

PROCEEDINGS
 or
 THE SOCIETY FOR PSYCHICAL RESEARCH,

July 17, 1882.

The first general meeting of the Society was held at Willis's Rooms, London, on July 17th, 1882.

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

The following address was delivered by the President :—

Before we proceed to what has been marked out as the business of this meeting, as it is the first general meeting of our new Society since the time it was definitely constituted, it has been thought that I should make a few brief remarks on the aims and methods of the Society, which will form a kind of explanation in supplement to our prospectus defining those aims and methods,—which, I suppose, has been seen by all the members, and perhaps by some who are not as yet members. This prospectus has not been subjected to much instructive public criticism. It has been received, either with entire cordiality, or with guarded neutrality, or with uninformative contempt. Still, several private criticisms on that prospectus and questions suggested by it have come to my notice; and it seems to me that I might perhaps employ the few minutes of your time that I wish to take up in no better way than in replying to these criticisms and objections.

The first question I have heard is, Why form a Society for Psychical Research at all at this time, including in its scope not merely the phenomena of thought-reading (to which your attention will be directed chiefly this afternoon), but also those of clairvoyance and mesmerism, and the mass of obscure phenomena commonly known as Spiritualistic? Well, in answering this, the first question, I shall be able to say something on which I hope we shall all agree; meaning by "we," not merely we who are in this room, but we and the scientific world outside; and as, unfortunately, I have but few observations to make on which so much agreement can be hoped for, it may be as well

to bring this into prominence, namely, that we are all agreed that the present state of things is a scandal to the enlightened age in which we live. That the dispute as to the reality of these marvellous phenomena,—of which it is quite impossible to exaggerate the scientific importance, if only a tenth part of what has been alleged by generally credible witnesses could be shewn to be true,—I say it is a scandal that the dispute as to the reality of these phenomena should still be going on, that so many competent witnesses should have declared their belief in them, that so many others should be profoundly interested in having the question determined, and yet that the educated world, as a body, should still be simply in the attitude of incredulity.

Now the primary aim of our Society, the thing which we all unite to promote, whether as believers or non-believers, is to make a sustained and systematic attempt to remove this scandal in one way or another. Some of those whom I address feel, no doubt, that this attempt can only lead to the proof of most of the alleged phenomena; some, again, think it probable that most, if not all, will be disproved; but regarded as a Society, we are quite unpledged, and as individuals, we are all agreed that any particular investigation that we may make should be carried on with a single-minded desire to ascertain the facts, and without any foregone conclusion as to their nature.

But then here comes the second question, which I have had put by many who are by no means unfriendly to our efforts,—that is, Why should this attempt succeed more than so many others that have been made during the last thirty years? To this question there are several answers. The first is, that the work has to go on. The matter is far too important to be left where it now is, and, indeed, considering the importance of the questions still in dispute, which we hope to try to solve, as compared with other scientific problems on which years of patient and unbroken investigation have been employed, we may say that no proportionate amount of labour has yet been devoted to our problems; so that even if we were to grant that previous efforts had completely failed, that would still be no adequate reason for not renewing them. But, again, I should say that previous efforts have not failed; it is only true that they have not completely succeeded. Important evidence has been accumulated, important experience has been gained, and important effects have been produced upon the public mind.

I say that important evidence has been accumulated; and here I should like to answer a criticism that I have privately heard which tends to place the work of our Society in a rather invidious aspect. It is supposed that we throw aside *en bloc* the results of previous inquiries as untrustworthy, and arrogate to ourselves a superior knowledge of scientific method or intrinsically greater trust-

worthiness—that we hope to be believed, whatever conclusions we may come to, by the scientific world, though previous inquirers have been uniformly distrusted. Certainly I am conscious of making no assumption of this kind. I do not presume to suppose that I could produce evidence better in quality than much that has been laid before the world by writers of indubitable scientific repute—men like Mr. Crookes, Mr. Wallace, and the late Professor de Morgan. But it is clear that from what I have defined as the aim of the Society, however good some of its evidence may be in quality, we require a great deal more of it. I do not mean to dispute,—it is not now the time to dispute,—with any individual who holds that reasonable persons, who have looked carefully into the evidence that has been so far obtained, ought to be convinced by that evidence; but the educated world, including many who have given much time and thought to this subject, are not yet convinced, and therefore we want more evidence.

If anyone asks me what I mean by, or how I define, sufficient scientific proof of thought-reading, clairvoyance, or the phenomena called Spiritualistic, I should ask to be allowed to evade the difficulties of determining in the abstract what constitutes adequate evidence. What I mean by *sufficient evidence* is evidence that will convince the scientific world, and for that we obviously require a good deal more than we have so far obtained. I do not mean that some effect in this direction has not been produced: if that were so we could not hope to do much. I think that something has been done; that the advocates of obstinate incredulity—I mean the incredulity that waives the whole affair aside as undeserving of any attention from rational beings—feel their case to be not *prima facie* so strong now as it was.

Thirty years ago it was thought that want of scientific culture was an adequate explanation of the vulgar belief in mesmerism and table-turning. Then, as one man of scientific repute after another came forward with the results of individual investigation, there was a quite ludicrous ingenuity exercised in finding reasons for discrediting his scientific culture. He was said to be an amateur, not a professional; or a specialist without adequate generality of view and training; or a mere discoverer not acquainted with the strict methods of experimental research; or he was not a Fellow of the Royal Society, or if he was it was by an unfortunate accident. Or again, national distrust came in; it was chiefly in America that these things went on; or as I was told myself, in Germany, some years ago, it was only in England, or America, or France, or Italy, or Russia, or some half-educated country, but not in the land of *Geist*. Well, these things are changed now, and though I do not think this kind of argument has quite gone out of use yet it has on the whole been found more difficult to work; and our obstinately incredulous friends, I think, are now generally content to regard the

interest that men of undisputed scientific culture take in these phenomena as an unexplained mystery, like the phenomena themselves.

Then again, to turn to a different class of objectors, I think, though I do not wish to overrate the change, that the attitude of the clergy has sensibly altered. A generation ago the investigator of the phenomena of Spiritualism was in danger of being assailed by a formidable alliance of scientific orthodoxy and religious orthodoxy; but I think that this alliance is now harder to bring about. Several of the more enlightened clergy and laity who attend to the state of religious evidences have come to feel that the general principles on which incredulous science explains off-hand the evidence for these modern marvels are at least equally cogent against the records of ancient miracles, that the two bodies of evidence must *primâ facie* stand or fall together, or at least must be dealt with by the same methods.

Then, again, a generation ago we were directed to go to the conjurers, and told that we should see that the whole thing was conjuring. I quite think that this direction was to a great extent just and important: it is highly desirable that the investigation of these matters should be carried on by men who have tried to acquaint themselves with the performances of conjurers. But we can no longer be told off-hand that all the marvels recorded by Mr. Crookes, Professor Zöllner, and others, are easy conjuring tricks, because we have the incontrovertible testimony of conjurers to the contrary. They may be conjuring tricks, but they are at any rate tricks that conjurers cannot find out.

For these various reasons I think we may say that on the whole matters are now more favourable for an impartial reception of the results of our investigation, so far as we can succeed in obtaining any positive results, than they were twenty years ago. In saying this I do not in the least wish to ignore or make light of the evidence that has been accumulated in recent years to shew that at least a great part of the extraordinary phenomena referred to Spiritual agency by Spiritualists in England and America are really due to trickery and fraud of some kind. I had this in view when I said just now that important experience had been gained by preceding investigations. This is certainly part of the experience, and I believe that no Spiritualist denies its importance. It would, however, be a mistake to suppose that investigators, or even believers in mesmerism or Spiritualistic phenomena, had not their eyes open twenty years ago to the part played in these phenomena by fraud.

My interest in this subject dates back for nearly twenty years, and I quite remember that when I began to look into the matter, nearly every educated Spiritualist that I came across, however firmly

convinced, warned me against fraud, and emphasised his warning by impressive anecdotes. It is merely a question of degree, and I think it would be generally admitted that recent experiences have changed the view of many Spiritualists with regard to the degree. I think that even educated and scientific Spiritualists were not quite prepared for the amount of fraud which has recently come to light, nor for the obstinacy with which the mediums against whom fraud has been proved have been afterwards defended, and have in fact been able to go on with what I may, without offence, call their trade, after exposure no less than before.

And this leads me to the point which is chiefly characteristic of the method of investigation which our Society will, I hope, in the main use. Though it would be a mistake to lay down a hard and fast rule that we may not avail ourselves of the services of paid performers or paid mediums, still we shall, as much as possible, direct our investigation to phenomena where no ordinary motives to fraud,—at any rate I may say no pecuniary motives,—can come in. There has, of course, always been a mass of evidence of this kind. In fact, I think everyone who has become convinced of the reality of the phenomena, or has become strongly and persistently convinced that there is a *prima facie* case for investigation, has had his attention first attracted by narratives of what has gone on in private families or private circles where none but relatives or intimate friends have been concerned.

Now, the great gain that I hope may accrue from the formation of this Society is that the occurrence of phenomena—*prima facie* inexplicable by any ordinary natural laws—may be more rapidly and more extensively communicated to us who desire to give our time to the investigation, so that in the first instance we may carefully sift the evidence, and guard against the danger of illusion or deception which even here may, of course, come in; and then, when the evidence has been sifted by accumulation of personal experiments, make it more available for the purpose of producing general conviction.

As I said before, I do not mean to claim for myself or my colleagues either any special aptitude for investigation, or any special claim to the credence of mankind, as compared with the members of private households or circles of friends where the phenomena may in the first instance occur. But in a matter so strange to ordinary experience I think we may say that it is only gradually that a man learns the complicated precautions that have to be taken in order to exclude all conceivable possibility of illusion or deception. Certainly my own experience is that I only learnt what had to be done in this way, and had to be guarded against, in a gradual way, by repeated experiments.

As regards the question of credibility, the important point.

to bear in mind is that every additional witness who, as De Morgan said, has a fair stock of credit to draw upon, is an important gain. Though his credit alone is not likely to suffice for the demand that is made on it, his draft will help. For we must not expect any decisive effect in the direction at which we primarily aim, on the common sense of mankind, from any single piece of evidence, however complete it has been made. Scientific incredulity has been so long in growing, and has so many and so strong roots, that we shall only kill it, if we are able to kill it at all as regards any of those questions, by burying it alive under a heap of facts. We must keep "pegging away," as Lincoln said; we must accumulate fact upon fact, and add experiment upon experiment, and, I should say, not wrangle too much with incredulous outsiders about the conclusiveness of any one, but trust to the mass of evidence for conviction. The highest degree of demonstrative force that we can obtain out of any single record of investigation is, of course, limited by the trustworthiness of the investigator. We have done all that we can when the critic has nothing left to allege except that the investigator is in the trick. But when he has nothing else left to allege he will allege that.

We shall, I hope, make a point of bringing no evidence before the public until we have got it to this pitch of cogency. I think it is desirable on various grounds, but one ground is, I think, this: It is due to the private families or private circles of friends whom we hope to persuade to allow us to take part in their experiments, not to leave the subject or the medium of the phenomena—when we have convinced ourselves, by our own methods, of the genuineness of the phenomena—to bear alone the injurious suggestions of any incredulous materialist who may find it needful to attack our experiments. We must drive the objector into the position of being forced either to admit the phenomena as inexplicable, at least by him, or to accuse the investigators either of lying or cheating or of a blindness or forgetfulness incompatible with any intellectual condition except absolute idiocy.

I am glad to say that this result, in my opinion, has been satisfactorily attained in the investigation of thought-reading. Professor Barrett will now bring before you a report which I hope will be only the first of a long series of similar reports which may have reached the same point of conclusiveness.

FIRST REPORT ON THOUGHT-READING.

By W. F. BARRETT, Professor of Physics in the Royal College of Science for Ireland; EDMUND GURNEY, M.A., Late Fellow of Trinity College, Cambridge; and F. W. H. MYERS, M.A., Late Fellow of Trinity College, Cambridge.

[Read to the Society, July 17, 1882.]

For several years past the members of this Committee have been gathering evidence on the obscure but important question of what may be termed *supersensuous perception*. Stray facts met with from time to time in the course of our own observations, or related to us by competent witnesses, led us to doubt the sufficiency of the popular physiological explanations to account for all cases, and encouraged us to persevere in an inquiry which may be stated in the form of the following proposition :—

Is there or is there not any existing or attainable evidence that can stand fair physiological criticism, to support a belief that a vivid impression or a distinct idea in one mind can be communicated to another mind without the intervening help of the recognised organs of sensation? And if such evidence be found, is the impression derived from a rare or partially developed and hitherto unrecognised sensory organ, or has the mental percept been evoked directly without any antecedent sense-percept? The *nature* and the *laws* of this direct action of mind on mind would of course form a subject of prolonged subsequent discussion and inquiry whenever the evidence in its favour had accumulated sufficiently. The object of the present report is to place on record the first instalment of the evidence which we have up to this time collected in reference to this subject.

The present state of scientific opinion throughout the world is not only hostile to any belief in the possibility of transmitting a single mental concept, except through the ordinary channels of sensation, but, generally speaking, it is hostile even to any inquiry upon the matter. Every leading physiologist and psychologist down to the present time has relegated what, for want of a better term, has been called "Thought-reading" to the limbo of exploded fallacies.* Dr. W. B. Carpenter, whose

* In the July number of the *Nineteenth Century* the senior assistant physician at Westminster Hospital expresses his amazement at the hardihood of anyone having the slightest pretence to scientific knowledge daring to put forth evidence in favour of thought-reading; and a recent writer in the *Saturday Review* gives expression to the general scientific point of view of the present day on this subject when he remarks that "we thought we had heard the last of thought-reading."

name and distinguished contributions to the science and literature of physiology command universal recognition and respect, finds in the so-called thought-reading a striking confirmation of views he has long advocated, that the "communications are made by unconscious muscular action on the part of one person and automatically interpreted by the other." Where collusion does not come into play all that Dr. Carpenter has ever seen or heard rests upon the "intermediation of those expressional signs which are *made* and *interpreted* alike unconsciously."* Dr. H. Maudsley in his "Pathology of Mind" takes the same view as Dr. Carpenter, treating the subject as hardly worthy of serious refutation. Collusion, hallucination, unconscious interpretation of unconsciously imparted signs, furnish, according to the physiologists of to-day, abundant explanation of the phenomena under investigation.

Twelve months ago, the performances of Mr. Irving Bishop having attracted considerable attention, a small committee of distinguished men investigated the matter, and after a few and rather hastily conducted experiments, a report, approved of by the other members of the committee, was drawn up by Mr. G. J. Romanes, and published in *Nature* for June 23rd, 1881. The report indicates that one member of the committee, Professor Ray Lankester, absolutely refused to countenance the idea of thought-reading, and objected to the other members—Professor Croom Robertson, Mr. F. Galton, and Mr. Romanes—giving even a fair trial to "so puerile a hypothesis." The trial was, however, made, and the result is thus stated: "From these experiments it is needless to say we did not anticipate any results, but, with the exception of Professor Lankester, we thought it worth while to make them, not only because Mr. Bishop seemed to desire it, but also to satisfy the general public that we had given the hypothesis of 'thought-reading' as well as that of 'muscle reading' a fair trial."

Mr. Stuart Cumberland has obtained considerable notoriety by experiments somewhat similar to those of Mr. Bishop, but his performances have no sort of relationship to our experiments, as he expressly disclaims thought-reading and denies the possibility of obtaining any results without contact. Mr. Bishop, on the other hand, professes to obtain results without contact, but the experiments for which he makes this claim are never obtained without the very closest proximity, nor without accompaniments of needless flurry and excited pantomime which are eminently calculated to distract and mislead the attention.†

* "Mesmerism, Spiritualism," &c., by Dr. W. B. Carpenter, pp. 53 and 55.

† It is due to Mr. Bishop, however, to say that he has more recently publicly tried (and one of us has also privately seen) experiments considerably better than his earlier ones, as regards success and the distance between himself and the person "willing."

Mr. Bishop's and Mr. Stuart Cumberland's performances are in some respects identical with those exhibited, some years past, by a Mr. Corey and others, in America. In a paper read before a scientific body in Detroit, and published in the *Detroit Review of Medicine* for August, 1875, Dr. T. A. McGraw describes as follows the method followed by Mr. Corey in his experiments: "Bringing himself," says Dr. McGraw, "into direct physical contact with some person, Mr. Corey was enabled to discover objects which that person had secreted, and to select from a multitude of objects the one upon which the willer was intent. All his performances were but variations upon these two strings. A hidden object was found, or a person, letter, or figure was picked out from a crowd of others. He usually brought himself into contact with his subject by grasping the subject's hand, and applying it to his own forehead, but sometimes placed his own hand also on the brow of his companion." The writer proceeds to shew that Mr. Corey's tests (like most of those of Mr. Bishop and Mr. Cumberland) are only ideas which can be expressed by the simplest kind of *action*. "He cannot detect any kind of an idea in such a way as to express it first by speech. Thus he cannot tell directly the date of a coin, nor can he discover it in any other manner than by choosing out the figures which represent it from among others on a table." It is obvious, as the writer goes on to say, that most of the actions "could be explained by the perception by a trained operator of involuntary and unconscious muscular movements."

"I myself," he continues, "experienced this tendency to involuntary action, when trying to carry out fairly one of Mr. Corey's tests. The object of the search in this case was the date of an old coin, and the operator was trying to discover it by choosing from among the figures on the table those of the proper date. While keeping my attention fixed on a certain figure, I became all at once aware that I was actually trying to force the hand of my associate towards it, so powerfully did the thought impel to the correspondent action."*

Notwithstanding this, Dr. McGraw does not believe the explanation he has just given covers *all* the phenomena he witnessed, for he adds: "It seemed to me that there were features in these exhibitions which could not be satisfactorily explained on the hypothesis of involuntary muscular action, for . . . we are required to believe a man could unwillingly, and in spite of himself, give information by unconscious and involuntary signs that he could not give under the same circumstances by voluntary and conscious action. . . . It seems to me there is a hint towards the possibility of the nervous system of

* The experience of all who have carefully attended to their sensations whilst making similar trials, will confirm this observation.

one individual being used by the active will of another to accomplish certain simple motions. There would be nothing inherently impossible in this when we recollect the strong similarities that exist between nervous and electrical forces; and as we know, it is possible to generate induced currents of electricity in coils of wire that are near to a primary electric coil; so we can imagine the nervous current to be continued into [induced in?] another body and act there upon the automatic centres of action. . . . The whole matter, however, needs as yet the most careful investigation before the phenomena can even be accepted as genuine."

Dr. Beard, of New York, professes to have supplied this need, and in various papers—on "Trance," on the "Scientific Basis of Delusion," on "The Physiology of Mind Reading," &c.—published in the American "Popular Science Monthly" for 1876, 1877, and 1879, has, according to the high authority of Professor Croom Robertson (*Nature*, July 14th, 1881), "given a varied record of facts, and a series of carefully drawn conclusions." We have carefully read what Dr. Beard has written, and failed to find much more than a singular exhibition of self-assertiveness; coupled with a marked disregard of many eminent names in the past and present records of scientific inquiry. Dr. Beard tells us that after incredible labour he has discovered six sources of error open to all who experiment with living human beings. "All of these errors are to be recognised, and systematically, and, if possible, simultaneously guarded against, if our results are to command the confidence and homage of science."

These six sources of error are as follows:—

1. *The phenomena of the involuntary life in both the experimenter and the subject*,—embracing under this head trance, as well as all actions below the plane of consciousness.

2. *Unconscious deception on the part of the subject experimented on*, which appears to be a particular instance of the general statement given in the first error.

3. *Intentional deception on the part of the subject*; experiments must be made without any regard to the moral character of the subject.

4. *Unintentional collusion of third parties*,—meaning by this bystanders or assistants, seen or unseen; to avoid this, the experiments must be made privately, or the audience kept absolutely silent.

5. *Intentional collusion of third parties, i.e., assistance designedly given*; difficult to guard against, for, as Dr. Beard remarks, intentional and deliberate deception is more common among the better classes than is generally imagined.

6. *Chance and coincidences*. Concerning this last, Dr. Beard remarks that the only way to eliminate this error is by making com-

parative experiments with all the sources of error removed except chance. "In this way," he continues, "it was shewn that mind-reading so-called, was really muscle-reading. In the researches I made on muscle-reading, it was shewn over and over that by pure chance only the blindfold subject would, under certain conditions, find the object looked for in one case, and sometimes in two cases out of twelve."

The first two sources of error are considered the most frequent and fatal, and to guard effectively against them "two, and only two, things are considered needful; one is a general knowledge of the phenomena of the involuntary life, and the other is so to deceive the subject experimented on that this involuntary action of his mind or body cannot come in and destroy the experiment."

But may not the experimenter himself be deceived by his foregone conclusions? In fact, we venture to think Dr. Beard and others have omitted one source of error more fatal to accuracy in interpreting the results obtained than perhaps any other. We allude to the strong prepossessions with which the subject is approached, a prejudice which concludes against their possibility, and which, if it does not preclude inquiry, destroys all calmness and impartiality in viewing the facts. It is undeniable that a strong mental bias in one direction is as objectionable on the side of scepticism as on the side of credulity. In either case it tends (1) to explain the facts in accordance with the mental bias, which may be erroneous; (2) to produce an actual mental disturbance, either perceptible or imperceptible, which in delicate mental operations may really be as fatal to their success as slight air disturbances in the indications of a galvanometer, or the introduction of a trace of a magnetic metal in the reading of a magnetometer.*

Hesitation in accepting any facts so novel, and, in many ways, suspicious, as mind-reading is, of course, perfectly justifiable; and we are quite prepared to expect much criticism and prolonged experiment, before any generalisation from the facts can meet with wide acceptance. Our own researches have now extended over a period of several years, and we have witnessed phenomena of more or less interest in a great variety of subjects. Broadly speaking, these phenomena may be grouped under the following heads:—

*An amusing instance of the existence of mental prejudice among eminent scientific men is given by the late Miss C. Fox, in her recently published journal; she relates that the late Provost of Trinity College, Dublin, said to her, "When in Dublin Sir W. Hamilton mentioned to Airey some striking mathematical fact. He paused a moment, when Airey interposed with 'No, it cannot be.' Sir William mildly remarked, 'I have been investigating it closely for the last five months, and cannot doubt its truth.' 'But,' said Airey, 'I've been at it for the last five minutes, and cannot see it at all!'" Similar interlocutory remarks, and even published replies, are not unknown to the members of this Sub-Committee.

I. Where some action is performed, the hands of the operator being in gentle contact with the subject of the experiment.

II. Where a similar result is obtained with the hands *not* in contact.

III. Where a number, name, word, or card has been guessed and expressed in speech or writing, without contact, and apparently without the possibility of the transmission of the idea by the ordinary channels of sensation.

IV. Where similar thoughts have simultaneously occurred, or impressions been made, in minds far apart.

I. Whenever the hands are in contact or even communicate by a tense cord with the subject of the experiment, it is almost impossible to exclude giving faint indications to the guesser, which with a sensitive subject are interpreted into a sense of rightness or wrongness that ultimately may lead them to the hidden object, "the communication," as Dr. Carpenter remarks, "being made by unconscious muscular action on the part of one person and automatically interpreted by the other." The most familiar illustration of this is to be found in the *willing game*, which may be described in Dr. Carpenter's words, as follows: "Several persons being assembled, one of them leaves the room, and during his absence some object is hidden. On the absentee's re-entrance, two persons who know the hiding-place stand, one on either side of him, and establish some personal contact with him, one method being to place one finger on the shoulder, while another is for each to place a hand on his body. He walks about the room between the two 'willers,' and generally succeeds before long in finding the hidden object, being led towards it, as careful observation and experiment have fully proved, by the involuntary muscular action of his unconscious guides, one or the other of them pressing more heavily when the object is on his side, and the finder as involuntarily turning towards that side." *

This well-known explanation doubtless accounts for very much that is witnessed in family circles, and which goes under the name of thought-reading. At the same time there is a difficulty in applying it to those cases wherein the subject has frequently failed to accomplish a simple task, and yet has accurately done a much more complicated one often with singular promptness and decision. Some striking cases of this kind will be found detailed in the Appendix to this report, but, though surprising, they are not in themselves of sufficient value to warrant an explanation by any new hypothesis. We therefore pass on to the second group of cases:—

II. Where actions are performed without contact with the person willing.

* Carpenter's "Mesmerism, Spiritualism, &c.," p. 54.

Here the involuntary guidance by the eyes of the rest of the party, or other indications of an almost imperceptible character, are swiftly and probably unconsciously interpreted by the guesser and lead him hesitatingly to do what is being willed. We might abundantly illustrate this type of cases from the mass of correspondence we have received and from what we have ourselves witnessed. Some cases are referred to in the Appendix. Even blindfolding the subject merely removes one risk of error. The doubtful interpretation of the best results obtained in this group has compelled us to attach comparatively little importance to them, and accordingly we pass on to the next group :—

III. *Where some number, word, or card has been guessed apparently without any of the ordinary means of communication between the willer and guesser.*

Though the errors arising from muscle-reading or involuntary guidance are here avoided, there are other sources of conscious or unconscious illusion to be guarded against. Collusion is one of the most obvious, and anyone who has witnessed what can be done by a code of signals such as is employed by Mr. Bishop,* or Mr. Heller, or Mr. Heriot with "Louie," will naturally distrust all observations where two particular persons are necessary for the results obtained. Imperceptible information may be given by one who knows the word selected, by means of the Morse code used in electric telegraphy, the long and short signs being readily communicated by sight, sound, or touch, as may be found requisite. And where collusion is out of the question an obvious danger lies in low whispering, or even soundless movement of the lips; whilst the faintest accent of approval or disapproval in question or comment may give a hint as to whether the effort is tending in the right direction, and thus guide to the mark by successive approximations. Any exhibition of the kind before a promiscuous company is nearly sure to be vitiated by one or other of these sources of error. It is obvious, in fact, that precision can only be attained by repeated experimentation in a limited circle of persons known to each other, and amenable to scientific control.

In the correspondence we have received there were two cases which seemed, upon inquiry, to be free from any *primæ facie* objections, and apparently indicative of true thought-reading. One of these cases is given in the Appendix, p. 55, but as we cannot from personal observation testify to the conditions under which the trials were made, we simply leave it aside. The other case was that of a family in Derbyshire, with whom we have had the opportunity of frequent and prolonged trials.†

* For Mr. Bishop also shews how anything can be intimated to a confederate by a code of questions and sounds.

† A preliminary note on this case was sent to *Nature*, and published in that journal for July 7th, 1881.

Our informant was Mr. Creery, a clergyman of unblemished character, and whose integrity indeed has, it so happens, been exceptionally tested. He has a family of five girls, ranging now between the ages of ten and seventeen, all thoroughly healthy, as free as possible from morbid or hysterical symptoms, and in manner perfectly simple and childlike. The father stated that any one of these children (except the youngest), as well as a young servant-girl who had lived with the family for two years, was frequently able to designate correctly, without contact or sign, a card or other object fixed on in the child's absence. During the year which has elapsed since we first heard of this family, seven visits, mostly of several days' duration, have been paid to the town where they live, by ourselves and several scientific friends, and on these occasions daily experiments have been made.

The inquiry has taken place partly in Mr. Creery's house, and partly in lodgings or in a private room of an hotel, occupied by some of our number. Having selected at random one child, whom we desired to leave the room and wait at some distance, we would choose a card from a pack, or write on paper a number or a name which occurred to us at the moment. Generally, but not always, this was shewn to the members of the family present in the room; but no one member was always present, and we were sometimes entirely alone. We then recalled the child, one of us always assuring himself that, when the door was suddenly opened, she was at a considerable distance, (in their own house at the further end of a passage), though this was usually a superfluity of caution, as our habit was to avoid all utterance of what was chosen. Before leaving the room the child had been informed of the general nature of the test we intended to select, as "this will be a card," or "this will be a name." On re-entering she stood—sometimes turned by us with her face to the wall, oftener with her eyes directed towards the ground, and usually close to us and remote from her family—for a period of silence varying from a few seconds to a minute, till she called out to us some number, card, or whatever it might be. If this was incorrect, we usually allowed a second trial, and occasionally a third.

To give an example: The following results were obtained on the evening of April 12th, in the presence of two of our number and the family. The first attempt of one of the children was to state (without searching) the hiding-place of some small object, the place having been chosen by ourselves, with the full range of the house, and then communicated to the other members of the family. This was effected in one case only out of four. The next attempt was to give the name of some familiar object agreed on in the child's absence, as "sponge," "pepper-castor," &c. This was successful on a first trial in six cases out of fourteen. We then chose a card from a full pack in the child's absence, and called upon her to name it

on her return. This was successful at once in six cases out of thirteen. We then tried holding small objects in the hand, as a "latch-key," a "half-sovereign," a "green ball"—which were at once rightly named in five cases out of six. A harder trial was now introduced. The maid-servant having left the room, one of us wrote down the name "Michael Davitt," shewed it round, and then put the paper in his pocket. The door was now opened and the girl recalled from the end of the passage. She stood close to the door amid absolute silence, and with her eyes on the ground—all of us meanwhile fixing our attention on the appointed name—and gave after a few seconds the name "Michael," and then almost immediately "Davitt." To avoid any association of ideas, we then chose imaginary names, made up by ourselves at the moment, as "Samuel Morris," "John Thomas Parker," "Phœbe Wilson." The names were given correctly *in toto* at the first trial in five cases out of ten. Three cases were complete failures, and in two the names given bore a strong resemblance to those selected by us, "Jacob Williams" being given as "Jacob Wild," and "Emily Walker" as "Enry Walker." It was now getting late, and both we and the younger children were very tired; and four attempts to guess the name of a town in England were all failures, though one of us had previously obtained remarkable success with this very experiment.

The results obtained when the family were present gain enormously in value if similar results can be shewn when none but strangers to the family know the word or card selected, or when the child who is the subject of the experiment is completely isolated from those who know the thing chosen. We will therefore describe two series of experiments of this character, which appear to us to be absolutely unexceptionable and conclusive, so far as they go.

"Easter, 1881. Present: Mr. and Mrs. Creery and family, and W. F. Barrett, the narrator. One of the children was sent into an adjoining room, the door of which I saw was closed. On returning to the sitting-room and closing its door also, I thought of some object in the house, fixed upon at random; writing the name down, I shewed it to the family present, the strictest silence being preserved throughout. We then all silently thought of the name of the thing selected. In a few seconds the door of the adjoining room was heard to open, and after a very short interval the child would enter the sitting-room, generally speaking with the object selected. No one was allowed to leave the sitting-room after the object had been fixed upon; no communication with the child was conceivable as her place was often changed. Further, the only instructions given to the child were to fetch some object in the house that I would fix upon, and, together with the family, silently keep in mind to the exclusion, as far

as possible, of all other ideas. In this way I wrote down, among other things, a *hair-brush*; it was brought: an *orange*; it was brought: a *wine glass*; it was brought: an *apple*; it was brought: a *toasting-fork*; failed on the first attempt, a pair of tongs being brought, but on a second trial it was brought. With another child (among other trials not here mentioned) a *cup* was written down by me; it was brought: a *saucer*; this was a failure, a plate being brought; no second trial allowed. The child being told it was a saucer, replied, 'That came into my head, but I hesitated as I thought it unlikely you would name saucer after cup as being too easy.'

This last trial, some would think, shews pure guesswork, and invalidates the other results; but we prefer to let it stand, as, taken in conjunction with our experience obtained in other ways, it indicates one source of failure, namely, that in delicate experiments of the kind here recorded (assuming them to be cases of thought transmission) the slightest effort of reason, or of will, on the part of the subject is sufficient to vitiate the success of the experiment. No doubt the chief source of failure is to be found in the difficulty of suppressing the more vivid impressions made on the mind by the ordinary channels of sensation. We may compare this to the action of a die in stamping; light pressure of the die will yield a delicate and faithful impression, or a blurred and imperfect one, or none at all, according to the nature of the material that is stamped, or the prior existence of any deeply cut impression.

The second series of experiments, which we venture to think are unexceptionable, were made by Mr. Myers and Mr. Gurney, together with two ladies who were entire strangers to the family. None of the family knew what we had selected, the type of thing being told only to the child chosen to guess. The experimenters took every precaution in order that no indication, however slight, should reach the child. She was recalled by one of the experimenters and stood near the door with downcast eyes. In this way the following results were obtained. The thing selected is printed in italics, and the only words spoken during the experiment are put in parentheses:—

“Experiments made on April 13th, 1882:

Objects to be named.

A White Penknife.—Correctly named, with the colour, the first trial.

Box of Almonds.—Correctly named.

Threepenny piece.—Failed.

Box of Chocolate.—Button-box said; no second trial given.

Penknife hidden.—Failed to name the place.

Numbers to be named.

Five.—Correctly given the first trial.

Fourteen.—Failed.

Thirty-three.—54 (No). 34 (No). 33 (Right).
Sixty-eight.—58 (No). 57 (No). 78 (No).

Fictitious names to be guessed.

Martha Billings.—Failed ; Biggis was said.

Catherine Smith.—Catherine Shaw said.

Henry Cowper.—Failed.

Cards to be named.

Two of clubs.—Right first time.

Queen of diamonds.—Right first time.

Four of spades.—Failed.

Four of hearts.—Right first time.

King of hearts.—Right first time.

Two of diamonds.—Right first time.

Ace of hearts.—Right first time.

Nine of spades.—Right first time.

Five of diamonds.—Four of diamonds (No). Four of hearts (No).

Five of diamonds (Right).

Two of spades.—Right first time.

Eight of diamonds.—Ace of diamonds said ; no second trial given.

Three of hearts.—Right first time.

Five of clubs.—Failed.

Ace of spades.—Failed."

The chances against success in the case of any one card are, of course, 51 to 1, assuming that there is no such thing as thought-reading, and that errors of experiment are avoided. Special precautions were taken to avoid such errors of experiment as are described by Dr. Beard, and the results shew that in the case of cards, out of *fourteen* successive trials *nine* were guessed rightly the first time, and only three trials can be said to have been complete failures. On none of these occasions was it even remotely possible for the child to obtain by any ordinary means a knowledge of the card selected. Our own facial expression was the only index open to her ; and even if we had not purposely looked as neutral as possible, it is difficult to imagine how we could have unconsciously carried, say, the two of diamonds written on our foreheads.

Now, if we apply to these two sets of experiments the sources of error enumerated by Dr. Beard, the conclusion, we venture to think, is inevitable that we have here very strong evidence in favour of a class of phenomena entirely new to science. *Involuntary actions*, such as movement of the lips, &c., could not reach the child when she was out

of sight and hearing, as was the case in the first series of experiments. *Conscious or unconscious deception* on the part of the subject does not apply, as the thing wished for was selected and written down by one of us. *Collusion* by a third party is avoided by the fact that none were allowed to enter or leave the room after we had selected the thing to be guessed, and in the second series of experiments by the exclusion of all members of the family, either from the room, or from participation in the requisite knowledge*; whilst *chance and coincidence* we have already dealt with. In many trials, such as the guessing of fictitious names, made up by us on the spur of the moment, the chances against success were, of course, incalculable; yet, as will be seen by the following record taken from our last day's experimenting, these names were guessed with as much ease as cards, where the chances against success were far less.

In the following experiments the thing selected was known to the family, who, however, never left their places after we had written down the word and silently handed it round, or drawn a card, exposed it, and then replaced it in absolute silence. The child was now recalled *by one of us*, and, as before, stood in complete silence near the door, *no sounds nor movements nor interrogatory remarks of any kind by anyone being permitted*. There were present Mr. Gurney and Mr. Myers (Professor Barrett having left the day before), and the family.

Morning of April 17th, 1882 :

Cards to be named, drawn at random from a full pack. The card selected is printed in italics, the guesses are given in Roman type, and the only remarks made, and those were by us, are put in parentheses.

Five of clubs.—King of hearts (No). Five of clubs (Right).

Two of spades.—Two of spades (Right).

Five of spades.—Four of diamonds (No).

Three of spades.—Three of hearts (No). Ace of spades (No).

Five of clubs.—Four of clubs (No). Ace of clubs (No).

Two of spades.—Two of clubs (No). Three of clubs (No).

Eight of spades.—Eight of clubs (No). Eight of spades (Right).

Knave of hearts.—Knave of hearts (Right).

Six of hearts.—Six of clubs (No). Seven of clubs (No).

Eight of hearts.—Seven of hearts (No). Seven of clubs (No).

Ace of clubs.—Queen of clubs (No). Ace of clubs (Right).

Two of clubs.—Two of clubs (Right).

* In subsequent experiments we obtained successful results by individual trials with each of the children, that is to say, the number, word or card was known to some *one* of us only.

- Seven of hearts.*—Two of diamonds (No). Three of hearts (No)
Two of spades.—Two of clubs (No). Two of spades (Right).
Six of diamonds.—Six of clubs (No). Six of diamonds (Right).
Three of hearts.—Four of hearts (No). Three of hearts (Right).
Eight of diamonds.—Five of diamonds (No). Seven of diamonds
 (No).
Eight of spades.—Nine of spades (No). Ten of spades (No).
 Eight of spades (Right).
King of spades.—King of clubs (No). Knave of clubs (No).
 King of diamonds (No).
Three of spades.—Three of spades (Right).
Knave of diamonds.—King of diamonds (No). Knave of diamonds
 (Right).
Nine of spades.—Nine of spades (Right).
Ten of clubs.—Ten of hearts (No). Queen of hearts (No).
Three of diamonds.—Three of diamonds (Right).
Six of spades.—Six of spades (Right).
Ten of diamonds.—Ten of diamonds (Right).
Knave of diamonds.—Ace of diamonds (No).

The trials so far were principally with the two children Maud and Alice; the eldest sister, Mary, was now tried, with the following results, *every experiment* being given in the order it was made.

- Six of spades.*—Eight of clubs (No). Eight of spades (No).
Ace of diamonds.—Ace of diamonds (Right).
Queen of hearts.—Queen of hearts (Right).
Two of clubs.—Two of clubs (Right).
Ten of spades.—Ten of spades (Right).
Ten of diamonds.—Ten of diamonds (Right).
Five of spades.—Five of spades (Right).
Two of spades.—Two of spades (Right).
Five of diamonds.—Five of diamonds (Right).
Three of clubs.—Four of clubs (No). Five of clubs (No). Three
 of clubs (Right).
King of clubs.—Ace of diamonds (No). Knave of clubs (No).
 King of clubs (Right).
Five of spades.—Four of spades (No). Five of spades (Right).
Seven of diamonds.—Five of diamonds (No). Five of clubs (No).
 Seven of diamonds (Right).
Queen of spades.—Queen of spades (Right).
Six of spades.—Six of spades (Right).
Three of spades.—Four of spades (No). Three of spades (Right).
Knave of diamonds.—Ace of diamonds (No). Knave of diamonds
 (Right).

Eight of hearts.—Nine of hearts (No). Eight of hearts (Right).
Nine of diamonds.—Nine of diamonds (Right).
Knave of clubs.—King of clubs (No). Knave of clubs (Right).
Four of clubs.—Four of clubs (Right).
Nine of hearts.—Five of hearts (No). Nine of hearts (Right).
Two of clubs.—Two of clubs (Right).
Six of clubs.—Six of clubs (Right).
King of clubs.—Knave of clubs (No). King of clubs (Right).
Nine of hearts.—Nine of diamonds (No). Nine of hearts (Right).
Ten of clubs.—Ten of clubs (Right).
Ace of clubs.—Ace of clubs (Right).
Five of clubs.—Five of clubs (Right).
Seven of clubs.—Five of diamonds (No). Seven of clubs (Right).
Knave of hearts.—Knave of clubs (No). Knave of diamonds (No).
 Knave of hearts (Right).

Fictitious words were now chosen ; during some of these trials Mr. Creery was absent. Miss Mary was the guesser in the first five trials, then Maud was selected ; the words chosen are again indicated by italics :—

William Stubbs.—William Stubbs.

Eliza Holmes.—Eliza H——.

Isaac Harding.—Isaac Harding.

Sophia Shaw.—Sophia Shaw.

Hester Willis.—Cassandra, then *Hester Wilson.*

John Jones.—John Jones.

Timothy Taylor.—Tom, then *Timothy Taylor.*

Esther Ogle.—Esther Ogle.

Arthur Higgins.—Arthur Higgins.

Alfred Henderson.—Alfred Henderson.

Amy Frogmore.—Amy Freemore. Amy Frogmore.

Albert Snelgrove.—Albert Singrore. Albert Grover.

In estimating our successes and failures, partial success is counted as a failure ; thus, seven of diamonds given instead of eight of diamonds, is counted wrong, and so in the names, Wilson given instead of Willis, and Grover instead of Snelgrove, are counted as failures.

The outline of results during the present investigation, which extended over six days, stands as follows :—Altogether 382 trials were made. In the case of letters of the alphabet, of cards, and of numbers of two figures, the chances against success on a first trial would naturally be 25 to 1, 51 to 1, and 89 to 1, respectively ; in the case of surnames they would of course be indefinitely greater. Cards were

far most frequently employed, and the odds in their case may be taken as a fair medium sample; according to which, out of the whole series of 382 trials, the average number of successes at the first attempt by an ordinary guesser would be $7\frac{1}{2}$. Of our trials 127 were successes on the first attempt, 56 on the second, 19 on the third, making 202 in all. On most of the occasions of failure, 180 in number, second trials were made; but in some cases the guesser professed inability, and declined to make more than one, and in others we allowed three; no trial beyond the third was ever allowed. During the last day or two of trial, after it had occurred to us to notice the point, we found that of the failures to guess a card at the first trial, those wrong both in suit and number were a small minority.

Our most striking piece of success, when the thing selected was divulged to none of the family, was five cards running named correctly on a first trial; the odds against this happening once in our series were considerably over a million to 1. We had altogether a good many similar batches, the two longest runs being 8 consecutive successes, once with cards and once with names; where the adverse odds in the former case were over 142 millions to 1, and in the latter something incalculably greater. If we add to these results others obtained on previous visits, it seems not too much to say that the hypothesis of mere *coincidence* is practically excluded.

We are aware that the exceptional nature of this inquiry goes far to invalidate arguments founded on character and demeanour; and on this head, therefore, will only state our conviction that any candid critic, present during the whole course of the experiments, would have carried away a far more vivid impression of their genuineness than the bare-printed record can possibly convey. Of more real importance is the hypothesis of exalted sensibility of the ordinary sense organs. We could discover no indication of this in any of its known forms; but by way of precaution, as has been already stated, we commonly avoided even whispering any word, number, or name that we had selected; and the position of the excluded child, when the door was opened, would in every case have satisfied the most exacting critic. The explanation which might be sought in unconscious indications given by the sitters, and especially in the movement of the lips, has been already adverted to.

Coming as we did to this investigation with considerable previous experience of the same kind, we were throughout strictly on our guard against giving such indications ourselves; the possibility of their being given by the family was of course excluded where the family were ignorant of the selected word or thing; and on the remaining occasions our perpetual vigilant watch never detected a trace of anything of the kind. The absolute docility of the children—both the guesser and the others—in taking any position in the room that we

indicated, was naturally an assistance to our precautions. It may be further mentioned that, on a previous visit made by one of us, the child called the required name through the shut door or from an adjoining room, having thus been completely isolated from the very beginning to the very end of the experiment.

It must be remembered that our great pre-occupation throughout was to guard against delusion. Had the phenomena been sufficiently established to allow of a systematic search for their underlying laws, we might have preferred a more unvarying method of experimentation; but in this preliminary stage it seemed desirable to meet *prima facie* possibilities of deception by frequent and unexpected changes of the various conditions. At the same time we endeavoured to gather such indications as we could of the way in which the impression flashed on the mind of the child.

The first question concerns the respective parts in the phenomena played by mental *eye* and mental *ear*. Among the experiments which we have counted as *failures* were very many where the number or card selected was guessed, as it were, piecemeal. For instance, the number 35 was selected, and the guesses were 45 and 43. So 57 was attempted as 47 and 45. So with cards: the seven of diamonds being chosen the guesses were six of diamonds and seven of hearts; the three of spades being chosen, the guesses were queen of spades and three of diamonds. These cases seem somewhat in favour of mental *eye*, the similarity in *sound* between three and thirty in 43 and 35, or between five and fifty in 45 and 57, not being extremely strong; while the *picture* of the 3 or the 5 is identical in either pair. A stronger argument on the same side is the frequent guessing of king for knave, and *vice versa*. On the other hand, names of approximate sound (also reckoned as failures) were often given instead of the true ones; as "Chester" for Leicester. "Biggis" for Billings. Frogmore was guessed first as "Freemore"; Snelgrove was given as "Singrore," the last part of the name was soon given as "Grover," and the attempt was then abandoned; the child remarking afterwards that she thought of "Snail" as the first syllable, but it had seemed to her too ridiculous. One of us has, moreover, successfully obtained from the maid-servant a German word of which she could have formed no visual image. The children's own account is usually to the effect that they "seem to see" the thing; but this, perhaps, does not come to much, as a known object, however suggested, is sure to be instantly visualised.

Another question would be as to the effect of greater or less distance between the sitters and the guesser, and of the intervention of obstacles. It will have been seen that, in the experiments conducted by one of us on a former occasion, the intervention of a door or wall seemed to make no difference. It would be interesting, again, to

discover whether numerical increase in the observers increases the effect, and how far the presence of special persons is influential. In our experience the presence of the father—though by no means essential and very often dispensed with—seemed decidedly to increase the percentage of successes.

A still more interesting and important question concerns such conditions of success and failure as may lie in the circumstances, disposition, general capacity, and mood of the subject, including such points as consanguinity and familiarity with members of the circle, and also in the temper and manner of the latter. We are dealing, not with chemical substances, but with childish minds, liable to be reduced to shyness and confusion by anything in the aspect or demeanour of visitors which inspires distaste or alarm. The importance of “a childly way with children,” and the slightness of the differences of manner which will either paralyse them into stupidity or evoke unexpected intelligence and power, are commonplaces to anyone whose duties have lain among them; and attention to such points may be as prime a factor of success in these delicate experiments as any other.

The delicacy of the conditions was illustrated in our own inquiry partly by the inexplicable fluctuations of success and failure affecting the whole household, partly by the wide difference observed in the capacities of particular members of it from day to day. The common notion that simplicity, and even comparative blankness of mind, are important conditions, seems somewhat doubtfully borne out by our experience; but of the favourable effect of freedom from constraint, and of a spice of pleasurable excitement, we can speak with entire assurance. The particular ill-success of a sitting which we held one close afternoon was attributed by the children themselves—and it seemed to us correctly—to inertness after their early dinner.

We could find no resemblances between these phenomena and those known as *mesmeric*; inasmuch as a perfectly normal state on the part of the subject seemed our first pre-requisite. Nor did we find any evidence that “strength of will” has any particular effect, except so far as both subject and circle may exercise it in patient attention. On one or two occasions it seemed of advantage to obtain vivid simultaneous realisation of the desired word on the part of all the sitters; which is most easily effected if some one slowly and gently claps time, and all mentally summon up the word with the beats.

Many further lines of the investigation suggest themselves; for instance, a great step would be made if a more complex idea, and one not habitually expressed by one definite sound or set of sounds, could be transmitted. An immense number of accurately recorded experiments will be necessary for the establishment of such special points; and possibly the present instalment may serve in some degree to stimulate

and concentrate various inquiries in the same direction, which, though widely spread, seem so far to have been for the most part of a lax and fitful sort. The material for such inquiries, as may be surmised from the present record, must be in large proportion children, who are fortunately not rare, and who may be congratulated on so grand an opportunity for combining utility with amusement.

The primary aim in all cases must be to get the results *without physical contact* or anything approaching it, a stage to which some practice with contact may be a necessary preliminary; in no other way can the hypothesis of "muscle-reading" be with certainty eliminated; while *en revanche*, the phenomena without contact, if once established, will afford solid ground for questioning the sufficiency of that hypothesis to account for all cases in which contact occurs.

As already mentioned our observations with this family have had the advantage of being confirmed by scientific and other friends. Professor Balfour Stewart, F.R.S., and Professor Alfred Hopkinson, M.A., have paid two or three visits to Buxton and the result of their experiments will be found in a subsequent paper. Before we pass from this part of our report we wish to express our sincere thanks to Mr. and Mrs. Creery for the kind and ready way in which they permitted us to conduct these experiments, at our own times and in our own way, giving them often, we fear, no little trouble and inconvenience.

We now come to the fourth group of cases.

IV. *Where similar thoughts have simultaneously occurred or impressions been made in minds far apart, without any known means of communication.*

Several cases of this kind have reached us, but they rest upon the testimony of others, and though we have no reason to doubt the accuracy of our informants, the evidence has necessarily a lower rank than the preceding. The following cases may be taken as a sample of other statements that have come to our knowledge. We are acquainted with, but not at liberty to publish, the names in the first case, which is related by the wife of General R——.

"On September 9th, 1848, at the siege of Mooltan, Major-General R——, C.B., then adjutant of his regiment, was most severely and dangerously wounded, and supposing himself dying, asked one of the officers with him to take the ring off his finger and send it to his wife, who, at the time, was fully 150 miles distant, at Ferozepore.

"On the night of September 9th, 1848, I was lying on my bed, between sleeping and waking, when I distinctly saw my husband being carried off the field, seriously wounded, and heard his voice saying, 'Take this ring off my finger, and send it to my wife.' All the next day I could not get the sight or the voice out of my mind. In due time

I heard of General R—— having been severely wounded in the assault on Mooltan. He survived, however, and is still living. It was not for some time after the siege that I heard from Colonel L——, the officer who helped to carry General R—— off the field, that the request as to the ring was actually made to him, just as I had heard it at Ferozepore at that very time.—M. A. R.”

“LESLIE LODGE, EALING, W., October 10th, 1876.

“DEAR SIR,—The circumstance about which you inquire is as follows:—I had left my house, ten miles from London, in the morning as usual, and in the course of the day was on my way to Victoria Street, Westminster, having reached Buckingham Palace, when in attempting to cross the road, recently made muddy and slippery by the water cart, I fell, and was nearly run over by a carriage coming in an opposite direction. The fall and the fright shook me considerably, but beyond that I was uninjured. On reaching home I found my wife waiting anxiously, and this is what she related to me: She was occupied wiping a cup in the kitchen, which she suddenly dropped, exclaiming, ‘My God! he’s hurt.’ Mrs. S., who was near her, heard the cry, and both agreed as to the details of time and so forth. I have often asked my wife why she cried out, but she is unable to explain the state of her feelings beyond saying, ‘I don’t know why; I felt some great danger was near you.’ These are simple facts, but other things more puzzling have happened in connection with the singular intuitions of my wife.—Yours truly,

“T. W. SMITH.”

The next case is more remarkable; our informant is a medical man, Mr. C. Ede, of Guildford, to whom the incident was related both by Lady G. and her sister.

“Lady G. and her sister had been spending the evening with their mother, who was in her usual health and spirits when they left her. In the middle of the night the sister awoke in a fright, and said to her husband, ‘I must go to my mother at once; do order the carriage. I am sure she is taken ill.’ The husband, after trying in vain to convince his wife that it was only a fancy, ordered the carriage. As she was approaching her mother’s house, where two roads meet, she saw Lady G.’s carriage. When they met, each asked the other why she was there. The same reply was made by both. ‘I could not sleep, feeling sure my mother was ill, and so I came to see.’ As they came in sight of the house, they saw their mother’s confidential maid at the door, who told them when they arrived, that their mother had been taken suddenly ill, and was dying, and had expressed an earnest wish to see her daughters.”

The following interesting letter from Mr. Ede accompanied this narrative.

WONERSH LODGE,

GUILDFORD, SURREY,

August 29th, 1877.

DEAR SIR,—The foregoing incident was told me as a simple narrative of what happened, both by Lady G. and her sister. The mother was a lady of strong will, and always had great influence over her daughters.

I, myself, have been persuaded that impressions and thoughts might be transmitted by the action of a powerful will upon sensitive brains at a distance, by some experiments which I made in mesmerism, being at first a strong disbeliever in all these things, and only convinced when testing the assertions of others. There must, it would seem, be some previous relation between the two brains, as in states of anxiety for the absent, or powerful longing. May not a material vibration in a strong brain affect another by its vibration, as light at a distance acts upon the retina of the eye, or sound upon the ear? We know that many sounds escape us if our attention be not directed to them, and, likewise, many objects may not be perceived. It is curious in the case of Lady G. and her sister, that both impressions were made in the night, when the attention was not diverted by surrounding sights or sounds.

This may have had some connection with the following incident which happened to myself lately. There is a house about half-a-mile from my own, inhabited by some ladies, friends of our family. They have a large alarm bell outside their house. One night I awoke suddenly and said to my wife, "I am sure I hear Mrs. F.'s alarm bell ringing." After listening for some time we heard nothing, and I went to sleep again. The next day Mrs. F. called upon my wife, and said to her, "We were wishing for your husband last night, for we were alarmed by thieves. We were all up, and I was about to pull the alarm bell, hoping he would hear it, saying to my daughters, I am sure it will soon bring your husband, but we did not ring it." My wife asked what time it was; Mrs. F. said it was about half-past one. That was the time I awoke thinking I heard the bell.

I could also give you many instances of the communication to another of a strong wish on my part, although unuttered, and unaccompanied by any gesture, or hint by look or action. I have often been amused at a concert, or other place of meeting, to single out some person who has their back to me, and will them to turn their head in a given direction towards me, and generally I succeed. It is common enough to have the same thoughts spoken by two people simultaneously, but, though the previous conversation might often suggest like ideas, I think it would not be difficult to sift out the cases of direct mental impressions from those of coincidence, suggestion, or sequence of thought arising from surrounding causes. When I have been strongly wishing to see a friend it constantly happens that he appears. May not the many extraordinary cases of apparitions be but the mental pictures produced by other minds on a sensitive subject? There is a well-known case recorded in the Colonial papers which supports this view.

Yours truly,

CHARLES EDE.

The Australian case to which Mr. Ede refers we have heard from different sources. If it be not a myth it is akin to the well-known case of Zchokke, which, if established, might throw much light upon apparitions. This subject we propose to deal with later, as it is beyond the scope of the present report.

It is obvious that any conclusions worth recording must rest upon a large induction of cases that cannot be obtained at will. We are, therefore, in a large measure dependent upon the testimony of correspondents, and would invite further information, which may be sent to any of us,* or to the secretary of the Society for transmission to us.

We cannot pretend that this inquiry is as yet more than in its infancy, and we would deprecate the premature formation of theories on the subject. The phenomena here described are so unlike any which have been brought within the sphere of recognised science, as to subject the mind to two opposite dangers. Wild hypotheses as to how they happen are confronted with equally wild assertions that they cannot happen at all. Of the two, the assumption of *a priori* impossibility is, perhaps, in the present state of our knowledge of Nature, the most to be deprecated; though it cannot be considered in any way surprising.

At the same time it may serve to disarm purely *a priori* criticism if we point out that the word "thought-reading" is merely used as a popular and provisional description, and is in no way intended to exclude an explanation resting on a physical basis. It is quite open to surmise some sort of analogy to the familiar phenomena of the transmission and reception of vibratory energy. A swinging pendulum suspended from a solid support will throw into synchronous vibration another pendulum attached to the same support if the period of oscillation of the two be the same; the medium of transmission here being the solid material of the support. One tuning-fork or string in unison with another will communicate its impulses through the medium of the air. Glowing particles of a gas, acting through the medium of the luminiferous ether, can throw into sympathetic vibration cool molecules of the same substance at a distance. A permanent magnet brought into a room will throw any surrounding iron into a condition similar to its own; and here the medium of communication is unknown, though the fact is undisputed. Similarly, we may conceive, if we please, with many modern philosophers, that for every thought there is a corresponding motion of the particles of the brain, and that this vibration of molecules of brain-stuff may be communicated to an intervening medium, and so pass under certain circumstances from one

* For postal address see list of members.

brain to another, with a corresponding simultaneity of impressions. No more than in the case of the magnetic phenomena is any investigator bound to determine the *medium* before inquiring into the *fact* of transit. On the other hand, the possibility must not be overlooked that further advances along the lines of research here indicated may, and we believe will, necessitate a modification of that general view of the relation of mind to matter to which modern science has long been gravitating.

NOTE ON THOUGHT-READING.

By BALFOUR STEWART, LL.D., F.R.S., &c., Professor of Physics at the Owens College, Manchester.

[*Read to the Society, July 17, 1882.*]

After the exhaustive report that we have had from Professor Barrett upon thought-reading, I shall only trouble you with a very few remarks. As one who has been engaged more in physical science than in anything else, I may perhaps be allowed to give an illustration from physical science that has reference to the best method of obtaining evidence of infrequent phenomena.

It so happens that there is in science a phenomenon that has been frequently observed by trustworthy observers, but that until very recently has hardly been accepted at all as anything that could possibly have occurred. I allude to the case of globular lightning. It was said in objection to all the evidence with reference to globular lightning, that is to say, a thunder-bolt travelling at a slow rate, and afterwards exploding and giving rise to lightnings of the ordinary kind, that what occurs is an electric discharge, and that all electric discharges must necessarily take place in a moment of time inappreciably small. Of late years, however, some physicists have suggested that this globular lightning, instead of being an ordinary electric discharge, is really a sort of travelling Leyden jar, and I believe one foreign observer has shewn in some experiments that something analogous to that on a small scale may be artificially produced. I think I am entitled to say that a change of tone has consequently taken place amongst physicists with regard to the evidence for globular lightning. The evidence of course remains as before. A little additional evidence accumulates now and then, but the great bulk remains as it was. The fact that we are able to explain this phenomenon without overthrowing entirely our received views on electricity, has certainly enabled people to accept evidence that they would not have accepted before.

Thus we see that the reason why this evidence was not accepted before was because the hypothesis with regard to electric discharges was insufficient. We imagined that there could not be anything but an ordinary electric discharge: we did not imagine the possibility of what may be called a travelling Leyden jar.

Now there is no question, I think, that the ordinary way in which we have communications from one human being to another, is by means of

what may be called the five senses. No one, of course, disputes that; but I do not know that this fact, any more than any other scientific fact, or any scientific law, should be taken as absolutely final and complete. Scientific experience has always shewn that we go from one generalisation to another. First of all we bind together a number of facts by what may be called a working hypothesis, which we may call a generalisation of the first order. Afterwards we find that there are slight departures from this working hypothesis, and then we are led to reflect on these departures, and are ultimately led to a higher law. Now if we were to treat this first generalisation or working hypothesis as something absolutely final, we should be able to gain no more information upon the subject. Surely it would not be the right way for any one who has come to a first generalisation to set his face against all extensions of it, neither making extensions himself nor trusting to the evidence of any others who may profess to have done so.

But this is exactly the position taken up by certain physiologists with regard to the possibility of thought-reading. It has been recognised throughout the world,—and all of us who are here recognise it as completely as any,—that the five senses are the ordinary and established means by which communications are made; but that physiologists should regard this as an absolutely final and complete statement is decidedly against all scientific analogy, and that they should decline, as some have done, to see experiments themselves or refuse credit to those who have done so, is to pursue a very objectionable method. I quite think that the mode in which our Chairman has put it is the best possible mode. We have, as he said, to bring evidence in such a way before the public that they must either believe the phenomena or be compelled to say, “We do not trust those who brought them forward;” and I think that in this respect the report that has been read by Professor Barrett, and the observations made by him and Mr. Myers and Mr. Gurney, have certainly succeeded wonderfully well. The only possible way of disputing the evidence is by hinting at the untrustworthiness of those gentlemen who have given it, and consequently I think their efforts must be regarded as successful.

Professor Barrett, Mr. Gurney, and Mr. Myers have, as I have said, put things in such a way that if they are to be denied you must dispute the trustworthiness of those gentlemen. Professor Hopkinson and myself have not perhaps obtained equally conclusive results; we had not the same time to devote to the inquiry. We, however, obtained results which neither of us was able to account for by any received hypothesis.

Our experiments were made in the same house and with the same host, and they are valuable, I think, at any rate, in confirming the conclusions arrived at by those gentlemen from their experiments.

If they are to be disputed on account of untrustworthiness, it is clear that the charge of untrustworthiness must be extended so as either to embrace Professor Hopkinson and myself, or the gentleman who was kind enough to give us the opportunity of seeing the experiments performed—perhaps to include us all—but I do not think that any of us will mind that very much.

I should like to say a word with regard to the last series of phenomena or the extension of thought-reading at a distance, which Professor Barrett brought before the meeting. I have devoted a great deal of attention to reading evidence on this particular point, and I certainly think that if we can rely upon evidence at all we have here a very strong body of evidence for some kind of action at a distance, particularly for the appearance of one individual to another at a distant place at the time of death. The reason for my bringing up this case is that while there is very strong evidence for something of the kind, I have been much surprised that it has not been put upon such a footing as would certainly commend itself to all men of science from without. Of course, it is a matter of delicacy for an individual who has received a communication of this kind to make it public, but it would be a great boon and an addition to our knowledge if he would do so either by an ordinary letter to a newspaper or by giving the communication in some kind of cypher. In such a case if, before the intelligence of the death can have arrived, a communication of this kind is published, either openly or in cypher, there will be unimpeachable evidence of a character to satisfy any candid inquirer, that something peculiar has taken place. In science, as in law, the evidence ought always to be the very best that can be brought into court.

We must bear in mind that coincidence will not certainly explain a thing of that kind. Suppose, for instance, that an appearance presented itself to an individual at a distance, and that death happened within ten minutes of this appearance. First of all, such an appearance is uncommon; then the probability of any person dying in a particular ten minutes is very small; and when the two things happen together you have to multiply the one probability by the other, and you will find that the probability of the united event is something which is inappreciably small, and consequently, if a thing of that kind happens, it cannot be accounted for by any such hypothesis as coincidence.

The few experiments which I took part in performing were performed at Buxton, at the house of a clergyman, who, I am glad to see, is present with us to-day. We paid two visits to his house. In the first instance, the thought-reader was outside a door. The object or thing thought of was written on paper and silently handed round to the company in the room. The thought-reader was then called in, and in the course, perhaps, of a minute, the answer was given. Definite

objects in the room, for instance, were first thought of, and generally the answer was right. Then cards were thought of, and in the majority of cases the answer was correct. Then numbers were thought of, and the answers were generally right; but, of course, there were some cases of error. Then names of towns were thought of, and a good many of these were right. Then fancy names were thought of. When my colleague, Professor Hopkinson, had gone away, I was asked to think of certain fancy names, and mark them down and hand them round to the company. I then thought of, and wrote on paper, "Blue-Beard," "Tom Thumb," "Cinderella," and the answers were all correct. I think it was the servant who answered "Cinderella." There was some hesitation in getting her to pronounce the name, as she seemed to think she did not know it.

After the first visit, one of my colleagues at Owens College remarked that it would be more conclusive if the thought-reader, instead of turning her face to the company, turned her face to the wall; and that was accordingly done on the second occasion. The percentage of success was about as large as in the first instance. In one case, while the thought-reader *remained* behind the door, a card was chosen. I chose the "ace of hearts," and the paper on which it was written down was handed round to the company. The thought-reader in a few moments called out, "Ace of hearts!"

These are all the experiments that I have to bring before you. While they cannot stand upon the same footing as those of Professor Barrett and his colleagues, they may be considered, I think, as corroborative of the experiments of these gentlemen. At any rate, if they are objected to, it will be necessary for our opponents to extend somewhat the area of untrustworthiness. I have no doubt when this operation is done again and again the objectors will get tired of it, and the laugh will then be turned against themselves.

The following is the detailed report of the experiments I have alluded to:—

On Saturday, November 12th, 1881, Professor Alfred Hopkinson and I went to the house of the Rev. A. M. Creery, at Buxton. There were present, besides Mr. Creery, Miss Mary Creery, also Alice, Emily, Maud, Kathleen, children; and the servant Jane.

After a few preliminary trials, the following guesses were made, the guesser going out of the room until some object was thought of by the company, when she came in and tried to guess what object was in the thoughts of all. No questions were asked nor observations made by the company:—

First.—DEFINITE OBJECTS THOUGHT OF.

1. *Pipe.*—Alice guessed plate, paper, then pipe.
2. *Fork.*—Maud guessed it at once.
3. *Cup.*—Emily guessed it at once.
4. *Corkscrew.*—Jane guessed it at once.
5. *Tongs.*—Miss Mary guessed fire-irons and then poker.

Second.—CARDS THOUGHT OF.

6. *Three of clubs.*—Jane guessed three of spades, then three of clubs.
7. *Queen of clubs.*—Miss Mary guessed three of diamonds.
8. *Four of clubs.*—Maud guessed five of clubs, then four of clubs.
9. *Ace of diamonds.*—Jane guessed ace of clubs, then ace of diamonds.
10. *King of spades.*—Jane guessed four of diamonds, then six of diamonds.
11. *King of hearts.*—Maud guessed knave of hearts, then king of hearts.
12. *Ace of spades.*—Maud guessed right at once.
13. *King of diamonds.*—Professor Stewart tried and guessed ten of diamonds.
14. *Three of diamonds.*—Miss Mary guessed right at once.
15. *Ace of hearts.*—Alice guessed right at once.
16. *King of clubs.*—Professor Hopkinson tried and guessed knave of spades, then four of hearts.
17. Mr. Creery and Professor Balfour Stewart tried but could not guess.

Third.—NUMBERS THOUGHT OF.

18. *Forty-eight thought of.*—Jane guessed 34, 44, 84.
19. *Sixty-seven thought of.*—Miss Mary guessed 66, then 67.
20. *Fifty-five thought of.*—Maud guessed 54, 56, then 55.
21. *Eighty-one thought of.*—Alice guessed 71, then 81.
22. *Thirty-one thought of.*—Emily did not guess it.
23. *Eleven thought of.*—Kathleen did not guess it.

Fourth.—OBJECTS THOUGHT OF.

Experiments 24, 25, 26, and 27, in which objects were thought of, were inconclusive, as the names of the things chosen might possibly have been surmised by the guesser.

Fifth.—NAMES OF TOWNS THOUGHT OF.

28. *Macclesfield.*—Jane did not guess rightly, then sat down, and shortly afterwards guessed rightly.
 29. *York.*—Maud guessed Ashford, then York.
 30. *Paris.*—Miss Mary did not guess rightly.
 31. *Chester.*—Jane guessed Manchester, then Chester.
 (N.B.—During this series also Mr. Creery was out of the room.)

Sixth.—FANCY NAMES.

32. *Peter Piper.*—Alice guessed at once.
 33. *Bluebeard.*—Jane guessed at once.
 34. *Tom Thumb.*—Jane guessed at once.
 35. *Cinderella.*—Jane guessed at once.

I ought to state that the object thought of was marked on paper by one of the company, and handed round silently, so that all present might be aware of it.

I ought also to mention that the thought-reader was aware of the general character of the things thought of; for instance, that it was definite objects in the first place; cards in the second, and so on.

Professor Hopkinson agrees with the above memorandum, except that after No. 29, Derby was put down as the name of a town, and Maud guessed right the first time.

EXPERIMENTS AT BUXTON, FEBRUARY 18TH, 1882.—Present: Mr. Creery and his five daughters; servant Jane; also Professors Hopkinson and Balfour Stewart.

GUESSES.	CARD SELECTED BY BALFOUR STEWART	RESULT.
1. Jane	Six of hearts	Wrong.
2. Miss Alice ...	Knave of clubs	"
3. Miss Maud...	Seven of hearts	Right 1st time.
4. " ...	Ten of spades.....	" 1st time.
5. " ...	King of diamonds	" 2nd time.
6. Miss Maud...	Ace of hearts.....	Wrong.
7. Miss Mary ...	Six of spades	"
8. Jane	Ten of hearts.....	Right 1st time.
9. "	Three of diamonds	Wrong.
10. "	Four of diamonds	"
11. "	Four of spades	"

GUESSER.	CARD SELECTED BY BALFOUR STEWART.	RESULT.
12. Miss Maud...	Five of clubs	Right 3rd time.
13. "	Six of hearts	" 2nd time.
14. "	Queen of hearts.....	" 1st time.
15. Miss Alice ...	Two of diamonds	" 2nd time.
16. "	Nine of diamonds	" 3rd time.
17. "	Three of clubs	" 3rd time.
18. "	Six of diamonds	" 3rd time.
19. "	King of spades	" 2nd time.
20. "	Queen of spades.....	" 3rd time.
21. "	Knave of diamonds	" 2nd time.
22. Miss Mary ...	Eight of clubs	Wrong.
23. Jane	Five of diamonds	Right 1st time.
24. "	Four of spades	" 1st time.
(In the three next experiments the guesser remained outside the door.)		
25. "	Ace of hearts	Right 1st time.
26. "	Five of spades	{ Wrong. Ace of Spades guessed.
27. "	Five of diamonds	{ Wrong. Ace of Diamonds guessed.

NUMBER SELECTED BY
BALFOUR STEWART.

28. Jane	22	Right 1st time.
29. "	46	Wrong.
30. "	10	Right 1st time.
31. "	12	Wrong.
32. Miss Maud...	44	Right 1st time.
33. "	37	" 1st time.
34. "	81	Wrong.
35. Miss Alice ...	33	Right 1st time.
36. "	27	" 2nd time.
37. "	55	Wrong.
38. Miss Mary ...	66	"
39. Jane	28	"
40. "	43	"
41. "	22	"

OBJECT SELECTED BY
BALFOUR STEWART.

42. Jane	A dish	Wrong.
43. Miss Maud...	Cream jug	"
44. Miss Alice ...	Scissors	Right 1st time.

GUESSER.	OBJECT SELECTED BY BALFOUR STEWART.	RESULT.
45. Miss Alice ...	Prof. Hopkinson's hat ...	Right 3rd time.
46. „ ...	Key	Wrong.
47. Miss Maud...	Clothes brush.....	Right 1st time.
48. „ ...	Umbrella	Wrong.
49. Jane	Candlestick	„
50. „	Teapot	„
51. Miss Alice ...	Watch	„
52. „ ...	Key	„
53. Miss Mary ...	Knife	Right 1st time.*
54. „ ...	Pencil.....	„ 1st time.
55. „ ...	Toothpick	Doubtful.
56. „ ...	A sovereign	Right 2nd time.
57. Miss Maud...	Purse	Wrong.

In all the above cases, except two or three, the guesser's back was turned to the company.

* The knife was also correctly described by the guesser. It had not been out of Professor Hopkinson's pocket until after Miss Mary had left the room.

NOTE ON THOUGHT-READING.

By the Rev. A. M. CREERY, B.A.

[*Read to the Society, July 17, 1882.*]

In the month of October, 1880, my attention was called to the phenomena of the "willing game," but being unable to determine how much of the results was due to simple willing, and how much to involuntary pushing, I resolved to thoroughly investigate the whole question of the action of mind on mind. For this purpose I employed four of my children between the ages of ten and sixteen, all being in perfectly robust health, and a maid-servant, about twenty years of age. Each went out of the room in turn, while I and the others fixed on some object which the absent one was to name on returning to the room. After a few trials the successes preponderated so much over the failures that we were all convinced there was something very wonderful coming under our notice. Night after night, for several months, we spent an hour or two each evening in varying the conditions of the experiments, and choosing new subjects for thought-transference. We began by selecting the simplest objects in the room; then chose names of towns, names of people, dates, cards out of a pack, lines from different poems, &c., in fact any things or series of ideas that those present could keep steadily before their minds; and when the children were in good humour, and excited by the wonderful nature of their successful guessing, they very seldom made a mistake. I have seen seventeen cards, chosen by myself, named right in succession, without any mistake. We soon found that a great deal depended on the steadiness with which the ideas were kept before the minds of "the thinkers," and upon the energy with which they willed the ideas to pass. Our worst experiments before strangers have invariably been when the company was dull and undemonstrative; and we are all convinced that when mistakes are made the fault rests, for the most part, with the thinkers, rather than with the thought-readers.

I may say that this faculty is not by any means confined to members of one family; it is much more general than we imagine. To verify this conclusion I invited two of a neighbour's children to join us in our experiments. On the first evening they were rather diffident, and did not succeed; on the second they improved, and on the third evening they were still better. Circumstances prevented them being able to

continue their visits to us, but I saw enough to make me feel perfectly sure that had they persevered they would have been quite equal to our own circle in the faculty of thought-reading.

Those who may be desirous of ascertaining the truth of the matter, can do so in their own families; and since it in no way interferes with the health of those engaged, it will be found a very interesting way of passing an hour on a winter's evening.

The distance between the thinkers and the thought-reader is of considerable consequence. As a rule the best results take place when this distance is not more than a yard or two; but under very favourable mental conditions we have often had four and five cards named right in succession, while the thought-reader was placed in a room on the landing above that in which the thinkers were assembled.

On questioning the children as to the *mode* by which they form their judgment of the ideas that come before their minds, I find them all agreed in this: two or three ideas of objects of the class with which we are experimenting come before their minds, and after a few moments' reflection they select that which stands out with the greatest vividness. At present we are not in a position to theorise very far on this subject, still we cannot help asking ourselves this question: How are the motions of the brains of the thinkers communicated to the brain of the thought-reader? Is there any such thing as direct action between mind and mind? or are "brain-waves" set up in some intervening medium, either in the luminiferous ether, or in a nerve atmosphere developed at the time in the cerebra of the thinkers, by which the corresponding idea is called up in the mind of the thought-reader? These are questions which, at present, we cannot definitely answer; but I am under the impression that the medium of communication is something more subtle than the vehicle that conveys heat and light.

When we began to investigate these curious phenomena we had no idea that the result of our little amusement would ever come before the public. But having been asked to deliver a lecture on some popular subject before a small philosophical society in Derby, I volunteered to give an account of the experiments in "Thought-reading" with which I was then engaged. A short report, which appeared in the local papers, I forwarded to Professor Barrett, who I knew was interested in such matters. He at once took it up, and paid us his first visit at Easter, 1881, the results of which he afterwards published in *Nature*; and should conclusions of any psychological value be ever deduced from the experiments that I commenced it will be mainly to him that science will be indebted.

P.S.—The last evening Professor Stewart was with us I asked a medical man in Buxton (Mr. Turner) to join us in our experiments. Professor Stewart was obliged to leave early, but Mr. Turner, in my

absence with Professor Stewart, continued the experiments, a record of which he has sent me, and is here subjoined.

“With a friend who appends his signature to these notes, which are copied from those taken on the moment, I visited the Rev. A. M. Creery on February 18th, 1882, for the purpose of witnessing the power of thought-reading possessed by his children. In the absence of Mr. Creery, I made an attempt to test the children’s power, and with the following results, roughly chronicled I know, and imperfect as a searching test, but accurate as to the results obtained.

“MISS ALICE CREERY.—

Expt. 1.—What do I hold in my hand? *Answer*—Spectacles. (Describe them.) Eye-glasses. (I had Mr. Orme’s eye-glasses concealed in my hand.)

Expt. 2.—What do I hold in my hand? *Answer*—Piece of paper. (No.) A knife. (Describe it.) It is white. (Describe further.) It has a toothpick and button-hook. (Correct; it had other implements useful to a smoker.)

Expt. 3.—What do I hold in my hand? *Answer*—A ring. (Describe it.) Has a buckle on it. (Correct.)

“MISS MAUD CREERY.—

Expt. 1.—What town have we thought of? *Answer*—Buxton. (Correct.)

Expt. 2.—What town have we thought of? *Answer*—Derby. (What part did you first think of?) Railway station. (So did I. Next.) The market-place. (So did I.)

Expt. 3.—What town have we thought of? *Answer*—Something commencing with L. (Pause of a minute.) Lincoln. (Correct.)

Expt. 4.—What town have we thought of? *Answer*—Stockport. (Correct.)

Expt. 5.—What town have we thought of? *Answer*—Fairfield. (What part did you think of first?) The road to it. (So did I. What part next?) The triangular green behind the Bull’s Head Inn. (So did I.)

“JANE DEAN, *the Maid Servant.*—

Expt. 1.—What do I take hold of in my pocket? *Answer*—Spectacle-case. (Does it contain anything?) It’s empty. (Correct.)

Expt. 2.—What have I placed under the piano? *Answer*—A key. (What is it the key of?) A club. (One and a-half minute’s pause.) No. The key of the Asylum. (It was the key of the Asylum grounds. No one knew that I had a private key; I am not officially connected with the Asylum.)

- Expt. 3.*—What have we agreed to think of? *Answer*—A flower. (What is the name of the flower? Slight hesitation, then answered.) Lily of the valley. (No.) Immediately pointed to some flowers in Mr. Orme's coat. Snowdrop. (Correct.)
- Expt. 4.*—What have I in my hand? *Answer*—A pin. (What colour?) Black. (What shape?) Bending her index finger and thumb into the shape of the letter C, she said, "That shape." (Unknown to anyone I had bent it to that shape.)
- Expt. 5.*—What card have I selected? *Answer*—Seven of hearts. (No.) Eight of hearts. (Correct. Which way is the point of the heart directed?) Upwards. (Correct.)
- Expt. 6.*—What card have I selected? *Answer*—Nine of spades. (Correct. Which way is the point of the spade directed?) Downwards. (Correct.)

"No-one knew of the previous card except Mr. Orme. No-one knew of the second card except myself.

"FREDK. TURNER, M.R.C.S., Grafton House, Buxton.
"JOHN H. ORME, Solicitor, Buxton.

"July 14th, 1882."

APPENDIX

TO THE REPORT ON THOUGHT-READING.

By W. F. BARRETT F.R.S.E., M.R.I.A., &c., Professor of Physics in
the Royal College of Science for Ireland.

It has been urged by those whose opinion is entitled to the highest respect, that this inquiry, if undertaken at all, should have been left in the hands of physiologists, or physicians, who, having a wider knowledge of the numerous forms which hysteria assumes, would be more keenly alive to certain sources of error, such as deceit, which might escape an ordinary observer. This is quite possible, though I venture to think that the experience gained by several years' persistent, though unpublished investigation of obscure mental phenomena occurring in all sorts and conditions of people, is not without its value. Only by wide and searching inquiry of this kind can any conception be formed of the pitfalls which beset the inquirer, arising mainly either from unconscious, involuntary actions, or from the extraordinary capacity there is in human nature for deception, often seemingly innocent, at other times resorted to for the sake of gaining notoriety. I confess I do not know how our vigilance would be increased, or our results become more trustworthy, by ability to diagnose any particular case, when experience has taught us to exercise habitual caution in all cases. The inquiry must ultimately resolve itself into a question of evidence, and demands the exercise of the faculty of careful observation, which a physicist is as likely to possess as a physiologist.

My own connection with the subject arose in this way. Some fifteen years ago, whilst staying with a friend in the country, I saw certain mesmeric experiments made on the children of one of my friend's tenants, which interested me greatly, in spite of my strong scepticism. Among other things, I noticed what appeared to be a transmission of impressions from the mind of the mesmeriser to that of the subject, without, so far as I could detect, any intervening sense of perception. For example, the mesmerised subject being in one room and my friend in an adjoining one, and completely out of sight of the subject (even supposing the sleep were feigned), I—placing myself between the two rooms—noticed that every time a substance was silently tasted by my friend, a corresponding motion of the lips, expressive of enjoyment or distaste, occurred in the mesmerised subject. In repeating these and other experiments on the same subject, I obtained what appeared

to me indubitable evidence of a reproduction in the subject's mind of any vivid sensation or idea that occurred in my own mind. I was so much impressed with these experiments, and others made at my own house subsequently, that I ventured to bring the matter before the British Association, at the meeting in Glasgow in 1876, in the hope that a committee of inquiry would be appointed. The discussion on this paper drifted, however, into other channels, and in a letter published in the *Times* in September, 1876, I wrote as follows :—

“I am inclined to believe that other mental phenomena—such, for example, as the influence of one mind upon another across space, without the intervention of the senses—demand a prior investigation. That cases of such mental action at a distance do really exist I, in common with others, have some reason to believe : but before they can be generally accepted, the evidence must accumulate and be thoroughly sifted. I hope that some one more competent, and having more leisure than myself, will ultimately take up this question ; meanwhile, I shall be glad to receive communications from anyone who can furnish me with trustworthy evidence on two points—of cases of the direct action of one mind upon another giving rise to an apparent transfusion of thought or feeling, occurring either in abnormal conditions produced by illness or ‘mesmeric trance’ ; or of cases where, under normal conditions, perception may seem to occur independent of the ordinary channels of sensation. I must beg those who kindly send me such cases to take great care lest sources of error be produced from unconscious muscular signs on the part of the observer, or from the keen ‘muscular sense’ and the general exaltation of the other senses, which, in any morbid condition, are likely to exist on the part of the subject. Whether careful inquiry will prove that every case can be referred to already known physiological laws or not, remains to be seen ; but many friends, to whose opinion I attach much weight, agree with me in thinking that such an inquiry should be made.”

As this letter was copied into other journals, and was followed by a similar request made by me in the columns of the *Athenæum* and elsewhere, I received a very large number of replies. The examination, sifting, and personal investigation of the best cases, has been a work of considerable labour, and has occupied a good deal of my time for the last five years.

Many of the cases were readily explicable on the theory of muscle-reading, others were of more interest and led to correspondence and personal interviews, in which I was joined by Mr. F. W. H. Myers and afterwards by Mr. Edmund Gurney, both of whom had been pursuing, in connection with our President, kindred inquiries for some time past. The later results of our investigation are contained in the foregoing report, but it may be useful to put on record some of our earlier experiences as affording typical illustrations of the debatable

border land between the discernment of muscular impressions and the supersensuous perception of an unexpressed wish.

The first case is a sample of the ordinary willing game that came under my notice in Easter, 1877.

Expt. 1.—The subject in this case was a young medical man, and the friends present were mostly medical men, sceptical of the operation of any agency beyond involuntary muscular action. The experiments were made in the house of a distinguished surgeon, Mr. Lawson Tait. A paperknife was placed by myself on the top of a folding screen during the subject's absence from the room; on recalling him two friends clasped hands round the subject's waist; he then closed his eyes, walked irresolutely to the spot, and took off the paperknife, placing it on the table. Here involuntary guidance to the spot may be assumed, but it is difficult to understand what should have made him lift up his hands suddenly and feel for an object out of sight. No indication of what was to be found was given beforehand.

Expt. 2.—The same subject again left the room, one of the number ascertaining that he was quite beyond eye or ear shot. This time we willed that he should move the fire-screen and double it back. On re-entering, my host, the surgeon, clasped him as before, and after a few moments of indecision, he went towards the spot and did as we had wished.

Expt. 3.—This time we fixed that the subject should turn out the gas of a particular bracket, one of several round the room. Loosely held round the waist, the subject in a few minutes went to the spot, lifted up his hands and turned off the gas.

These three experiments are of interest, inasmuch as in each one the hands had to be lifted up, muscles being used distant from the part in contact with the willers. Similar results were obtained in July, 1877, with Miss R. as the subject; one example will suffice.

Expt. 4.—During the absence of the subject, it was agreed that a mark should be made with a pencil round a sixpence, which happened to be lying near a sheet of paper on the table before the subject left the room. In this case the hands of the willers were placed round Miss R.'s neck, and the action fixed upon silently willed. In a few moments Miss R. walked to the table, took up a pencil, and deliberately made a mark round the sixpence.

A long series of experiments extending over several days in May, 1879, were made by me with another subject. In this case, the sister of the lady seemed to have the most power over her. Among numerous trials that were made, the following may be quoted.

Expt. 5.—In her absence, the subject was willed to take up a little agate jewel box, standing with some twenty other small objects on a shelf, put it inside a certain covered jar in another part of the room, reopen the jar, remove the ornament and hand it to one of the friends present. This was done swiftly and correctly to the smallest detail.

Expt. 6.—Selected notes on the piano were four times in succession correctly struck. Here, and in Expt. 5, the hands gently touched the head. In some of the next experiments the hands did not actually touch.

Expt. 7.—Certain books in a book-case (containing some 100 volumes) were chosen by me in the absence of the subject. In six consecutive trials the right book was taken down.

Out of a total of 130 trials with this subject, of which the foregoing are fair samples, about 100 were correctly performed. Instead of giving the details of all these experiments I may be permitted to summarise them by saying that while in ~~very many~~ ^{some} cases the muscular sense might have been a sufficient explanation, there were many others very carefully tested, which could not easily be so explained, and which pointed in the direction of something new, for example, as mind-reading, as their only satisfactory explanation. In fact, the intervention of a second person, who was entirely ignorant of what had to be done, between the willer and the subject, the hands of each resting on the shoulders of the one in front, did not seriously interfere with the results obtained. Under such conditions difficult things were correctly done, involving complicated muscular actions, whilst we failed to do similar, and even much simpler, things under the influence of deliberate conscious guidance.

Besides these cases we have received evidence of similar performances in private families in different parts of England—at Southampton, Southport, Cirencester, Yarmouth, Cork, Edinburgh, Glasgow, Norwich, &c.; in all these cases we are greatly indebted to our informants, to whom we have given considerable trouble in correspondence; but none of these cases were of such a nature as to justify a personal visit, and moreover the hypothesis of muscle-reading might, *primâ jactâ*, be taken to account for many of them. Two cases, however, one in London and

one on the south coast, seemed deserving of more careful inquiry. In these, as in all the other cases recorded, the subjects freely placed themselves in our hands, a kindness we desire gratefully to acknowledge, regretting the unrequited trouble we have given them.

The case in London, that of Miss C., has been investigated by each of the members of the Committee on Thought-reading. Here is the record of four typical experiments made by Mr. Myers, on November 30th and December 7th, 1877.

“The mother of the young lady placed three of her fingers, not including the thumb, on the back of Miss C.’s head, the fingers resting apparently quite lightly.

Expt. 1.—I drew on a piece of paper a rough sketch of a house and shewed the sketch to Mrs. C. Miss C.’s head was averted the whole time; no look was interchanged between her and Mrs. C., no other part of their persons was in contact. No one but Mrs. C. saw the drawing. I watched Mrs. C.’s fingers closely in full gaslight; they seemed to rest lightly on Miss C.’s head; no signals perceptible. The drawing was rudely reproduced, as though by a person drawing in the dark; one of the windows being drawn outside the outline of the house.

Expt. 2.—I wrote a sentence and shewed it to Mrs. C., taking care that Miss C. should not see it. Miss C. then wrote it under the same conditions as above. I chose sentences in foreign languages, that guidance might be less easy.

Tu regere imperia.

Se deſe prender.

These were correctly written.

Expt. 3.—Miss C. then pushed up her sleeve. Mrs. C. placed three fingers on Miss C.’s arm above the elbow, and in like manner Miss C. wrote (without having previously seen the words):—

Palma.

This man.

Expt. 4.—The Greek words *μεθυ* and *αμαξ* were then written under the same conditions. They were very rudely written, but each letter was distinguishable.”

Notwithstanding these surprising results, we were convinced that, granting the hypothesis of involuntary muscular action, and of

extreme sensibility on the part of the subject, the probably unconscious and certainly undiscernible movements of the touching fingers might possibly serve to convey a sufficient guidance to the girl's delicate skin and responsive organisation, even though she might be unaware of her own response.

The other somewhat similar case that reached me was on the south coast, and here also Mr. Myers visited the family and reported as follows :—

“NOTES OF EXPERIMENTS WITH THE MISSES B., OCTOBER 31st, 1877.

Miss M. B., henceforward called M.

Miss R. B., ” ” R.

I put my hand on M.'s shoulders. I thought of what I wished her to do, and told nobody except in Expts. 5, 6, 15, and 16.

Expt. 1.—I wished her to take a very small ornament from the chimney-piece—a little china cat an inch high. As soon as my hands were on her shoulders she rushed to the chimney-piece, so quickly that I had difficulty in keeping my hands on her, and instantly picked up the cat, which was inconspicuously placed among many ornaments.

Expt. 2 & 3.—Two failures followed; she said she felt strong but confused influence.

Expt. 4.—I wished her to go to a book of photographs—one of several in the room—open it, and pause at a certain photograph. She rushed quickly to the book and opened it, but became confused.

Expt. 5.—Mr. B. took one end of a stick and M. the other. M. took a strap from a table and gave it to a lady at some distance; the test agreed on while M. was out of the room.

Expt. 6.—A thread was substituted for the stick. M. moved an object previously agreed on—an umbrella in corner of room; but this time after a good deal of hesitation and fumbling.

Expt. 7.—I put my hands on R.'s shoulders and willed her to pick up and eat a biscuit from a plate in corner of room. She at once picked up a biscuit but did not eat it.

Expt. 8.—I willed her to shake hands with her mother. She rushed to her mother and stroked her hands.

- Expt. 9.*—I willed her to pick up grape from bunch. She rushed to grapes and picked a few up.
- Expt. 10.*—I willed her to pick up a hat in distant part of room. The instant my hands touched her she turned sharply round, rushed to the hat, and picked it up.
- Expt. 11.*—A similar wish failed.
- Expt. 12.*—I willed her to nod. She stood still and bent her head.
- Expt. 13.*—I willed her to clap her hands. She did nothing.
- Expt. 14.*—I willed her to strike on the piano tenth note from right hand end. She did so after a few seconds' fumbling. As I had opened the piano she might guess I wished her to go to the piano, but she could not surmise the right note to strike.
- Expt. 15.*—Eight persons present contributed trifling articles—a half-crown, two pencil-cases, small knife, key, handkerchief, two small purses. These were put in the pocket of a lady present, while R. was out of room. R. re-entered room; M. touched her shoulders. R. rushed to the lady who had the objects, pulled them out one by one and with shut eyes gave each to its owner—M. withdrawing her hands during part of the process, which was extremely rapid. R. said she did not know to whom she was giving the things; had no sense of connection between the things and the people—merely an impulse to move first one way and then another.
- Expt. 16.*—I wrote the letters of the alphabet on scraps of paper. I then thought of the word CLARA and shewed it to M. behind R.'s back, R. sitting at the table. M. put her hands on R.'s shoulders, and R. with shut eyes picked out the letters C L A R V—taking the V apparently for a second A, which was not in the pack—and laid them in a heap. She did not know, she said, what letters she had selected. No impulse had consciously passed through her mind, only she had felt her hands impelled to pick up certain bits of paper.

“This was a good case as *apparently* excluding *pushing*. The scraps were in a confused heap in front of R., who kept still further confusing

them, picking them up and letting them drop with great rapidity. M.'s hands remained apparently motionless on R.'s shoulders, and one can hardly conceive that indications could be given by *pressure*, from the rapid and snatching manner in which R. collected the right letters, touching several letters in the course of a second. M., however, told me that it was always necessary that she, M., should *see* the letters which R. was to pick up.

"Mr. B. said that M. used at one time to write automatically the thoughts of persons sitting near her—though quite unconscious of what those thoughts were—the hand being moved without any perceptible influence on the brain.

"NOVEMBER 1ST, 1877.—On a second visit similar phenomena occurred, with one new and instructive experiment, viz.,

Expt. 17.—M. held one end of a stick and R. the other. I shewed M. certain words which I thought of, behind R.'s back; R. then picked out letters, with the hand which was not holding the stick, from a confused pile. She made the words correctly. *When a thread was substituted for the stick she failed to do so.*"

Other experiments were subsequently made with this family by two members of our Committee. But, marvellous as were some of the things done, nevertheless had we no other case than this to rely upon, I do not think we should be justified in calling in the aid of any new hypothesis to explain the phenomena: in fact, the last experiment shews that in some cases true thought-reading certainly was *not* the cause of the success attained. I may here observe that our President and Mrs. Sidgwick, who made somewhat similar experiments with two other ladies, arrived at the conclusion that all the results witnessed by them personally were capable of explanation by the hypothesis of unconscious perception of unconscious muscular indications. Mrs. Sidgwick writes: "They certainly did very wonderful things, but they did not succeed in any, even very simple, experiments which appeared completely to exclude the muscular hypothesis, except after several attempts. My brother and I both found that with the hands of one of the sisters on our shoulders, we could succeed in doing things fairly well, though slowly; not, however, by feeling any impulse to do anything, but by concentrating our minds on the hands, and trying to make out from them whether their owner was satisfied or dissatisfied. In this way he succeeded, *e.g.*, in selecting the desired card from a number on a table. We found that the close attention necessary for success was assisted by closing the eyes. I should add that I discussed the theory of unconscious muscular action with the Misses X. but.

they did not think it would account for either their own sensations or some experiments they had succeeded with in their own family circle."

On the other hand it must be admitted that some of the results obtained by Mr. Myers would be far more easily explained by thought-reading, if that were once recognised as a *vera causa*, and the following prior experiments with the same ladies, sent to me by an eye-witness—whose integrity I have no reason to doubt—seem quite beyond any power of muscle-reading.

"SEPTEMBER, 1876.

Expt. 1.—Miss B. seated at the table, with her eyes bandaged, and a pencil in her hand. I stood *behind* her; no word was spoken. I took my spectacles and held them in my hand; she wrote 'Spectacles'; then my dog-whistle; after this a key; then a pencil; all these she wrote down correctly

Expt. 2.—The same young lady, M. B., seated at the table with her eyes bandaged, pencil in hand. Her uncle, standing about twelve feet distance, asked, 'What word am I thinking of?' M.B. wrote 'Homo.' This was right.

Expt. 3.—My daughter, who had recently returned from a visit to her brother at his vicarage, asked M. B. (who was again seated with eyes bandaged and pencil in hand), 'Who preached at my brother's church last Sunday evening?' the answer to the question being known to my daughter *only*. M. B. wrote the first six letters of the name, viz, "Westmo—" and then said, 'I feel no more influence.' My daughter said, 'Lean your head against me.' M. B. did so, and then wrote the rest of the name, making it quite right—'Westmore.'

Expt. 4.—My daughter then asked her the following questions: 'What is the name of the hotel I was staying at in Paris last month?' This was answered correctly. 'The name of the opera I heard?' Also answered correctly."

Since the publication of our article on Thought-reading in the *Nineteenth Century* for June last, a friend has sent me the report of some experiments tried in their own circle, both with and without contact of any kind. Amongst the latter may be mentioned the following:—

Expt.—"Mrs. H. was the subject. We tried the names of four towns. *Edinburgh* was the first. Mrs. H. guessed *Edinburgh* the first trial. *Dover* was next fixed on. Mrs. H. said *Plymouth* first, and then *Dover*. *Canterbury* was next chosen. Mrs. H. said *Chester*, *Lancaster*, *Manchester*. *Brighton* was then fixed upon. Mrs. H. said *Birmingham*. In these two failures it will be noticed there is a slight resemblance in the words wrongly named."

Experiments of this kind, however, even if not merely fortuitous in their results, have little scientific value unless one knows all the conditions under which they are performed; their interest consists in the indication they afford of a widening of the area of experiment, which is an important step.

It was after seeing the "willing game" in a friend's house that the Rev. A. M. Creery, of Buxton, was led to try his own children for an evening's amusement. At first entirely sceptical of obtaining any results without contact, he was astonished at the success he soon obtained under these conditions, whereupon, in January, 1881, he wrote to me, having read in the papers my letter requesting information on this subject. It will be needless, in view of our own observations, to detail all the early experiments Mr. Creery tried at my request. One or two experiments, however, are of interest, as they are of a somewhat different nature.

MARCH 21ST, 1881.—Present: Mr. and Mrs. Creery and family, and two friends, Mr. and Mrs. F., who add their testimony to the accuracy of the report.

Fifteen experiments were made in the naming of objects or of English towns selected in the child's absence; there were only three complete failures. Short sentences were now fixed upon, namely: "What time is it?" "Will you have some supper?" "Will you go to bed?" "Were you at the sale to-day?" In each case the sentence was correctly named by the subject on her return to the room; every care being taken that no information could be derived through the ordinary channels of sensation. In these experiments the time that elapsed from the moment the subject entered the room to the utterance of the word selected, was found to vary from a few seconds to two minutes; in one case four minutes elapsed. In another series of experiments Mr. Creery tried placing the subject at various distances from the "willers," and obtained successful results in the naming of cards even when the subject was placed in a bedroom upstairs whilst the willers remained in a room downstairs which was not under the bedroom.

So far as the children's testimony, and our own and their parents'

observation went, no injurious effect to the health has accompanied these experiments; the children immediately afterwards engage in play with the same zest as if they had not been interrupted by our trials. But it is right to state that several correspondents have mentioned that trials with the willing game produce dizziness and sometimes hysteria and incipient trance in the subject of experiment.* This is confirmed by a letter from the Rev. G. Henslow, published in *Nature* for June 23rd, 1881; and by a recent letter in *Knowledge*, the editor of which has devoted several articles to the exposition of our experiments on thought-reading, and has stated his own belief in the direct action of mind upon mind.

Interesting and able articles on thought-reading have also recently appeared in the *Spectator*, together with several letters on the subject.

* The following letter from a correspondent, whose name and address I am not at liberty to publish, bears upon this point:—"The one to be 'willed' would go to the other end of the house, if desired, whilst we agreed upon the thing to be done, and before re-entering the room, the child was always effectually blindfolded. Then amidst total silence two of us would place our fingers lightly on the child's waist, when after a moment's pause, he or she would suddenly dart off towards the object of the 'willing,' passing round the various articles of furniture, as if seeing them, without ever disturbing them, and often so rapidly that we could not keep up with him, and therefore detaching himself from our touch. We used to will the children to do the most unlikely things, such as to take a shoe off one person's foot, and put it on another's head; or to find something hidden up, and then present it on their knees to one of the company, &c., &c. In fact they were such adepts at 'the game,' that the more outlandish the 'willing' was, the more they liked it, and they were rarely, if ever, unsuccessful; the feat was usually performed very quickly, and when unbandaged they were amazed at what they had done and would laugh heartily over it. The children stated that they had no idea of what they were doing, but felt, as it were, a blind force compelling them to certain aimless actions. Now I come to what seems to me a most important feature in 'the game,' and which is the true cause of my troubling you with this letter, viz., that it always tired the children very much, even to making the girls sometimes hysterical. Indeed we found it so exhausting to them (sometimes also giving them a queer and *égaré* look afterwards), that we at last forbade it altogether, except on very rare occasions, to shew the curious phenomena to some special friends. I think, therefore, the fact of this exhaustion in strong, healthy children, from about twelve to sixteen years of age, after a performance that never lasted more than two or three minutes *at most*, and was never allowed to be attempted by the same child more than twice in the same evening, goes far to prove that the abnormal powers displayed by them were not the effect of mere muscular action, either voluntary or involuntary, upon the children, and also that those abnormal powers were genuine. I should add that the most rigid silence was enjoined by the children, and adhered to, and that they were always particular to be *thoroughly* blindfolded—as the exercise in *any* degree of their normal senses of hearing and sight, seemed to interfere with the abnormal senses induced by being 'willed.'" Another correspondent writes:—"The doctor has forbidden my daughter trying these experiments again. The last time she attempted them she went off into violent hysterics, ending with a dead faint." These pathological facts deserve careful inquiry.

The term *will-impression*, rather than thought-reading, is proposed by one correspondent in the *Spectator*, and with much justice; the committee have accepted the ordinary phraseology simply because it has come into general use. Among the letters in the *Spectator* the following may be cited:—

“I had one day been spending the morning in shopping, and returned by train just in time to sit down with my children to our early family dinner. My youngest child—a sensitive, quick-witted, little maiden of two years and six weeks old—was one of the circle. Dinner had just commenced, when I suddenly recollected an incident in my morning’s experience which I had intended to tell her, and I looked at the child with the full intention of saying, ‘Mother saw a big, black dog in a shop, with curly hair,’ catching her eyes in mine, as I paused an instant before speaking. Just then something called off my attention, and the sentence was not uttered. What was my amazement, about two minutes afterwards, to hear my little lady announce, ‘Mother saw a big dog in a shop.’ I gasped. ‘Yes, I did!’ I answered; ‘but how did you know?’ ‘With funny hair?’ she added, quite calmly, and ignoring my question. ‘What colour was it, Evelyn?’ said one of her elder brothers; ‘was it black?’ She said, ‘Yes.’

“Now, it was simply impossible that she could have received any hint of the incident verbally. I had had no friend with me when I had seen the dog. All the children had been at home, in our house in the country, four miles from the town; I had returned, as I said, just in time for the children’s dinner, and I had not even remembered the circumstance until the moment when I fixed my eyes upon my little daughter’s. We have had in our family circle numerous examples of spiritual or mental insight or foresight; but this, I think, is decidedly the most remarkable that has ever come under my notice.—I am, Sir, &c.

“CAROLINE BARBER.

“Ferndene, Abbeydale, near Sheffield, June 22nd.”

To the same journal the Suffragan Bishop of Bedford, Dr. Walsham How, contributes a remarkable case of perception at a distance, which came under the observation of his father. In this case the whole details of a distant *scene* were perceived in a dream and with a minuteness that seemed to exclude any mere freak of the imagination on the part of the seer. Cases of this kind, and of “presence at a distance,” such as the curious incident related by Dr. J. H. Gilbert, F.R.S.,* may be said to adjoin the scope of the present inquiry and they deserve and will receive separate investigation.

* See *Spectator* for September 2nd and 23rd, 1882.

Several cases have come under my notice lately, of an accident or a wound in battle (*e.g.*, one in the Zulu war, and one in the present campaign in Egypt), occurring to some individual, and at the same instant a distant friend has received an intimation of the occurrence, very much as if a nervous thrill had passed through the intervening space, awakening a response in a sympathetic mind. These cases fall under the *fourth group of phenomena* mentioned in this report, and two typical illustrations are given on pages 30 and 31. I am indebted to Mrs. G. Bidder for the following additional evidence under this head:—"A connection of mine was staying with a friend whose husband was engaged in making a line of railway in Spain. My friend was roused one night by her hostess, who was in a terrible fright, and said she was certain her husband was killed in a railway accident. She had been wakened with a start, and then had either seen the occurrence or been told in some way, but how, she could not remember. My friend reminded her that the railway he was engaged on did not open till the next day, so that the accident was unlikely. It turned out, however, that her husband had been doubtful of the safety of one part of the line, and had insisted on running an engine over it in the night, to try it for the next day's opening, and he had been killed."

In the memoir of the late Bishop Wilberforce, a similar transmission of an impression is recorded in the following words:—"The Bishop was in his library at Cuddesdon, with three or four of his clergy writing with him at the same table. The Bishop suddenly raised his hand to his head, and exclaimed: 'I am certain that something has happened to one of my sons.' It afterwards transpired that just at that time his eldest son, who was at sea, had had his foot badly crushed by an accident on board his ship." The Bishop himself records this circumstance in a letter written at the time, and dated March 4th, 1847. "It is curious," the Bishop writes, "that at the time of his accident, I was so possessed with the depressing consciousness of some evil having befallen my son Herbert, that at last on the third day after, the 13th, I wrote down that I was quite unable to shake off the impression that something had happened to him, and noted this down for remembrance."

Dr. Wilton, of Sutton, Surrey, is my authority for the following case:—

"A patient of mine, Mr. J. T——, a solicitor, about sixty years of age, lived a short distance out of London, with his family, consisting of a wife and step-daughter, Miss W——. One December he was asked to go to Edinburgh, to arbitrate in some matter of business. Accordingly he left London, expecting to be away nearly a week.

"In the early morning of the third day after his departure, Mrs. T—— awoke, and was surprised to find her husband, as she thought,

standing by her bedside. She exclaimed, 'How did you get in without my hearing you? Wait while I light the candle.' She struck a match, and was very astonished at not seeing her husband in the room. While she was thinking over this singularly vivid delusion, her step-daughter, who occupied an adjoining room, knocked at the door, and on being admitted, said, 'Oh! mother, I have had a horrible dream about father, and cannot sleep; I am afraid something has happened to him.' In the morning they both told their stories to their maid, and subsequently to a gentleman who called while they were at breakfast. In the course of the forenoon a telegram arrived from Mr. T——, saying there had been an accident to the train in which he had been a passenger, that he was not hurt, and would be home in the course of the day.

"It appears that he had arranged his business much quicker than he had expected, and was able to leave Edinburgh by the night train; a collision took place a few miles from London, owing to a thick fog, and about the time when the two ladies were disturbed by their dreams. There was no doubt whatever of the truth of this strange coincidence, the ladies having told their dreams long before the arrival of the telegram. I attended the family many years, and although Mr. T—— did not appear to have sustained injury at the time, he never recovered from the nervous shock."

The *Spectator* publishes the following:—

"My eldest brother went to New Zealand. One morning my sister Emily came down to breakfast, looking very white and queer, and directly she entered the room, said,—'Ben has met with an accident.' Disregarding our incredulous amusement, she declared she had seen him with his arm bandaged up, lying in a room where there were other beds. We were longer than usual in hearing from my brother; he explained the delay, saying his arm had been broken, and that he had been for some time in the hospital. Comparing dates, we found he was injured the day my sister had her vision.—I am, Sir,

"ANTHONY ASHLEY."

"3, Buxton Villas, Stratford, August 7th."

Other cases are doubtless known to many who read this, for a multitude of similar stories are in existence. Hitherto, as these facts arose, the general explanation has been coincidence. It has been said, "How many thousands of accidents occur, and no knowledge of them has been conveyed to others, except through the ordinary means; but when, by a fortuitous circumstance or a natural foreboding, some friend fancies an accident has occurred, and it turns out more or less as imagined, then such coincidences are talked about as if they were representative, or indicative of a law, whereas they are really nothing more than chance shots." This would be a legitimate argument if the

cases were excessively rare, and so far as our knowledge of the facts extends at present, we are not in a position to do more than assert that enough well-authenticated cases are on record to render explanation by coincidence difficult to entertain with any degree of confidence. When to this is added the facts detailed in our report—from which I venture to think the only fair conclusion is that some mode of supersensuous perception not improbably exists—then it seems to me unphilosophical to reject, as unworthy of serious examination, all stories such as those just narrated. On the other hand—reiterating what has been emphatically stated already—wide generalisations are altogether premature. Our object here, as elsewhere, is simply to collect, collate, and weigh the facts, using, if need be, as a working hypothesis, the conclusions drawn from our Buxton experiments.

Nor must we forget that other workers have been in this or an adjoining field. A list of well-known names might easily be compiled who have testified from critical observation that during the mesmeric sleep the mind of the mesmeriser can influence that of the subject, independently of the ordinary channels of sensation.

The late Dr. Bush, a distinguished scholar, and Professor in the University of New York, writes:—"I know that the conceptions of my own mind have been reproduced in another mind without any outward signs, and I know that I have not been deceived as to the facts averred."

Dr. Mayo, F.R.S., who was Professor of Physiology and Anatomy in King's College, London, and the author of an important treatise on "The Nervous System and its Functions," gives similar testimony. In connection with this subject, he remarks:—"A number of incidents are frequently turning up, for the most part on trivial occasions, which we put aside for fear of being thought superstitious, because as yet a natural solution is not at hand for them. Sympathy in general, the spread of panic fears, the simultaneous occurrence of the same thoughts to two persons, the intuitive knowledge of mankind possessed by some, the magnetic fascination of others, may eventually be found to have to do with a special and unsuspected cause."

The principle underlying these occurrences Dr. Mayo believes to be the same that is found in a more striking form in mesmeric phenomena. Of the singular relationship that exists between the minds of the mesmeriser and his subject, producing an apparent community of thought and sensation, Dr. Mayo gives experimental evidence precisely similar to what has come under my own observation, and in conclusion he states:—"I hold that the mind of a living person in its most normal state, is always, to a certain extent, acting exoneurally, or beyond the limits of the bodily person." He remarks that:—"It will be said the cases, in which I suppose this power manifested, are of too trivial a

nature to justify so novel a hypothesis. My answer is, the cases are few and trivial only because the subject has not been attended to. For how many centuries were the laws of electricity pre-indicated by the single fact that a piece of amber, when rubbed, would attract light bodies. Again, the school of physiological materialists will, of course, be opposed to it. They hold that the mind is but a function or product of the brain.*

As we have stated in the Report, we have been anxious to accumulate and sift experimental evidence as to the facts before us, rather than to indulge in theories as to the cause of the phenomena. We may, however, conceive of nervous energy acting by induction across space as well as by conduction along the nerve fibres. In fact, the numerous analogies between electricity and nervous stimuli would lead to some such inference as the above. Or the brain might be regarded as the seat of radiant energy like a glowing or a sounding body. In this case, the reception of the energy would depend upon a possibility of synchronous vibration in the absorbing body; which, moreover, may be constituted like a sensitive flame, in a state of unstable equilibrium, so that a distant mental disturbance might suddenly and profoundly agitate particular minds, whilst others might remain quiescent. Further, we may conceive that, just as a vibrating tuning fork or string spends its energy most swiftly when it is exciting another similar fork or string in unison with itself, so the activity of the brain may be more speedily exhausted by the presence of other brains capable of sympathetic vibration with itself.

But speculations such as these are merely of use in suggesting lines of experiment. For my own part, I have little doubt that a wider and more exact knowledge of psychological phenomena will shew the insufficiency of any physical analogy or materialistic explanation, and thus should tend to accelerate the passage of the existing wave of materialism, the crest of which, there seems reason to believe, has already gone over us.

POSTSCRIPT.

The following extract from a letter in the *Sussex Daily News* is of some interest in connection with the mode of experiment pursued by Mr. Bishop. The subject of Thought-reading having been discussed, "It was proposed that we should attempt the experiment. Accordingly I was blindfolded and left the room. Whilst I was absent a reel of black cotton was secreted in a flower-pot near the window. On pressing the hand of the gentleman who had secreted it against my forehead, and requesting him to think of the object he had hidden, I saw plainly with my blindfolded eyes, as though in a dream, the figure of a reel of black

* "Truths contained in Popular Superstitions," p. 68 et seq.

cotton floating before me. I then told him to think of where he had hidden it, and I saw and led him to a bureau at the opposite end of the room to the window. This he said was wrong, but on inquiry I found that he had originally intended to have placed it there, but had altered his mind. We then tried the question of localising a pain. Being blindfolded, and holding my friend's left hand against my forehead, I told him to imagine a pain. Almost immediately I felt a peculiar, indescribable sensation on the right side of my face, and told him that he was thinking of a pain there. He was, in fact, imagining a violent attack of neuralgia in the right upper jaw. Other experiments were tried and have been tried since, some successful, some unsuccessful, but I have seen quite enough to convince me that there is truth in it. I don't pretend to offer a reason, but I would say to those who disbelieve it, 'Try for yourselves.' All do not possess the power. I was the only one of a party of six or seven who was thus affected, but, doubtless, there are very many who could perform precisely the same experiments, and by continued inquiry it may be that the mystery will be solved.—I am yours, &c.,

"HENRY EDMONDS, B.Sc. (London)."

"Brighton School of Science and Art."

The following extract from a letter published in *Light* shews that a Mr. Smith, of Brighton, has powers analogous to those claimed by Mr. Bishop:—"The way Mr. Smith conducts his experiment is this: He places himself *en rapport* with myself by taking my hands; and a strong concentration of will and mental vision on my part has enabled him to read my thoughts with an accuracy that approaches the miraculous. Not only can he, with slight hesitation, read numbers, words, and even whole sentences which I alone have seen, but the sympathy between us has been developed to such a degree that he rarely fails to experience the taste of any liquid or solid I choose to imagine. He has named, described, or discovered small articles he has never seen when they have been concealed by me in the most unusual places, and on two occasions he has successfully described portions of a scene which I either imagined or actually saw.

"DOUGLAS BLACKBURN, Editor of *Brightonian*."

"24, Duke-street, Brighton."

Mr. Bishop has lately been good enough to give me an opportunity of trying his powers. In the first instance, by means of a confederate, he shewed the wonderful perfection to which he has carried fictitious thought-reading, indicating objects, names, or figures by means of a pre-arranged code. Thus his confederate, who was seated back to us and blindfolded in another part of the room, told us—in answer to a succession of seemingly casual questions on the part of

Mr. Bishop—the whole particulars of a Bank of England cheque which a friend of mine happened to have in his pocket, the nature, number, and date of the cheque, the person in whose favour it was drawn, the person who drew the cheque, and the bankers in whose favour it was crossed, Mr. Bishop of course looking at the cheque from time to time. Mr. Bishop then shewed what he believes to be the genuine power of thought-reading that he possesses. Some striking things were done; for example he wrote with his finger on the door certain figures corresponding to those that I had put on paper and was thinking of but had shewn to no one else, his hand pressing mine tightly against his forehead. He also twice discovered the exact locality of a pain that I fixed upon, in one case with extreme accuracy, but he did not succeed so well with a friend; in this case also he pressed the hand of the subject against his own forehead. He next tried some experiments without contact, his hand being held very near mine; in this way he moved backwards and forwards across the room in the precise direction that I had previously, in his absence, written down; other experiments without contact were not quite so successful. It is, however, very difficult to arrive at any satisfactory conclusions from these experiments, owing to the extraordinary pantomime and wriggling excited action which Mr. Bishop invariably employs, so utterly different from the silent, passive condition to which we have been accustomed in the experiments recorded in our report.