

P R O C E E D I N G S

O F T H E M E E T I N G O F T H E S O C I E T Y O N

April 24, 1883.

The third general meeting of the Society was held at Willis's Rooms, St. James's, London, on April 24th, 1883.

HENRY SIDGWICK, ESQ., PRESIDENT, IN THE CHAIR.

I.

T H I R D R E P O R T O N

T H O U G H T - T R A N S F E R E N C E .

Committee :—EDMUND GURNEY, M.A.,* late Fellow of Trinity College, Cambridge; F. W. H. MYERS, M.A.,* late Fellow of Trinity College, Cambridge; F. PODMORE, B.A.; and W. F. BARRETT,* Professor of Physics in the Royal College of Science for Ireland, *Hon. Secretary*.

At the close of the last Report a series of experiments were recorded illustrating the reproduction of drawings without any discernible communication passing between the Agent, Mr. Blackburn, who had momentarily seen the drawing made by one of us, and the Percipient, Mr. G. A. Smith, who was blindfolded throughout the experiment. In these early experiments Mr. Smith held Mr. Blackburn's hand for a few moments, and then, releasing it, drew his impression of the figure. In this way we obtained a rough, but recognisable, reproduction of the nine figures which we had drawn. The figures and the reproductions are published in Part II. of our Proceedings. We have now to record a further extension of our inquiry in this direction, the experiments being made under conditions still more stringent than those at first imposed. We have also endeavoured to ascertain how far the curious inversion of the figures, which had been noticed in the descriptions but not in the pencil reproductions of the drawings, was accidental or otherwise.

At the invitation of the Committee, Mr. Blackburn and Mr. Smith

[* THE COUNCIL HOLDS ITSELF GENERALLY RESPONSIBLE FOR THE REPORTS OF ITS COMMITTEES. AT THE HEAD OF EACH REPORT THE NAMES OF THOSE MEMBERS OF COMMITTEE WHO ARE SPECIALLY RESPONSIBLE FOR ITS COMPOSITION ARE MARKED WITH ASTERISKS.]

came from Brighton in January last, and met us at the rooms of the Society in Dean's Yard, where all the experiments about to be described were conducted. As Mr. Blackburn came only in answer to the urgent request of the Committee, and at considerable inconvenience to himself, we feel it our duty to mention this fact, and, at the same time, to express our hearty obligations to him for the unrecompensed trouble which we have so frequently imposed upon him.

It is almost needless to point out that in these observations, so foreign to our common experience, it is indispensable to be minutely careful and conscientious in recording the exact conditions of each experiment. This we have striven to be ; and the reader will thus be enabled to form an independent judgment by making allowance for whatever mental bias he may discover in our conclusions. He has thus, moreover, the means afforded him of detecting possible errors, or of suggesting precautions which we may have overlooked.

Our *modus operandi* is as follows : The Percipient, Mr. Smith, is seated blindfolded at a table in our own room ; a paper and pencil are within his reach, and a member of the Committee is seated by his side. Another member of the Committee leaves the room, and outside the closed door draws some figure at random. Mr. Blackburn, who, so far, has remained in the room with Mr. Smith, is now called out, and the door closed ; the drawing is then held before him for a few seconds, till its impression is stamped upon his mind. Then, closing his eyes, Mr. Blackburn is led back into the room and placed standing or sitting behind Mr. Smith, at a distance of some two feet from him. A brief period of intense mental concentration on Mr. Blackburn's part now follows. Presently, Mr. Smith takes up the pencil amidst the unbroken and absolute silence of all present, and attempts to reproduce on paper the impression he has gained. He is allowed to do as he pleases as regards the bandage round his eyes ; sometimes he pulls it down before he begins to draw, but if the figures be not distinctly present to his mind, he prefers to let it remain on, and draws fragments of the figure as they are perceived. During all this time, Mr. Blackburn's eyes are, generally, firmly closed (sometimes he requests us to bandage his eyes tightly as an aid to concentration), and except when it is distinctly recorded, he has not touched Mr. Smith, and has not gone in front of him, or in any way within his possible field of vision, since he re-entered the room.*

* This precaution was not attended to in the experiments of one afternoon ; but these experiments, and these alone, are omitted from the series discussed below, as having been rendered nugatory through accidental circumstances which were calculated to exercise, and obviously did exercise, a distracting effect on Mr. Blackburn's mind.

When Mr. Smith has drawn what he can, the original drawing, which has so far remained outside the room, is brought in, and compared with the reproduction. Both are marked by the Committee and put away in a secure place. The drawings and reproductions, given at the end of the Report, are in every case fac-similes of the untouched originals, from which they have been photographed on the wood blocks.

The experiments began on January 19th, 1883, and were continued for three or four days in succession. During this series of experiments a considerable number of drawings were made, nearly all of which were exhibited at the following General Meeting. Another series of experiments were made in April. All the drawings may be seen at the Society's rooms; but it has been thought needlessly expensive to reproduce the whole number here. Those which are omitted, however, are by no means failures, and in fact only 8 experiments, out of the total of 37, can be put down as unsuccessful, Mr. Smith in 4 cases failing to see anything and in 4 cases giving so imperfect a representation that it might be called a failure. The first 4 figures were obtained after Mr. Blackburn had for a few minutes grasped Mr. Smith's hand—a procedure to which they were accustomed—as a supposed aid to Mr. Smith in visualising Mr. Blackburn's mental picture. We, however, could allow no exception to our cardinal axiom on this subject, that no experiment where contact of any sort is allowed can be decisive; and though in the present instance the drawings were of such an irregular character that their description would have been extremely difficult to convey by imperceptible tracing or by any subtle code of pressure-signs, yet, assuming Mr. Blackburn and Mr. Smith to have been in collusion, the hypothesis was at least conceivable. Accordingly, we requested Mr. Blackburn to dispense altogether with the preliminary contact; and it must be understood that all the rest of the successful drawings (with the exception of two, not here reproduced and of Fig. 13^b as explained below) were done without any contact whatever, in the manner already indicated on p. 162. Down to Fig. 9 we had made rude geometrical drawings; at this point, one member of the Committee, *without giving the least indication of his intention*, now drew Fig. 10 outside the room as usual. The grotesque reproduction by Mr. Smith is decidedly striking; and so also is the reproduction of the next figure, when Mr. Smith again apparently imagined that a geometrical figure had been drawn.

In some of the less accurate reproductions Mr. Blackburn complained of the difficulty he had in keeping the original drawing steadily in his mental view; and on one or two occasions we asked Mr. Blackburn to draw his recollection of the picture simultaneously with Mr.

Smith (the two, of course, being kept out of sight of each other). We found that the main errors in Mr. Smith's reproduction existed already in Mr. Blackburn's recollection of the drawing. A striking illustration of this is given in Fig. 16, where the reproduction closely resembles Mr. Blackburn's drawing of what he remembered. It is, in fact, by no means easy to keep vividly and correctly in mind for several minutes any irregular figure which has only been actually before the eye for a few seconds. We tried one experiment to test the effect of refreshing Mr. Blackburn's memory. Fig. 13 was drawn by us; and its reproduction, Fig. 13^a, was made by Smith, in the usual way. The reproduction is very imperfect, being a sinuous, instead of a spiral line. No contact between the operators having so far occurred, we now asked Mr. Blackburn to touch Smith's hand for a few moments; on releasing it the reproduction, 13^b, was obtained. Mr. Blackburn was now asked to stand (as at first) behind Mr. Smith, who remained blindfolded. The original drawing was now brought into the room, and held in front of Mr. Blackburn's eyes, and, therefore, some distance from the back of Smith's head. The latter now made the reproduction, 13^c, which is an exact copy of the original. We need hardly add that there were absolutely no means (such as mirrors, &c.) by which Smith, even if not blindfolded, could have gained any glimpse of the drawing, and, as we have already remarked, the most complete silence was preserved throughout these experiments.

We have now to consider whether it was possible that any information of the character of the designs drawn could have reached Smith through the ordinary avenues of sense. Of the five recognised gateways of knowledge, four—tasting, smelling, touch, and sight—were excluded by the conditions of the experiment. There remains the sense of *hearing*, which was but partially interfered with by the bandage over the eyes and ears. But the information can certainly not have been conveyed by speech; our ears were as near to Mr. Blackburn as Mr. Smith's, and our eyes would have caught the slightest movement of his lips.

There remains the hypothesis of a code, consisting of audible signals other than oral speech; and it would, no doubt, be an exaggeration to affirm that the possibility of such signals was absolutely excluded. We shall endeavour so to vary the conditions of subsequent experiments as to exclude this hypothesis completely: at present we will only point out the very great improbabilities which it involves, quite independently of our reliance on the integrity of Mr. Blackburn and Mr. Smith, which nothing has occurred to shake in the slightest degree.

Let our readers, who may be familiar with the Morse or any other code of signals, try in some such way to convey a description of some of our drawings, to a friend who is blindfolded and has not seen the original;

we venture to assert that, even if audible signs were allowed, several minutes at least would be required to convey the notion of the figures correctly. It is probably no exaggeration to say that several scores, if not hundreds, of precise signs would be required to convey an idea as exact as that implied in many of Mr. Smith's representations. But in our experiments what sort of range existed for this mode of communication? The material for possible signs appears to be reduced to shuffling on the carpet, coughing, and modes of breathing. Anything distinctly unusual in any of these directions must inevitably have been noticed; and since our attention, during this part of the experiment, was of course concentrated on the relation between Mr. Blackburn and Mr. Smith we are at a loss to conceive how any signalling, sufficient in amount to convey the required ideas, could have passed undetected. Furthermore, it must be observed that the reproductions were not made in a tentative, hesitating manner as if waiting for signals; but deliberately and continuously as if copying a drawing that is seen. Moreover, in almost every instance the *proportions* of the different parts of the original figure were reproduced more accurately than were its more easily describable details. However, with the view of removing all doubts that might arise as to possible auditory communications, we on one occasion stopped Mr. Smith's ears with putty, then tied a bandage round his eyes and ears, then fastened a bolster-case over the head, and over all threw a blanket which enveloped his entire head and trunk. Fig. 22 was now drawn by one of us, and shown outside the room to Mr. Blackburn, who on his return sat behind Mr. Smith, and in no contact with him whatever, and as perfectly still as it is possible for a human being to sit who is not concentrating his attention on keeping motionless to the exclusion of every other object. In a few minutes Mr. Smith took up the pencil and gave the successive reproductions shewn below.

To profit by a code in this case, Mr. Smith would have had to extract the putty from his ears unobserved by us, (an action the possibility of which the heavy swathings rendered just conceivable,) and then, still smothered in bolster-case and blanket, to detect periodic variations in Mr. Blackburn's breathing imperceptible to us; to identify them as proceeding from Mr. Blackburn, and to interpret them into a description of the figure given below. This hypothesis seems to us an extreme one, but, as we have already said, we intend to meet it by yet further varying and narrowing the conditions of future experiments.

We have now to describe some experiments which were undertaken to test whether the mental inversion of the object that had been noticed in some of the early trials was accidental or otherwise. Mr. Smith, having been carefully blindfolded, sat with his back to us, in a darkened room—some heavy opaque curtains being between him and us. An

arrow having been drawn on a sheet of white paper, it was held by one of us in sight of Mr. Blackburn, who remained in our presence, and sat facing the same way as Mr. Smith. In answer to the query, "How is the arrow pointing?" spoken by one of the Committee in a uniform tone of voice, Mr. Smith called out the direction as he mentally perceived it. We turned the arrow noiselessly, and at random, in different directions, and noted the following series of replies:—

TRUE POSITION OF ARROW.	POSITION AS STATED BY SMITH.	TRUE POSITION OF ARROW.	POSITION AS STATED BY SMITH.
1. Pointing up:	Pointing up.	22. Pointg. to right:	Pntng. to right.
2. " to left:	" to right.	23. " to left:	" to left.
3. " down:	" down.	24. " to left:	" to right.
4. " to right:	" to right.	25. " up:	" up.
5. " up:	" up.	26. " down:	" down.
6. " to left:	" to right.	27. " up:	" up.
7. " up:	" up.	28. " up:	" up.
8. " up:	" up.	29. " to left:	" to left.
9. " up:	" up.	30. " to right:	" to left.
10. " down:	" down.	31. " up:	" up.
11. " to left:	" down.	32. " to right:	" down.
12. " to left:	" down.	33. " to right:	" to left.
13. " up:	" up.	34. " down:	" down.
14. " to right:	" to left.	35. " up:	" up.
15. " to right:	" to right.	36. " to right:	" to right.
16. " up:	" up.	37. " down:	" down.
17. " up:	" up.	38. " to left:	" down.
18. " up:	" up.	39. " up:	" to right.
19. " to left:	" to right.	40. " down:	" to right.
20. " to right:	" to left.	41. " to right:	" to right.
21. " down:	" down.	42. " up:	" up.

After the 37th trial, Mr. Blackburn was obliged to leave; but we continued the experiments, one or two of the Committee taking Mr. Blackburn's place, and with fair success. Counting these last, we made in all 42 trials. In these the arrow was held in a perpendicular position, up or down, 23 times; and of these cases 20 were guessed rightly, 3 wrongly. It was held in a horizontal position, right or left, 19 times; and of these cases 7 guessed rightly, 12 wrongly. The three wrong guesses when the arrow was in a perpendicular position occurred after Mr. Blackburn had left us; and in these cases the error was not one of inversion. Of the 12 wrong guesses, when the arrow was held horizontally, 8 were lateral inversions of the position of the arrow, as if it were seen in a mirror. Hence we see that 87 per cent. of the answers were correct for the perpendicular position, and barely 37 per cent. for the horizontal position; and, further,

that it was about an even chance, when the arrow was horizontal, whether the image was described as laterally inverted or not.*

The interest which this subject is exciting throughout the country leads us to hope that experiments in various directions may be made wherever anyone shows indications of possessing the percipient faculty. † Experiments are of value even when they do not point to thought-transference, as long as the nature of the conditions is accurately noted. Thus, among others, the Rev. E. H. Sugden, B.Sc., of Bradford, has, by practice, developed an acute power of sense perception whereby he can write down any number thought of by following (according to his own account) the unconscious indications given by the thinker. At the same time, Mr. Sugden, while fully convinced of the possibility of genuine thought-transference *without* contact, expressly asserts the necessity of that condition for *his* results: hence his experiments and explanations, which are admirably clear and well-described, testify to the accuracy of the distinction prefixed by us to our second Report. (Proc. S.P.R. p. 70.)

SUMMARY OF RESULTS.

It will be interesting here to review the numerical results of our experiments on thought-transference. These are summed up in the accompanying tabular form.

PRELIMINARY EXPERIMENTS.

Made by Prof. Barrett, at Buxton, Easter, 1881. Proc. S.P.R. p. 21.

Things chosen.	No. of trials.	Total right.	Remarks.
Objects	33	25	Many of these guesses were right on the first trial, but no exact record was kept of this. At least a score of these trials were made with the guesser in an adjoining room. Adverse chances at least 50 to 1.
Names	35	26	
Cards	7	7	
Totals.....	75	58	

* Mr. Smith described the impression he obtained as that of a white arrow on a dark ground. We used at first an arrow drawn in ink on white paper, Without informing Mr. Smith (who remained behind the opaque curtain) of our intention, we cut an arrow out of white paper and placed it on a crimson cloth; Mr. Smith at once perceived the difference, and said he saw a kind of greenish arrow—this being (though *we* could hardly perceive it) the subjective colour given to the arrow from its contiguity with the crimson cloth. We had no facilities at the moment for trying further experiments in this direction, and merely mention this result, which might have been accidental, as indicating another avenue of inquiry.

† Since this Report was drawn up, we have learnt with much satisfaction that a paper has been read before the Literary and Philosophical Society of Liverpool,

MR. AND MRS. HENRY SIDGWICK'S EXPERIMENTS. Buxton, August, 1881.*

	No. of trials.	No. right on		Total right	If first guess only is counted, the result by experiment was	The chance of success by accident was
		1st guess.	2nd guess.			
Playing Cards...	23	6	6	13	1 quite right in $3\frac{3}{4}$	1 right in 52

In addition to the numbers which were entirely right in the first response, the suit was named rightly 4 times, and the number of the card 6 times, on the first guess.

There was also an ambiguous case not included in the above; here the seven of spades was thought of, but Mrs. Sidgwick accidentally thought of the seven of clubs, and this latter card was named by the percipient. A few trials were also made with objects, but with less success, the thing in many cases being partially guessed, as crumb-brush was said for clothes-brush, cigar for cigar-case, &c. As it would be difficult to tabulate these they are omitted.

PROF. BALFOUR STEWART'S EXPERIMENTS.

Buxton, November, 1881, and February, 1882. Proc. S.P.R. p. 38.

Things chosen.	No. of trials.	No. right on		Total right†	If first guess only is counted, the result by experiment was	The chance of success by accident was
		1st guess.	2nd guess.			
Cards	36	10	9	24	1 right guess in $3\frac{1}{2}$	1 right in 52
Nos. 10-100 ...	20	5	3	9	1 ,, 4	1 ,, 90
Objects	21	7	1	10	1 ,, 3	1 ,, 40
Names	8	4	3	7	1 ,, 2	Something inde- finitely small.
Totals	85	26	16	50		

by Mr. Malcolm Guthrie and Mr. Birchall, giving a carefully compiled record of a series of independent experiments on Thought-transference. The Hon. Secretary of this Committee has visited Liverpool, and seen the minute and jealous care with which the experiments were conducted, and witnessed some striking successes. We hope that Messrs. Guthrie and Birchall will shortly contribute a *résumé* of their work to this Society.

* These experiments have not before been recorded in the Proceedings.

† Inclusive of an occasional third attempt.

COMMITTEE'S EXPERIMENTS.

Buxton, Easter, 1882. Proc. S.P.R. p. 22 *et seq.*

Things chosen.	No. of trials.	No. right on		Total right*	If first guess only is counted, the result by experiment was	The chance of success by accident was
		1st guess.	2nd guess.			
Cards	194	61	39	106	1 right guess in 3	1 right in 52
Objects	55	21	2	28	1 " 2½	1 " 50
Names	64	35	7	42	1 " 2	{ Something in- definitely small.
Nos. 10-100 ...	33	5	3	10	1 " 6	1 right in 90
Miscellaneous...	36	5	5	16	1 " 7	1 " 20
Totals.....	382	127	56	202		

COMMITTEE'S EXPERIMENTS.

Cambridge, August, 1882. Proc. S.P.R. p. 71 *et seq.*

Things chosen	No. of trials	No. right on		Total right*	If first guess only is counted the result by expt. was	The chance of success by accident was
		1st guess	2nd guess			
Cards	248	22	18	40	1 right guess in 11	1 right in 52
Numbers ...	64	5	6	11	1 " 13	1 " 90
Totals.....	312	27	24	51		

PROF. BARRETT'S EXPERIMENTS.

Dublin, Decèmer, 1882. Proc. S.P.R. p. 76.

Things chosen	No. of trials	No. right on		Total right*	If first guess only is counted the result by expt. was	The chance of success by accident was
		1st guess	2nd guess			
Cards	109	19	7	26	1 right guess in 6	1 right in 52
Numbers ...	143	53	23	76	1 " 3	1 " 16
Words	50	25	10	35	1 " 2	1 " 4
Totals.....	302	97	40	137		

The next table shows the results obtained when the possibility of collusion was excluded, unless our own veracity be impeached, the thing selected being known to *the Committee only*. Perfect silence, as in all our trials, was preserved in each experiment:—

* Inclusive of an occasional third attempt.

TABLE SHOWING SUCCESS OBTAINED WHEN THE SELECTED OBJECT WAS KNOWN TO ONE OR MORE OF THE COMMITTEE ONLY.
To Test Hypothesis of Collusion.

Place of Trial.	Things Chosen.	No. of Trials.	No. Right on		Total Right*	If 1st Guess only is counted Experiment gave	The Chance of Success by Accident was
			1st Guess.	2nd Guess.			
a. Buxton Expts.	Playing cards †	14	9	0	10	1 right guess in $1\frac{1}{2}$	1 right in 52 trials
" "	Numbers, &c.	15	4	0	5	1 " " $3\frac{1}{4}$	1 " 90 "
b. Cambridge "	Cards †	216	17	18	38	1 " " 13	1 " 52 "
" "	Numbers	64	5	6	11	1 " " $12\frac{1}{2}$	1 " 90 "
c. Dublin "	Cards †	30	3	0	3	1 " " 10	1 " 52 "
" "	Numbers, &c.	108	32	11	43	1 " " $3\frac{1}{2}$	1 " 12 "
" "	Words	50	25	10	35	1 " " 2	1 " 4 "
	Totals.....	497	95	45	145	1 " " $5\frac{1}{4}$	1 " 43 "

a Present, Mr. F. W. H. Myers, Mr. Edmund Gurney and Miss Mason. b Present, Mr. Myers, Mr. Gurney, Mr. Barrett and Mrs. Myers. c Present, Mr. Barrett only.

* Inclusive of an occasional third guess. † A full pack of cards was invariably used, from which a card was drawn at random.

Excluding the Dublin trials, which were made before one member of our Committee only, and where the odds against success by accident also were smaller, and confining our estimate to the *first answer alone*, the experiments show that when the Committee alone knew the card or number selected there were :—

- 260 Experiments made with playing cards ; the first responses gave 1 quite right in 9 trials ; whereas the responses, if pure chance, would be 1 quite right in 52 trials.
- 79 Experiments made with numbers of two figures ; the first responses gave 1 quite right in 9 trials ; whereas the responses, if pure chance, would be 1 quite right in 90 trials.

A more favourable result would have been shown had we made these test experiments earlier, or included those of the preliminary trials made by Mr. Barrett, when the Percipient was shut in an adjoining room during the experiment. Although the proportion of success was then greater than subsequently, those trials were undertaken before the joint inquiry commenced, and before precision in our methods and records had been attained.

All these experiments were made with the Misses Creery. It may be noted that the power of these children, collectively or separately, gradually diminished during these months, so that at the end of 1882 they could not do, under the easiest conditions, what they could do under the most stringent in 1881. This gradual decline of power seemed quite independent of the tests applied, and resembled the disappearance of a transitory pathological condition, being the very opposite of what might have been expected from a growing proficiency in code-communication.

Having above exhibited the results of a long series of experiments where *collusion* was out of the question, we subjoin in the next table a general summary of our results, shewing the insufficiency of mere chance *coincidences* as an explanation. From this table, which summarises the results obtained both with the Creery family and with Messrs. Blackburn and Smith, we have excluded all those trials when the chances against success were less than 50 to 1 :—

TABLE OF EXPERIMENTS ON THOUGHT-TRANSFERENCE,

Showing success obtained under stringent conditions, when the adverse chances were beyond 50 to 1.

(To test the hypothesis of chance coincidence.)

Date.	Authority.	No. of trials.	No. Right on		Total Right*	Total Percentage Right.	Objects Selected.	Peripients.
			1st guess.	2nd guess.				
1881. Easter	Prof. Barrett	75	—	—	58	PER CENT. 77	Playing cards, fictitious names, numbers of two figures, &c., chosen in each case by one of us.	The Misses Creery
„ August	{ Mr. and Mrs. Sidgwick }	23	6	6	13	56		„
„ Nov. & Dec.	{ Profs. Stewart and Hopkinson }	64	19	15	40	62		„
1882. Easter	Committee	346	122	51	186	54	„	„
„ August	„	312	27	24	51	16	„	„
„ December	Prof. Barrett	109	19	7	26	24	„	„
„ „	Committee	23	8	6	14	61	Pains, colours, words, figures.	Mr. G. A. Smith.
	Totals.....	952	201	109	388	40·7		

* Inclusive of an occasional third attempt.

Thus, giving *every* experiment when the chances against success were beyond 50 to 1, we find upwards of 40 per cent. correctly answered if we include the second and an occasional third response. But excluding these, and confining ourselves to the *first responses only*, 21 per cent. were correctly answered. Pure chance would have given certainly less than from one to two per cent.

As the chances of hitting by pure guesswork on a fictitious name, invented by one of us, would be at least one to many thousands, and as none of the chances were less than 1 to 50, we may roughly say that if pure guesswork only were concerned, we should not have had more than :—

I quite right in 100 trials :

whereas our experiments showed we had, *if the first responses only* be allowed,

I quite right in $4\frac{1}{2}$ trials.

In these results we have not included our recent experiments on the reproduction of drawings. Here obviously an incalculable number of trials might be made, at any rate in the case of the more random and eccentric figures, before pure guesswork would hit upon a resemblance as near as that obtained in almost every case by Mr. G. A. Smith.

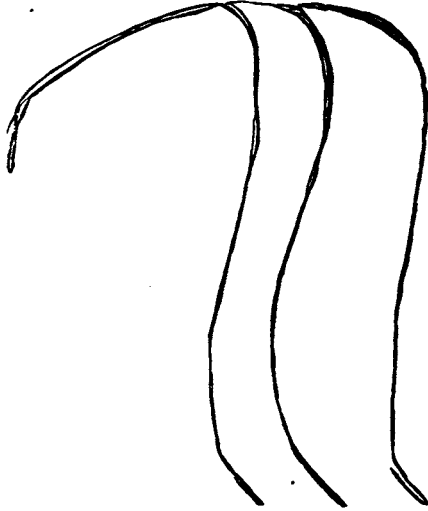
It will, we think, be evident to any candid inquirer who has carefully followed our investigations so far, that our experiments derive much strength and coherence from their very multitude and variety. In a question where the antecedent improbability of our conclusions seems so great, we could not be surprised if any single experiment—even an experiment in which sources of error were so completely excluded as in the cases where the Creery family correctly told cards, &c., unseen by anyone except the investigating Committee—should leave the reader's mind still unconvinced. But we venture to assert that the *cumulative* character of the evidence which we have now amassed, and the extent to which we have eliminated the hypotheses of collusion, chance coincidence, and muscle or sign-reading, render our claim to have established the reality of this novel class of phenomena a very strong one. We continue carefully to consider all adverse criticism; but we venture to think that much of it really depends on an *a priori* presumption of impossibility which, natural though it may be, cannot of course be legitimately opposed to positive evidence.

Hitherto, we have confined our inquiry to cases where the subject is in his or her normal waking state. But there already exists a large amount of historic evidence, of varying degrees of value, which shows that transference of thought and sensation occurs far more readily

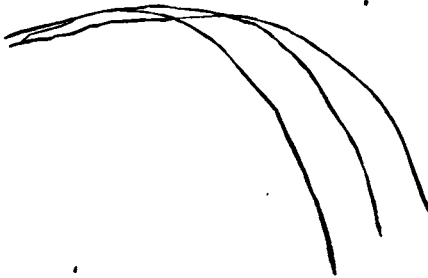
when certain changes are induced in the Percipient's nervous system by the so-called Mesmeric Sleep, or Induced Somnambulism. Some evidence of this kind is afforded in a paper which the Hon. Secretary of this Committee read before the British Association in 1876; and a portion of which is re-printed in the present part of the Proceedings. A description of many further experiments is given in the ensuing Report of the Mesmeric Committee.

The accompanying diagrams are facsimiles of the original drawings which were obtained in the manner described on page 162. The accuracy of the engraving has been ensured by photographing the original drawings and transferring the photographs on to the wood blocks.

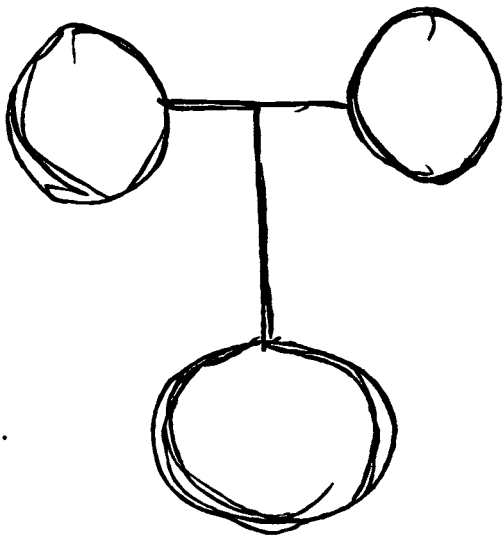
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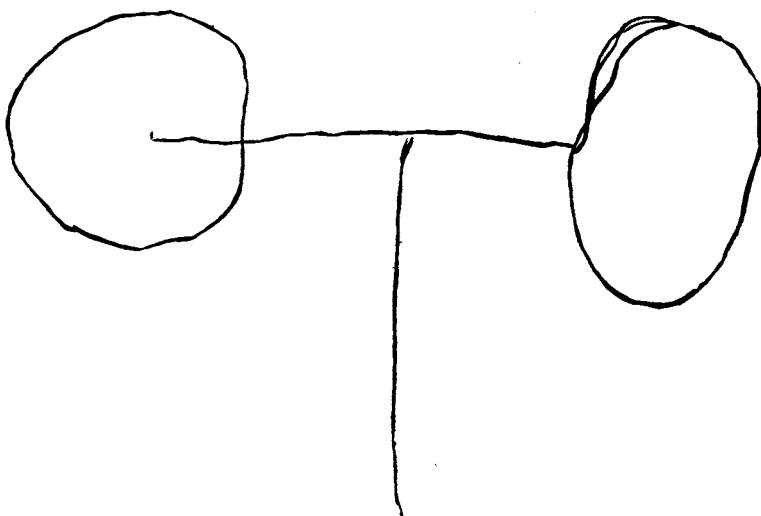
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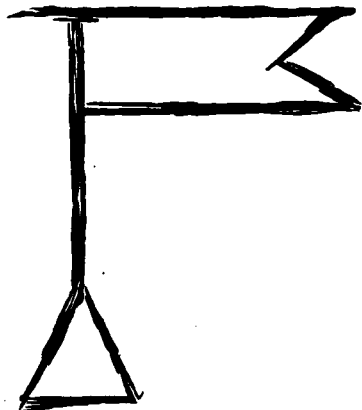
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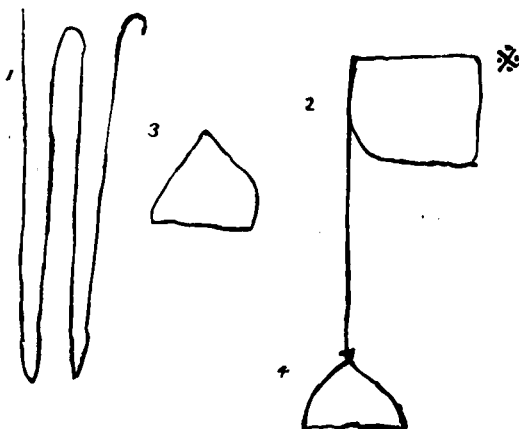
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NO. 3. ORIGINAL DRAWING.

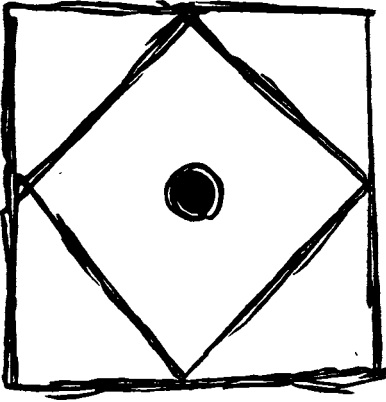


NO. 3. REPRODUCTION.

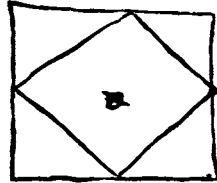


The figures indicate the order in which the drawings were made. At the close Mr. Smith said 1 should be "put on here somewhere," pointing to the spot where the asterisk is shown.

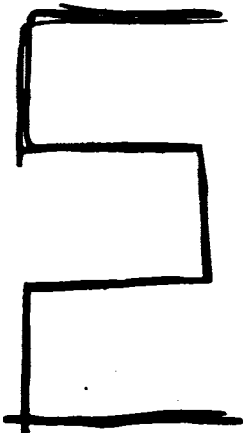
NO. 4. ORIGINAL DRAWING.



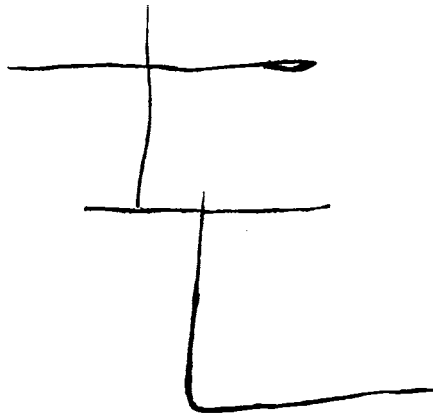
NO. 4. REPRODUCTION.



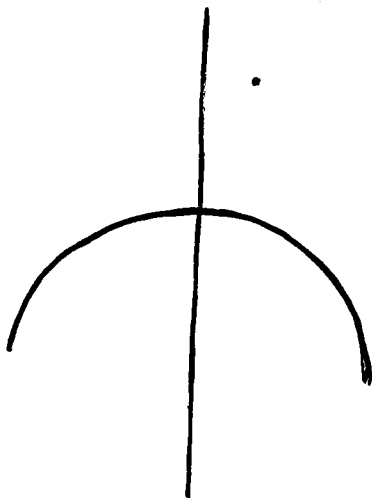
NO. 5. ORIGINAL DRAWING.



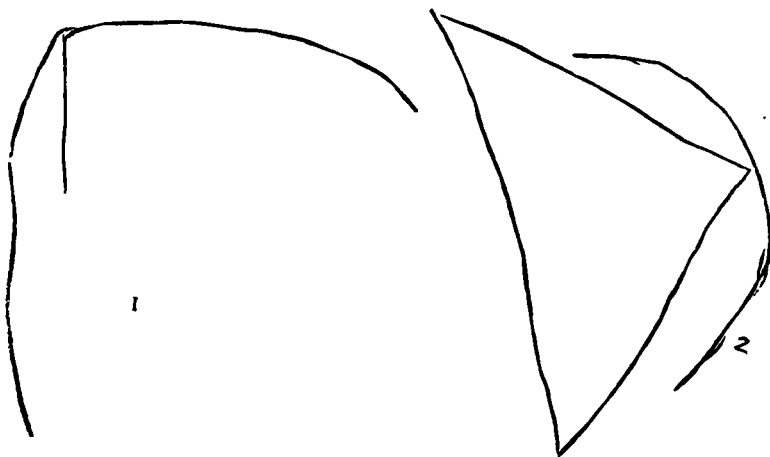
NO. 5. REPRODUCTION.



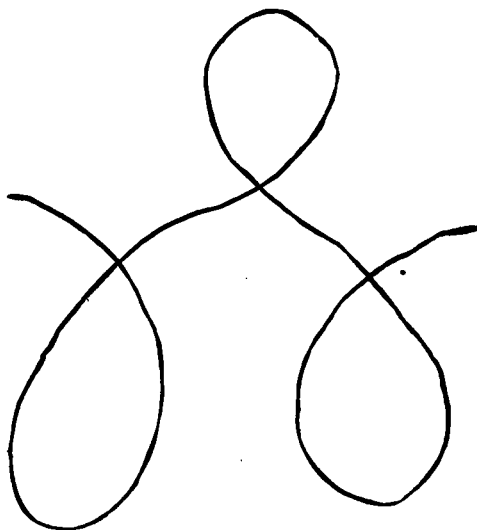
NO. 15. ORIGINAL DRAWING.



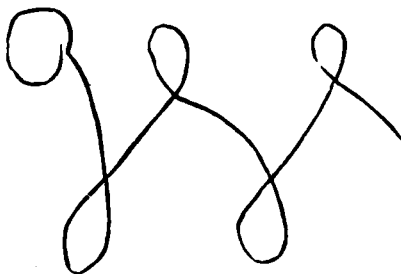
NO. 15. REPRODUCTION.



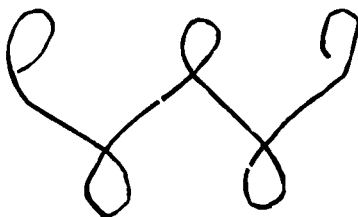
NO. 16. ORIGINAL DRAWING.



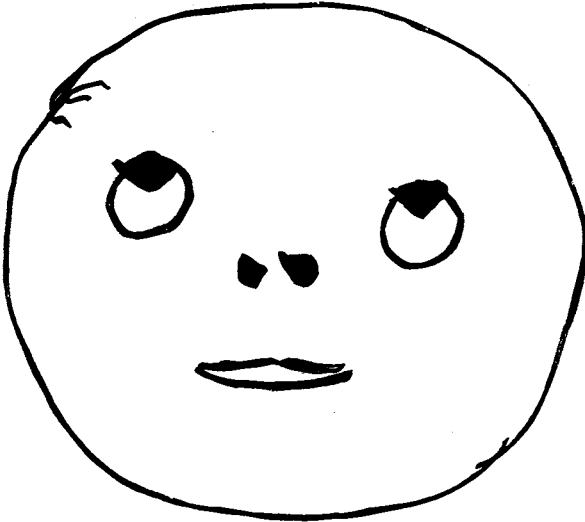
NO. 16. REPRODUCTION.



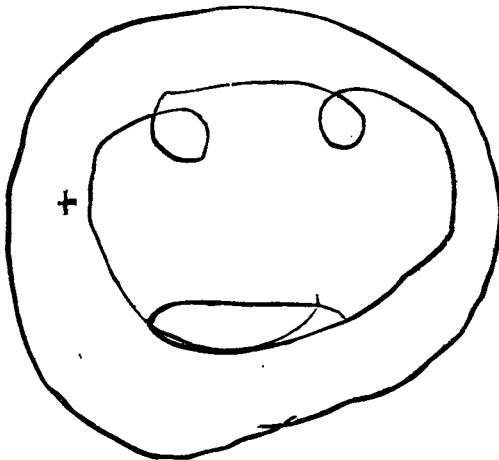
Mr. Blackburn had not precisely remembered the figure, and drew the following as representing what he had in his mind.



No. 17. ORIGINAL DRAWING.



No. 17. REPRODUCTION.



Inner circle begun at point marked +, and then carried round in one continuous line from left to right.

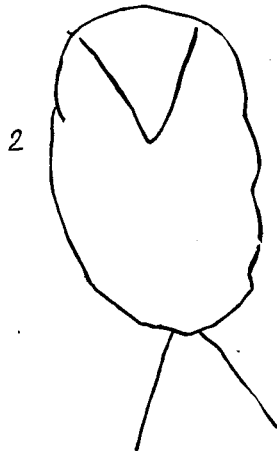
NO. 18. ORIGINAL DRAWING.



NO. 18. REPRODUCTION.

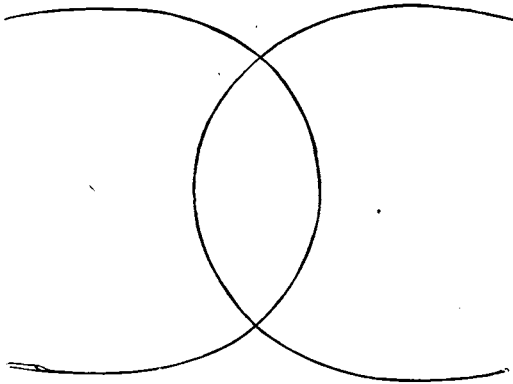


NO. 18. REPRODUCTION.

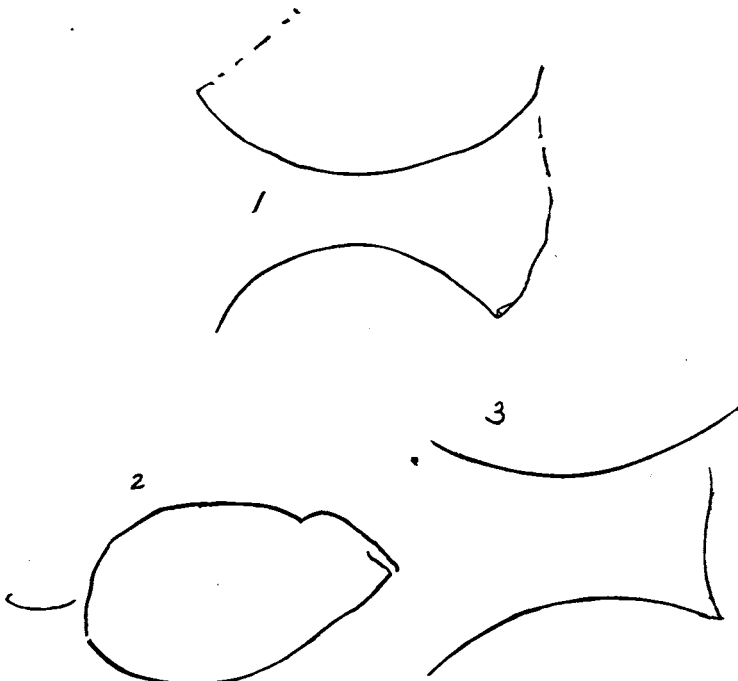


Mr. Blackburn forgot the eyes.

NO. 19. ORIGINAL DRAWING.

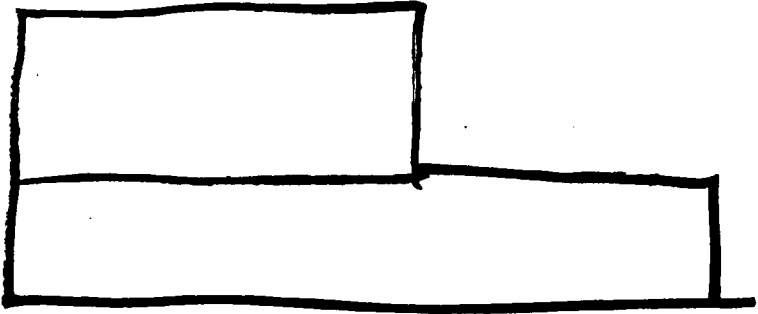


NO. 19. REPRODUCTION.

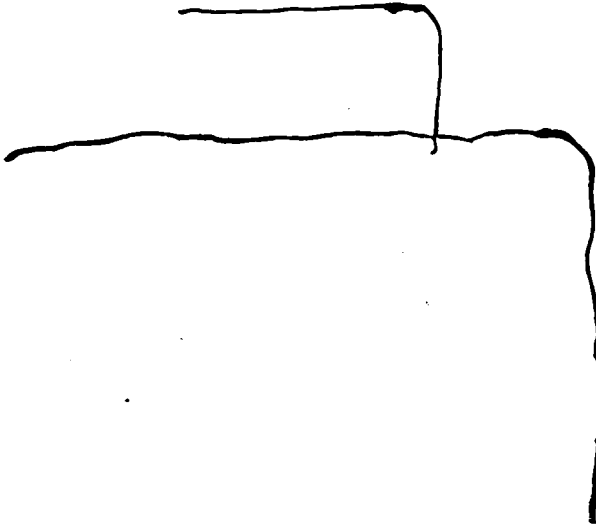


Mr. Blackburn was fixing his mind on the oval, in order to make Mr. Smith connect the lines he had got.

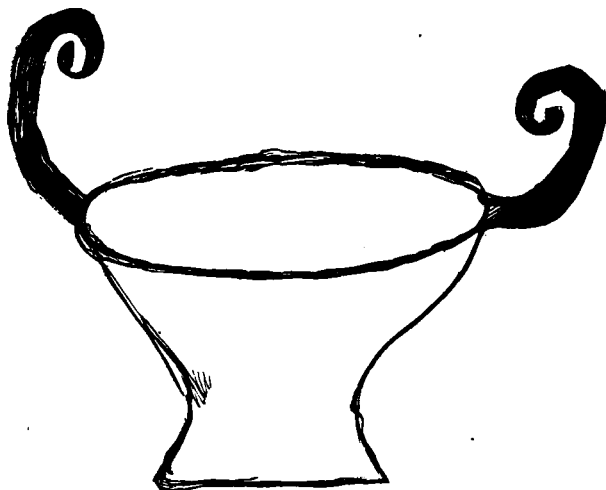
NO. 20. ORIGINAL DRAWING.



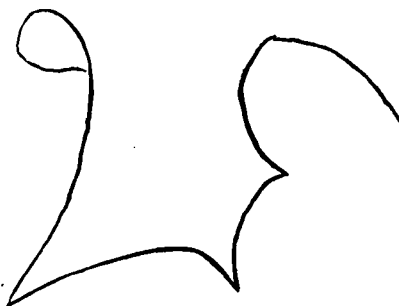
NO. 20. REPRODUCTION.



NO. 21. ORIGINAL DRAWING.

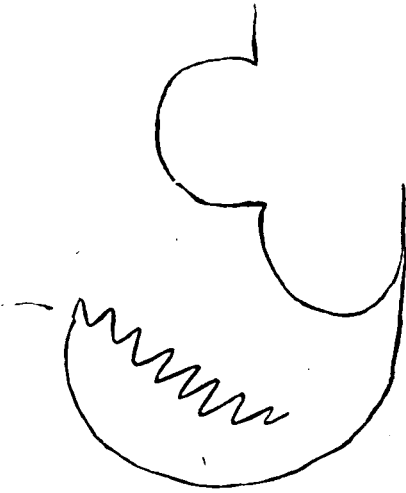


NO. 21. REPRODUCTION.

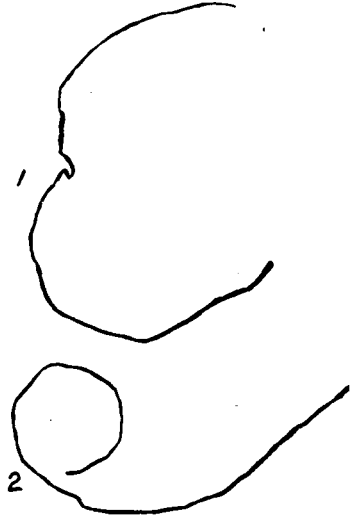


Mr. Blackburn was imagining the handles as turned *outwards*.

No. 22. ORIGINAL DRAWING.



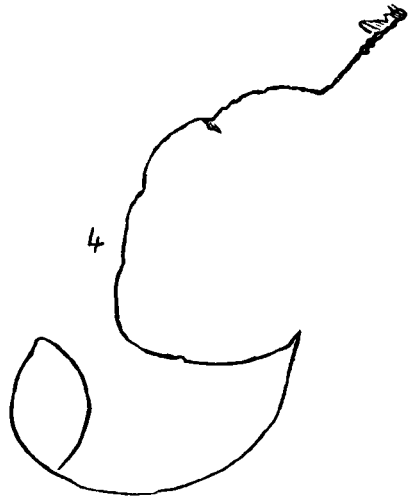
No. 22. REPRODUCTION.



No. 22. REPRODUCTION.



No. 22. REPRODUCTION.



For a description of this experiment see page 165.

II.
FIRST REPORT
OF THE
COMMITTEE ON MESMERISM.

Committee :—W. F. BARRETT, F.R.S.E.*; EDMUND GURNEY, M.A.*; FREDERIC W. H. MYERS, M.A.*; HENRY N. RIDLEY, M.A., F.L.S.; W. H. STONE, M.A., M.B.; GEORGE WYLD, M.D.; and FRANK PODMORE, B.A.,* *Hon. Secretary.*

The work which lies before the Committee on Mesmerism differs, to some extent, from that appropriated to other Committees of this Society. In most of the subjects which we have included under the designation of "Psychical Research," the ground was so inadequately explored as to afford no basis for any confident scientific induction. In mesmerism, on the other hand, the investigators have been both numerous and intelligent; the ground has been traversed and retraversed; the literature of the subject already forms a small library, and the evidence, if impartially studied, appears to be harmonious, and on many points complete. Nevertheless, if we turn from the advocates of mesmerism to our natural guides on such subjects, we find that scientific writers, both at home and abroad, have united in ridiculing the pretensions of the mesmerists—at first attributing all the phenomena to trickery and delusion, and subsequently admitting many of the facts, but explaining them as within the domain of well-recognised psychological or physiological laws. And if we compare the scientific utterances of to-day with those of half a century back, we shall see that the discredit of mesmerism, as such, has been distinctly on the increase. True, the orthodox scientific attitude was never anything but hostile. Thus, in 1839, the then leading medical organ, edited by Sir John Forbes, wrote as follows: "Considering the high sanction which even a temporary belief in the powers of animal magnetism has obtained in this country, we look upon its recent progress, and its abrupt and shameful fall, as calculated to degrade the profession. . . . English practitioners are now ashamed of the very name." But the scepticism was not entirely thorough-going; for, six years later, the same journal remarked: "Mesmerism has hardly received fair play at the hands of many of our professional brethren, and its pretensions are too well supported to justify an opposition made up almost exclusively of ridicule and contempt. We think it is proved, or, to say the least, we think it to be

made in the highest degree probable, that there is a reality in the simple phenomena of mesmerism ;” and the writer ends by declaring “his full belief in the reality of some of the facts which have often been set down as sheer delusion or imposture.”* But this opinion has not been shared by later authorities in medicine or physiology. Dr. Carpenter, for instance, “does not hesitate to express the conviction, based on long, protracted, and careful examination of the evidence adduced to prove the existence of a mesmeric force, acting independently of the consciousness of the ‘subject,’ that there is none which possesses the least claim to acceptance as scientific truth. * * * Mesmerisers who assert they could send particular individuals to sleep have altogether failed to do so when the ‘subjects’ were carefully kept from any suspicion that such will was being exercised. * * * Nothing is more easy than to explain the peculiar *rapport* between the mesmeriser and his ‘subject’ on the principle of dominant ideas.”† And in his published lectures on “Mesmerism, &c.,” Dr. Carpenter draws a broad line of distinction between the phenomena of induced somnambulism, which he admits to be now a recognised physiological condition, and the alleged facts of mesmerism, such as the “*rapport*” between the mesmerist and his subject, their community of sensation, the induction of general or local anæsthesia (so far as this is not dependent on suggested ideas), the effluence of any peculiar power from the mesmerist, &c., &c.

And this leads us to a most important observation, namely, that the main cause of the increasing incredulity and contempt shown towards mesmerism, as such, has been, not an error, but a truth, or at least a partial truth,—the discovery, namely, of a real means of explaining many of the facts, without resorting to any “mesmeric.” hypothesis. The credit of this discovery is due to a countryman of our own, the late Mr. Braid, whose name deserves a wider reputation than it has received. He shewed, by a long and admirable series of experiments, that mere fixation of the eyes in a strained position was often enough to throw the subject into a condition in which many of the phenomena attributed to mesmeric influence could be easily produced. Similar experiments have been lately conducted by Professor Heidenhain, of Breslau, whose conclusions are decidedly in advance of anything contained in the standard treatises on physiology.‡ His explanation wholly rejects

* *British and Foreign Medical Review*, 1845, pp. 440 and 485.

† *Mental Physiology*, 2nd Edition, pp. 619-623.

‡ A summary of the views advanced by Heidenhain is to be found in Dr. McKendrick’s article on Mesmerism in the *Encyc. Brit.* Ninth Edit. Other important psycho-physiological investigations into this hypnotic state are being carried on in America, France, Italy, and Germany, as well as in our own country. We hope to notice these on a future occasion.

"expectant attention," "dominant ideas," and all mental factors whatsoever, and refers the phenomena to what is practically a wide extension of the range of "reflex action." He conceives that, in the hypnotic condition, stimulation by word or gesture of the lower sensory centres in the "subject's" brain, instead of passing on in the usual way to the higher portion of that organ and there giving rise to consciousness and volition, passes by a direct path to the immediate centres of motion, and there gives rise to automatic responses, which may take the form of mimicry or of unconscious carrying out of simple orders. In his view, the opening of this direct path, with its result of a certain and involuntary response, is due to *inhibition*, brought about by monotonous sensory impressions of the functions of those higher cortical centres which are associated with choice and reasoning, and which normally control the lower motor centres. It will thus be evident that anything of the nature of a specific effluence or influence in mesmerism is rejected in our day by Heidenhain, as it was by Braid 40 years ago. Hypnotism or induced somnambulism, whether accompanied by consciousness or not, has been regarded as covering *the whole ground*, and thus, in proportion as it has become more and more orthodox to admit many of the facts commonly known as "mesmeric," has it become more and more heretical to attribute them to "mesmeric agencies."

This point in the history of the subject is of such prime importance that a brief reference to it was absolutely indispensable. We do not, however, propose to enter at present upon anything like a historical survey. Such a task needs, indeed, to be accomplished; but it would require much more space than could fitly be devoted to it in this Report. We will content ourselves with a single remark; namely, that the great divergences in the conclusions reached by different observers,—say, by the first French Commission as contrasted with the second,* or by Dr. Carpenter as contrasted with Dr. Elliotson or (we may add) with Heidenhain—are by no means necessarily to be attributed either to prejudice on the one side or to credulity on the other. It seems to us that the champions of the different sides, who have bandied these terms so freely, have generally failed to realise the immense *variety* of the phenomena which these abnormal states present to our study, and have been too apt to assume the completeness of their own particular observations. And the conflict of opinion which prevails at any rate renders the duty of this Committee doubly clear, viz., to submit the whole subject to renewed and careful experiment. If, as we have hinted, the differences of previous observers have largely depended

* The first was appointed to investigate and report on mesmerism in 1784; the second reported in 1825. Both included distinguished physicians and trained observers; and the second report, which contains a mass of carefully conducted experiments, was favourable to the claims made by mesmerists.

on the limited scope (or on the varying luck) of their observations, that is the most cogent reason for making the basis of our inductions as complete as possible.

From what has been said, it will readily be understood that the term "*mesmerism*," which occurs in the title of the Committee, and which will be used throughout this Report, has been selected as the most general description, and not as involving any theory, or pledging us to any particular explanation of the facts, such as is suggested, *e.g.*, in the terms *animal magnetism* or *electro-biology*. But while for convenience sake we have adopted the most general name, we must state at once that we anticipate, as we advance, the necessity of limiting and specialising the meaning of the word "*mesmerism*." For since the term "*hypnotism*," as just explained, is confined to phenomena which may be produced *without any special influence or effluence* passing from the operator to the subject, and has been adopted as a complete designation of these phenomena by those who emphatically deny that any such influence or effluence can exist, it will be natural for us, if we come across further facts to which the "*hypnotic*" hypothesis proves inapplicable, to describe these facts as *par excellence* "*mesmeric*." And, as we have seen, it would be no unfair description of these two classes of alleged phenomena, to say that the line between them is the line which, so far, recognised science has not overstepped: by the science of this country, at any rate, at the present day, "*hypnotism*" is pretty widely acknowledged, and "*mesmerism*" almost universally rejected.

Before recounting our more consecutive experiments, we ought to mention that we have tried on several occasions to influence various persons—boys of from 12 to 20 years old, in the manner described by Braid, but, hitherto, with little success. The method is as follows: The person to be operated on is placed in a comfortable position in a chair. Perfect silence is observed, and every precaution is taken not to distract the attention of the patient. He is then bidden to look at a coin, or other bright object, held about 15 inches from his forehead, in such a position as to produce in his eyes a slight inward and upward squint. Braid states that he found the great majority of the persons on whom he operated susceptible to this method. We, on the other hand, have only had even partial success in one case, that of Mr. W. North, late lecturer at Westminster Hospital. As a full account of this experiment will be published elsewhere, it will be sufficient here to state that Mr. North, after gazing intently for upwards of half-an-hour at a bright copper disc, succeeded in bringing himself into a condition in which some of the phenomena observed by Braid and Heidenhain were successfully demonstrated, namely, (partial) insensibility to pain, extreme muscular irrita-

bility, and a deadening of the mental faculties. Equally characteristic was Mr. North's very imperfect subsequent recollection of what had taken place.

But the rest of the phenomena here described were preceded by the conditions ordinarily associated with mesmeric influence. They were observed, for the most part, in a willing and intelligent young man of 20, Fred Wells by name—the son of a baker in Brighton. Other youths have also been tried, and some are now under experiment. The operator in every case has been Mr. G. A. Smith, of Dulwich, S.E., and lately of Brighton. Mr. Smith's method with his "subjects" is as follows: The subject is placed in a chair, with his hands in his lap, and he is told to direct his attention exclusively to a coin or other bright disc of metal, which is placed in his hands. Mr. Smith, meanwhile, draws his hands, at intervals, slowly downwards across the subject's head and face, always in the same direction. His hands, generally, do not touch the surface of the skin, nor even approach very near to it. After a time varying from two to twenty minutes has been thus occupied, Mr. Smith raises the subject's head, closes the eyes, and presses his thumb on the forehead between the eyes. He then bids him open his eyes. If the boy succeeds in doing so without difficulty, the whole operation is repeated; and if on a second trial no effect is produced, the subject is dismissed. But it not infrequently happens that the boy, when told to open his eyes, finds himself unable to do so; or only succeeds after many efforts. Mr. Smith then strokes the muscles at the corner of the mouth; and, after a short interval, both eyes and mouth being closed, he is told to open them. If the subject is a good one, he fails to do this, and it is very strange to watch the contortion of his features, and his evident vexation, whilst he endeavours to thwart the mysterious influence which has sealed his lips and eyes. This curious phenomenon was amusingly illustrated in the person of a friend of one of us, Mr. Harold Wolferstan, of Tavistock. Mr. Wolferstan had been brought into the mesmeric state by Miss Smith, sister of the Mr. G. A. Smith above-mentioned, in the presence of Dr. Myers and Mr. Podmore. Believing him to be, at the time, unconscious, these gentlemen were talking about him pretty freely; and it was not until Mr. Wolferstan had been restored to his normal condition that they discovered that he had heard the whole of the conversation, but had been unable to open his lips or make any other movement to shew his interest in it. On another occasion, a friend of ours, after subjection to the usual process, was left alone, apparently asleep, in an arm-chair, while the Committee turned their attention to other "subjects." Meanwhile our friend awoke, but found himself unable to move his limbs, and the first intimation which we had of his condition was the plaintive request, "Please come and undo me."

Influence of Suggestion.—When the “subject” has been rendered so far amenable to the operator’s manipulation, other singular phenomena may be observed. It is easy to make him the victim of any hallucination which the fancy of those present may suggest. That he is, for the time, really under the influence of the suggested idea, and genuinely believes what he is told, cannot be doubted, at any rate after a sufficiently long and varied experience. To suppose that the multifarious gestures and movements, performed in support of the characters which they are bidden to assume, are parts of a conscious and deliberate scheme of deception, would be to attribute to the half-educated boys who formed the subjects of these experiments, a sustained capacity for acting a part, as well as rare genius for mimicry and power of self-control. Moreover, that the hallucination is, in most cases, complete, is the more readily perceived by its incompleteness on certain occasions. Sometimes the reasoning faculties are but partially subdued, and the boy offers a half incredulous resistance to the suggested impression. A very striking instance of this kind was the following. Mr. Smith dandled a handkerchief before a boy, telling him that it was a baby. The boy listened, but half convinced, and smiled incredulously. But he was gradually overcome by the idea suggested, and taking the handkerchief, laid it carefully across his arms, in orthodox nursery fashion. No sooner, however, did Mr. Smith divert his attention, than reason began to assert itself again. The boy discovered an unusual deficiency in his nursling; he kept furtively looking round, with most genuine anxiety and hopeless bewilderment, to discover the head of his baby. In the midst of his perplexity he was recalled to his proper senses, and joined with us in laughing at his own discomfiture.

The illusion, however, is generally untroubled by any doubts. On one occasion, for example, Wells was given a candle, which he was assured was a sponge-cake. He broke it in pieces, remarking that it was very stale, and actually ate about an inch and a-half of it. Shortly afterwards, he began to feel the effects of his unusual meal; and, when pressed, flatly declined to have any more of “Mr. Gurney’s sponge-cakes.” On another occasion, he ate salt greedily, when told that it was sugar; and rejected sugar in great haste under the impression that it was cayenne pepper. When white pepper was blown up his nostrils, he being under the impression that it was mignonnette, not only did he not sneeze, but his eyes did not water to any appreciable extent, a fact which was ascertained by opening the lids. Other experiments intervened, and no sneezing occurred until some ten minutes afterwards, when he was given common salt, and told it was snuff. He smelt at it and then sneezed violently, with the characteristic spasm, for some little time. He drank a spoonful of vinegar with much relish, believing

it to be cream, and subsequently ate a slice of bread and mustard as plumcake, eagerly asking for more.

On another occasion Wells, then in his normal condition, was bidden to look fixedly at Mr. Smith, and was then impressed with the idea that Mr. Smith had left the room, though Mr. Smith was immediately in front of him. He anxiously looked for him all over the room and even left the room to search over the house. When Mr. Smith was pointed out he was unable to recognise him—"had never seen him before." His power of imitation under the influence of a suggested idea was most remarkable. Thus he admirably mimicked at different times a parrot, a worm, a clock, a statue, a bear, and a frog. His leaps under the influence of the last-named impression were so energetic and so reckless that it became necessary to discontinue the experiment, lest he should do himself some injury. When he was told on another evening that he was a *nightingale* it was anticipated that he would confine himself as in the case of the parrot, to vocal imitation mainly. But his interpretation of the part, though sufficiently thorough, was quite unconnected with our expectations. He rushed, without hesitation, at a set of high book-shelves, well filled with books, which lined one wall of the room, mounted—we might almost say fluttered—up them with wonderful speed, and then crouched in a corner on the top of the shelves, with his head against the ceiling, violently and ineffectually flapping his arms, as a bird, accidentally imprisoned in a room, will flap its wings. Nor was his assumption of *combined* parts less complete. He at once succumbed, for example, to the suggestion that one side of him was a nurse, and the other a windmill; and for many minutes his sedulous though left-handed attentions to an imaginary infant were quite unimpaired by the no less sedulous revolutions of his right arm.

From these lighter stages of the induced condition, the sensitive would be roused by a loud clap of the hands, and the word "Right" shouted at him by the operator. He would wake suddenly with a dazed expression, and would as a rule be found to retain not the slightest recollection of what had taken place. Sometimes, as in Mr. North's case, there was a slight remembrance of the main incidents. And it was found that if a command or a prohibition were strongly impressed upon the "subject" in the sleep, he would execute the behest on waking. In obedience to such a command Wells was made to leave the house, on going home, without his hat: indeed, his subjection to the command was so thorough that the hat could only be put on his head by force. This sensitive was frequently made the subject of such commands. He invariably executed them in the waking state, but, as a rule, with some hesitation and reluctance—not unnatural, perhaps, in view of the tasks demanded of him. Thus he, on one occasion, threw

his waistcoat on to the fire in obedience to the dominant idea impressed upon him in the sleep. Similarly at another time he thrust his fingers between the bars into the lighted fire, but was of course prevented from injuring himself. The effort to remember these and other commands, where many consecutive actions were enjoined, made his head ache, and thus at last caused the failure of the experiment. But in no case had he any recollection at all of the circumstances under which the command was given. His own account of the matter was that, on waking, he would feel that he "had to do something;" but the exact nature of the task would, as a rule, become clear to him only after a few minutes' reflection. He appeared generally to resist the idea thus suggested, until its influence became too strong for him.

The above examples, which are merely selections from a long and varied list, may suffice to indicate our complete adhesion to the view (taken by the advocates of hypnotism and mesmerism alike) that in certain states of the nervous centres suggested ideas may acquire a dominant and practically irresistible force. Of course experimentation of this kind can scarcely exclude all sources of error with scientific precision—that is to say, the state of subjection to a dominant idea can be, to a certain extent, simulated, as epilepsy or coma can be simulated; and in the one case, as in the other, some very painful tests might seem necessary to establish the genuineness of the alleged state. But the need of such tests *here* is rather apparent than real. For this particular phenomenon—on public platform or in private consulting-room—rarely fails in practically *demonstrating itself*; and observers of all shades of opinion, who have witnessed this state of subjection to a suggested idea, are hardly more disposed to deny that the state is, at any rate in the majority of cases, a genuine one, than observers of epilepsy are disposed to deny that such a thing as true epilepsy exists.

Community of Sensation.—But we come now to a thesis more controverted and more controvertible. We maintain (and here we are supported by one section only of previous inquirers) that we have frequently observed a very remarkable *community of sensation* between operator and "subject"; or, as the phenomenon might be, perhaps, more correctly described, a transference of sensation from the operator to the "subject." This phenomenon is obviously closely allied to those which have been occupying the Committee on Thought-transference. It differs, however, from the experiments recorded by that Committee in that the Percipient is here in the mesmeric sleep, and not, therefore, in his normal state. The experiments were conducted as follows. Fred Wells was placed in a chair blindfolded, and Mr. Smith stood behind him. Wells was then sent into the mesmeric sleep through passes made by Mr. Smith. Some part of the latter's body would then be pricked or pinched tolerably severely—the operation lasting,

generally, one or two minutes. Perfect silence was observed throughout, except for the simple and uniform question: "Do you feel anything?" This question was asked by Mr. Smith, as the subject appeared not to hear any other speaker. In the first set of experiments Mr. Smith held one of Wells' hands, but this was found subsequently to be unnecessary, and the later experiments were performed without contact of any kind between Mr. Smith and the sensitive.

First Series. January 4th, 1883.

1. The upper part of Mr. Smith's right arm was pinched continuously. Wells, after an interval of about two minutes, began to rub the corresponding part on his own body.
2. Back of the neck pinched. Same result.
3. Calf of left leg slapped. Same result.
4. Lobe of left ear pinched. Same result.
5. Outside of left wrist pinched. Same result.
6. Upper part of back slapped. Same result.
7. Hair pulled. Wells localised the pain on his left arm.
8. Right shoulder slapped. The corresponding part was correctly indicated.
9. Outside of left wrist pricked. Same result.
10. Back of neck pricked. Same result.
11. Left toe trodden on. No indication given.
12. Left ear pricked. The corresponding part was correctly indicated.
13. Back of left shoulder slapped. Same result.
14. Calf of right leg pinched. Wells touched his arm.
15. Inside of left wrist pricked. The corresponding part was correctly indicated.
16. Neck below right ear pricked. Same result.

In the next series of these experiments Wells was blindfolded, as before; but in this case a screen was interposed between Mr. Smith and Wells; and there was no contact whatever between them. During two or three of the trials Mr. Smith was in an adjoining room, separated from Wells by thick curtains.

Second Series. April 10th, 1883.

17. Upper part of Mr. Smith's left ear pinched. After the lapse of about two minutes, Wells cried out: "Who's pinching me?" and began to rub the corresponding part.
18. Upper part of Mr. Smith's left arm pinched. Wells indicated the corresponding part almost at once.

19. Mr. Smith's right ear pinched. Wells struck his own right ear, after the lapse of about a minute, as if catching a troublesome fly, crying out: "Settled him that time."
20. Mr. Smith's chin was pinched. Wells indicated the right part almost immediately.
21. The hair at the back of Mr. Smith's head was pulled. No indication.
22. Back of Mr. Smith's neck pinched. Wells pointed, after a short interval, to the corresponding part.
23. Mr. Smith's left ear pinched. Same result.

After this, Mr. Smith being now in an adjoining room, Well, began, as he said, "to go to sleep"; and said that he "didn't want to be bothered." He was partially waked up, and the experiments were resumed.

24. Salt was put into Mr. Smith's mouth. Wells cried out, "I don't like candle to eat" (an idea possibly suggested by the word "candle" having been mentioned in his hearing a few minutes before).
25. Powdered ginger, of a particularly hot description, put into Mr. Smith's mouth. Wells presently exclaimed, "I don't like hot things; what do you want to give me cayenne for?"
26. Salt was then again placed in Mr. Smith's mouth. Wells exclaimed, "Why do you give me nasty hot sweetmeats?"
27. Wormwood in Mr. Smith's mouth. Wells cried, "Makes my eyes smart: don't like mustard."

It will be noticed that in these last two experiments, the taste of the ginger apparently persisted, and obscured all later sensations.

28. Mr. Smith's right calf pinched. Wells was very sulky, and for a long time refused to speak. At last he violently drew up his right leg, and began rubbing the calf.

After this Wells became still more sulky, and refused in the next experiment to give any indication whatever. With considerable acuteness he explained the reasons for his contumacy. "I ain't going to tell you, for if I don't tell you, you won't go on pinching me. You only do it to make me tell." Then he added, in reply to a remonstrance from Mr. Smith, "What do *you* want me to tell for? they ain't hurting *you*, and *I* can stand their pinching." All this time Mr. Smith's left calf was being very severely pinched.

Thus out of a total of 24 experiments in transference of pains, the exact spot was correctly indicated by the subject no less than 20 times. Two out of the four failures had been anticipated, previous experience having shewn that the experiment rarely succeeded when the

infliction consisted in pulling the hair. There remain, then, but two failures unaccounted for, and in only one of these cases was a *wrong* indication given, the boy merely remaining silent on the other occasion. It would be hazardous to draw any positive conclusions from the results of the four trials with *tastes*. But we shall hope to continue our experiments in this direction at no distant date.*

It is obvious that the impressions here recorded as having been transferred by sympathy from operator to subject might conceivably have been conveyed by a code, with less difficulty, at any rate, than in the case of the diagrams given in our report on Thought-transference. where, nevertheless, our precautions may appear to have been more elaborate than *here*. But the fact is that we never attempted these experiments in "mesmeric sympathy" until we had satisfied ourselves of the genuineness and completeness of the "mesmeric sleep." That state was, as we think, tolerably unmistakable; nor did any one circumstance occur during the whole course of our experiments which threw any doubt on its reality, or on the perfect integrity of the operator.

Rigidity and Anæsthesia.—But a more distinct and definite testimony to the genuineness of this induced condition is to be found in our experiments on *anæsthesia*. We satisfied ourselves, by a great number of carefully-varied experiments, that it was possible to induce in the subject: (1) either a general insusceptibility to pain inflicted on any part of his person—and this state generally existed even while he was acutely sensitive to pains inflicted on the *operator*; (2) or an anæsthesia of some specified part of the subject's body, chosen by ourselves. Thus, a limb or a portion of a limb, after being stroked two or three times by the operator's hands, would assume a condition of perfect rigidity, in which pinching, pricking, burning, or strong electric shocks might be applied without producing the slightest protest or sign of pain.

This condition would last for a considerable period; indeed, the Committee have not yet observed a case in which rigidity, when once fully induced in a sensitive subject, has disappeared of its own accord. The limb is soon restored to its normal condition when stroked with the hand as before—the passes being made, however, in the reverse direction. It is noticeable that the insensibility is often very strictly defined. All parts above a definite line, apparently limited by the range of the "passes," are in the normal condition; all below seem as void of sensation

* Some independent observations of one of our Committee strongly corroborate this "mesmeric sympathy." See the facts recorded by Professor Barrett in a paper read before the British Association in 1876, and reprinted in the present part of our Proceedings, as to the transference of impressions from *himself*, as mesmeriser, to an ignorant Irish peasant girl, who was his subject.

as a piece of shoe-leather. Occasionally, whilst all sense of pain is destroyed, the sense of contact is, to a certain extent, preserved; then the "subject" will feel a pin touch the skin, but cannot feel it driven into the flesh. In our observation the two phenomena of rigidity and insensibility to pain have always been conjoined; but it is believed that this is not always the case.

In the case of Fred Wells, above-mentioned, Harry Manson, a tradesman's apprentice, and several other "subjects" whom the Committee have employed, this insensibility has been very thoroughly tested; and the results abundantly confirm the observations of Esdaile, Elliotson, and others. To cite one experiment only, out of many which were tried upon Wells—a very willing subject—in order to demonstrate this induced insensibility to pain. One arm having been bared and stiffened, the wires from a powerful induction coil were applied to the skin, so as to affect the flexor muscles of the fingers. The boy, who was otherwise in the normal state, watched with tranquil curiosity the action of the battery, which he had never seen before; he was greatly amused and delighted with the rapid and spasmodic movements of his fingers. The wires were then suddenly applied to a sensitive part, and the boy jumped out of his chair from the violence of the shock.

It may be noted that in the case of the eye, insensibility seems to be produced as part of the general condition—at any rate without any special manipulation of the organ. If the sleep is profound, and the eyes, which are firmly closed, are forced open, the sensibility of the conjunctiva may be proved to have almost entirely disappeared. The contractility of the pupils is also very much lessened; as may be conveniently demonstrated by bringing a lighted candle close to the eyes, when but a very slight movement is observed.*

We have dealt thus far with three main phenomena connected with the mesmeric state, viz.: (1) The dominance of a suggested idea; (2) transference of sensations, without suggestion, from operator to patient; (3) induction of general or local anæsthesia. The first of these three theses is, we believe, on the high road to universal acceptance. The mass of recorded testimony to it is enormous; the experiments are not difficult to repeat; and the discussions of physiologists are beginning to turn on the explanation rather than on the

* Experiments on pain, even when as in these cases their result is to prove their own absolute *painlessness*, should be sparingly employed; and we hardly anticipate any such scepticism as to the induced anæsthesia of mesmerism as can make it needful to renew them. We need, perhaps, scarcely add that the Committee are fully aware of certain special dangers attending mesmeric experiments. The strictest precautions have always been taken to avoid undue interference on the part of any onlooker with the mesmerised "subject"; and our care in this respect will not be relaxed in future.

existence of the phenomenon. The *second* thesis is, of course, much more keenly contested. We think that we have added something to the facts recorded in its favour, and we have every hope of adding more. This mesmeric sympathy is, as we have suggested, entirely consonant with our experiments in Thought-transference in the normal state; and as we learn more of the philosophy of the subject it may be hoped that the two inquiries will throw reciprocal light upon each other. The *third* point—the production of anæsthesia—has been already established by Esdaile and others with what seems to us even overwhelming completeness, and is to a certain extent admitted by many exponents of modern physiology. But here we come face to face with one form of what must now be accounted as the fundamental problem of the whole inquiry. Is this anæsthesia produced by mere expectant attention exercised in a particular state of the nervous system? Is it (in somewhat different terms) the culminating example of the dominance of a suggested idea? Or is it, again, the result of the inhibition of certain sensory centres in consequence of prolonged stimulation of the peripheral extremities of the nerves. This is the explanation given by Heidenhain, and, with some modifications, previously by Braid; but it only covers the cases where the passes have been accompanied by actual contact, which in our experiments has by no means always been the case. Or is it, lastly, the result of some specific effluence from the operator which may act without actual contact, independently of the subject's knowledge or expectation? It is on this question that we are now concentrating our attention; and it is only fair to say that our results point strongly in the direction of the *third*—the least antecedently probable, the least generally accepted explanation. But the question of this specific influence—of *mesmerism*, as opposed to *hypnotism*—is too complex and important to be approached in a fragmentary manner. It admits of direct investigation in several ways: and we prefer to defer the publication of results until a more complete reproduction of the experiments of others, with added tests of our own, may have afforded a wider basis for discussion.

III.

FIRST REPORT

OF THE

"REICHENBACH" COMMITTEE.

Committee.—W. F. BARRETT, F.R.S.E.*; Rev. MAXWELL H. CLOSE, M.A.; ST. GEORGE LANE-FOX*; EDMUND GURNEY, M.A.; FREDERIC W. H. MYERS, M.A.; A. T. MYERS, M.D.; EDWARD R. PEASE; HENRY N. RIDLEY, M.A., F.L.S.; W. H. STONE, M.A., M.B.; and WALTER H. COFFIN, F.L.S., F.C.S.,* *Hon. Sec.*

In accordance with their preliminary Report, the Committee appointed to repeat Baron Reichenbach's experiments have, so far, confined themselves entirely to his original and fundamental observations upon the magnet, and particularly to the alleged "Magnetic Light."

If a luminous effect of ordinary magnetism, though rarely seen, is, as Reichenbach believed, an actual physical phenomenon, and demonstrable as such, its establishment would not only have a high scientific interest outside the main objects of this Society, but distinctly lend importance and a degree of credibility hitherto wanting to his description of correlative and less purely objective phenomena.

The necessary experimental conditions have latterly been fairly well realised by the Committee: A room on the Society's premises, about 13 feet square and 12 feet high, has been so arranged as to be easily and perfectly darkened. On a bright, sunny day, prolonged immersion in this "*camera obscura*" fails to reveal to any eye the faintest glimmering of ordinary light; but in view of certain hypotheses, trials with various large white and polished surfaces, lenses, and silvered concave reflectors, have not rendered visible any of the light assumed to still linger in the room. Furthermore to exclude the effect of phosphorescence exhibited more or less by all substances, the apartment was usually darkened for an hour or more before each series of observations.

The comparative sensitiveness to ordinary faint light of the normal human eye, as between individuals, has not, so far as the Committee are aware, been very systematically studied; their own rough and casual estimates of this, by means of phosphorescent sulphides of the "luminous paint" description, indicate only a slight and inconstant difference between healthy eyes. Of the two observers to be specially mentioned hereafter, one seemed to be a little above, the other slightly below the average sensibility to the ordinarily visible rays of the spectrum.

The magnetic objects employed have been permanent magnets of various shapes and sizes, distributed on tables or shelves around the room, or mounted and standing on the floor;—a small portable electro-magnet, whose position could be readily changed;—and a larger and more powerful one kindly lent by Dr. Stone (hereafter called "the electro-magnet"), with limbs about eight inches long, the same distance apart, of flattened section of $2\frac{1}{4}$ by 7 inches, reduced to $2\frac{1}{4}$ inch circular at the polar surfaces, the whole 24 inches length of the magnet being wound with wire, and so mounted on trunnions in a massive wooden stand that it could be inclined and clamped in any position. It was excited by the current of eight large (10 by 6 inch plates) Smee cells, led to it from a "commutator," (that is, a contact-maker and current reverser,) in the adjoining room.

These facilities have been used by the Committee in their regular experiments of from $2\frac{1}{2}$ to 3 hours' duration, and for numerous informal trials conducted by individual members, when at least one hour has always been allowed for the visual accommodation to the darkness. The longest observations in the dark have been about three hours, the average duration of them being an hour and a-half, and it is quite possible that a much longer period, with perhaps an interval of sleep or rest, may be found necessary for maximum sensitiveness.

The main results, however, have been that no member of the Committee (or of the Society so far tested) has yet seen anything unusual connected with a magnet, the human body, or other object; and that after careful and repeated trials with 45 "subjects" of both sexes, and of ages between 16 and 60, only *three* of these professed to see luminous appearances; a much smaller proportion of "sensitives" than, according to Reichenbach, should be found. The testimony, however, of these observers, who declared their entire ignorance of Reichenbach's works, was so remarkable, and the series of tests by which it was confirmed is so striking, that the Chairman of the Committee, recounting a portion of it in the *Philosophical Magazine*,* says, "Of the fact that, to certain eyes, a luminosity accompanies the creation of a magnetic field, the evidence, so far as it goes, seems to me absolutely unexceptionable." In view of this opinion, it will be useful to record some of the experiments more fully than they are described in the "Note" just quoted.

The following, for instance, is an account of one of several similar and nearly equally satisfactory meetings of the Committee: Mr. G. A. Smith, a young man of 19, of whom further mention is made in

* Note on the Alleged Luminosity of the Magnetic Field. By W. F. Barrett, Prof. of Experimental Physics in the Royal College of Science, Dublin. *Phil. Mag.*, April, 1883.

the Mesmeric Report, and a lad, Fred Wells, who had been a "subject" of his mesmeric experiments, were, on the evening of January 2nd, 1883, seated in the dark room with Dr. A. T. Myers, Mr. F. W. H. Myers, Mr. H. N. Ridley, and Professor W. F. Barrett. The large electro-magnet was in the centre of the room with its poles upward, and about 4ft. 6in. above the floor. This was magnetised at intervals, but for the first half hour without being observed. At the end of that time both sensitives described something in the centre of the room, as, in their own words, "more like a faint smoke than a light," and successively led Professor Barrett's hand directly to the poles of the electro-magnet as its source. The "commutator," which made and broke the current animating the electro-magnet, worked quite silently, and was in charge of Mr. E. R. Pease, Mr. Edmund Gurney, and Mr. Walter H. Coffin in the adjoining room, where every remark of the "sensitives," or any of the Committee, could, through the darkening screens, be plainly heard and noted. The current was suddenly and unexpectedly broken, when Smith said instantly, "It is gone" (alluding to the "smoke" or faint light described), and it being as suddenly remade in a few seconds, he exclaimed as quickly, "Now I see it." A few less decisive observations then followed, until, on breaking the current, Smith instantly announced the fact; and from this moment commenced a remarkable number of uniform successes. In the words of Professor Barrett:—

A consecutive series of observations, extending over an hour, was then made by Mr. Smith. From time to time, during this period, the observers in the next room silently and unexpectedly closed or interrupted the current, the intervals being purposely varied from a few seconds to several minutes. In this way *fourteen* consecutive trials were made; and in every case, except one, the exclamations made by Mr. Smith, such as "Now I see it," "Now it's gone," were absolutely simultaneous with the movement of the commutator—according to the unanimous report of the witnesses in the adjoining room. In the one exception referred to, a delay of five seconds occurred between the breaking of the current and the exclamation: this, however, may easily have been due to a momentary relaxation of attention on the part of Mr. Smith. The strain on the attention was indeed so severe, that after the fourteenth observation, Mr. Smith complained of considerable pain in his eyes and head, and was obviously much exhausted. During a succeeding half hour two or three further experiments were made; but the results were uncertain, and may, I think, be fairly excluded.

It is obvious that a series of accidental coincidences between the act of closing or opening of the circuit and the exclamation of the observer cannot explain the facts here noted. As there are 3,600 seconds in an hour, to hit off any one right moment by pure chance would be very improbable; but the chances against success increase in geometric progression when fourteen right moments are *successively* hit off. The probabilities against mere coincidence as an explanation are therefore many millions to one.

More important was the possibility of indications being afforded by the

act of magnetization and demagnetization, which might give notice to the observer and suggest to the imagination the conversion of an illusion into a fancied reality.

Of these indications the so-called "magnetic tick" at once suggested itself. Knowing precisely what to listen for, and therefore more keenly alive to the sound than Mr. Smith, who presumably knew nothing of this molecular crepitation, I failed to detect the faintest sound on the "making" of the circuit; and a barely audible tick on "breaking" contact was heard only when my ear was in close contact with the magnet or its support.* This was due to the massive character of the magnet and stand, which also prevented any other discernible movement when the magnet was excited. Further, I satisfied myself that, at the distance at which Mr. Smith stood from the magnet, it was impossible to discover when the circuit was completed or interrupted by the attraction of any magnetic substance about one's body; as a precaution, however, Mr. Smith emptied his pockets beforehand. At the same time it is quite possible a skilful operator, bent upon deceiving us, might be able to detect the moment of magnetization and demagnetization by feeling the movement of a concealed compass-needle. Against this hypothesis must be placed the fact that no information was given to Mr. Smith beforehand of the nature of the experiment. Ultimately all scientific observation rests upon the good faith of the observers; and there was nothing to arouse the smallest suspicion of the good faith of the observer in the present instance.

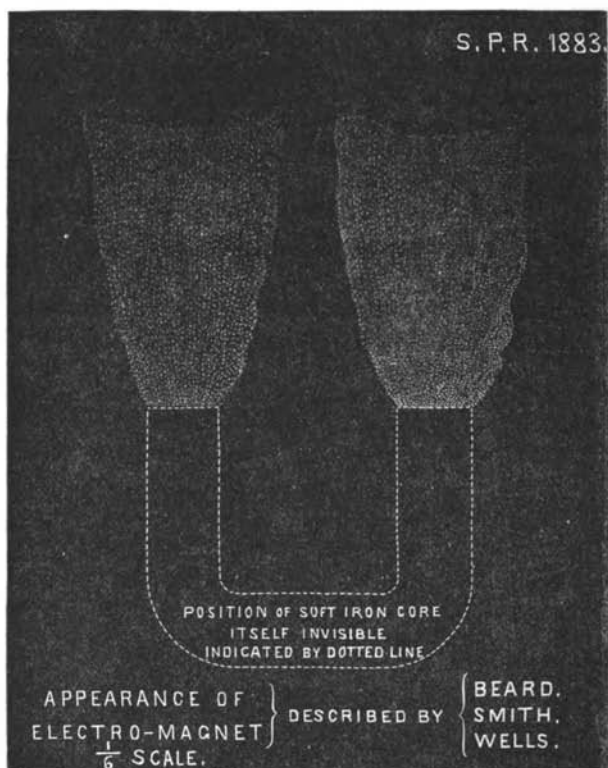
Similar experiments were made on another evening with the boy Wells, with fairly satisfactory results. In the case of Wells the luminosity, from his description, must have appeared to be brighter and larger; and on the interruption of the circuit it was not instantly extinguished, but gradually died away†; his frequent exclamation on breaking the current was, "Oh, you are spoiling it."

The description given by the three "sensitives" (including Mr. Sidney Beard, in addition to the above mentioned) of the luminous appearance was that of two rounded or blunted cones, apices downward, one of each being directly over and upon a pole of the magnet; that over the north-seeking pole being the brighter. They were said to be unsteady, waving in form, and flickering or variable in intensity. The breath deflected without extinguishing them. According to two of the observers with whom the experiment was tried the luminosity was not intercepted or cut off by a black velvet cloth or a thick board laid flat over the poles, but was hid from view like an ordinary source of light by an opaque body between it and the eye.

Faint lights have been described by one of these sensitives on permanent steel magnets, and in particular upon a small laminar

* The Committee have subsequently found that under proper conditions (of perfect silence, &c.) the ticks of both "making" and "breaking" are audible to the ear placed directly in contact with the polar surface of the magnet.

† There was a considerable amount of residual magnetism in the electro-magnet.



"Jamin" magnet having a concentrated intense field,—but the tests to which the Committee have as yet been able to put these statements are not entirely conclusive.

On several occasions, two of these "sensitives" and a member of the Committee have experienced peculiar sensations in the face and head which appear to be an effect of the magnetic field. This member, placing his forehead exactly between the poles of the electro-magnet, was certainly able to distinguish accurately,—and according to him by feelings in the face and eyes,—whether the magnet was excited or not. Mr. Smith, in the light, and in presence of the Committee, with his face placed in the strongest part of the magnetic field, has—as he alleges by singular effects upon his eyes and temples—accurately announced within one or two seconds, 21 successive "makes" and "breaks" of the current. In these, as in the other experiments, every precaution was taken against any direct or indirect knowledge, other

than by the magnetization, of the opening or closing of the circuit reaching the observer.

The Committee regret that under apparently identical circumstances, similar effects have not been always forthcoming, and think it reasonable to consider that the phenomena depend upon conditions, possibly physiological, not yet understood.

The objections that have been urged to even admitting the possibility of luminous magnetic phenomena, as apparently attributing a continuous evolution of radiant energy to a magnet, are, the Committee think, sufficiently met if it be remembered that on the one hand the source of energy may be outside the magnet altogether, and on the other the *amount* of this energy *must* be so small that many undetected sources of it probably exist.

In confirmation of this opinion it is gratifying to find that so distinguished and high an authority as Professor G. F. Fitzgerald, F.R.S., F.T.C.D., in a paper read before the Royal Dublin Society, has shown that these luminous appearances do not necessarily contravene the laws of thermo-dynamics or of the conservation of energy. Professor Fitzgerald has been good enough to state, in a letter (appended by permission to this Report), his hypothesis accounting for the origin of the light. In fact, the amount of molecular disturbance which may be competent to excite vision is immeasurably small. Thus a rush light can be seen through a radius of, say, a mile or more. How infinitesimal is the energy which produces sensation at that distance! Also, a phosphorescent powder, illuminated for one second, is found to be visible after uninterruptedly expending for five or six hours the trace of energy imparted to it by a momentary insolation. How minute is the amount of energy which excites our vision at the 20,000th second! But, notwithstanding the amazing delicacy of the organ of sight, photographic plates can now transcend our sense perception, and unlike the sensory effect, the image on such plates grows in distinctness by prolonged exposure to very feeble radiations. Hence the Committee were anxious to try photography; but preliminary efforts have been disappointing, no effect being produced by a magnet on very sensitive dry plates. The matter, however, will be returned to; meanwhile the eminent astronomer, Dr. Huggins, F.R.S., whose experience in this kind of photography is not exceeded by that of any other experimenter, has also tried, but unsuccessfully, to photograph these luminous phenomena. An interesting letter from Dr. Huggins is, by his permission, included in the Appendix. Other scientific men, in communication with the Committee, are found by no means hostile to these views; among them being Mr. J. Rand Capron, F.R.A.S., who refers to the subject in his work on "Auroræ," p. 165, and has favoured the Committee with further particulars of his experiments.

On the whole, while undoubtedly the evidence is yet too slight to draw more definite conclusions, the Committee feel at least justified in recording :—

Firstly, that three observers, separately, on distinct occasions were in some way immediately aware when an electro-magnet was secretly "made" and "unmade," under such precautions as were devised to suppress ordinary means of knowing, and to exclude chance and deception; and identified such magnetization with luminous appearances which as described agreed generally with the evidence recorded by Reichenbach ;—

Secondly, that there were, though less decisively, indications of other sensory effects of magnetism.

In view of these apparent confirmations of previous testimony, the Committee incline to the opinion that, among other unknown phenomena associated with magnetism, there is a *prima facie* case for the existence, under conditions not yet determined, of a peculiar and unexplained luminosity resembling phosphorescence, in the region immediately around the magnetic poles, and visible only to certain individuals.

APPENDIX.

40, Trinity College, Dublin, 23rd April, 1883.

MY DEAR BARRETT,—In my paper read last Monday at the Royal Dublin Society I proposed an experiment to decide between the ordinary electrical theories of direct action at a distance and Clerk Maxwell's theory of action by means of the ether. Upon the latter theory it should be possible to originate either stationary or progressive waves, and I showed that it might be possible to originate experimentally such short waves as to be within range of observation, for the principal difficulty is that owing to the velocity of propagation being the same as that of light it is very difficult to get short waves. I proposed to get over the difficulty by using the rapidly alternating currents produced when a condenser is discharged through a small resistance. As I was on the subject of the electrical origination of light I mentioned some suggestions as to a possible origin for the light said to be seen near a magnet. I suggested that the fact that the air molecules are moving in an intense magnetic field might very likely disturb the distribution of vibrations when they collide : the sudden change of direction in their motion may induce electric currents or vibrations in the molecules which are sufficiently rapid to be visible. I pointed out that this got over all difficulty in respect of the first Law of Thermo-dynamics, for that the source of energy was the air and not the permanent magnet. I suggested some ways in which it might not be opposed to the second Law, although it seems as if you were getting energy from the air by cooling it down below its surroundings. I pointed out that the action might not be permanent in still air and that the action might be like evoking a phosphorescence out of the air which would ultimately settle down

to a new distribution of velocities, but that practically air currents would make the action permanent. I pointed out that a somewhat similar action was supposed to take place when the magnet is stated to be able to separate the paramagnetic oxygen from the nitrogen in the air, for that as the gases are not of the same density there is a greater energy in them when apart than when mixed, and I suggested that the two effects might be in some way connected with one another, for that in order to separate them there must be some disturbance of the intra-molecular impacts. I refrained from suggesting obvious experiments to test the suggestions, such as putting the magnets in vacuo, or in a simple gas, or in a liquid or transparent solid, partly because they were obvious and partly because it seems so hard to catch the experimenter who can see the phenomenon. Mr. Moss suggested putting dry photographic plates for a long time near a magnet to see would they get at all fogged. This might very well be tried, but it would require a long course of experiments, as plates get accidentally fogged in so many ways.—Yours very sincerely,

GEO. FRAS. FITZGERALD.

P.S.—I think I stated that contrary to what many people state I can see no *a priori* difficulty in the supposition of direct action at a distance.

Upper Tulse Hill, S. W., 6th May, 1883.

DEAR PROFESSOR BARRETT,—Mr. Grubb, doubtless, gave to you the photographic plate, with the data of the experiment. I gave it to him the same day, and had not the opportunity of careful examination when dry. If you can see even the very faintest suspicion of any form, it may be well to strengthen with bichloride of mercury and ammonia. * This photograph was taken under the impression that you rather suggested the possibility of *ultra-violet* light. It is difficult to understand, from what we know of the magnetic lines of force, that there is a *radiant* energy which can be brought to focus on the retina. Your experiment with *screens* seems to suggest that the force is physical and not purely psychical. May it be energy in the form of *infra-red* vibrations? If our eyes were like Abney's plates every object would be luminous, and men and women and all warm-blooded creatures would be pre-eminently "*shining ones*." In one case the oxidation necessary to life does manifest itself in vibrational energy within the eye's range, namely, the little phosphorescent beasts in the sea, and also similarly in fire-flies and glow-worms. When you experiment again, will you try screens of glass and very thin ebonite? The former is very imperfectly transparent to short vibrations, and the ebonite is opaque to all but low *infra-red* rays, which pass freely through it. It would be well to try magnets with Abney's plates. If there is sufficient radiated energy to affect the eye, it might be well to see if, with the most sensitive pile, there is any indication of a rise of temperature from the magnetic field. In this case, if an electro-magnet is used, care must be taken that any rise of temperature in the wire from the current may not be allowed to affect the pile. This would be a good preliminary experiment before trying for photographic evidence of red or *infra-red* rays.—Yours very sincerely,

WILLIAM HUGGINS.

* I could see no trace of any form at all on the plate.—W. F. B.

IV.

ON SOME PHENOMENA ASSOCIATED WITH ABNORMAL CONDITIONS OF MIND.*

BY W. F. BARRETT, F.R.S.E., M.R.I.A., &c.

There are certain conditions of the mind, either temporarily induced or habitual, which appear to be associated with many remarkable phenomena that have hitherto received but partial attention from scientific men. On various occasions during the last ten years I have had the opportunity of observing some of these singular states, and in the hope of eliciting further information or of stimulating inquiry by those more competent than myself, I venture to bring the following facts under the notice of the British Association.

The observations to be described belong mainly to the class known by the names of mesmerism, hypnotism, or induced somnambulism, for these terms express very similar mental states. The experiments of the late Dr. Braid have led physiologists to recognise the existence of the fact that an extraordinary condition of the mind can be induced in certain susceptible or sensitive individuals by merely fixing the attention rigidly for several minutes upon any bright object. Whilst staying with a friend in Westmeath, now some years ago, I had the opportunity of frequently witnessing the production of this morbid condition, and, further, of observing some phenomena that are usually denied by eminent physiologists of the present day.

Selecting some of the village children and placing them in a quiet room, giving each some small object to look at steadily, it was found that one amongst the number readily passed into a state of reverie, resembling that dreamy condition between sleeping and waking. In this state the subject could readily be made to believe the most extravagant statements, such as that the table was a mountain, a chair a pony, a mark on the floor an insuperable obstacle. As Dr. Maudsley observes in his *Mental Physiology*, "the mind of the patient becomes possessed with the ideas the operator suggests, so that his body becomes an automatic machine set in motion by them."

In the Proceedings of the British Association for 1855, is a paper by Mr. Braid in which the phenomena of mesmerism are referred to

* This paper was originally read before the British Association, at the Glasgow Meeting, September, 1876. By permission of the Council of the S.P.E., it was included in the proceedings of the meeting on April 24th, after a verbal revision and the omission of a few paragraphs. Though this paper was the subject of much animadversion when first read, students of the Reports published by the S.P.E. will be able to judge how far the opinions expressed seven years ago have since received confirmation.

what is termed a mono-ideo-dynamic action, the ideo-motor force of the present day. Many other writers might be quoted, but the main facts are not now denied ; in fact, this peculiar physiological state is referred to all in recent works on the mind.

The fact that one mind can thus readily be thrown into a state of passive obedience to another mind is undoubtedly a fact of much importance. It is important, not only as exhibiting a state into which certain minds are liable to be exposed, but also as probably affording a clue to some of the extraordinary assertions that have been made by credible witnesses as to the elongation and levitation of the human body, the handling of fire and the like. These facts are testified to by eminent men whose word one cannot for a moment question. Either the narrators *saw* the things they describe or they *thought* they saw them. The following considerations seem to render it highly probable that the latter affords a provisional explanation.

Mr. Herbert Spencer has compared the ordinarily vivid mental impressions produced by the perceptions of external things which are simultaneously present in our consciousness with the fainter ideas produced by reflection, memory, or imagination—to two parallel streams of consciousness, constantly varying in their relative breadth, as the outer or the inner world predominates. During states of activity we are receiving a crowd of impressions from the senses, and hence the stream of consciousness derived from the external world causes the faint manifestations derived from within to sink into insignificance. But when the vivid manifestations produced by the senses are enfeebled—*e.g.*, by closing the eyes, stillness, &c.—the usually faint stream of consciousness becomes predominant ; the heterogeneous current of ideas grows more distinct, and almost excludes the vivid order of impressions, and finally, on lapsing into sleep, the manifestations of the vivid order cease to be distinguishable as such, and those of the faint order come to be mistaken for them.

It is highly probable that the vivid stream of consciousness produced by sensation, having been reduced by quietness and twilight, the minds of those who testify to the feats referred to, would readily yield themselves to any emphatic suggestion on the part of the operator. However, to put this matter to the test of experiment, I selected (in the manner already described) a young lad, who in the course of fifteen minutes was hypnotised, as Mr. Braid would say. The lad now readily believed any assertion I made, with evident relish going through the farce of eating and drinking because I suggested the act, though the only materials I gave him were a book and an empty vase. When subsequently he partly awoke, he was under the conviction that he had had his tea, yet could not understand how it was, as he associated the actual books with the forced idea of bread and butter, and the struggle of reason and memory

was curious to witness. On another occasion, when the lad was hypnotised, I placed my shoes on the table and forcibly drew his attention to them. I then suggested that I was standing in them, and after he had given his assent, I said, "Now I am going to rise up and float round the room." So saying I raised my hand, and directing his sight upwards, pointed out the successive stages in my imaginary flight. He followed the direction I indicated with intense and anxious interest, and on my slowly depressing my hand, and asserting I was once more on the ground, he drew a sigh of relief. On awaking he held to the belief that I had in some indistinct way floated round the room and pointed to the course I had taken. I had not the slightest doubt that after a few trials, this extravagant idea might have been fixed in the lad's mind with the greatest ease.

I adduce the foregoing experiment to shew the powerful influence of *suggestion* when the mind is in certain states such as reverie, and hence the need of guarding against illusion. At the same time, I do not wish it to be supposed that I dogmatically assert this must have been the explanation of the phenomena described by Lord Lindsay and others; all I assert is, in our present state of knowledge, it is an easier explanation than to assume the actual occurrence of the marvels. Nevertheless, in justice to those who hold an opposite view, I am bound to mention a case that came under my own repeated observation, wherein certain inexplicable physical phenomena occurred in broad daylight, and for which I could find no satisfactory solution either on the ground of hallucination or of fraud.* . . .

Returning to the experience gained at my friend's house in Westmeath, the girl there mesmerised passed on another occasion into a state of deeper sleep or trance, wherein no sensation whatever was experienced unless accompanied by pressure over the eyebrows of the subject. When the pressure of the fingers was removed, the girl fell back in her chair utterly unconscious of all around, and had lost all control over her voluntary muscles. On re-applying the pressure, though her eyes remained closed, she sat up and answered questions readily, but the manner in which she answered them, her acts and expressions, were capable of wonderful diversity by merely altering the place on the head where the pressure was applied. So sudden and marked were the changes produced by a movement of the fingers that the operation seemed very like playing on some musical instrument.† I mention these facts simply to ask whether a careful and systematic study of them

* The description of this case is here omitted, not from any doubt of its genuineness, but because it is thought better to refer it to the special Committee of the Society appointed to deal with this class of phenomena.

† The deep mesmeric sleep and the complete insensibility of the subject seemed to be the best guarantee against a clever course of deception on her part.

might not throw some additional light on the localisation of the functions of the brain. For extraordinary as it may appear that moderate pressure on the skull could produce any local irritation of the brain, yet it must be borne in mind that we are here dealing with the brain in an abnormal condition, probably a state of unstable equilibrium, so that a slight disturbance might produce an altogether disproportionate effect.

On a third occasion the subject, after passing through what has been termed the "biological" and "phrenological" states, became at length keenly and wonderfully sensitive to the voice or acts of the operator. It was impossible for the latter to call the girl by her name, however faintly and inaudibly to those around, without at once eliciting a prompt response. Even when the operator left the house, and at intervals gently called the girl's name, at the same time indicating the fact by signs to those within sight, she still responded, more and more faintly, it is true, as the distance became greater. This extraordinary and unnatural sensibility surprised me greatly, as it exceeded anything I had heard or read, and I regretted being unable, at the time, to carry out more experiments in this direction.

In his *Mental Physiology*, Dr. Carpenter states that he has seen abundant evidence that the sensibility of a hypnotised subject may be exalted to an extraordinary degree in regard to some particular class of impressions, this being due to the concentration of the whole attention upon the objects which excited them. Thus, he has known a youth in the hypnotised state find out, by the sense of smell, the owner of a glove which was placed in his hand, from amongst a party of more than sixty persons, scenting at each of them one after the other, until he came to the right individual. In another case, the owner of a ring was unhesitatingly found out from amongst a company of twelve, the ring having been withdrawn from the finger before the somnambule was introduced. He has seen other cases, again, in which the sense of temperature was extraordinarily exalted, very slight differences, inappreciable to ordinary touch, being at once detected.

Without denying the possibility of such an extraordinary sensibility, other facts I witnessed pointed in the direction of a *community of sensation* between the mesmeriser and the subject, for I noticed that if the operator tasted, smelt, or touched anything, or experienced any sudden sensation of warmth or cold, a corresponding effect was instantly produced on the subject, though nothing was said, nor could the subject have seen what had taken place upon the operator. To be assured of this, I bandaged the girl's eyes with great care, and the operator having gone behind the girl to the other end of the room, I watched him and the girl, and repeatedly assured myself of this fact. If he placed his hand over the lighted lamp, the girl instantly withdrew hers, as if in pain; if he

tasted salt or sugar, corresponding expressions of dislike and approval were indicated by the girl. If, however, anyone else in the room other than the operator tried the experiment, I could perceive no indications on the part of the subject. Certainly, so far as my observations extended, there did seem to be a vast difference between the influence exerted on the subject by the operator, and that which could be exerted by anyone else. Dr. Carpenter believes, however, that there is no foundation for the "*rapport*" which is so often asserted to exist between a mesmerised subject and the operator. On this point he remarks: "If the subject be 'possessed' with the previous conviction that a particular individual is destined to exert a special influence over him, the suggestions of that individual are obviously received with greater readiness, and are responded to with greater certainty, than are those of any bystander. This is the whole mystery of the relationship between the 'biologiser' and his 'subject.'"

For my own part, I do not think that the whole mystery of this so-called "*rapport*" can be disposed of quite so easily. Not only do the facts I have just given negative Dr. Carpenter's easy solution, but the following still more remarkable experiments shew, at any rate, that the question is one deserving of more extended inquiry.

When the subject was in the state of trance or profound hypnotism, I noticed that not only sensations but also ideas or emotions occurring in the operator appeared to be reproduced in the subject without the intervention of any sign, or visible or audible communication. Having mesmerised the girl myself, I took a card at random from a pack that was in a drawer in another room. Glancing at the card to see what it was, I placed it within a book, and in this state brought it to the girl. Giving her the closed book, I asked her to tell me what I had put within its leaves. She held the book close to the side of her head and said, "I see something inside with red spots on it." "Count the spots," I told her; she did so, and said there were five red spots. The card was the five of diamonds. Another card, chosen and concealed in a similar way, was also correctly named; and when a Bank of Ireland note was substituted she said, "Oh now I see a number of heads; so many I can't count them." She sometimes failed to guess correctly, asserting the things were dim, and invariably I found she could give me no information of what was within the book, unless I had previously known what it was myself. More remarkable still, I asked her to go in imagination to Regent Street, in London, and tell me what she saw. The girl had never been out of her remote Irish village, but she correctly described to me the shop of Mr. Ladd, the optician, of which I happened to be thinking—referring to some large crystals (of Iceland spar) and to other things in

the shop—and when she mentally left the shop she noticed the large clock that overhangs the entrance to Beak Street.

In many other ways I convinced myself that the existence of a distinct idea in my own mind gave rise to an image of the idea in the subject's mind ; not always a clear image, but one that could not fail to be recognised as a more or less distorted reflection of my own thought. The important point is that every care was taken to prevent any unconscious movement of the lips, or otherwise giving any indication to the subject, although one could hardly reveal the contents of an optician's shop by facial indications.*

This power of "thought-reading," as it has been termed, has often been described by writers on mesmerism, but little credence has been given to it by physiologists or psychologists.

Some assert that this state extends even further ; that subjects in this condition are able to perceive occurrences at remote distances which are not known to any present, and yet are subsequently verified. I

* The following interesting communication from my friend, Mr. W. E. Wilson, of Co. Westmeath, reached me in September, 1876, after the foregoing was written, and gives us a glimpse of something even beyond thought-reading, but many more experiments are necessary before a *prima facie* case in favour of so-called "clairvoyance" can be said to have been established.

Mr. Wilson writes in reference to the above-mentioned card experiment :—

" You are correct, as I remember several experiments of the same kind, I think we proved beyond all doubt that the subject is able to read the thoughts of the mesmeriser. Also that they are able to see through things which are to us optically opaque, provided that they could touch the objects or hold them in their hand. At any distance beyond that I don't think we have evidence that they can see things unless the mesmeriser knows them, in which case it of course becomes thought-reading. A lady subject has often told us the time by a gold hunting watch, which was put in a box after the hands were altered to any extent by the keyless arrangement, so that no one knew their position. I remember one instance with her. There were some friends in the room looking on. The hands of the watch were twisted round promiscuously; it was then put in a box and the closed box put in her hand. She at once said what o'clock it was. My father opened the watch to see if she was right, but found to his astonishment that she was wrong. He told her so, and gave her the watch to try again. She at once said she was right. He told her to look again, but she got crusty and refused to look for some time. He pressed her to look once more. She still said she was right, but that it was now a minute past the time she first said. My father opened the watch to shew those present the mistake she made, but found that she was perfectly right, that he had made a mistake himself. In that instance the thoughts of the mesmeriser were against her. Another instance I remember was with a country boy. He was mesmerised in a room which we made perfectly dark. Cards were given to him at random from a pack. He told fourteen correctly without a mistake, and I have no doubt would have gone through the pack if we liked. Of course you know that they don't try to use the eyes to see with. They always, without exception, put whatever is put in their hand to the side of their head, a little behind the ear, and about six inches from them. They always say that everything is greatly diminished. Ordinary book print they describe as fine lines."

have had cases of this kind described to me by those whom I esteem as careful and conscientious observers; but as nothing of the sort has ever come under my own observation, I refrain from stating what I cannot vouch for myself. Even as regards the facts I have myself witnessed, I do not pretend that they do more than justify further inquiry, as a large amount of similar evidence must be obtained by well qualified men before these phenomena can be accepted unreservedly. All I wish to urge is, that it is not wise to allow a natural feeling of incredulity on this matter to become a barrier to a possible extension of knowledge.

Dr. Carpenter himself remarks, that "everyone who admits that 'there are more things in heaven and earth than are dreamt of in our philosophy,' will be wise in maintaining 'a reserve of possibility' as to phenomena which are not altogether opposed to the laws of physics or physiology, but rather transcend them"; and he adds (*Mental Physiology*, p. 633), "some of his own experiences have led him to suspect that a power of intuitively perceiving what is passing in the mind of another, which has been designated as 'thought-reading,' may, like certain forms of sense-perception, be extraordinarily exalted by entire concentration of the attention. So far, however, as we are acquainted with the conditions of its exercise, it seems to depend upon the unconscious interpretation of indications (many of them indefinable) furnished by the expression of the countenance, by style of conversation, and by various involuntary movements; that interpretation, however, going, in many instances, far beyond what can have been learned by experience as the *meaning* of such indications."

It will be noticed that whilst Dr. Carpenter does not deny the possibility of thought-reading or some analogous kind of divining power, he distinctly asserts that everything *he* has seen is explicable by sign or "muscle-reading." The evidence that I have here adduced, on the other hand, indicates that when a person is thrown into a hypnotic or passive condition, the nervous action associated with thought can be excited by a corresponding action in an adjoining individual, and this across space and *without* the intervention of the recognised organs of sensation. Nor does this seem an altogether incredible fact. The energy of electricity exerts itself in two ways, by transmission along a material conductor and by influence, or induction as it is termed, across space. May not nerve energy, whatever be its nature, also act by influence as well as conduction? For many years I have held this view, and it has been confirmed by what I have witnessed from time to time. My main object in bringing this paper before the Section is to direct attention to the subject in the hope that those who have any evidence to offer in support of this view, or any good grounds for opposing it, may favour me with their experience.