260n.

Now the removal of the wax from the water is recorded in Dr Crandon's notes¹ in the words "Something is heard coming out of the water, and some wax as if it was hitting the table". Unfortunately no times are recorded, but after this note there takes place a good deal of talk from Walter, the gradual rousing of the medium, the examination of the adhesive strapping and rope, the examination of the medium by Lady Barrett and the changing back of the medium into her usual clothing. We are quite unable to believe that all this could have been compressed into 15 minutes, which is the longest possible time for the wax to have remained soft.

(3) The conditions of the sitting of 8 December (where Woolley and Brown held the lashed hands throughout the experiment) exclude the normal production of the prints.

This seems irrelevant to Dr Crandon's contention, since the hands were in fact held only for a part of the experiment, namely from the time that the second piece of wax was put in the hot water until the old half piece, that is, "a lump of wax in a cloth" as mentioned in Dr Crandon's notes, was picked up by Dr Brown. It is not clear what prints are referred to since only one print (the Old Lady) was produced at this sitting.

(4) Both of the "Walter" prints² and also the "Old Lady" print show only the unique print desired.

This also seems irrelevant to the point under discussion, which is the presence of the medium's finger impression on a quite different piece, namely, the "squeezed lump." It would clearly be a great advantage if we could establish with certainty from the wax itself whether the impression is on the half piece A2 or the whole piece (B or C: for argument see Part II) which were welded into one on 8 December. The appearances suggest that it is on the whole piece which we believe to have been wrapped round the half piece so as to enclose it more or less completely. It is not possible to be certain on the point without softening the wax and we are unwilling to do this at the present time. But even without this we consider that we have sufficient evidence, from the wax and Dr Crandon's notes, to make it reasonably certain that by some means or other an impression of the medium's right index finger was made on the wax at a time when she was secured to her chair with the adhesive tapes which Dr Crandon habitually uses, and we consider therefore that that method of control is ineffective in preventing the medium from handling objects on the table, and thus useless for its purpose.

¹ *Psychic Research*, xxiv. 262.

² Both these Walter prints were made on 7 December.

II. THE PRODUCTION OF THE SO-CALLED "OLD LADY"
THUMB IMPRESSION ON 8 DECEMBER.

By V. J. WOOLLEY.

My original intention was to publish no further comments on these sittings, which I regarded as exclusively Dr Crandon's investigation at which I was merely an onlooker. In his report however he has described one particular experiment in which I was allowed to take a leading part, and it seems possible that some of his readers may be led to believe that I endorse the account there given.

The experiment in question consisted in the making by Walter of a thumb impression which was said to be that of a well-known lady who had died recently, but whose name was not given. At the sitting Walter announced that the impression would be made while the medium's hands were held by Dr Brown and myself. The account dictated by Dr Crandon at the time is so inadequate that it even fails to mention, except quite indirectly, in reporting Walter, the fact that two pieces of wax, which in Part I of this paper were distinguished as B and C, were used during the evening.

In Part I of this paper has been given our own account of the events of the sitting, and I will later consider how this can be checked by reference to admitted facts. Before the sitting began I was asked by Dr Crandon to provide myself with several pieces of Kerr wax, each differently marked. I marked three pieces by making deep notches in the side. The three pieces were marked with one, two and three notches respectively, and the notches could easily be felt and counted in the dark. I understood from Dr Crandon that I was to keep these markings secret and not show them to any other sitter. The first important event was the placing by me in the hot water of the first piece of wax. I am quite confident that this was the piece marked with one notch and distinguished as B. The red light was then turned out, and after an interval Walter said that the water was too cold and this piece was spoiled. Lord Charles Hope was then directed to put hot water into the dish and Dr Crandon and Mrs Brackenbury left the *séance*-room. I took the medium's right hand, as described in the published notes, having first put into the hot water the second piece of wax marked with two notches, and distinguished as C.

After an interval Walter said that the impression was made and directed Dr Brown to take the wax off the table. Dr Brown first picked up the old half A2 from the previous sitting which had been on the table throughout, and then the piece which Walter said was the one on which the Old Lady had impressed her thumb.

Later in the sitting a hard object fell on the floor, and when this was

examined afterwards it was found to consist of one and a half cakes of Kerr wax squeezed together. It obviously consisted of one of the two cakes softened during this sitting, together with the old half cake from the night before. At the close of the sitting, when I was able to examine in light the so-called Old Lady impression, I found that it had only one notch, and therefore was the first piece of wax softened and not the second.

There were only two possible alternative explanations of the facts I had observed. The first was that I had made an inexcusable blunder and had miscounted my own marks on the wax, putting the double notched piece C into the water thinking it was the single notched one B. The second was that Walter was in error in his assertions that the impression was made on the second piece of wax and that the first piece was spoiled. In the bustle and excitement of the sitting I saw no way to decide between these two alternatives, and in fact it was some time before the matter became clear. As the sequence of events is somewhat confusing I propose to give a brief statement of that sequence in the form of numbered paragraphs.

1. At the beginning of the sitting there is on the table a half cake of wax, A2, which had been softened and squeezed together on the previous day.
2. The first piece B or C is put into the hot water.
3. It is taken out of the hot water by Walter, who says the water is too cold. (It is impossible to suppose that it was left in the hot water dish, which had to be refilled, and where it might adhere to and interfere with the second piece.)
4. The hot water dish is refilled: a second piece of wax, C or B, is put into it and the medium's hands are held by Dr Brown and myself.
5. Dr Brown is directed to pick up the finished impression. He picks up the old half piece A2 which was on the table from the previous day.
6. He finds what is said to be the right impression and hands it to me.
7. The medium's hands are let go by Dr Brown and me, and Mrs Brackenbury and Dr Crandon return to the *séance*-room.
8. After some remarks from Walter he says "Here you are, Woolley," and something is heard to fall on the floor. On being picked up after the sitting this is found to consist of the old half piece A2 which Dr Brown had picked up first and a whole piece C, the two being squeezed firmly into one.

Now if Walter is correct in saying that the impression was made on the second piece of wax, it necessarily follows that the piece squeezed up with the old half piece was the first piece which he said

was spoiled. But this first piece was taken out of the water (which he said was too cold to soften it properly) in paragraph 3 above. It ought to have been cooling on the table, or in the cold water, through all the events detailed in paragraphs 4, 5, and 6, because up to this point the half piece (which was afterwards squeezed up with it) was still separate, and was picked up by Dr Brown. Actually the combined one and a half pieces have been squeezed up when one of them at any rate was very soft, as can be seen from the complete obliteration of the original outlines. It would have been quite impossible to squeeze so completely a piece of wax which had been cooling for so long.

These considerations seem to me to make it quite clear that the second alternative I mentioned was the correct one and that I had not made any mistake in my marked pieces of wax.

At the time however I had neither the various data which have accumulated since nor the time to consider what data I had. All I could do was to dictate a very guarded note which I print below, and which, so far as it goes, bears out my present statement that I had noticed the notch on the wax and was not inclined to agree with Walter's claim without further evidence: "Dr Crandon was not in the *séance*-room from the time that the second marked piece of wax was put into the dish until the time of the picking up of the impressed piece by Dr Brown." As soon as I was satisfied as to the fate of the two pieces of wax I communicated my opinion to Dr Crandon by letter of 30 January, at the same time that I informed him of the presence on the spoilt wax of the medium's finger impression which forms the subject of Part I of this paper.

I have given above a summary of the course of events during the sitting and have tried to show that their time relations are inconsistent with the claim which Walter made. I have now to consider how far that sequence is consistent with my view that the impression was made on the first piece and not on the second.

I suggest that the first piece B was softened in the usual way and was impressed with the thumb mark which it now bears. It was then left on the table between the cold water dish and the medium, there being also present near the dish the old half piece of wax A2 from the previous sitting. Walter then announced that the wax was spoiled and that the second piece C would be impressed by the Old Lady while the medium's hands were held. The second piece was accordingly put in the hot water dish by me and remained there until after the first piece B, bearing the impression, had been picked up by Dr Brown and the medium's hands were released. At some point after this the second piece C was taken out of the hot water and squeezed up with the old half piece A2. From the fact that it now bears the impression of the medium's finger I deduce that Walter

used the medium's right hand to squeeze it up. It was then thrown on the floor very soon after the return of Dr Crandon to the *séance*-room.

These suggestions seem to me to be consistent both with the sequence of events and with the present condition of the pieces of wax. The claim that the "Old Lady" print was made on the second piece of wax seems consistent with neither.

In conclusion, it seems worth while to draw attention to one detail in which the procedure at this sitting deviated from that usually followed by Dr Crandon. It is usual, I believe, for the hot water dish to be filled in red light : this is obviously more convenient and safer. On this occasion complete darkness was maintained from the time that the first piece was put into the hot water until after Dr Crandon's return to the room. If I am right in suggesting that the finished Old Lady impression was lying on the table at the time when the hot water dish was being refilled for the second cake of wax, there is sufficient reason for Walter's maintaining complete darkness during that part of the experiment.

REVIEW.

HENRY MONNIER, *Etude médicale de quelques guérisons survenues à Lourdes*. 8vo, pp. 67, 6 ill. Paris : Editions Médicales N. Maloine, 1930. Price 14 fr.

The above work is a "thèse pour le doctorat". Dr Monnier (of Neuilly-sur-Seine) defended it before the Paris Faculty of Medicine on 7 April 1930 with complete success ("Mention très honorable"). This fact is perhaps as interesting in a way as the cures themselves. We are not sure that it was the first time a physician selected the Lourdes phenomena for his thesis; but even granted that Dr Monnier had predecessors in this respect, these must have been very few indeed.

We heartily congratulate the learned Faculty on the toleration and breadth of view manifested by it in admitting so unorthodox a thesis; in this we concur *mutatis mutandis* with a personality of no less exalted rank than Pope Pius XI, to whom the writer—a most devout Roman Catholic—sent a copy of his work, and who, in thanking Dr Monnier, expresses the opinion in a letter signed by his Cardinal Secretary of State (which letter is reproduced in the volume in *facsimile*) that the aforesaid admission is a "happy symptom".

Dr Monnier's religious fervour is further attested by the fact of his "study" being dedicated *inter alia* to Jesus "Christ, Saviour and Master", the Virgin Mary and the (Roman Catholic) Church. This circumstance will possibly prejudice against him some of our members. Though I can understand their standpoint, I prefer welcoming the author's frankness and sincerity. I will also avail myself of this opportunity respectfully to remind those members that in the present phase of its existence Dr Monnier's Church by no means views alleged contemporary "miracles" with undue favour. It is true that the Lourdes phenomena seem to be privileged in this respect (though belief in them is not *de fide*), but the attitude of that Church towards the Konnersreuth stigmatica, for instance, is a proof of its present circumspection, a circumspection which in some other cases has been akin to hostility. It would be therefore unjust, I think, to regard the Roman Catholic fervour of Dr Monnier as detracting from the value of his observations and deductions.

These deal with three cases : that of Mme Augault, born at Craon

(Mayenne) in 1877, cured in August 1926; that of Charlotte Renauld, born in 1874, cured in 1892 (this case was investigated by the writer's father, Dr Louis Monnier); and that of Mlle Emilie Cailleux, specially observed by Dr Pierre Goret, cured in August 1921.

In the first case—the only one, so far as I can see, which appears in print for the first time—we have to do with a fibroma, whose existence is attested by four physicians; in Charlotte Renauld's case with a shortening by three centimetres of her left leg (this shortening seems to have begun, by the way, in the patient's fifteenth year: a circumstance which in the eyes of some may be an argument in favour of its "hysterical" character); in Mlle Cailleux's case with Pott's disease and Paraplegia (besides Dr Goret who specially investigated this case we have attestations of other physicians too). All the three patients were cured at Lourdes, the two first apparently instantaneously, the third more gradually, and these cures appear to have been definitive.

There seems to be no reasonable doubt that viewed from the standpoint of orthodox medicine they are "absolutely extraordinary" (Dr L. Monnier on the Charlotte Renauld case). Nature does not produce such results even with time at its disposal, Dr Goret thinks *à propos* of some particulars of Mlle Cailleux's case.

Some readers will no doubt regret that only in the Augault case has our author been to a certain extent (a very limited one) in personal contact with the patient. (Mme Augault's ordinary physician was Dr Faligant, who informed Dr Monnier in December 1929 that since her return from Lourdes he had not to attend her at all). Whatever may be thought on that particular point, I for my part do not hesitate to state that I am ready to accept these three cures—as well as several others of the same character—on their face value. I am willing to believe the medical evidence adduced in this and some other cases adequate. I have almost no doubt that the cures were effected by means of which we know little—if anything—and which seem to be entirely, or almost entirely, beyond our control at present. But more than this it does not seem to me safe to assert.

Dr Monnier is no doubt right in showing (in the chapter entitled "La cause de ces guérisons") that suggestion, as he understands it, had nothing to do with the results. Mme Augault, for instance, "if suggestion had the power attributed to it by some, would have died at Lourdes", in consequence of what Dr Faligant had categorically stated to her. But of course only *direct* suggestion can be meant here. Also more or less direct autosuggestion which the writer also attempts to disprove (pp. 53-55). Emilie Cailleux, he says, did not ask to be sent to Lourdes, she merely consented to go

there. It is true that she seems to have had complete confidence in the Virgin Mary ("Ces quelques lignes ne sont pour ainsi dire, qu'une répétition du mot : confiance"), but she had no initiative, and merely let "events come". As to Charlotte Renauld, "psychological documents" bearing on her case are missing, and so Dr Monnier "abandons her to those who think that an act of her will could have suddenly added the three missing centimètres to her inferior limb" (p. 55).

Here I cannot follow the author, I confess. And his line of reasoning does not convince me. *Primo*: How does he know that autosuggestion *cannot* produce in our organism profound modifications such as have never been provoked nor are likely to be induced, at least for a very long time to come if ever, by direct suggestion? *Secundo*: Why identify autosuggestion with the patient's *conscious* will, *conscious* convictions or *conscious* expectations? These may help; but why should they be indispensable?

On the other hand: that stigmata à la Therese Neumann—to return to her—may be due not to a "miracle", that is to the intervention of a superhuman entity, but to recondite psychological and physiological processes will now be admitted even by many of Dr Monnier's co-religionists. Out of some 300 stigmatised persons known to history only a fraction have been canonised by the Roman Catholic Church, which proves that even Roman Catholics are by no means bound to regard stigmata *per se* as of a supernatural origin. It is true that from them to some of the Lourdes "miracles" there seems at times a very long way; but may we not suspect that the difference is not so much qualitative as quantitative?

The more I think on these matters the more I am inclined tentatively to postulate as a working hypothesis that religious faith may be one of the most powerful factors in human existence. I will compare it in this respect to the sexual instinct. The latter, which is much more powerful, still creates life incessantly and everywhere. Religious faith on the other hand is apt, we may suppose, to affect living tissues, to fathom the depths both of the human psyche and of the human physical organism, to produce in both radical changes in a way of which we have barely an idea. Why can it tap in this fashion the hidden energies of the ego? We do not know. It is one more mystery of Nature, but a mystery for whose reality there is, I think, some little evidence.

Of course there must be limits to the power of religious feeling. It will never cause a house or a mountain to move, any more than it will bring a dead man back to life. I also strongly suspect that if I have my right hand chopped off, no amount of faith will cause it to coalesce again with my arm. But, barring these extreme cases, I think that we should be very circumspect as to what faith, religious

feeling, can or cannot achieve in a diseased human body, especially in particularly appropriate surroundings (such as Lourdes), where we may tentatively suppose there may have accumulated in the course of years something like a huge reservoir of mysterious psychical elements laden with latent potentialities. At times, in a way of which we know as yet nothing, some of these elements, in a moment of high tension (induced by a religious procession carrying the Blessed Sacrament, for instance) will possibly "explode"—and a sudden cure may be the result. We have, of course, no evidence that such a psychic reservoir exists, but I submit that its reality is nevertheless much more plausible than for instance that of a cosmic reservoir of memories in which some people are inclined to believe!

If we postulate such an hypothesis, faith direct and conscious need not by any means be a *sine qua non*, though surely, as I said, it may contribute to the cures. So much for Dr Monnier's assertion that the three cases he has analysed had the "Christian Faith" as their cause. Let us now turn to his conclusion (pp. 61-63).

In the first we are told that the *malades* in question, afflicted as they were with various functional and organic troubles acknowledged by their physicians to be either incurable or "very slowly curable", have been suddenly cured. As to this I have practically no doubt.

From which Dr Monnier draws two practical conclusions: (a) A physician must not advise patients whose state can not be improved by his methods and those of his confrères, not to go to Lourdes; (b) he is not to be blamed if he advised them to go there. Few of us—and I least of all—will be inclined to dispute this.

On the other hand, I cannot agree with Dr Monnier when he continues (conclusion 3, a): "If we call them (the cures) spontaneous, if we say that they have been caused by unknown natural forces, we shall merely veil our ignorance with words, but shall not explain them."

I do not think we shall deserve this reproach if we postulate tentatively, as I have done, that there is some evidence to show that by itself religious belief—apart from its supposed "truth"¹—is a very potent factor not only of psychological but also of physiological order, to whose operation limits should not be too hastily assigned.

In conclusion 3, b, we read *inter alia* that the *malades* had not been submitted to any psychotherapeutic action which besides "could not have cured them owing to the nature of their disease". If here suggestion in some form or other is meant the writer is very probably right, but this is quite beside the point, as I have attempted to show.

In conclusion 4 Dr Monnier notes that the state of the three

¹ Into the very interesting question why this evidence appears to be much more abundant in the Roman Catholic confession than in others I purposely do not enter here.

patients had been either stationary or had grown worse outside of Lourdes ; “ loin de la cause présumée pas de guérison ” ; therefore the “ Christian faith ” was the cause of the cures in question.

The last formula is—involuntarily—far too vague, not to say too diplomatic : for no intelligent agnostic—other agnostics do not interest me—would, I think, refuse to subscribe to it, granting the facts adduced to be real. It is obvious that, whatever the explanation finally adopted, the “ Christian ” (*i.e.* Roman Catholic) religion has something to do with the cures. But from this it does not logically at all follow that the latter prove that religion to be the true one. Now, though Dr Monnier does not say so *expressis verbis*, he obviously had in mind something of the kind. His formula is therefore inadequate from his standpoint.

I am not sure that the argument that the *malades* had not recovered outside of Lourdes (“ Loin de Lourdes, ou d’un lieu équivalent, aucune de ces malades ne guérissait avant longtemps, si tant est que l’une d’elles eut guéri ”, p. 59), is quite sound under the pen of a convinced Roman Catholic : for surely Dr Monnier believes that his Church can work cures *everywhere* ?

In conclusion 5 we read (p. 63) : “ If it be objected to us that we are here outside the domain of medicine, we shall answer that science has no other limits than those of being : no truth is foreign to the physician.”

Bravo ! Let therefore the Lourdes “ miracles ” be investigated in the same spirit in which all phenomena of Nature should be. Let there be on one side due reverence, complete objectivity and readiness to account any conclusion warranted by adequate observation, even if such a conclusion were peculiarly unpalatable to the observers ; on the other side let there be willingness to acquiesce in all reasonable conditions and to admit that the investigators have the moral right to ask for criteria and tests of approximately the same character as they would in any other sphere.

Conducted on such lines as these a scientific enquiry into the Lourdes problem might be fruitful. Unfortunately I have my doubts as to the possibility of *such* an enquiry.

PEROVSKY-PETROVO-SOLOVOVO.

REPORT OF A SERIES OF EXPERIMENTS IN CLAIR-
VOYANCE CONDUCTED AT A DISTANCE UNDER
APPROXIMATELY FRAUD-PROOF CONDITIONS

- I. Introduction, and Description of the Technique Employed.
By Theodore Besterman.
- II. Statistical Analysis. By S. G. Soal.
- III. Theoretical Analysis, with Suggestions for further Research.
By Ina Jephson.

[*This paper is a sequel to Miss Jephson's paper on "Evidence for Clairvoyance in Card-guessing" published in Proceedings, vol. xxxviii. The objects of this further investigation were to repeat the experiment under stricter conditions in order to confirm, if possible, Miss Jephson's results and perhaps to discover one or two percipients with outstanding clairvoyant powers. The reader must be warned before troubling himself with details that the results have been entirely negative in both respects. The present experiments do not afford evidence of clairvoyance, nor do they exhibit what Miss Jephson called the "fatigue curve" in series of guesses (Proceedings, xxxviii. 230 ff.). This curve occurring as it did in the previous experiments with considerable regularity with different percipients independently, and agreeing as it seemed to do with the experiences of some experimenters in telepathy, appeared to confirm the view that there was some regular cause other than chance at work in producing the figures obtained. The Committee of Reference, when the present paper was referred to them, felt no doubt that the result should be reported to the Society, but they hesitated to recommend the Council, especially in view of the expense involved, to print in full detail this account of an experiment which had a purely negative issue. The conditions, however, have been thought out by the investigators with so much care, and the result so carefully analysed, that the Committee believe the account will in itself be of value to future investigators and moreover that as investigations into clairvoyance must go on, it is important to know in detail about conditions in which it does not appear to occur.—Hon. Ed.]*

I. INTRODUCTION, AND DESCRIPTION OF THE TECHNIQUE EMPLOYED

BY THEODORE BESTERMAN

Cards I tel you . . . they are parlous beasts.

—*Hay any Worke for Cooper*, f. iii v.

IN December 1928 Miss Ina Jephson published (*Proceedings*, xxxviii. 223-71) the results of a novel experiment in clairvoyance in the form of card-guessing. The conclusions which that experiment appeared to suggest were of considerable importance, but the percipients having been uncontrolled, various difficulties (described below by Mr Soal) arose. It seemed important, therefore, to repeat the experiment, making use of a more evidential technique. Mr Soal's long-distance experiments in telepathy coming to an end in February 1929, a valuable opportunity presented itself to make use of his numerous percipients for the purpose of repeating Miss Jephson's experiment. Sanction was obtained from the Council, and the investigation was undertaken by Miss Jephson, Mr Soal, and myself, jointly. The experimental technique devised for the purpose of this experiment is described in the present part of our Report. In the next part Mr Soal will deal with the scoring and with the statistical analysis of the results. Finally, Miss Jephson will analyse the results theoretically and make suggestions for further experiments.

It must first be explained that the experiments reported on in the present paper consist of five distinct series. The first, forming the main part of the experiment, produced 6,361 guesses. The second consisted of a small series, producing 200 guesses, which was conducted with the eight percipients in the first series whose scores presented positive deviations greater than $2 \times SD$. The third series was conducted with 95 members of the Boston S.P.R., producing 2,031 guesses. The fourth series was conducted by Professor Gardner Murphy at Columbia University with 157 percipients and produced 784 guesses. The fifth series was carried out by Professor J. C. Flugel with 24 students of University College, London, producing 120 guesses. For the sake of convenience and uniformity these five series will be referred to throughout this paper as the Joint, Supplementary, Boston, Murphy, and Flugel series, and Miss Jephson's original experiment will be referred to by her name. These particulars are tabulated in Table I.

TABLE I

Name of Experiment.	No. of Percipients.	No. of Guesses.
Joint	283	6,361
Supplementary	8	200
Boston	95	2,031
Murphy	157	784
Flugel	24	120
Total	567 ¹	9,496

Proceeding now to a description of the technique, it is necessary to emphasise again at the outset a point to which attention has already been drawn in the title of this paper : that the technique devised for these experiments does not purport to be strictly fraud-proof so far as the percipients are concerned ; nor do I believe that it would be possible to devise such a technique. What human ingenuity could devise, human ingenuity could circumvent. As in the classic illustration of this fact, the unsinkable battleship always goes to the bottom in the long run, and the irresistible gun is resisted. Our aim was not so much absolutely to prevent any possible fraud on the part of the percipients, as to prevent the more or less innocent forms of fraud due to thoughtlessness and to vanity, or to a misplaced sense of humour. It was also intended to make fraud so difficult as to make it probable that cases of successful fraud, if any, would be so few as not to affect the cumulative result. And I may as well say here, once and for all, that although we think we succeeded in doing this, we have no reason whatever to suppose that any of our percipients either wished or attempted to deceive us.

So far as the percipients were concerned, therefore, we were perforce content with conditions that were only approximately fraud-proof. But we introduced a novel feature into the experiments : we devised means to make them to all intents and purposes fraud-proof so far as the experimenters themselves, and their colleagues, were concerned. In ordinary scientific experiments the integrity of those conducting the investigation is assumed. Indeed, as Professor Sidgwick pointed out in his fourth Presidential Address,² it is taken so very much as a matter of course that the assumption passes unnoticed : it is nevertheless very real. A few cases are even on record in recent years where this assumption led to the acceptance of dishonest work.

¹In this total the eight percipients who took part in the Supplementary series of course appear twice.

²*Proceedings S.P.R.* (1884), ii. 155.

In psychical research the position is different. The whole subject matter of our researches is so doubtful and obscure, so widely questioned, that where results have been published capable of a normal explanation only by assuming the dishonesty of the investigator, that assumption has been made as the lesser of the two evils. A good deal could be said as to the propriety of such an attitude, but it is not my business to enter into that here. I have merely to explain that we thought it worth while to see whether it is possible to conduct such an investigation as the present one in such a way as to make it impossible for those conducting the experiment to fake the results. This we undoubtedly succeeded in doing, as will be seen below, but only at the cost of so much unnecessary work and misplaced ingenuity, that we should all be sorry to have to undergo the experience again.

An important element in the technique employed was that of the materials used, a point to which a good deal of thought and research were devoted. The main objective was to discover a comparatively fraud-proof and not too extravagantly costly method of sending playing-cards through the post to a considerable number of percipients in such a way that literally nobody should know what cards were being sent to the percipient.

It was assumed for the purpose of the experiment (since the possibility cannot be ruled out) that clairvoyance, if there is such a thing, obeys the laws of normal vision. The first thing to do, therefore, was to avoid anything that might interfere with the clear vision of a playing-card enclosed in an envelope, supposing that what takes place is that the card is seen through the envelope. Now a normal playing-card has a more or less elaborately coloured design on the back, and this would clearly prove a difficulty, an important object of these designs being in fact to prevent the cards being seen through while being played. A special supply of cards of strong quality and with blank backs was therefore obtained from the Universal Playing Card Company Ltd., Crown Point Works, Leeds.

The next point to be considered was that of the envelopes in which the cards were to be enclosed. It was necessary that it should not be possible, on the one hand, to open and reclose the envelope without leaving distinct traces, and, on the other hand, to see normally the playing-card while it was in the envelope. These two necessities were met jointly thus : (1) An envelope was used of such a size that the playing-card fitted into it exactly, giving it no free play at all ; (2) the envelope was made of a strong but rather coarse textured paper which offers a very good surface for adhesive purposes, and which cannot be rendered transparent by strong light, X-rays, water, alcohol, ether or the like ; (3) the envelopes used were blue, so that the colour would not interfere with, or suggest,



FIG. 1

The actual colour of the envelopes is rather darker and of the strips of paper rather lighter.

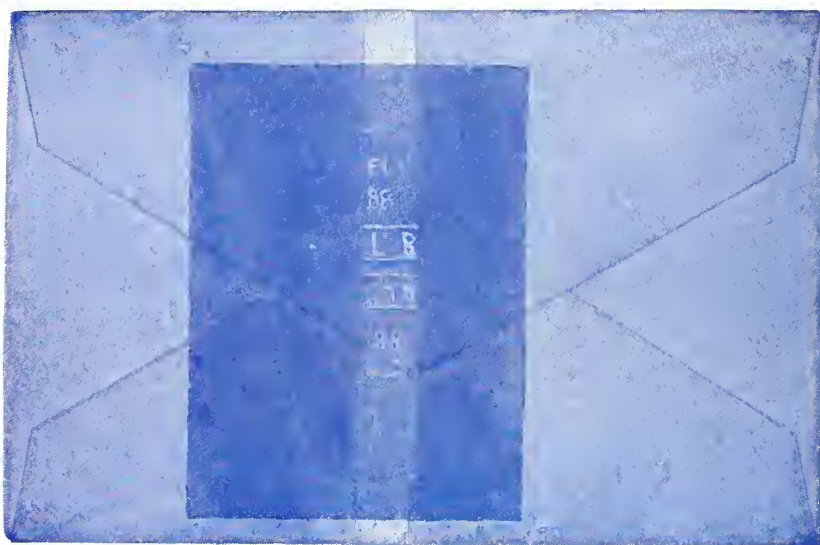


FIG. 2

The actual colour of the envelopes is rather darker and of the strips of paper rather lighter.

the red and black of the playing cards ; (4) round the envelope was placed by the manufacturers a strip of figured paper such as is used for cheques, the printing ink on such paper being liable to run if submitted to excessive moisture ; this strip began on the closed flap of the envelope and ran all round the envelope, projecting about half-an-inch over the open flap of the envelope (see Fig. 1) so that when closed the upper flap was pasted on to one end of the strip and the projecting end also came down on the strip. The colour of this strip was grey on white ; (5) when the envelope was closed a piece of dark blue tissue paper was gummed on to the back of the envelope, entirely covering the opening. This tissue paper shows the slightest moisture and cannot be removed without leaving traces ; (6) finally, the Society's relief stamp was impressed on the envelope, marking both sides of it and consequently the card contained in it (see Fig. 2). These envelopes with the strips attached, ready for closing, were manufactured for us by Messrs Alfred H. Atkins Ltd, Fetter Lane, London.

I now turn to the preparation of the envelopes for despatch. The empty envelopes having been delivered at the Rooms of the Society, Miss Jephson and I, with Miss Newton, Miss Horsell, and Miss Carruthers, assembled in the Secretary's Room. A quantity of playing-cards was placed face downwards on the table and the cards were placed by us face downwards in the envelopes, having been supplied already shuffled. The cards and envelopes were of course all alike and, to all intents and purposes, indistinguishable. When a sufficient number of envelopes had been thus filled, they were sent to Mudie's Bookbinding Department, New Oxford Street, London, and were there closed down with a specially prepared paste of a particularly adhesive quality. They were then returned to us. At this point already there was nobody who knew what any particular envelope contained, unless one or more of the five persons who placed the cards in the envelopes had been able, in the presence of the others, to make a secret mark on one or more envelopes and now had the opportunity (none of us in fact had) to search these out from the parcels of closed envelopes returned to us by Mudie's. The envelopes had been sent to Mudie's, and were returned by them, in no particular order. Even if the course described had been possible it would have been useless, as will be seen.

The next process was to stamp the envelopes with some identifying mark to enable us to keep track of them. After much discussion a combination of letters was decided on. Letters were preferred to numbers so as to avoid any possibility of the percipient being influenced in his guess by the number on the envelope. For the same reason the letter A (suggesting Ace) was avoided. The identifying marks thus were BB, BC . . . CB, and so on ; a number of three-

letter marks were also used as the two-letter combinations were exhausted. Each of these combinations was stamped on five envelopes, since a set of five guesses was to form the unit of the experiment. In order to preserve the identity and sequence of the envelopes in each set of five, they were distinguished by writing the letters v, w, x, y, z respectively, on the backs of the envelopes. At the same time a scoring slip for each set was stamped with its identification mark.

At this point the only person who may have known the contents of any of the envelopes was myself, for the whole of the stamping was done by me. I was therefore the only one who had the opportunity, while stamping the envelopes, to recognise marks which I might previously have made on them while placing the cards in them. This knowledge, however, would have been of no use to me unless I could now arrange for the marked envelopes or sets of envelopes to be sent to specific percipients with whom I should have arranged a conspiracy and whom I would then inform what cards were contained in the envelopes they were about to receive. (So far as I am aware I have not met any of Mr Soal's percipients except in the most casual way.) All this was rendered impossible.

The appropriate scoring slip, instructions, and a stamped envelope addressed to the Society, were placed with each set of five cards and safeguarded with an elastic band. In the meanwhile, a set of outer envelopes, in which the sets of five were to be sent through the post to the percipients, had been prepared by Miss Carruthers. These outer envelopes and the sets of five were then handed to Lieut.-Colonel Maxwell Dick, M.B., B.S., LL.B., who shuffled both the sets and the envelopes as he pleased, and who put the sets into the envelopes, in the presence of Miss Carruthers. At this stage, therefore, not only did nobody know, but nobody could have known, what card was contained in any envelope (assuming, of course, that there was no general conspiracy between the investigators, their colleagues, and Cols. Dick and Hayward, for the latter of whom see p. 384 below).

The instructions sent out with the first set were as follows (drafted, it will be realised, to interest the percipients) :

“ It has long been believed that there is some connection between the mental faculties of *Telepathy* and of *Clairvoyance* or second sight.

“ As our experiments in *Telepathy* are now coming to an end, the opportunity seems a favourable one for testing the theory that good telepathists are also good clairvoyants.

“ I am therefore asking for your kind help in a few simple experiments that can be carried out in your own time at any moment when you are free. We hope to begin the experiments as soon as possible after the Easter holidays.

“ Briefly, the scheme is this : Five sealed envelopes, marked V.W.X.Y.Z. [this should have been v.w.x.y.z.] will be sent to you, each containing an ordinary playing card. You will be asked not to open the envelopes but to return them with seals unbroken, together with a record of your impressions of what each hidden card is, within, say, the course of a week.

“ The experiment (which will not tie you down to any fixed times of concentration, as did the telepathy tests) will then be repeated with 5 fresh envelopes during the following week, until three or four such sets of results have been obtained.

“ Will you kindly let us know, when sending in your last set of Telepathy impressions, whether you are able to take part in this additional experiment ? We shall then send you detailed instructions [these were not found necessary]. *As it will be one of the most important of its kind ever attempted in Psychological Research, we shall greatly appreciate your help.*”

With the second to the fifth sets the following form was sent :

“ We are enclosing the [fourth] set of five envelopes for the Clairvoyant Experiment, and would be glad if you would kindly return them with your scoring slip and number as soon as possible.”

When the first sets had been returned, with the scoring-slips completed, in the addressed envelopes supplied to percipients, Colonel Maxwell Dick opened the outer envelopes, took out each set of blue envelopes, examined it, and called out the identification mark to Miss Carruthers, together with the identification number of the percipient. Miss Carruthers then stuck the completed scoring-slips, which had been prepared with a gummed edge, on to the record sheets prepared for each percipient. In this way the original record made of his own guesses by the percipient was preserved and formed the basis of the subsequent scoring (Fig. 3).

The sets of envelopes (except a few disqualified because of damage in the post) were then added to the remaining stock, from which the second week's set had in the meanwhile been sent out. The same procedure was followed for the second, third, fourth and fifth sets of five. When the great majority of the fifth set of five envelopes had been returned by the percipients the time came to open the blue envelopes (the contents of which were of course still unknown) and to enter the actual card against the guesses made by the percipients on their scoring-slips. The opening of the envelopes was conducted with elaborate precautions. As Colonel Dick opened each envelope and called out the card contained in it he was watched and checked by one of the investigators (who took turn and turn about in this and the other duties I am describing). The entering of the cards so called out was done by Miss Carruthers, under similar supervision. In this way, humanly speaking, no error can have been made.

Name: <i>(Name and address omitted)</i>		<u>98.</u>	
Address			
Date	Impressions	Remarks, etc.	Time
Number <i>98</i> Date <i>Sept 27th 1929</i> P X CARD SCORE		Number <i>98</i> Date <i>Oct 5th 1929</i> R E CARD SCORE	
Number <i>98</i> Date <i>Friday - Oct 11th 1929</i> G. J. CARD SCORE			
v Seven of Clubs <i>Ace Hearts</i>	v Three of Diamonds <i>2 Clubs</i>	v Knave of Diamonds <i>5 Spades</i>	<small>(Office use only)</small>
w Four of Diamonds <i>5 Diamonds</i>	w Nine of Diamonds <i>Queen Hearts</i>	w Ten of Clubs <i>King Hearts</i>	<small>(Office use only)</small>
x Knave of Spades <i>Queen Hearts</i>	x Two of Spades <i>4 Hearts</i>	x Ace of Hearts <i>Queen Clubs</i>	<small>(Office use only)</small>
y Three of Hearts <i>4 Hearts</i>	y Knave of Hearts <i>3 diamonds</i>	y Two of Spades <i>2 diamonds</i>	<small>(Office use only)</small>
z Queen of Hearts <i>6 Spades</i>	z Ace of Clubs <i>Queen Clubs</i>	z Seven of Diamonds <i>9 diam.</i>	<small>(Office use only)</small>
TIME Before Guessing First Card <i>2 o'clock P.M.</i>	TIME After Guessing Last Card. <i>2.25 P.M.</i>	TIME Before Guessing First Card <i>5.25 P.M.</i>	TIME After Guessing Last Card. <i>5.40 P.M.</i>
TIME Before Guessing First Card <i>6.43 P.M.</i>	TIME After Guessing Last Card. <i>6.58 P.M.</i>		
Number <i>98</i> Date <i>Oct 17th 1929</i> F. L. CARD SCORE		Number <i>98</i> Date <i>Oct 24th 1929</i> D. S. CARD SCORE	
v Two of Spades <i>Jacks Clubs</i>	v Two of Clubs <i>5 Spades</i>	v Seven of Diamonds <i>8 Clubs</i>	<small>(Office use only)</small>
w King of Hearts <i>3 Clubs</i>	w Knave of Spades <i>6 Hearts</i>	w Eight of Spades <i>8 Clubs</i>	<small>(Office use only)</small>
x Seven of Clubs <i>10 Hearts</i>	x Ace of Hearts <i>5 Hearts</i>		
y Five of Hearts <i>6 Clubs</i>			<small>(Office use only)</small>
z Nine of Diamonds <i>5 Hearts</i>			<small>(Office use only)</small>
TIME Before Guessing First Card <i>2.45 P.M.</i>	TIME After Guessing Last Card. <i>2.58.</i>	TIME Before Guessing First Card <i>2.55 P.M.</i>	TIME After Guessing Last Card. <i>3.7 P.M.</i>

P.T.O.

FIG. 3

* ¹ These headings have no significance, forms left over from a previous experiment having been used.

When each envelope had been opened and the card contained in it recorded, the card was replaced in the envelope and the opened envelopes with the cards in them were placed in a strong-box, locked by Colonel Dick and deposited by him at the Westminster Bank, Tavistock Square, where only he, accompanied by one of the investigators, had access to it. There the envelopes and cards will remain until the publication of this report, after which they will be preserved in the Rooms of the Society.

When the whole of the percipients' sheets had been completed so far as possible, from them were prepared, with the same elaborate precautions, duplicate sets, independently compiled and checked, of records showing the history of each set of five envelopes. A section of these records is reproduced in Table II, in the third column of which are shown the percipients who received the sets; thus, QO was sent to percipient no. 519 the first week, to no. 120 the second week, and so on. It will be seen also that percipient no. 529 received set QP the third week, and set QQ the fifth week.

TABLE II

QO v	5 Hearts	519
w	8 clubs	120
x	2 clubs	153
y	6 hearts	8
z	6 spades	424
QP v	jack diamonds	175
w	2 diamonds	372
x	7 spades	529
y	7 hearts	122
z	8 spades	62
QQ v	6 hearts	335
w	5 diamonds	568
x	jack clubs	478
y	6 diamonds	283
z	10 spades	529

Finally, each operation being again supervised and checked, the completed sheets were dictated to a typist. These typed records were prepared in three copies, which were independently scored by the investigators, as described by Mr Soal below. The appropriate part of one of these sets was eventually sent to each percipient. This completed the actual work of the experiment. A specimen record, that of the percipient shown on Fig. 3, completely scored, is shown in Table III.

TABLE III

No. of Percipient.	Card guessed.	Actual Card.	Class.	Score.	Total.
98	7e	1h	OR	3.78	61.71
	4d	5d	SR	19.89	
	J _s	Q _h	POR	16.45	
	3h	4h	SR	19.89	
	Q _h	6s	POO	1.70	
	3d	2c	OR	3.78	41.87
	9d	Q _h	OC	8.79	
	2s	4h	OR	3.78	
	J _h	3d	POC	8.67	
	1e	Q _c	OS	16.85	
	J _d	5s	POO	1.70	
	10e	K _h	OO	.73	
	1h	Q _e	OO	.73	
	2s	2d	ON	30.54	
	7d	9d	SR	19.89	
	2s	J _e	OC	8.79	53.59
	K _h	3e	POO	1.70	
	7c	10h	OR	3.78	
	5h	6c	OR	3.78	
	9d	5h	CR	11.84	
	2e	5s	CR	11.84	
	7d	2c	OR	3.78	
	J _d	6h	POC	8.67	
	8s	8c	NC	38.60	
	1h	5h	SR	19.89	

Mean Score = 10.79

Total = 269.84

The same procedure was followed with the Boston series, with two exceptions: (1) the sets of five, instead of being sent individually to each percipient, were sent in bulk, already prepared and stamped for the post (with U.S. stamps previously obtained), to Boston, where they were put in the post; and (2) that in the later stages of the work of examining the envelopes, etc., Colonel Diek was obliged by pressure of other duties to give up this extremely lengthy, tedious and laborious work, and was replaced by Lieut.-Colonel W. D. Hayward, M.B. The Society's best thanks are due to these two gentlemen, who put in a vast amount of work of the dullest and most

boring kind imaginable, with the utmost conscientiousness and with unrelaxing attention. The exhaustive reports prepared by Col. Dick on his work are available for inspection in the Rooms of the Society.

The conduct of the Murphy series can be easily gathered from the following instructions, which were prepared by Professor Murphy :

1. Each subject receives a pack of 5 blue envelopes, held together by an elastic.
2. The subjects are informed that each envelope contains a card from an ordinary deck of 52 playing cards. Removing the elastic, each envelope is to be held for a moment by the subject, who is told simply to make a random guess as to what card it contains and to record the guess, *immediately*, on the front face of the envelope. The cards are to be guessed in the order in which they come ; and it is important that, when putting on the elastic again, they make doubly sure that the envelopes are in the order in which they were received.
3. If the subjects take more than 30 seconds to do the series of guesses, hurry them up, telling them "not to go into a trance, but to *guess at random*."
4. When the guesses are all recorded, each subject returns his pack of 5, in the right order with the elastic on it, to the instructor.

To this need only be added that the blue envelopes in this series were separately obtained by Professor Murphy, and were not the same as those used in the Joint, Supplementary, Boston, and Flugel series.

The Flugel series was conducted in the same way as that of Professor Murphy, except that our own blue envelopes were used.

II. STATISTICAL ANALYSIS

BY S. G. SOAL

We must speak by the card or equivocation will undo us.

—*Hamlet*, V. i.

1. *Introduction.* The Jephson experiment produced some interesting results. The deviations from the expected numbers in the categories of Colour, Suit and Value were so great as to make it certain that some factor other than chance had operated in favour of successful guessing. The following table gives a brief summary of these important deviations.

TABLE IV
[No. of guesses = 6,000.]

	Actual Number.	Number expected.	Deviation from Expected Number.	Standard Deviation.	Deviation ÷ Standard Deviation.
<i>Colour</i> $p=1/2$ Guessed correctly	3307	3000	+ 307	±39	nearly 8
<i>Suit</i> $p=1/4$ Guessed correctly	1832	1500	+ 332	±33	nearly 10
<i>Value</i> $p=1/13$ Guessed correctly	664	461	+ 203	±21	nearly 10

It will appear from the above table that the actual deviations approximate to 8 and 10 times the theoretical standard deviations and three factors of this magnitude are conclusive as regards the existence of an extra-chance influence or influences. We have therefore either to postulate the existence of a widespread faculty of clairvoyance possessed by a large percentage of the human race and hitherto unnoticed by science or else to envisage alternative explanations. These alternatives readily suggest themselves. The large majority of the persons who performed the Jephson experiment did so in their own homes using their own cards and under no supervision by the experimenter.

We may readily envisage one or more of the following possibilities operating in favour of successful results. We may suppose :

(a) A considerable proportion of the percipients failed to carry out Miss Jephson's instructions. Instead of making and recording the five guesses prescribed for each sitting these percipients performed many more than five, and either selected their best guesses or waited till a run of success appeared before recording their five guesses.

(b) A considerable percentage of the percipients used cards whose backs were worn or soiled, and of such a nature as to lead to unconscious associations of the face values with slight markings on the backs.

(c) Careless manipulation of the cards coupled with hyperaesthesia of vision enabled certain percipients to glimpse and partially or wholly to read the faces of the cards.

(d) Deliberate faking of results on the part of percipients.

I do not for a moment suggest that (d) operated to any serious extent. I merely envisage it as a theoretical possibility. I would

indeed be the last to take such a lamentable view of human nature and I think it far more probable that conditions (a), (b) and (c) prevailed in varying degrees. After reading Miss Jephson's paper I felt it urgent that her experiment should be repeated under conditions of control which would definitely exclude possibilities (a), (b) and (c), and render (d) at least difficult. As described by Mr Besterman, an opportunity to do this soon occurred and the present report is the result.

2. *System of scoring.* This system of scoring adopted throughout the present series of experiments is based on the revised scheme given by Dr Fisher in an Appendix to Miss Jephson's first paper (*Proceedings*, xxxviii. 269-271) thus :

TABLE V

<i>Plain Card Said.</i>		<i>Picture Card Said.</i> [King, Queen or Knave]	
<i>Category</i>	<i>Score</i>	<i>Category</i>	<i>Score</i>
OO	.73	POO	1.70
OR	3.78	POR	16.45
OC	8.79	POC	8.67
CR	11.84	PCR	23.42
OS	16.85	POS	15.64
SR	19.89	PSR	30.39
ON	30.54	PON	27.50
NC	38.60	PNC	34.47
NS	46.65	PNS	41.44

The individual scores are so adjusted that the theoretical Mean of n guesses is 11.18 and the Standard Deviation of the Mean of n guesses from 11.18 is $\frac{10}{\sqrt{n}}$.

Thus the Standard Deviation of the Mean Score of 25 guesses $= \frac{10}{\sqrt{25}} = 2$ and the theoretical distribution is approximately normal for $n \geq 25$.

With a set of 25 guesses the Mean Score would by chance alone fall outside the limits (7.18 - 15.18) (*i.e.* 11.18 ± 4) about once in 20 sets.

3. *Method of Scoring.* The 6,361 guesses in the Joint experiment were scored by myself between August and October 1929. They were *independently* scored under Miss Jephson's direction, and about two-thirds of the whole were also *independently* scored by Mr

Besterman. Each guess therefore has been scored independently at least twice. On 17 September, by which date about 6,168 guesses were available, the Committee met to compare scores and totals. Each experimenter in turn read aloud the card categories, scores, totals of 5 and complete totals from his set of the typewritten sheets described by Mr Besterman (Table III), the two remaining experimenters following his reading from their own slips and stopping him at any discrepancy. In this way any mistakes that had been made were corrected. Between 17 and 25 September a few more sets of envelopes were returned and these were scored by all three experimenters at a Committee meeting held on 25 September. A few odd sets continued to arrive at intervals until 30 October, by which date the whole of the 6,361 Joint guesses had been scored.

4. *Incomplete and Multiple Guesses.* In the very rare cases in which a percipient gave two guesses for the same card the score was allocated to the *first* of the two guesses in order of recording, the second guess being ignored whether it was better than the first or not.

In the case of a guess that was incomplete we made it a rule to award the *lowest* score that was consistent with the data at our disposal

Thus the following examples are scored as shown :

TABLE VI

Card guessed.	Actual Card.	Class.	Score.
diamond . . .	10h	POC	8.67
knave . . .	3d	POO	1.70
ace . . .	Kc	OO	.73
red ace . . .	3h	CR	11.84

44 such incomplete guesses occurred among the 6,361 Joint guesses. It will be observed that in her analysis of card preferences Miss Jephson quotes the total of the Joint guesses as 6,317. Since Miss Jephson is only able to deal with *complete* guesses in her section of the work, the difference between 6,361 and 6,317 is thus fully accounted for by the 44 incomplete guesses.

5. *The Scoring of the Boston Guesses.* The scoring of the 2,031 guesses produced by the Boston experiment was done under Miss Jephson's direction. A set of the typewritten slips bearing the scores and totals was put into my hands and I have checked all the scores, partial totals of 5 and page totals as also the grand total score. These scores and totals have also been checked by Miss Jephson herself. The counting of the number of guesses which fall

within the categories of Colour correct, Suit correct, Value correct and Card correct has been done independently by Miss Jephson's assistant and myself, an agreement being arrived at for each category.

6. *Analysis of the Joint Experiment.* 283 persons took part in this experiment; 276 of these were residents of the British Isles who during the previous nine months had taken part in my mass experiments in long-distance telepathy. The remaining seven were members of M. René Warcollier's group in experimental telepathy and all of them had also acted as percipients in experiments with my own group.

A considerable percentage of the 276 British percipients had in September 1928 written letters to the S.P.R. in which they claimed to have had spontaneous experiences in telepathy or clairvoyance or to have been successful in card-guessing. A small number of the percipients claimed to be automatic writers or spirit mediums.

The 283 sets of guesses sent in may be classified as follows :

TABLE VII

178 <i>complete</i>	sets of	25	guesses each	=	4450	guesses
25 <i>incomplete</i>	, ,	24	, ,	=	600	, ,
6	, ,	23	, ,	=	138	, ,
1	, ,	22	, ,	=	22	, ,
3	, ,	21	, ,	=	63	, ,
35	, ,	20	, ,	=	700	, ,
6	, ,	19	, ,	=	114	, ,
5	, ,	15	, ,	=	75	, ,
3	, ,	14	, ,	=	42	, ,
1	, ,	13	, ,	=	13	, ,
8	, ,	10	, ,	=	80	, ,
1	, ,	9	, ,	=	9	, ,
11	, ,	5	, ,	=	55	, ,
<hr/>					<hr/>	
283 sets					6361 guesses	

Total number of guesses	=	6361
Total score on Fisher's System	=	70832·08
Theoretical Mean Score	=	11·18
Actual Mean Score	=	11·14
Deviation	=	-·04
Standard Deviation from		
11·18 for 6361 guesses		

$$= \frac{10}{\sqrt{6361}} = \cdot 12$$

Hence *Actual Deviation* = one-third of *Standard Deviation*.

Result is therefore negative and there is no evidence of the existence of any extra-chance factor.

7. *Analysis of Colour, Suit, Value and Rank.* We next proceed to inquire if there has been any general ability to detect the Colour, Suit, or Value of the card in the envelope or to recognise that the card is a picture. The theoretical number of cases of Colour correct, Suit correct, Value correct, and Card absolutely correct will be obtained by multiplying the number of guesses (6,361) by $1/2$, $1/4$, $1/13$ and $1/52$ respectively.

Now, although complete packs of cards were used in filling the envelopes it so happened that since certain percipients were more punctual than others in returning their envelopes some sets of 5 were sent out to percipients more often than others. Taking as our basis for calculation the numbers of "actual" cards occurring, under each of the 13 values in Miss Jephson's Table¹ we find that there is a distinct excess of *kings* and a deficiency of *queens*. Each "actual" card ought theoretically to occur about 486 times with a standard deviation from this mean of ± 21 . The numbers of all the values except those of *king* and *queen* fall within the limits $486 \pm 2 \times 21$. But the *king* occurs 569 times and the *queen* 441 times.

The total number of actual cards of Red colour = 3179.

The total number of actual cards of Black colour = 3182.

The difference between Red and Black is so small that we may safely use the factor $p = 1/2$ in estimating the expected number of colour correct.

Again the expected number of each suit among the "actual" cards is about 1590 with a standard deviation = ± 34 .

A glance at Miss Jephson's Table shows that the numbers for the different suits all fall within the limits $1590 \pm 2 \times 34$. We may therefore without serious error take $p = 1/4$ in estimating the expected number of cards with suit correct.

The expected number of cases in which the "guess" and "actual" cards are both picture cards would theoretically be found by multiplying the total number of picture or *P*-guesses by the factor $3/13 = .2308$.

Owing to the fact that the number of actual picture cards is slightly in excess of the theoretical number I have thought it best not to use this factor but to count the number of actual picture cards and find the ratio this number bears to the total number of actual cards.

¹This Table is not printed in the present paper, but will be available at a later date, when certain investigations connected with it are completed.

I find :

Number of " actual " picture cards = 1524

Theoretical number = $\frac{3}{13} \times 6361$ = 1468

I therefore take as my factor $p = \frac{1524}{6361} = .2396$

Now actual number of *P*-guesses
recorded by percipients = 1487

Hence the expected number of cases in which both the " guess card " and the " actual card " are pictures = $1487 \times .2396 = 356$.

The Standard Deviation for each category is calculated from the formula

$$\text{S.D.} = \sqrt{Np(1-p)}$$

where N^* = total number of guesses = 6361 and $p = 1/2, 1/4, 1/13, 1/52$, in the case of Colour, Suit, Value and " Card Correct " respectively.

TABLE VIII. [Joint Experiment]

Number of Guesses = 6361.

	Actual Number.	Expected Number.	Deviation.	Standard Deviation.
Number of guesses with <i>Colour</i> correct	3218	3180	+ 38	± 40
Number of guesses with <i>Suit</i> correct	1609	1590	+ 19	± 34
Number of guesses with <i>Value</i> correct	466	489	- 23	± 21
Number of guesses entirely correct	122	122	± 0	± 11
Number of cases in which card is <i>correctly</i> divined to be a <i>Picture</i>	343	356	- 13	± 16

A glance at the last two columns of the above table shows that no one of the deviations even approaches double the corresponding standard deviation. In fact the highest deviations do no more than barely exceed their standard deviations. The results therefore are totally negative as regards any supposed faculty of clairvoyance.

8. *Distribution of Scores of Complete Sets.* Of the 178 complete sets of 25 guesses there were eight sets with mean scores exceeding

* For the expected number of " Picture Card detected " guesses N = total number of *P*-guesses = 1487.

15·18, *i.e.* with positive deviations greater than $2 \times \text{S.D.}$ Only two complete sets had mean scores of lower value than 7·18, *i.e.* negative deviations exceeding $2 \times \text{S.D.}$

There are therefore 10 sets with mean scores whose deviations, positive or negative, exceed twice the standard deviation. But the expected number of such sets = $178 \times \cdot 0455 = 8$. Hence the actual number of remarkable scores is only slightly in excess of the theoretical number. The table which follows gives the details of these exceptional scores.

TABLE IX

Identity number of Percipient.	Mean Score.	Remarks.
623	15·32	Percipient claimed to have had pre-visual dreams and telepathic experiences.
98	15·46	
233	15·46	Percipient claims to have had experience of clairvoyance, prevision and psychic ability to diagnose disease.
256	15·47	Percipient believes himself to be without any psychic faculties. Lays claim to no special faculties or qualifications except honesty. Approximates to $3 \times \text{S.D.}$
319	16·02	
366	16·77	
112	17·01	
118	17·96	
200	7·15	Apparently has had no psychic experiences but interested in scientific research.
718	6·79	Claims to have experimented successfully with telepathy under his own roof.

We note that only one percipient (no. 118) obtains a positive deviation exceeding 3 times the standard deviation, but another (no. 112) obtains a deviation just less than $3 \times \text{S.D.}$

Theoretically we should expect such a deviation (exceeding $3 \times \text{S.D.}$) to occur on an average once in 370 complete sets. It may well be therefore that the one deviation expected in the 370 sets has

happened to fall in the first 178 of these and the result cannot be considered in any way remarkable or significant.

We may note in passing that percipient no. 112 guessed colour correctly 22 times out of 25, suit correctly 15 times and value correctly only once. No card was absolutely correct. He clearly scored on colour and suit.

Percipient no. 118 on the other hand obtained nothing remarkable as regards colour or even suit, which were guessed accurately 14 times and 9 times respectively. But no. 118 guessed the value correctly 6 times out of 25 including 2 guesses which were entirely correct (NS).

9. *Determination of the Observed Mean and Observed Standard Deviation for the Complete Sets.* In addition to the 178 complete sets of 25 there were 25 "nearly complete" sets of 24 and 6 "nearly complete" sets of 23. An incomplete set of 24 often arose through the percipient writing "Joker" as a guess and such a guess of course could not be scored. In the work which follows I have affiliated these "nearly complete" sets of 24 and 23 to the complete sets of 25 thus obtaining $178+25+6=209$ sets of which I analyse the distribution of scores. I therefore assume for the theoretical standard deviation of each set a "weighted mean" $\sigma=2.01$, which has been computed on statistical principles.

By the usual methods of statistical analysis I find for the 209 sets

<i>Observed Mean</i> of a single set	= 11.12
<i>Observed Standard Deviation</i> σ'	= 1.97
Hence we have <i>Theoretical Mean</i>	= 11.18
<i>Observed Mean</i>	= 11.12
<i>Theoretical Mean - Observed Mean</i>	= .06

Standard Deviation of the Mean = $\frac{\sigma}{\sqrt{N}}$ where

N = number of sets = 209 and σ = theoretical standard deviation of the mean of a set = 2.01

Hence S.D. of Mean = $\frac{2.01}{\sqrt{209}} = .14$

Hence $\frac{\text{Theoretical Mean} - \text{Observed Mean}}{\text{Standard Deviation of Mean}} = \frac{.06}{.14} = \frac{1}{2}$ approximately.

Hence there is no significant difference between the Observed Mean and the Theoretical Mean.

Again we have <i>Theoretical S.D.</i> = σ	= 2.01
<i>Observed S.D.</i> = σ'	= 1.97
<i>Hence</i> $\sigma' - \sigma$	= -0.04

And for a normal distribution in which the number of sets N is large we have

$$\text{Standard Error of a Standard Deviation} = \frac{\sigma}{\sqrt{2N}}$$

$$\begin{aligned} \text{Hence the Standard Deviation of } \sigma &= \frac{2.01}{\sqrt{2 \times 209}} \\ &= .098 \end{aligned}$$

$$\text{Hence } \frac{\sigma' - \sigma}{\text{Standard error in } \sigma} = \frac{-.04}{.098} = .4 \text{ (nearly)}$$

Hence there is no significant difference between the theoretical Standard Deviation and the value obtained by experiment.

We find in fact that the experimental distribution of mean scores coincides very closely with the theoretical distribution based on chance expectation. To save unnecessary expense of printing the details of the above calculations have been omitted, but copies of the tables connected with them will be lent to any person who wishes to study them.

10. *The Further Testing of Percipients with High Scores.* Although the number of complete sets with scores exceeding 15.18 did not seriously differ from the chance expectation, it was clearly advisable that all percipients with scores greater than 15.18 should be asked to submit to an additional test of their powers. With one exception the first eight percipients mentioned in Table IX each performed an extra set of 25 guesses. The same technique was used as in the main experiment. As before the experiment was spread over a period of 5 weeks, each percipient receiving five sealed envelopes per week.

Percipient no. 366 was not tested because the last set completing his original 25 guesses did not arrive till the new experiment was well under way. His total score in fact was discovered too late for him to be included with the others.

Table X gives the mean scores of this new experiment and for comparison the original high scores. It will be seen that not one of the seven percipients gets a score approaching even 14. We are thus confirmed in our view that these original high scores were merely the extreme fluctuations of chance to be expected when a sufficiently large number of sets is examined.

11. *Additional Investigation.* It has been suggested that if the faculty of clairvoyance bore any analogy to an imperfect visualisation of the pips of the cards then it might happen that, although the percipient was unable to obtain an exact impression of the number of pips, he yet might be able to distinguish vaguely between a card with many pips and one with few pips. He might, for instance,

mistake an ace for a two or a three, a four for a five, or an eight for a ten. The ordinary scoring system would of course give no credit for such approximate guesses. To test this hypothesis I have constructed four tables taking as my basis the 6168 joint guesses which were available by 17 September 1929. These Tables show (a) the number of times each "Actual Card" occurs for each guess value, (b) the *expected* number of times each actual card should occur for each guess value, (c) the standard deviations from expected numbers for each "actual card" value and (d) the deviations for each actual card value from the expected number. Each table contains 169 entries.

TABLE X
[Supplementary Series.]

Identity No. of Percipient.	Original Mean Score in Main Experiment.	Mean Score in New Experiment.
98	15.46	10.79
112	17.01	11.95
118	17.96	11.85
221*	15.10	11.94
233	15.46	12.73
256	15.47	9.27
319	16.02	13.03
366	16.77	Not tested
623	15.32	11.97

A careful study of these tables warrants me in asserting that the present series of experiments show no evidence of any systematic tendencies to mistake certain cards for certain other cards or to approximate to the number of pips on a card.

The actual tables and analysis are, to save expense, not printed, but may be examined by any person interested in the question.

12. *Analysis of the Boston Experiment.* Ninety-five persons took part in the experiment and the total number of guesses was 2031.

* No. 221 was tested again since his original score was only slightly less than 15.18.

These may be classified as follows :

TABLE XI

52 complete	sets of	25 guesses	each =	1300 guesses	
5 incomplete	„ „	24 „ „	=	120 „	„
1 „	set „	23 „ „	=	23 „	„
18 „	sets „	20 „ „	=	360 „	„
5 „	„ „	19 „ „	=	95 „	„
4 „	„ „	15 „ „	=	60 „	„
1 „	set „	14 „ „	=	14 „	„
1 „	„ „	11 „ „	=	11 „	„
2 „	sets „	10 „ „	=	20 „	„
4 „	„ „	5 „ „	=	20 „	„
2 „	„ „	4 „ „	=	8 „	„
95 sets			Total	2031 guesses.	

Total Number of guesses	= 2031
Total Score on Fisher's System	= 22817·65
Actual Mean Score	= 11·23
Theoretical Mean Score	= 11·18
Deviation	= +·05

Standard Deviation from
11·18 for 2031 guesses

$$= \frac{10}{\sqrt{2031}} = \cdot 22$$

Hence *Actual Deviation* = 1/4 of *Standard Deviation* (approx.).

The result is therefore negative and there is no evidence of the existence of any extra-chance factor.

13. *Analysis of Colour, Suit, Value and Rank.* As in the case of the Joint series I have calculated the theoretical number of cases with Colour correct, Suit correct, Value correct and Card absolutely correct by multiplying 2031, the total number of guesses, by 1/2, 1/4, 1/13 and 1/52 respectively. The number of "actual" picture cards was counted and found to be 478 as against $2031 \times 3/13 = 469$, the expected number. Now $478/2031 = \cdot 2353$ and as the number of picture guesses recorded by the percipients = 515 we see that the expected number of cases in which both the "card said" and the "actual card" are pictures = $515 \times \cdot 2353 = 121$. We thus obtain Table XII.

Table XII shows that none of the deviations attain even to the standard deviation and there are no indications of any ability to detect any of the characteristics in question.

TABLE XII

	Actual Number.	Expected Number.	Deviation.	Standard Deviation.
Number of guesses with <i>Colour</i> correct	1022	1015	+7	±23
Number of guesses with <i>Suit</i> correct	489	508	-19	±20
Number of guesses with <i>Value</i> correct	154	156	-2	±12
Number of guesses <i>entirely</i> correct	41	39	+2	±6
Number of cases in which card is correctly divined to be a <i>picture</i>	130	121	+9	±10

14. *Combined results of Joint and Boston Series.*

TABLE XIII

Total Number of Guesses	= 8392
Total Score	= 93649.73
Actual Mean Score	= 11.16
Theoretical Mean Score	= 11.18
Deviation from 11.18	= -.02
Standard Deviation from 11.18 of 8392 guesses	$= \frac{10}{\sqrt{8392}} = .11$

Hence *Actual* deviation = about 1/5 of Standard Deviation. Hence for the combined series the result is negative.

15. *Analysis of Combined Series for Colour, Suit, Value, Rank.*

A glance at Table XIV shows that in the combined series none of the deviations are significant.

16. *Distribution of Scores in the Combined Series.* In the combined series we have

230 complete sets of 25 guesses
30 "nearly complete" sets of 24 guesses
7 " " " " 23 " "
<u>Total 267 sets</u>

TABLE XIV (8392 guesses.)

	Actual Number.	Expected Number.	Deviation.	Standard Deviation.
Number of guesses with <i>Colour</i> correct	4240	4196	+ 44	±46
Number of guesses with <i>Suit</i> correct	2098	2098	0	±40
Number of guesses with <i>Value</i> correct	620	646	- 26	±24
Number of guesses entirely correct (NS)	163	161	+ 2	±12
Number of cases in which card is <i>correctly</i> divined to be a <i>picture</i>	473	477	- 4	±19

We take as our theoretical Standard Deviation a "weighted" standard deviation computed to be $\sigma=2.01$, and proceed to find the observed values of the mean and standard deviations of a single set.

$$\text{We find Observed Mean} = 11.14$$

$$\text{Observed Standard Deviation} = 2.03$$

$$\text{Hence Theoretical Mean} - \text{Observed Mean} = 11.18 - 11.14 = .04.$$

$$\text{And Standard Deviation of Mean} = \frac{2.01}{\sqrt{267}} = .12.$$

$$\text{Hence } \frac{\text{Theoretical Mean} - \text{Observed Mean}}{\text{Standard Deviation}} = \frac{.04}{.12} = \frac{1}{3}$$

$$\text{Again Observed S.D.} - \text{Theoretical S.D.} = 2.03 - 2.01 = .02.$$

$$\text{And Standard Error of the Standard Deviation} = \frac{2.01}{\sqrt{2 \times 267}} = .09.$$

$$\text{Hence } \frac{\text{Observed S.D.} - \text{Theoretical S.D.}}{\text{Standard Error of S.D.}} = \frac{.02}{.09} = \frac{1}{4} \text{ (approx.).}$$

Hence for the combined series the theoretical mean and Standard Deviation agree very closely with the experimental values of these quantities and the actual distribution is found to agree with that expected on the assumption of chance. The details of the above computations may be examined by any person who wishes to see them.

17. *Note on Exceptional Scores [Boston Series].* Out of the 52 complete sets of 25 there were three sets with mean scores less than 7.18 and three sets with mean scores exceeding 15.18 making in all six sets with positive or negative deviations numerically exceeding $2 \times \text{S.D.}$ The number of such sets on the basis of chance expectation is about 3. These exceptional scores are shown below.

TABLE XV
(Boston Series)

Identity Number of Percipient.	Mean Score.
944	15.95
1002	15.29
1114	16.15
915	6.97
967	7.10
1111	6.75

There is actually an excess of such sets as compared with chance expectation. We have not however made further tests with these few percipients.

18. *The Murphy Series.* In this series Professor Murphy informs us 157 percipients took part. The total number of guesses obtained was 784, and the percipients with one exception did five guesses each. These 784 guesses have been scored under Miss Jephson's direction with the following results :

TABLE XVI

Number of Guesses	=	784
Number of Percipients	=	157
Total Score	=	8150.92
Mean Score	=	10.40
<i>Theoretical Mean Score</i>	=	11.18
Deviation from 11.18	=	-.78
Standard Deviation for 784 guesses		
	=	$\frac{10}{\sqrt{784}} = .36$

In this case the *actual* deviation just exceeds *twice* the standard deviation, but as this deviation is negative it is not evidence of any positive clairvoyance.

In Table XVII we analyze the separate characteristics of this series.

TABLE XVII
(Total = 784 guesses.)

	Actual Number.	Expected Number.	Deviation.	Standard Deviation.
Number of guesses with <i>Colour</i> correct	371	392	- 21	±14
Number of guesses with <i>Suit</i> correct	177	196	- 19	±12
Number of guesses with <i>Value</i> correct	50	60	- 10	±7
Number of guesses with card entirely correct	12	15	- 3	±4
Number of cases in which card is correctly divined to be a <i>picture</i>	48	56	- 8	±6.6

19. *The Flugel Series.* Finally a series of 120 guesses was obtained by Professor Flugel. In this experiment 24 percipients took part, each percipient making 5 guesses. These guesses were scored under Miss Jephson's direction and the results were as follow :

TABLE XVIII

Number of Guesses	=	120
Number of percipients	=	24
Total Score	=	1291.86
Mean Score	=	10.76
<i>Theoretical Mean Score</i>	=	11.18
Deviation from 11.18	=	- .42
Standard Deviation for 120 guesses	=	±.91

Hence results are negative.

Table XIX shows the analysis of Colour, Suit, Value and Rank for this experiment.

Again it will be seen that the results tally closely with the chance expectations. There were in this series no outstanding sets of five.

TABLE XIX
(Total=120 guesses.)

	Actual Number.	Expected Number.	Deviation.	Standard Deviation.
Number of guesses with <i>Colour</i> correct	61	60	+1	±5·5
Number of guesses with <i>Suit</i> correct	24	30	-6	±5·0
Number of guesses with <i>Value</i> correct	10	9	+1	±3·0
Number of guesses with card entirely correct	2	2	±0	±1·0
Number of cases in which card is divined correctly to be a <i>picture</i>	5	7	-2	±2·5

20. *Total Score for All Five Experiments.* We give in this section the total score for all the guesses at our disposal. We have :

TABLE XX

Series.	Guesses.	Score.
(a) Joint	6361	70832·08
(b) Supplementary	200	2338·49
(c) Boston	2031	22817·65
(d) Murphy	784	8150·92
(e) Flugel	120	1291·86
Total	9496	105431·00

Total number of Percipients who took part	=	559
Total Score for 9496 guesses	=	105431·00
Mean Score	=	11·10
<i>Theoretical Mean Score</i>	=	11·18
Deviation from 11·18	=	-·08
Standard Deviation	=	±·10

Hence the actual deviation is just less than the Standard Deviation and the result of the combined series must be considered negative.

21. *Conclusion.* The present series of experiments provide no confirmation of the hypothesis that the faculty of clairvoyance is a

normal faculty possessed in a slight degree by the majority of civilised persons. Whether we should have arrived at different conclusions if we had worked with 500,000 percipients instead of 559 it is of course impossible to say. So far as we have gone our results are in agreement with those obtained by J. E. Coover¹ in 1917. Coover experimented with 105 percipients of normal mentality working in conjunction with 97 normal agents. Each percipient made 100 guesses at playing cards drawn at random by the experimenter from a full pack. In 5,000 out of the 10,000 guesses the card chosen was not seen by the agent or experimenter until the percipient had recorded his guess. These 5,000 guesses therefore were experiments in clairvoyance and Coover found that the results obtained entirely conformed to the expectation of chance.

It has been objected to Coover's experiments that the conditions were very unfavourable to the percipients who were allowed only two minutes for each guess with a minute's interval between successive guesses. It might, I think, be urged legitimately that it was asking too much to expect the delicate faculty of clairvoyance to function at a moment's notice without regard to the mood of the percipient or to his idiosyncrasies. Moreover the fact that the percipient knew that he was to end his period of concentration on the tap of the experimenter's pencil might have injuriously affected the operation of spontaneous clairvoyance.

Now such objections will, I think, hardly be urged against our own experiments in which the percipient was given a whole week in which to record five guesses. He was able to choose both his own time and place.

It will probably be suggested that in our own series the enclosure of the card in an envelope inhibited the faculty of clairvoyance. Against this, however, I would point out that almost the only serious *experimental* evidence for the existence of this power is provided by the gifts of M. Stefan Ossowiecki whose chief speciality is the divination of writing enclosed often in several sealed envelopes. In the case of M. Ossowiecki at least the envelope does not inhibit clairvoyance.

It will perhaps be contended with more reason that playing cards do not possess sufficient emotional appeal to stimulate the supposed latent powers of clairvoyance. Moreover, to request a person to guess at a playing card will frequently have the effect of inducing in his mind the image of his favourite card or of his lucky suit. This image may be so persistent and spontaneous as to interfere with the mental passivity which some claim is an essential condition for clairvoyance.

¹ *Experiments in Psychological Research* (Stanford University, California), pp. 50 ff.

The absence of emotional significance in the subject-matter of experiment might be remedied by using a small set of cards on each of which was inscribed a picture with a distinct and separate emotional appeal. By performing large numbers of experiments with this limited set the statistical advantages inherent in the use of playing cards might be retained without the drawback of an insipid subject matter. The possible inhibitory effect of mental preference on the exercise of the supposed clairvoyance would however still remain.

In conclusion my special thanks are due to Dr E. S. Pearson of the Statistical Department of University College, London, for his very kind demonstration of a simple practical method of finding the observed values of the mean and standard deviations.

III. THEORETICAL ANALYSIS, WITH SUGGESTIONS FOR FURTHER RESEARCH

BY INA JEPHSON

It don't come out altogether as plain as to please me, but it's on the cards.
—*Bleak House*, iv

WHEN Mr Soal suggested that a further experiment should be made with playing cards, using a more evidential technique than in the Jephson experiments, I welcomed the suggestion for many reasons. First, because it was essential that a control experiment should be made with which the results of mine might be compared ; secondly, because the results achieved in past experiments with cards in opaque envelopes (a few in the Jephson series and an interesting collection by Professor Richet ¹) gave plausible ground for hope that a larger series might show some traces of the faculty in question.

That the results of the Joint experiment must be ascribed to chance I found very disappointing, but before discussing the various causes which may possibly account for the remarkable difference in scoring in the free and the controlled experiments, there is one more aspect of the results to be examined, and that is whether there is any confirmation in the Joint series for the first or early guess success observed in the Jephson collection, and whether there is any confirmation for the theory of fatigue put forward in my report.

¹ "Further Experiments in Hypnotic Lucidity or Clairvoyance," *Proceedings* (1889), vi. 66ff.

For the reader's information I will recapitulate the conditions of the Jephson experiment ;¹ 240 people took part, each guessed five sets of five cards taken at random from the pack. Only a small percentage of experiments were witnessed, and as none of my percipients knew what was expected I relied largely on the first or early guess success as evidence of the unknown faculty and as evidence for true recording. I was not sufficiently aware that I should be unable to meet (except by a personal opinion) the criticisms made of this method. Dr Fisher was the first to make clear to me—and it is a perfectly fair criticism—that if my percipients waited to record their results till chance gave them a success I should be given exactly the effect of clairvoyance in the early guesses. This criticism did not take into account that good corroborative evidence had been given by the experiments made by Dr Estabrook in similar experiments in “telepathy” and also by other experimenters, where selection of results was not possible. Still, my own experiment was on a large enough scale to have to make its own case and to meet criticisms on its own merits. It is a legitimate demand that an investigator must meet objections not by reiterating faith in the bona-fides of the experimenters but by the evidence of test experiments where the sources of error have been excluded. The uncontrolled experiments and the test experiments being exactly comparable in plan, the analysis can be very simply stated, and the results compared, with the view to discovering whether there is any confirmation for my early observations.

The theory of fatigue put forward in my report was briefly this. I attributed the diminution of success which occurred in the results of a total of 6,000 card guesses in the Jephson experiment (the 1200 first guesses being definitely more successful than the 1200 fifth guesses), to a fatigue of the faculty of clairvoyance. I assumed the faculty to be a spontaneous one, and likely to be at its most successful in the initial effort, before the conscious mind had time to interfere.

I therefore studied the results with a view to testing this, two analyses being made, one of the amount of success in each guess from the first of the little series of five to the fifth, and in the other I counted each little set of five as a whole to discover whether the earlier sets had higher average scores than the later sets.

I will begin by showing the analysis of the five consecutive guesses in the Joint experiment and comparing it to the similar analysis of the Jephson series. The scale on the left gives the score, and on line *A* is recorded the average score for all 1st or *A* guesses, on line *B* the average score for all second or *B* guesses and so on.

¹The Jephson series has been re-scored according to Dr Fisher's latest system so as to be exactly comparable with the present experiment.

TABLE XXI

	A	B	C	D	E	No. of guesses.
Jephson X . . .	13·65	12·76	12·73	12·81	13·23	6,000
Joint Y . . .	10·69	11·33	11·20	11·05	11·50	6,317

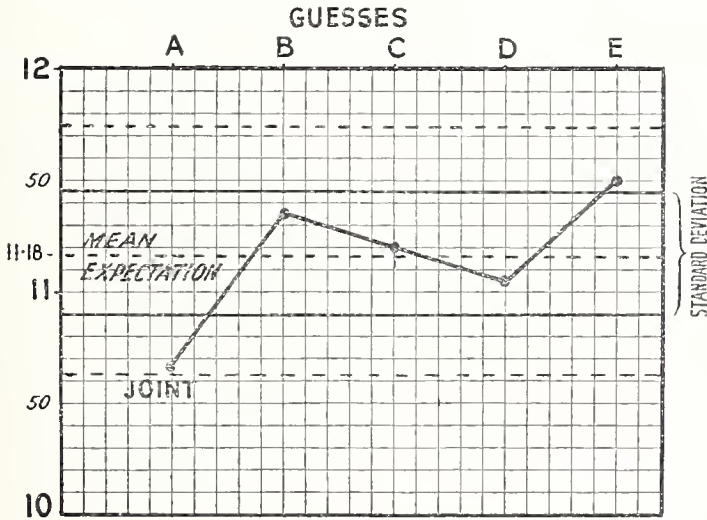


FIG. 4

It will be seen that the curve in no way fulfils my hopes, in fact, it is reversed, the last guess being definitely the best, and the only one which even approaches the upper line of standard deviation.

If we examine the Boston and Murphy experiments on the same basis, we find that in both there is deterioration in scoring from first to last guesses in a set of five. The following diagram gives the success from first to last guess in the two American experiments.

TABLE XXII
(Average Score for Guesses)

	A or 1st	B or 2nd	C or 3rd	D or 4th	E or 5th	No. of card guesses.
Jephson X . . .	13·65	12·76	12·73	12·81	13·23	6,000
Boston Y . . .	11·24	10·94	11·42	11·56	10·99	2,031
Murphy Z . . .	11·14	9·60	10·40	10·22	10·63	784

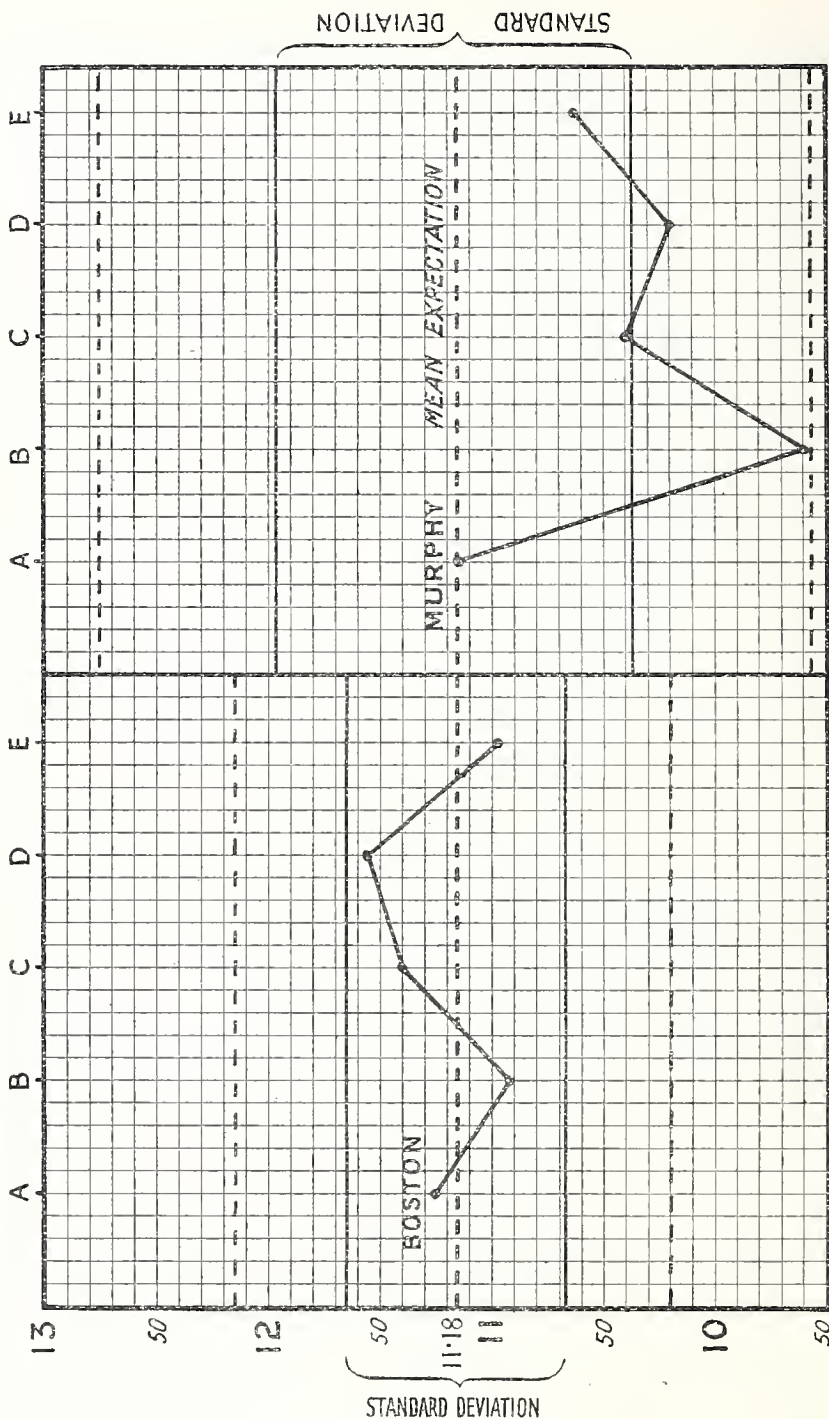


FIG. 5

(The marked variations in the Murphy experiment can be accounted for by the fact that only a small number of guesses were involved. With a larger number the averages tend to steady down to nearer 11.18.)

There remain the Supplementary and the Flugel experiments. In the former a slight deterioration occurs from first guess to last, but on this scale it is negligible, and most unimpressive when compared to the rise in scoring for the "D" guesses. In such a small experiment again a very wide margin must be allowed for chance variations.

In the same table below appear the results of the experiment done by Professor Flugel with his class at University College, 24 percipients.

TABLE XXIII

	A	B	C	D	E	No. of guesses.
Supplementary	10.78	10.60	12.33	14.63	10.38	197
Flugel	6.42	11.09	11.61	10.52	14.17	120

Both results must be considered pure chance results, and as in a certain number of experiments the fluctuations must be beyond S.D., the below and above S.D. results in Professor Flugel's experiments are chance variations.

To sum up the results of the present experiments, so far as my theory of deterioration from first guess to last is concerned, we get the following results :

TABLE XXIV

Experiments in which deterioration occurs from first to last guesses

Boston	2031 guesses	11.34 to 10.99
Murphy	784 ,,	11.14 to 10.63
Supp.	197 ,,	10.78 to 10.38
	<u>3012</u>	

TABLE XXV

Experiments where the effect is reversed

Joint	6317	10.69 to 11.50
Flugel	<u>120</u>	6.42 to 14.17
	<u>6437</u>	

So though if we count by experiments I am given a majority for deterioration on the actual number of guesses the larger Joint experiment swamps the effect, and we get the total curve :

TABLE XXVI

	A	B	C	D	E	No. of guesses.
(5 experiments combined)	10·79	11·08	11·21	11·16	11·33	9449

which is definitely unsatisfactory from the point of view of my theory.

If we now turn to the secondary analysis for deterioration to discover whether we get confirmation for the more prolonged deterioration observed in the Jephson experiment, which occurred from the first set of five guesses to the last set of five guesses the following is the result. It was not possible to make this analysis in all five experiments because in Professor Flugel's and in Professor Murphy's each percipient had only guessed one set of five, but the following table gives the figures for the remaining three experiments where the guessers had each tried five sets of five :

TABLE XXVII * (Sets)

	1st	2nd	3rd	4th	5th	No. of Sets.
Joint	11·09	11·08	10·97	11·17	10·74	845
Boston	11·15	12·78	19·84	10·97	11·44	235
Supplementary	13·23	11·31	12·71	9·01	11·58	40
						<u>1120</u>

Or for clearness we can re-arrange the table thus :

TABLE XXVIII

Experiments where deterioration in scoring occurs from first to last sets.

Joint	845 sets	11·09 to 10·74
Supplementary	40 „	13·23 to 11·58

Experiment where there is no deterioration.

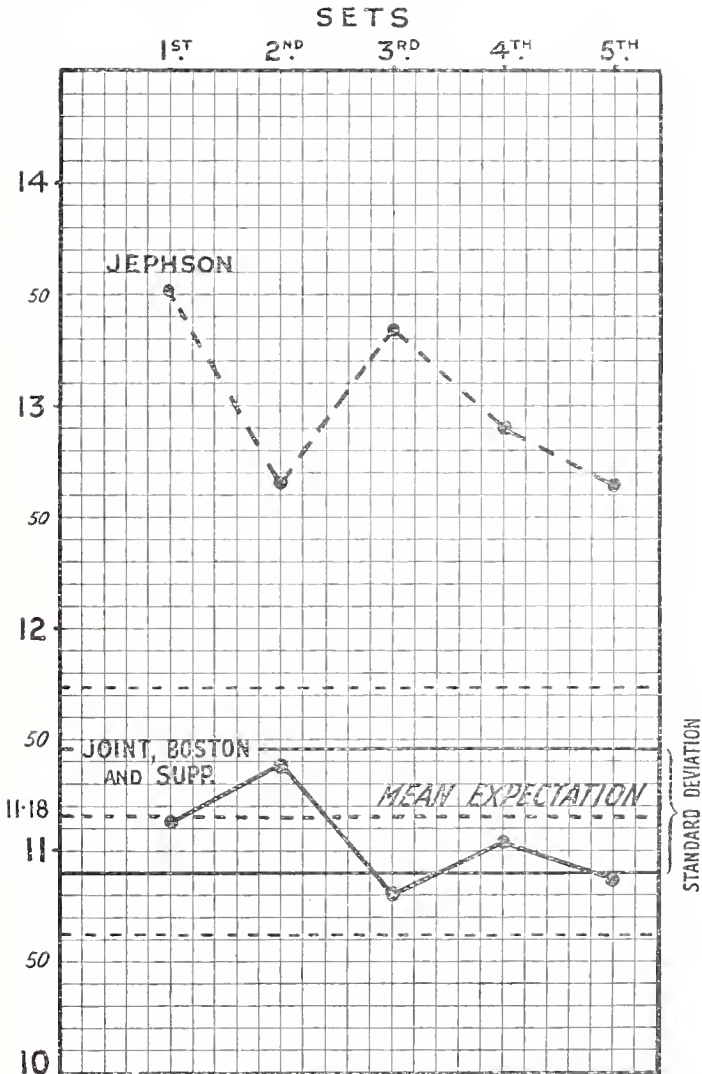
Boston	235 sets	11·15 to 11·44
--------	----------	----------------

Here we get a clear majority for a fatigue curve, and it is rather tempting to imagine that it is a real sign of the increasing inhibition, boredom or fatigue of the percipients. The diagram will show the drop in scoring, compared to the similar deterioration which I had observed in the Jephson experiment (see Fig. 6).

* Where my figures differ from those in other parts of this report, it is because I make use only of entirely complete sets.

TABLE XXIX (Sets)

	1st	2nd	3rd	4th	5th	No. of Sets.
Jephson	13-57	12-66	13-37	12-93	12-64	1240
Joint, Boston and Supp.	11-16	11-38	10-83	11-07	10-89	1200



THE S.D. FOR THE JEPHSON CURVE IS NOT SHOWN AS IT IS ALMOST IDENTICAL WITH THE JOINT, BOSTON AND SUPP. S.D.

FIG. 6

It is very doubtful whether, as in the present experiments all scores lie well within the realm of chance, any significance at all can be attached to them. Chance alone in a variety of experiments is bound to give a certain number of like curves, and the result must be considered quite inconclusive.

A subsidiary enquiry which had interested me in the Jephson experiment and which I investigated again here, was whether women had had more success than men, as that is what is conventionally expected where intuitive faculties are concerned. Seventy-five per cent. of the women guessers were found to show fatigue curves against forty-four per cent. of the men. Their scores were slightly higher. This tendency to better scoring is confirmed in the Joint experiment as the following table shows, though perhaps as they are only superior by a decimal point it is hardly fair to call attention to it.

TABLE XXX

*Joint Series**101 men.*

	Average score per set.
1st set of five	10·97
2nd " " "	11·36
3rd " " "	11·16
4th " " "	10·92
5th " " "	10·62

113 women.

1st set of five	11·20
2nd " " "	10·82
3rd " " "	10·80
4th " " "	11·39
5th " " "	10·85

The average total score for the five sets for men was 275·26, and for women 275·41.

If we turn to the Boston experiment and study there the figures for deterioration from first set of five to last, we find the last set definitely the best, and in this case the men score considerably higher than the women. The following table gives the result. I have taken all those sets which were recorded as running consecutively, but postal difficulties and in some cases unrecorded dates or doubtful dates made it difficult to be sure in what exact sequence the sets had been done. Therefore I have only taken those complete sets about which there was no doubt, and this reduced the number considerably. A certain number of percipients only recorded four sets, which I do not record here.

TABLE XXXI

*Boston Series**22 men.*

	Average score per set.
1st set of five	11.86
2nd " " "	13.52
3rd " " "	10.42
4th " " "	10.88
5th " " "	10.94

26 women.

1st set of five	11.02
2nd " " "	12.37
3rd " " "	9.19
4th " " "	11.16
5th " " "	12.56

The average total score for five sets for men was 252.73, and for women was 229.35.

The Supplementary experiment I had examined with extra interest. On the assumption that the highest scores were due to a supernormal faculty a fatigue curve might be encouraging, and I was delighted to discover the hoped-for effect. However, a fatal industry led me to examine the results of the above-standard-deviation group analysed in part II above, and I found there was no drop from first to last guess but the reverse, and no deterioration from first to last set but an improvement. I do not mean that by these facts we can possibly decide whether the above-standard-deviation scores were supernormally acquired or not, but merely that on the particular analysis chosen the balance of evidence in this small experiment is adverse to my suggestion.

Finally then, three experiments out of five give deterioration from first to last guess. Two experiments out of three give deterioration from first to last sets.

This result is highly inconclusive, and when we also take into consideration that the variations within the curve are more marked than is the drop from first to last, it becomes more likely than ever that we must ascribe the results to chance.

The main facts of the present experiments being now before the reader, some attempt must be made at a summary, but I cannot do so without discussing for a moment the criticisms on the Jephson experiment. I am glad that everything which can be suspected of my percipients can be suspected of myself in an even greater degree, as my results figure among those recorded, and had I wished to avail myself of it I had every opportunity of excluding such results as did not tend to confirm my hopes. All the charges brought against my

collection are perfectly legitimate and Dr Fisher's objections are particularly convincing. His explanation covers many of the facts observed and explains the high scoring not by fraud but by the fact that as I did not know exactly how many of my percipients had failed to return their records, I could not meet the objection that those who tried and were unsuccessful might have thought it not worth while to send me their results. This alone would ensure that the collection would be better than unaided chance would achieve. It would be, in reality, a chance collection with the poorer results weeded out of it. His second suggestion is that my unknown guessers may not have begun to record their guesses until chance gave them a success. As chance in the long run would tend not to give two successes running, there would be a drop in score after the first card, and thus automatically the effect of clairvoyant vision of the first card is given. This criticism makes it quite impossible for me to prove my theories on the experiment in question, but I still cannot forget that the experiments which I witnessed gave the same type of result as the unwitnessed, and how are we to account for the fact that exactly the same effect of high scoring and early success appears in Dr Estabrook's experiment in telepathy where there was no scope for selection or for arranging the moment for beginning to score? All that the jury can decide is that selection and clairvoyance give the same results, and though (as is obvious) common sense and scientific respectability depend on accepting the most normal explanation, I intend to consider the question still open. For the sake of the revolutionary possibilities involved I will gladly defer any claim to scientific respectability till I have done some more exploring.

It has been assumed throughout this research that the test experiments and the uncontrolled experiment were exactly comparable. But in what may be a most important divergence the conditions were totally different. My suggestion is highly theoretical and will reduce to despair those readers to whom the facts of supernormal perception are delusion. But how can we be sure that in using absolutely new cards as we did in the present experiments, for the sake of uniformity, we are not taking away from the percipients precisely the source of information whence they get their knowledge? I cannot see what else we are to do at this stage of research than to accept at its face value the collected evidence of experiences recorded by supposedly clairvoyant people. It is their assumption that an object can carry with it impressions relevant to it; so that if they are in contact with an object they are also in some way linked with the human experience with which that object has been associated. The recent S.P.R. "Absent-Sitter" experiments with Mrs Warren Elliot are the only experiments I know of under

control conditions which sought to test the theory that objects carry some emotional influence. One cannot ignore the accepted tradition of many honest and intelligent sensitives, and the question raised here is so fundamental that one is tempted to think that it is the first problem which research should tackle. If we could get information as to whether a clairvoyant gets an impression or information direct from a material and physical object, we might decide in that case that the faculty was in some form a material one that ultimately could be classified (the analogy of wireless at once occurs to one) on some wave theory. If, on the other hand, a clairvoyant is only in touch with and can only report on *thoughts* which have been associated with an object, the whole operation is shifted on to a psychical or mental sphere. It would be interesting to try experiments with mediumistic people, asking them to guess alternatively absolutely new cards with which no thoughts are associated, and then to try much-played cards. What, I wonder, would happen? If it is only thoughts with which clairvoyants are in touch the whole problem becomes definitely simplified. Underneath all our theories of telepathy is the assumption that a thought somehow ceases to be dynamic once its moment is over. In telepathy we picture an *active* thought impressing the passive mind of the percipient. If we give up this assumption and picture the thought as permanently dynamic (and here again we have normal psychology on our side) telepathy and clairvoyance merge as it were imperceptibly into each other and the same faculty is responsible for both. In the case of telepathy the percipient gets the impression of a thought simultaneously to the thought taking place in the mind of the thinker; and in clairvoyance the percipient gets an impression of a thought which may not be quite so recent.

The primitive and superstitious view which ascribes to relics a real importance may have a grain of truth attached to it, apart from normal associations. Any used card, such as those in the Jephson series, we may consider, if we choose, as a relic, with its own appropriate image attached, and it may have been these images with which the percipients were able to get into touch and which gave them their information. As in the Joint experiment the cards were absolutely new from the printers, it is arguable that they therefore had no associative thoughts attached to them, so that in reality, when we had sent out the envelopes containing the new cards, we had sent out, from the point of view of the clairvoyant, exactly nothing at all. We might just as well have sent the empty envelopes.

It will be quite rightly considered entirely premature to suggest such wild and not even original speculations, and I am quite willing to agree that it may quite likely be a normal reaction to scoring so many thousand cards from several different points of view. I am

tempted to speculate also perhaps, on account of the more promising nature of my new series where the experiments have been planned so as to give the percipient the maximum of spontaneity, and to eliminate as much as possible the use of the conscious mind.

Whilst I must acknowledge that on the analysis made the results of the Joint experiment are chance results, I claim that it is too early to consider the verdict of the experiment in any way final. We can, and need only say, that under the given conditions, the experiment was negative.

I cannot end without thanking on behalf of the three investigators the chief people who helped in the prolonged work of the experiments. Dr Fisher's help and advice were invaluable ; for at least, whatever my theories, his scoring system and his supervision of the statistics, enable the facts to be given without prejudice so that the reader is free to come to his own conclusions. Professor Murphy and Professor Flugel helped materially by contributing their most welcome experiments, and the encouragement of their co-operation, and the collaboration of many members of the Boston S.P.R. was a most valuable addition to the experiment. Miss Newton and Miss Carruthers were an ever-present help in times of toil, and Dr Woolley's help in connection with the envelopes is gratefully acknowledged.

REVIEW.

W. F. PRINCE, *The Enchanted Boundary, being a Survey of Negative Reactions to Claims of Psychic Phenomena, 1820-1830.* 8vo. pp. xi. 348. Boston Society for Psychic Research, 1930.

The difficulties encountered in psychical research are so many, the opposition met with by the investigator so strong and usually so uninformed, the criticism received even from those who are interested in the subject often so irrelevant, that the active worker not seldom feels despondent about the position and possibilities of the whole subject. In such a mood Dr Prince's new book can be safely prescribed as an effective tonic. Dr Prince is controversial, but in a subject in which we are too inclined, from sheer weariness, to offer the other cheek when buffeted on the one, it is highly suitable that we should be able to return to our adversaries, though but vicariously, one tremendous and irresistible blow. For, though Dr Prince's pugilistic style is polished and though he unquestionably hits above the belt, his very science makes his victory the more overwhelming.

The book opens with a reprint of Dr Prince's paper in *The Case for and Against Psychological Belief*. Next follows an article he wrote some years ago, enlarged to well over 100 pages. Dr Prince assumes the polite fiction that when otherwise able and learned men harry psychical research an enchantment falls on them which prevents them from seeing clearly. Hence the title of *The Enchanted Boundary*. In this part of his book Dr Prince examines the criticisms of psychical research published by a number of distinguished men, from Faraday, Tyndall, Huxley, and Macaulay, to such lesser luminaries as Professor Rine (an Adventist), the Rev. Charles Reynolds Brown (Dean of Yale Divinity School), Mr Morris H. Turk (a theological writer), and the Rev. I. M. Haldeman (who has been described as "the greatest prophet of the Lord now standing in any pulpit" in America). It is impossible to question Dr Prince's claim that he has established that none of these men has squarely faced and fairly discussed the real evidence, that most of them are often guilty of elementary blunders, that some of them quote incorrectly, that many of them show ignorance of the literature, that they are guilty of amazing solecisms in logic, that they exhibit "impatience, 'loathing,' and other emotional aberrations," that they entrench themselves

behind the very scientific maxims which are in dispute, and that they manifest a liking for what Dr Prince happily calls "technical billingsgate."

Under the title of "Some Sample Explanations" Dr Prince then deals with the Lehmann and Hansen theory of unconscious whispering, with Mr Salter's review of *The Road to Endor*, and with the notorious, but elusive, mediumistic Blue Book.

The next chapter is a very important one. In it Dr Prince discusses the parallels and contrasts between the late Harry Houdini and Sir Arthur Conan Doyle. Apart from the psychological interest of Dr Prince's demonstration of the similarity of the mental processes which led these two men to diametrically opposite conclusions, Dr Prince's discussion should serve to give the final blow to two very mistaken notions. One is that the creation of Sherlock Holmes was evidence of superior detective ability in his creator; those who are inclined to believe this will find it useful to read pp. 150-151 of Dr Prince's book. The other notion demolished by Dr Prince is that skill in conjuring is sufficient qualification for investigating psychical research. This is far too often alleged as a self-evident fact, whereas it is one of those facts for which there is no evidence, however plausible they may seem at first sight. The best critical work in psychical research has been done by men with very little or no knowledge of conjuring, and some of the most expert conjurers have been notoriously tricked (as by Davey). It seems, indeed, that a conjuror's resistance to the marvellous is capable of being more easily and more completely broken down than that of the average person. It is usually the conjuror who gives no serious attention to the phenomena who remains militantly sceptical.

Dr Prince's next tilt is against the anonymous *Essay on Capacity and Genius* of about 1820. In this work the usual sort of popular account is given of the circumstances under which ghosts appear. Dr Prince takes this account point by point and shows it to be almost completely erroneous, so far, at any rate, as the S.P.R. material goes. He deals in a similar manner with an address by Professor Persifor Frazer delivered in 1875, and with Hibbert's *Sketches of the Philosophy of Apparitions* (1824).

The next chapter is a summary of Mr Piddington's comparison of visceral hallucinations with those studied by psychical research,¹ in which Dr Prince refers very happily to Mr Piddington's "mcti-eulosity" (p. 203).

The whole of the remaining chapters are occupied by a discussion evoked by a questionnaire sent out by Dr Prince to, it would appear, all persons appearing in the American *Who's Who*. It is somewhat to be regretted that this questionnaire is not printed in the book

¹ *Proceedings* (1905), xix., 267 ff.

before us : it is often rather difficult to follow allusions to it. However, Dr Prince prints (p. 209) the particular question with which he is most concerned, a selection of replies to it, and of his own answers to these replies. Here is another cause for regret. Dr Prince has "improved" his own letters for publication. Knowing what we do of Dr Prince's accuracy, we can feel quite sure that his alterations are only verbal and "literary." Still, it is a pity to expose oneself to criticism in such a matter; nobody expects literary polish in controversial correspondence of this kind.

The first chapter of this part of the book gives a number of specimen replies to a request for a statement of the reactions of the persons addressed to a very strong (supposedly hypothetical) case of supernormal knowledge. The evasions produced by this question are remarkable in their variety. Perhaps the most interesting is the answer of a Professor of Bacteriology, who wrote amiably: "You will find 'Pancrobilin Pills, Plain,' Reed and Carnrick, a splendid 'antidote'" (p. 211). It is an interesting illustration of American standards that the precise religious denomination of the writers is given in most cases; these denominations range from Church of the Brethren to Roman Catholic.

The remaining chapters deal with replies alleging that belief in psychical events is the result of pathological states, arguing from negatived "premonitions," attributing the events to coincidence, and so on. Under all these heads Dr Prince replies to his correspondents with a courtesy equalled only by his forcefulness, putting forth all the arguments of sweetness and reasonableness, though seemingly with but little effect on the minds of those addressed. It would appear, indeed, that Dr Prince's labours will not benefit those to whom the questionnaire was sent, but rather students of psychical research, in the vivid light Dr Prince's discussion throws on the psychology of the opposition to the subject by "scientific" and other "distinguished" men.

A few points of special interest may be briefly referred to. In answer to the question, "Do psychical researchers all agree that any types of phenomena are supernormal?" Dr Prince cites four types which he thinks all students accept. These are telepathy, veridical apparitions, mediumistic deliverances, and psychometry. As to telepathy Dr Prince is clearly right, and also in regard to veridical apparitions. Dr Prince's explanation of the latter (p. 15) shows that he has phantasms of the dying in mind; but he does not say whether in his view these demonstrate anything more than telepathy. The same holds of mediumistic deliverances, which are hardly a type of phenomenon in themselves. As to psychometry there can be little doubt that Dr Prince is too optimistic in supposing there to be general agreement about this phenomenon. I could name several psychical

researchers who do not accept it at all. Apart from this, it would have been interesting to have been told whether Dr Prince believes in any forms of psychometry beyond such as can be explained by telepathy.

Dr Prince doubts (pp. 130-1) whether it will ever be possible to put telepathy to the pragmatic test. But Dr Schiller has been maintaining for many years that only by this very means will telepathy ever come to be generally accepted.

In the letters quoted at pp. 304 ff. Dr Prince is at some pains to explain what is meant by the word supernormal. His correspondents appear to think that the word is given a philosophical significance. It cannot be too often stressed that in psychical research supernormal is simply used to indicate that which is not accepted by orthodox science, psychical research itself being the study of supernormal faculties in man.

Our President, it will be obvious, has written a most valuable work, one which will greatly stimulate all serious students of psychical research and which will provide them with controversial material for a long time to come.

THEODORE BESTERMAN.

ERRATA

Vol. xxxviii, pp. 409, 449, 450: for "Rondon" read "Kordon."

Vol. xxxix, p. 273, footnote: for "Private Meeting" read "General Meeting."

p. 283, line 38: for "Huxley" read "Tyndall."

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PROCEEDINGS

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