

## IV.

"THE REALITY OF PSYCHIC PHENOMENA."<sup>1</sup>

By W. WHATELY SMITH.

## I.

IT is probable that no branch of Psychical Research has proved more uniformly disappointing to investigators than the study of what are commonly known as "physical phenomena." The history of such investigations forms an almost unbroken record of fraud and malobservation, of initial plausibility and subsequent exposure. Again and again cases have been reported of mediums who could produce the most striking phenomena "under the strictest test conditions," and again and again it has been found on further investigation that insufficient precautions had been taken to exclude trickery. Even the cases in which no fraud has been found have usually borne so close a resemblance to others in which it has that the cautious student, arguing by analogy, rightly hesitates to attach any great importance to them. Moreover, as we learn more of spurious methods, so the standard we demand becomes increasingly strict, and for this reason we are obliged still further to discount some of the older records.

In face of all this it looks rather like asking for trouble to profess a strong opinion in favour of any phenomena of this class.

None the less I believe that "The Reality of Psychic Phenomena" is likely to become a classic of the subject, for, as I hope to show, the experiments described therein present features not to be found in any similar research.

<sup>1</sup> *The Reality of Psychic Phenomena.* By W. J. Crawford, D.Sc. John M. Watkins. London, 1916. 4s. 6d. net.

I need hardly say that I do not proffer this opinion in a spirit of dogmatic assertion. I am well aware that a single sitting, however satisfactory, is not sufficient evidence for the formation of *final* judgments, especially on so precarious a subject. But, in the last analysis *all* sound judgments are provisional; even our so-called "certainties" are never more than assessments of probabilities at a fraction closely approaching unity, and I think that on the most conservative estimate we can fairly say that Dr. Crawford's work has raised "physical" phenomena to a position very near indeed to the top of the "credibility scale."

It is true that results of similar importance would not be unreservedly accepted, by Physicists for example, without thorough confirmation by independent investigators, and, in spite of the rarity of good cases of this kind, it is especially necessary in Psychological Research to relax in no smallest degree the most rigorous canons of Scientific Caution. Still, I see no reason for professing doubts which I do not feel or inventing criticisms merely for the sake of appearing judicious, so I may as well state outright that I quite agree with Dr. Crawford when he says: "To all visitors the phenomena are so manifestly and palpably genuine that they are never troubled again with doubts as to whether there is such a thing as psychic force."

## II.

The experiments dealt with cover some three years' work concentrated almost exclusively on the two phenomena of "raps" and "levitation without contact."

The salient features of the results, all of which were obtained through the remarkable mediumship of Miss Kathleen Goligher of Belfast, are as follows:

### A. LEVITATION.

(i) Levitation without contact of any kind has been obtained on numberless occasions and with the utmost regularity. It is by no means capricious or erratic in

its occurrence, but can almost invariably be produced on demand.

(ii) During levitation the weight of the medium is increased by an amount practically equal to that of the table.

(iii) There is, normally, no pressure on the floor vertically under the table during levitation.

(iv) If a balance-supported platform is inserted under the table, a downward pressure is exerted on it during levitation, provided that its height above the floor exceeds 3-4 inches. This pressure increases with the height of the platform above the floor and may be considerably in excess of the weight of the table.

(v) There appears to be a definite connection between the medium and the table possessing all the usual characteristics of a rigid structure except visibility and palpability. It is approximately of V form, one limb supporting the table while the other is attached in some fashion to the body of the medium.

(vi) A thin rod may be passed freely across the space occupied by this structure without producing any effect, but the interposition of a large object appears to interfere with the structure and causes the table to drop.

(vii) This structure can resist or transmit torsional, compressional, tensional or shearing stresses of considerable magnitude.

#### B. RAPS.

(i) Raps of every degree of loudness up to "sledge-hammer blows" have been obtained in great abundance.

(ii) They cannot be produced unless the medium's weight is first reduced. The usual reduction is about 8 lbs.

(iii) The intensity of the rap is apparently directly proportional to the decrease in weight of the medium.

(iv) The loss of weight is only temporary.

(v) The loss of weight is not effected suddenly but, on the contrary, quite gradually.

(vi) After a time (for a given experiment) the loss reaches a final amount and thereafter does not vary.

(vii) Any rap or blow produces a momentary reaction on the medium.

## C. GENERAL.

(i) The phenomena are under the control of some kind of intelligence which seems to co-operate actively with the experimenter.

(ii) The production of phenomena is apparently accompanied by the removal of actual matter, in some form or other, from the body of the medium, and this process seems to be an indispensable preliminary to their occurrence.

(iii) These experiments have all been performed in a good red light—not in darkness.

(iv) Copious *quantitative* measurements have been made of the forces involved.

The foregoing is no more than a bald *résumé* of results and inferences which lack of space prevents me from giving *in extenso*. For the full details of the actual experiments and reasoning reference should be made to Dr. Crawford's book.

It is particularly fortunate that the study of these very interesting phenomena should have fallen to the lot of a scientist accustomed to look at things from a mechanical standpoint. Every investigator must have a natural tendency to employ the methods with which he is most familiar, and there can be little doubt that Dr. Crawford's position as Lecturer in Mechanical Engineering at the Municipal Technical Institute of Belfast must have influenced his decision to begin his research with a thorough analysis of the fundamental mechanics of the phenomena. It is unlikely that any other line of attack would have yielded such satisfactory results, and it has, in addition, automatically introduced a far greater measure of control than would have accompanied more recondite methods.

For the benefit of those who are not familiar with Dr. Crawford's book, and in order to give a general idea of the kind of experiment on which his conclusions are based, I will here quote a few paragraphs dealing with some of the more important examples.

*"Experiment 2: Reaction on the medium during levitation.*

The table used was No. I. (the ordinary séance table, . . . ). I accurately balanced the weight of medium,

chair on which she was sitting, and drawing board. [On the weighing machine which frequently supported the medium in these experiments. W. W. S.] The medium sitting perfectly still, I asked the operators<sup>1</sup> to levitate the table and keep it as steadily levitated as they possibly could—i.e. without up-and-down or to-and-fro motion—while I was making my observations. Immediately on request the table rose about 8 in. into the air in an approximately horizontal plane . . . and became to all appearance quite steady in the air. As soon as I was satisfied that all was right and that the medium was sitting perfectly still as I had placed her, I examined the weighing machine. The steelyard, which before the levitation was just on the balance, was now hard up against the top stop. I moved the rider along until it again just balanced. The following were the readings:

Weight of medium + chair + draw- ing board, before levitation, - }	= 9 st. 4 lbs. 14 oz.
Weight of medium + chair + draw- ing board during steady levitation }	= 10 " 0 " 10 "
Increase in medium's weight due to levitation. - - - - }	9 " 12 "
Weight of table,	10 " 6 "

*Conclusion.* The increase in weight of the medium due to levitation is 10 oz. short of the weight of table.

*Notes.* The levitation was as nearly perfect as could be, and time was not a factor, as I had concluded my observations and there were no signs of the table descending. I had in fact to inform the operators that I had finished, and to ask them to drop the table, which they did suddenly, so that it reached the floor with a crash."

*"Experiment 7: The effect on the medium's weight of levitated table jerking vertically up and down in the air.*

The table used was No. I. (the ordinary séance table).

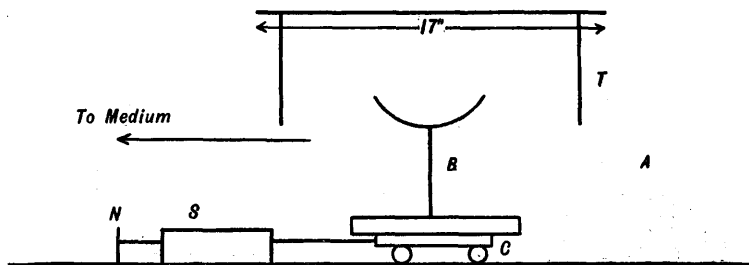
<sup>1</sup>Dr. Crawford considers that the phenomena are directed by "the spirits of human beings who have passed into the Beyond." These are the "operators" referred to. There can be no doubt that the phenomena are under intelligent control of some kind; but I myself prefer to suspend judgment as to its nature. W. W. S.

*Method.* The medium being seated quietly on the weighing machine, with hands on knees as in Experiment 2, I asked for steady levitation of the table, which was immediately given. The weight of the medium + chair + drawing board before levitation was 9 st. 4 lbs. During the period of steady levitation the combined weight was 10 st. 0 lb. 8 ozs. Having balanced the machine at this, I asked the operators to jerk the table (which up to then was steadily levitated) vertically upwards into the air. This they at once did, the table rising quickly from 6 in. to 8 in. into the air. I asked them to do it several times. The result was always the same. At each upward jerk of the table the steelyard of the weighing-machine rose and pressed against the top stop and then returned to its position of balance.

I also asked the operators to let the table slowly sag vertically in the air, and to arrest its motion suddenly before it reached the floor. This they did several times. I found that this motion also synchronised with instantaneous and temporary increase of the medium's weight in addition to the increased weight due to steady levitation.

*Conclusion.* When the table is steadily levitated the medium's weight is increased by an amount practically equal to the weight of the table. If the table is jerked up and down in the air, there is an additional instantaneous weight on the medium while the jerking is proceeding."

"*Experiment 48: To find the horizontal component of the reaction [exerted during levitation on a compression balance placed under the table. W. W. S.].*



. . . B is the compression balance (reading to 14 lb.) placed on top of an iron carriage C, which runs on ball bearings, and which is so free from friction that a force of  $\frac{1}{16}$  lb. suffices to pull it along the floor. N is a nail driven into the floor. S is an ordinary Salter tension spring balance reading to 20 lb., tied to the nail N and to the carriage C. T is the levitated table. Between A and N is my approximate position of observation.

I placed a finger of the right hand on the pointer of the tension balance S, and a finger of the left hand on the pointer of the compression balance B. I then asked the operators to levitate the table, when in the usual way the pointer on B gradually moved to  $14\frac{1}{2}$  lb. against the stop, and then the table sprang up into the air. The pointer on the tension balance also simultaneously moved along the scale, and the average of half a dozen levitations gave for it a rough value of about 4 lb. . . .

*Conclusion.* The horizontal component of the reaction when the séance table of weight  $10\frac{3}{8}$  lb. is levitated above the 14 lb. balance is apparently about 4 lb.; and this component acts directly outwards from the medium."

There are many other experiments I should like to quote if space permitted, but the foregoing, selected almost at random, are typical and should suffice to indicate their simplicity and directness and what I may call the commonsense nature of the methods employed.

The following account of the sitting witnessed by myself is taken from my contemporary notes. For the sake of brevity I have omitted a few irrelevant details, but none which would affect the main issues involved.

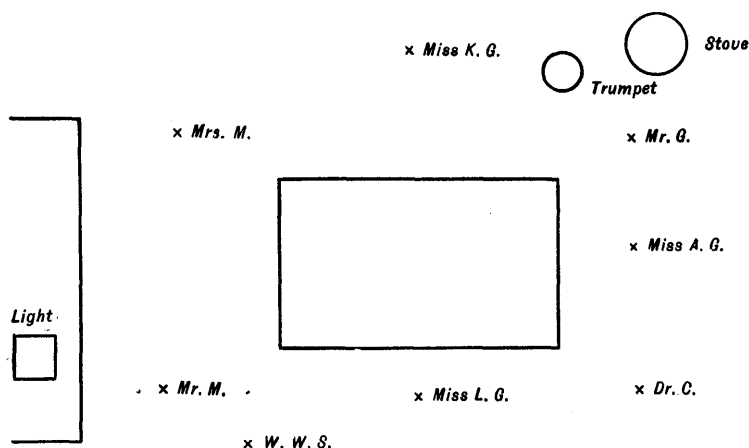
This sitting took place in Belfast on Saturday, Dec. 9th, 1916, at 8.0 p.m. It was held in the room in which most of Dr. Crawford's work has been done, at the house of Mr. Goligher, the father of the medium. There were present Mr. Goligher, Miss Anna Goligher, Miss Lily Goligher, Mr. Morison, Mrs. Morison and Miss Kathleen Goligher, the medium.

These formed a circle of about 5 ft. in diameter, sitting

with joined hands in the order named, *i.e.*—with Miss Kathleen Goligher between Mrs. Morison and Mr. Goligher.

Dr. Crawford and I sat outside the circle and were free to move about as we pleased.

The ordinary séance table was placed in the centre of the circle and a large two-piece metal trumpet stood just outside the circle, between Mr. Goligher and the medium. The arrangement is shown in the diagram.



The room was lighted by a fish-tail gas burner enclosed in a sheet-metal box fitted with ruby glass sides and placed on the chimney-piece to the medium's right front as shown. It was warmed by a gas-stove which stood on the floor diagonally opposite the light. This was rather important, as will appear later.

It is difficult to give any precise idea of the degree of illumination given by the gas. I can, perhaps, best indicate it by saying that it was a good deal stronger than I should care to use in a photographic dark-room. I found that when my eyes had become accustomed to the light, *i.e.* after about ten minutes, I could clearly see every object in the room unless it happened to be in deep shadow.

The proceedings opened with singing. After a few minutes, and while the singing was still in progress, strong raps were heard which beat time to the tune. These



were apparently produced on the floor in the neighbourhood of the medium. They sounded very definite, that is to say, as if someone were knocking firmly on the floor with a piece of hard wood; they in no way resembled the sound of an electric discharge as some raps have been said to do by certain observers.

The singing stopped and the proceedings proper began.

First came a variety of raps of all kinds from scarcely audible taps to real "sledge hammer" blows which shook the whole floor. These latter could not normally have been produced without the aid of some heavy percussive instrument or violent kicks with the heel of a boot.

The members of the circle were holding hands and all hands were clearly visible to me. I am sure that no one present could have made sufficiently violent movements with their feet without attracting my attention.

When the raps ceased the large metal trumpet already mentioned moved into the circle sliding along the ground apparently under its own power, so to speak, the sitters next to it (Miss Kathleen Goligher and Mr. Goligher) raising their hands to allow it to pass. It then fell on the floor under the table, and after a few moments' scuffling about it was separated into its two component parts. These two parts then rose into the air and projected towards me from under the table, being at this juncture not more than 18 inches from me.

I was invited to take hold of these two parts and I accordingly grasped each in turn.

I found, in each case, that I could move the end which I held to and fro in any direction with the greatest ease, although I was conscious of a slight elastic resistance. But when I tried to twist either of them about a longitudinal axis I was quite unable to do so. So great was the resistance to torque that I can only describe it by saying that it felt as if the lower ends of the two parts were embedded in a large mass of solid concrete, freely suspended so as to allow of transverse and longitudinal movement, but so heavy as to preclude twisting.

After a few moments these two parts of the trumpet

fell to the ground, and shortly after the table began to move about.

This table was about 2 feet long and  $1\frac{1}{2}$  feet broad and was made of dark painted or stained wood. It had four legs of the ordinary turned variety, which had no cross-bars between them, and weighed about ten pounds.

First it moved to and fro over a range of about a foot. Then it was rotated about a vertical axis at the rate of about 15-20 revolutions per minute. At the request of Dr. Crawford the direction of rotation was reversed without delay and apparently without difficulty. This rotation was distinctly jerky rather than smooth, and on the whole I should say that this irregularity was due rather to the intermittent nature of the rotating impulses than to inequalities of friction against the floor.

The table then moved again slightly, to adjust itself apparently, gave one or two tilts, and finally rose clear off the floor to a height of at least 12 inches.

In the course of the evening it did this some six or more separate times. On each occasion I bent down and looked clear under the table. I was particularly well situated for this observation, since, as already explained, the gas stove used for warming the room was diagonally opposite me and emitted a reddish glow from the heated metal, as well as gleams of light from cracks or the like. It was easy, as the table swayed gently to and fro in the air, to bring each leg in turn in line with this glow—by moving my head slightly from side to side—and thus to satisfy myself that there was nothing in contact with any of the legs.

On two occasions when the table was clear off the ground all the members of the circle lifted their hands above their heads, in which position they were verified by me.

After two or three of these preliminary levitations I was invited to step inside the circle, and I accordingly did so. I grasped the table firmly with both hands and did my utmost to prevent it moving, but I was quite unsuccessful. By dint of great exertion I could prevent it from moving in any one direction and could keep it

steady for a second or so, but it instantly moved in some other direction, the force changing with great rapidity. The amount of force exerted was quite extraordinary, indeed incredible to anyone who has not actually experienced it. I estimate that at times I exerted pressures of fully 100 lbs. weight. At one time the table was made so heavy that I could not lift it. At another time, when I had for a moment relaxed my grip, it levitated within six inches of me. While it was thus suspended in the air I again took hold of it and found that although I could move it, within limits, easily in any direction in the plane of its top, I encountered a remarkably solid resistance when I tried to push it downwards and towards the medium at an angle of about 45 degrees. So great was the resistance in this direction that it felt like pushing against a solid strut of wood or metal.

During the whole of this time I was standing within three feet of the medium and, most of the time, facing her. I could see distinctly the whole of her body down to the knees, and the light from the lamp fell directly on to her lap. Her feet were in shadow and I could not make them out distinctly. This is natural as she always sits with them tucked under her chair and her heels against its crossbar.

I could infallibly have detected any movement of the medium, and I can certify that she sat absolutely motionless during the whole time that the table was performing these violent evolutions.

I later sat on the table and, with my feet clear of the floor, was moved a distance of about six or eight inches. In addition, the table was three times tilted up to such an angle that I was unable to retain my seat.

Finally, after I had dismounted, it pushed me to the extreme edge of the circle, moving to a distance of fully four feet from the medium in the process. In this position I tried my hardest to push it back. Again it felt like pushing against a solid strut. By putting out all my strength I was only able to move it an inch or so.

Certain minor incidents also took place, and one or two interesting variations on the above were introduced.

For instance, raps were produced on the under surface of the table while I rested my hands on the top, and I could plainly feel the wood quivering under the blows.

Again, at one time the table was thrown upon the ground, levitated in this position (legs horizontal and pointing towards the medium), and finally restored to its upright position. This last process was performed with difficulty and only succeeded after several attempts. It was done by a series of strong jerks, exactly as if manipulated by an invisible hand which appeared to try to change its grip rapidly but sometimes missed it.

### III.

In discussing these phenomena the first question to be dealt with is that of genuineness, for, as Dr. Crawford says " . . . all the experiments recorded . . . depend for any value they may have on the fact that the table movements, levitations, raps, blows and other phenomena are genuine productions due to the action of psychic force and in no way caused by fraudulent action on the part of the medium or members of the circle."

The two possibilities which must be considered in this connection are Illusion and Fraud.

*ILLUSION.* (i) It may be doubted whether true illusion has ever been a source of error in the study of cases of this type. By 'true illusion' I mean the direct imposition by a trickster of an hallucination which has no foundation in objective reality, as opposed to faulty conclusions arising from malobservation, or unwarranted extrapolation from correct observations or unsound reasoning, which have, of course, been common enough, as have also false *suggestions* by the performer. Such hallucinations may be impossible on suitably sensitive subjects in certain hypnotic states, but I am not aware of any evidence which suggests that normal persons can be so deluded.

(ii) One of the mainstays of the professional illusionist is the creation in the minds of his audience of an anticipation that a certain event is about to happen, followed by

a suggestion that it has happened and commonly supplemented by an adroit distraction of attention at the moment when it is supposed to happen but does not. I may say in parenthesis that several excellent examples of this method are to be found in Zöllner's account of his sittings with Slade, who seems to have been a past-master at this sort of thing. But the method can only work when the illusionist is in charge of the proceedings, which is not the case here.

(iii) It is impossible to reconcile the idea of illusion, whether imposed or self-induced, with results recorded by mechanical apparatus.

(iv) Finally, speaking personally, I cannot believe that my own very vigorous "wrestle" with the table—a considerable exertion I may say—could have been an hallucination imposed and removed without my knowledge.

I do not think it likely, however, that anyone will attempt to explain away the phenomena along these lines, so I will say no more, except that if anyone does seriously think that illusion is a probable explanation and can adduce evidence to support his views I shall be pleased to undertake a more exhaustive analysis of the possibilities.

FRAUD. The possibility of trickery demands much more serious consideration, and I am sure that neither Dr. Crawford nor the members of the circle will resent my discussing it with the utmost care, especially as I anticipate that the great majority of those who are disinclined to accept the experiments at their face value will base their objections on an accusation of fraud. Nor do I blame them, for fraud has occurred so frequently throughout the history of Psychical Research, has stultified so much research and vitiated so many cases in which there is reason to suppose that genuine phenomena were also produced, that it is both natural and necessary to suspect its presence in contemporary cases of similar type.

But this attitude of suspicion, right and proper though it is, arises from an argument by analogy, and is only legitimate in proportion as the analogy is exact. Essentially this argument runs as follows :

"It is found from experience that there is a chance "P" of any member of a class "A" of objects  $a_1, a_2, a_3, \dots$  etc., possessing a certain quality "B": the object  $a_n$  belongs to the class "A"; therefore there is a chance "P" that the object  $a_n$  possesses the quality "B."

This is no more than a thinly disguised form of the syllogistic type: "All S is P: M is S; therefore M is P." Its soundness in this particular case is most likely to be vitiated by the inaccuracy of the minor premise—"the object  $a_n$  belongs to the class "A."

In other words, we have found from experience that of a certain class of cases 99 out of 100, say, are fraudulent; we therefore argue that the chances are about 99 to 1 in favour of another case of the same class being also fraudulent. But this argument only holds good provided that the characteristics of the particular case under consideration are sufficiently like those of the previous cases to warrant our placing it unreservedly in the same class with them.

If they are wholly dissimilar, the special case cannot be placed in the previously experienced class at all and the argument from analogy is wholly inapplicable.

If the resemblance is only partial we must proportionately discount our deduced chance of the presence of the quality "B" (in this case fraud).

Now although at first sight the fact that this case is one of a "physical" phenomenon may seem sufficient to warrant our classing it without more ado with all other cases of "physical" phenomena, I do not think that this will hold good in practice. In the first place, physical phenomena shade off gradually into non-physical, and it is hard to draw any exact boundary between the two. It would be unwise, for instance, unreservedly to class telekinetic phenomena, such as these, with parakinetic<sup>1</sup>, or with the obscure manifestations of the "Direct

<sup>1</sup>The distinction between "telekinetic" and "parakinetic" phenomena is that in the former there is no contact of any kind between the object moved and the members of the circle, whereas in the latter there *is* contact, but not of a kind to account for the observed movements on normal lines. Ordinary "table-turning" is an example of parakinesis.

Voice," which, if all that is claimed for them be true, must be at least quasi-physical.

Moreover, the characteristics by which various cases are classified for the purpose of forming such an *a priori* expectation must be those presented by them *before* the question of their genuineness was decided by the "reagent" of a special investigation *ad hoc* conducted under test conditions, may be, or by particularly stringent observers.

In other words, the special conditions which determined the frequency of association of the quality "B" with members of the class "A" must not be used for defining the class "A" or for identifying the object  $a_n$  as a member of that class.

In this case, for example, it would be fallacious to argue that phenomena originally produced in a good light are, for the purpose of assessment of the *a priori* chance of fraud, to be counted, without qualification, as members of the same class which includes phenomena originally produced in darkness and subsequently found to be fraudulent when the additional factor of good light was added.

In fact, all *a priori* judgments are comparative judgments based on evidence available up to a certain point, and it is essential that this point should be the same for the two sets of experiences compared. It may be defined, for present purposes at any rate, as the point where casual observation ends and systematic observation begins. If we place it later for early cases possessing a known probability of fraudulence we must do the same for the present case, and we are at once committed to the formation of a judgment which is not *a priori* at all.

It follows that *a priori* judgments have their logical basis in the similarity of *superficial* characteristics, and that our resultant expectation must be suitably discounted if the similarity is imperfect.

I do not pretend, of course, that this reasoning can be applied with quantitative precision to this or any other special case of the type we are considering.

The point I wish to make is that we are only entitled

to approach the case with a strong expectation of fraud if we have first satisfied ourselves that its superficial characteristics are truly similar to those of cases with which we have come to associate fraud in the past, and we must discount this expectation in proportion to any dissimilarity we may find.

I have dwelt on this point at some length because a priori judgments are too commonly formed without any real comprehension of the logical processes involved.

If now we compare what I have called the superficial characteristics of these experiments with those of past cases of "physical" phenomena I think we shall find a number of points of marked difference. I am somewhat handicapped by writing without books, so I cannot bring out these differences point by point, giving chapter and verse as I should like to do. But I do not think it likely that anyone well acquainted with the literature of the subject could fail to be struck by the difference between the general "atmosphere" of these experiments and that of other previous cases of which I suppose that of Eusapia Palladino is the classical example. Of special points the following are among the more obvious characteristics where important differences may be looked for:

- (i) The type of phenomenon produced.
- (ii) The order of their intensity.
- (iii) The regularity and ease with which they are produced.
- (iv) The degree of light in which they occur.
- (v) Whether the medium is entranced or not.
- (vi) Whether he or she is a paid professional or not.
- (vii) His or her past history.

I am sure that any comparison of this kind is bound to lead us to the conclusion that our *a priori* prepossessions in favour of fraud must be heavily discounted by reason of the difference between the superficial characteristics of this and previous cases.<sup>1</sup>

<sup>1</sup> I wish to make it clear that I am not in any way trying to depreciate the value of the evidence obtained from the study of these "previous cases," which was of the utmost intrinsic value. My point is that the conditions obtaining at Belfast have been so different from and, incidentally, better than those of



It must be noted that this is an argument from mere differences as such and is in no way dependent on the *sense* of these differences. It would apply with equal force even if all the differences were of such a nature as to *increase* the suspicion of fraud when forming an *a posteriori* judgment. This is not the case here as I shall now proceed to show.

Fraud is frequently discussed under the two heads of Conscious and Unconscious, but in this case the distinction is unnecessary for the question is one of fact only. Is it *possible* for the medium to produce the phenomena observed by ordinary physical means, in spite of the prolonged and close scrutiny of Dr. Crawford and in view of their characteristic features? If it is possible, does she in fact do so?

These are questions which must be answered, and I think that the evidence at our disposal is sufficient to enable us to do so with a very high degree of certainty.

I do not propose to dwell at length on whether there might be a desire or motive for deception, preferring to treat the question as a matter of bare fact.

None the less, it is well to point out that the sittings at which these phenomena are produced are entirely private in nature, and therefore cupidity or the desire for notoriety, which may form a powerful incentive in the case of the professional medium, are here entirely lacking. Dr. Crawford has never paid a penny for the facilities accorded him—often at the cost of considerable inconvenience—by the medium and her family. Nor do the members of the circle encourage the promiscuous introduction of persons other than their own friends; and as a matter of fact, the sittings have recently been restricted solely to scientific work.

previous cases that we must exercise considerable care in “carrying forward” past experience and applying it in this case.

The records of earlier investigators who claim to have obtained levitations without contact (cp. Sir William Crookes’ “Researches in Spiritualism” and the Naples experiments with Eusapia Palladino, reported in *Proc. S.P.R.*, Vol. XXIII.) are, of course, greatly strengthened, if we accept the Belfast phenomena as genuine.

I feel that it amounts to something very like an impertinence on my part to presume to testify to the integrity of those concerned; but, for the purpose of mere scientific record, I may say that the members of the circle struck me as being eminently upright, honourable and likeable people of the best type—quite incapable of practising a mean and objectless trickery.

But these considerations, however much they may weigh with those who have come under their immediate influence, cannot be expected to appeal with the same force to others who approach the subject only at second hand. Nor are they admissible as evidence of the first order from the strictly scientific point of view.

It is right, therefore, for the purpose of the immediate discussion, to assume that the desire to deceive may exist, and that it may be coupled with the utmost imaginable cunning and ingenuity.

As already indicated, my own strong opinion is that the phenomena are genuine. To be more specific, I believe that when the table is levitated it is not supported either by the hands or feet of the medium or of any member or members of the circle, or by any mechanical contrivance devised or controlled by her or by them or by a confederate. I do not mean by this, of course, to dissent from Dr. Crawford's "cantilever" theory, with which I entirely agree; but this is not a "mechanical contrivance" in any usual sense of the word.

I think the reasons supporting this view may conveniently be divided into two groups. First, there are general considerations obtained from Dr. Crawford's own records and from the comparison of this case with preceding cases; and, secondly, those arising from my own personal observations. It should be noted that the latter inevitably strengthen the former. Even a single sitting by an independent observer is a very valuable check on the original observer's work. Unless I and all other observers who have seen the phenomena are *ipso facto* suspect, it eliminates the possibility of Dr. Crawford being mad, or himself implicated in a fraud—and even such far-fetched possibilities are worth putting out of court.

Again, if the main features of a piece of work of this kind are substantiated, there is little need to worry about the minor points.

As regards general considerations, then, it may first be noted that a comparison of such features of this case as exert a direct influence on the possibility of fraud with the corresponding features of preceding cases is heavily in favour of these phenomena being genuine.

This comparison is, of course, essentially a matter of the *sense* of differences, not of their mere existence, as was the case when I was discussing the formation of a *priori* prepossessions, and includes the results of "systematic observation" as well as the "superficial characteristics."

(i) It is well known that a trick which appears mystifying at first sight is easier to detect every time that it is repeated. Dr. Crawford has observed some hundreds of levitations, has worked under and all round the table, both with and without apparatus, and the conditions of observation have been uniformly favourable.

(ii) The phenomena occur with the utmost regularity. It is easy to make errors of observation when events take the observer by surprise and when it is necessary to look for everything at once. But this is not the case when a phenomenon is repeated again and again at the word of command and with variations caused only by the observer's own modifications of conditions, for this means that he can attend to various points one by one and with any amount of checking that he pleases.

(iii) The lighting has been good. This is enormously important, for our sense of sight is by far the most reliable that we possess, and without its help an investigation is very heavily handicapped. I do not know of any well-authenticated case of phenomena of this intensity occurring in a good light, except perhaps those of D. D. Home many years ago. But in this case the light available for observation is undeniably good, although it is, of course, to be hoped that Dr. Crawford may ultimately be able to use a much stronger light by finding and eliminating the particular wave-lengths which prevent phenomena developing.

(iv) There is a great difference between quantitative and qualitative work. The former, which has been abundant in this case, postulates an intimate control of the phenomena and cannot be extensively undertaken when they are fugitive or sporadic. Conditions must be very good indeed for qualitative observation before they become possible for quantitative measurements.

(v) Difficult as it is to see any opening for fraud at all, it becomes much more so when we reflect that the fraudulent person or persons would have been obliged not only to simulate the phenomena but to "fake" the readings of the apparatus at the same time, and, moreover, in such a way as to give concordant results uniformly pointing towards a definite theory, and maintaining this concordance even in the face of experiments especially designed by Dr. Crawford to test it, and whose import was unknown to the members of the circle.

Dr. Crawford has dealt very thoroughly with this aspect of the question on pp. 16-26 of his book, and there is therefore no need for me to enlarge further on it.

(vi) The research has been continuously conducted throughout by the one observer only—with checks on the main features by various independent observers, such as myself. It is clearly far more difficult for a trickster to maintain a deception in the face of continuous observation of this kind—unless the chief investigator is quite extraordinarily gullible—than it is in cases where the investigation takes the non-cumulative form of the observation of a few sittings each by a series of committees who do not know quite what to look for and who can easily be put off, if need be, by a sudden "failure of power," "bad conditions," or the like.

(vii) As already pointed out, there seems no *motive* for fraud.

In addition to these general considerations there are more specific reasons for believing fraud to be absent.

A spurious levitation could be accomplished in one of three ways only:

(a) The medium or one of the other members of the circle might use their hands or feet directly.

(b) They might use hands or feet indirectly, that is to say, by the intermediary of rods, clamps or similar appliances.

(c) A special and elaborate apparatus might be installed, operating, *e.g.* through trap-doors in the floor, and controlled either by a member of the circle or by a confederate.

The last possibility is inserted only for the sake of logical completeness. It may be eliminated at once, for, in addition to its inherent improbability, it must be remembered that all the principal phenomena in question have, on occasion, been produced in Dr. Crawford's house and in various other rooms. The direct or indirect use of hands is also eliminated, because, as already stated, I have myself twice seen the sitters raise their joined hands above their heads while the table was levitated clear of the ground.

We are left with the possibility of the direct or indirect use of feet by the medium or a member of the circle.

The members of the circle other than the medium may be left out of the question, because, in the first place, all Dr. Crawford's quantitative work goes to show that the medium is primarily responsible for the phenomena, and, in the second, during the time I was within the circle myself I moved all round the table except into the space between it and the medium. This would have prevented any member of the circle from using his or her feet, because my own legs would have been in the way.

The question, therefore, reduces to whether the medium uses her feet or not.

The following points seem to me to answer this satisfactorily:

(i) I tried to lift the table with my feet myself but failed. The legs have no cross-bars and it is impossible to get a proper grip.

(ii) When the table was levitated I was in a position to see each leg in turn against the light from the stove—as already explained—and there was nothing in contact with any of them.

(iii) When the table was thrown into the horizontal position with the legs pointing towards the medium there was, naturally, no shadow thrown by its top to hinder observation. In this position all four legs were clearly visible simultaneously and were not in contact with anything.

(iv) In the course of the proceedings the table moved to a distance of at least four feet from the medium, which is well beyond the reach of a person sitting upright in a chair.

(v) The leverage exerted by a ten-pound table at a radius of the legs is considerable, as anyone may test for themselves. Even if it were possible for the medium, by long practice or the use of specially devised clamps, to raise the table from the ground, it would be physically impossible for her to keep it steady for any length of time. This is doubly so when the very heavy pressures exerted by myself are taken into account.

(vi) I know a little of the way muscle behaves when fatigued. Throughout the séance the table moved with a poise and precision which did not at all convey the impression of muscular action.

(vii) When the table was levitated I tried the effect of pushing it in various directions. The direction of maximum resistance was about 45° downwards and towards the medium. This is just about the least favourable angle for resistance on the assumption that the medium was using her feet, because at that angle my pressure would exert an approximately maximal turning moment about the medium's hips. The direction most favourable for resistance would, of course, be along the line of the legs—i.e. more or less horizontally towards the medium, but somewhat upwards, since she could not raise her legs above the horizontal without moving her knees, which were in full view. In practice the resistance was extraordinary and quite beyond the medium's normal physical power.

(viii) At the time that I was within the circle until I was pushed to its confines by the table, I was within three feet of the medium, and, as already explained,

I could see clearly the whole of her body down to her knees. Even if we postulate the most ingenious mechanism for grasping the table and a physical strength on the part of the medium quite disproportionate to her weight and general build, it is surely impossible that she could have sat absolutely motionless, as she did, while the table writhed and twisted beneath my hands with the utmost violence.

(ix) Several times while pushing hard against the table I suddenly relaxed my pressure. If the medium had been pushing against me she could not have restrained a synchronous jerk. No such movement took place.

In concluding this account of my reasons for believing the phenomena to be wholly genuine I cannot refrain from saying that I consider that a certain *onus probandi* rests with those who support the theory of fraud as well as on those who do not. No accusation of trickery should be made unless its author is prepared to show, roughly, at any rate, how such trickery could be effected. I do not think anyone has answered Dr. Crawford's challenge (see pp. 21-26), but, speaking as one to whom the explanation of fraud has always seemed the most likely in these cases, I should be exceedingly interested to hear of any really plausible explanation on these lines, if any reader can produce one, *which will cover the facts and contains a sketch of the 'modus operandi.'*

It may be noted here that Dr. Crawford's work has already had the effect of greatly lightening the labours of future students of telekinetic phenomena. When next we find a good case of the type, we need not begin *de novo* groping in the dark. We can get to work right away confirming his fundamental experiments and following up the methods which he has inaugurated. This should result in a great economy of time and of rare material.

#### IV.

In view of the reasons which I have adduced in support of the genuineness of the phenomena it seems permissible

to conclude with some observations based on the assumption that fraud may be regarded as eliminated.

I do not propose to discuss the ultimate origin of the phenomena, or whether the controlling intelligence is incarnate or discarnate, a primary or a secondary personality. Such questions, associated with the problem of survival, do not constitute the principal and peculiar interest of phenomena of this type. The evidence for survival must always reduce to a matter of identity and of identity alone, and although the evidential matter necessary for its study may be obtained by "physical" phenomena—e.g. by raps—its evidential value, if any, is to be found in its content and not in the manner by which it is received.

The peculiar interest of telekinetic phenomena lies, then, in their proximate rather than their ultimate causes. There can be no doubt, in this case at least, that they are directed by an intelligence of some kind or other, and it follows that we have a case of a Consciousness<sup>1</sup> acting upon matter otherwise than through the usual intermediary of brain, nerve and muscle to which we are accustomed.

This being so, it is clearly probable that a study of the means whereby this control is exercised may throw light on the relation which matter and consciousness bear to one another.

Consciousness normally acts on matter through the body, and hitherto we have not succeeded in discovering the precise nature of the connection between the two, largely by reason of the experimental difficulties involved.

In this case some mechanism, using the word in its widest sense, seems to be employed which must be of a very different nature.

On the one hand, since it is under intelligent control, it must be in contact, so to speak, with Consciousness; on the other, since it affects matter, it must be quasi-physical.

<sup>1</sup>"Consciousness" is admittedly a somewhat vague and controversial word to use here. It must not be taken as indicating any special philosophic or metaphysical doctrine on my part beyond a general belief in the "transmissive" view of the relation between brain and consciousness.



It forms, as it were, a mean proportional between the physical and mental realms, and if we succeed in elucidating its precise nature we shall, I believe, have obtained the missing link which will enable us to understand something of the relation between the two and to express both in terms of a common and coherent whole.

This might almost be described as the primary aim of Psychic and Psychological Science.

The results so far obtained by Dr. Crawford do not enable us to form any definite conclusions on the subject of a positive nature. They do, however, allow us to eliminate certain possibilities with considerable assurance.

I do not propose to deal with the sort of explanation which speaks vaguely of undefined "forces" or "negative gravity" or postulates the existence of an "etheric duplicate" of the material table. These fantastic "explanations" simply do not explain: for explanation consists essentially of the restatement of the highly obscure in terms of the less obscure and is not effected by the introduction of new concepts as unintelligible as the original problem.

It is therefore necessary, in accordance with the universal scientific principle of minimum assumption, to exhaust all known modes of action and analogues thereof before launching out into entirely unprecedented lines of speculation.

This method has been pursued by Dr. Crawford with the happiest results.

Before dealing with the line of explanation to which he has been led I will first mention one or two possibilities which may be definitely eliminated from the start.

Matter moves only when force is applied to it, and forces may normally be transmitted in one of two ways only.

(i) Through the intermediary of matter, *e.g.* by the direct action of a mechanical structure capable of transmitting tension, compression, torque or shear, or by the impact of material particles as in the case of the pressure of a gas.

(ii) Without the intervention of matter, that is to say, when the force is transmitted solely by the ether, as in

electrical or magnetic actions or gravitation. In all cases of this class the forces obey the law of inverse squares and are propagated rectilinearly.

The whole of this second type of force transmitted may safely be rejected at once. The behaviour of the table in no way resembles the effect of any of the forces comprised in this group. I am sure that no one who has seen the phenomena and has any acquaintance with the nature of electro-magnetic forces could possibly think that the movements of the table are produced by the action of any force obeying the inverse square law and emanating from the medium or any other source or sources.

The same applies to any supposition that the table is bombarded by a stream of material particles projected from the medium or elsewhere. This idea is negatived by the normal absence of any reaction on the floor vertically under the table, by the failure of a manometer to detect any pressure and by the extreme rapidity with which the force changes its direction.

I may say, however, that although this line of explanation seems untenable in its simplest form, I should not be surprised to find that it entered into the complete explanation in a modified form and in an ancillary capacity.

As opposed to these possibilities, with which the observed phenomena present no analogy, we find a striking and indeed perfect resemblance to the simplest of all forms of force transmission, namely, a rigid structure.

The direction and magnitude of the forces measured correspond accurately with those which would obtain, if the connection between the medium and the table were a material beam of a certain shape and possessing considerable rigidity.

Dr. Crawford has been at pains to test this hypothesis by deduction and experiment, and the results have been uniformly coherent and confirmatory. We may therefore say, at least, that although we can neither see nor feel any material structure between the medium and the table, yet the latter behaves precisely as if such a structure

were there. Not only is this the case as regards the forces called into play during the phenomena, but, in addition, it is possible to locate the structure and to determine approximately its size and form by observing in what positions an interposed solid object causes the levitated table to drop, and by tracing out the lines of stress in the space between the medium and the table. These methods confirm the conclusions drawn from measurements of the forces.

At this point we are faced by a somewhat awkward dilemma. Short of denying the facts—a course which I hold to be unjustifiable on the evidence—we must admit one of two things.

Either we must say that there exists a concatenation of forces of an entirely unknown nature and so adjusted as to produce a perfect but illusory simulacrum of a rigid cantilever, or we must admit that there can exist a material, or quasi-material, structure which combines the properties of rigidity and impalpability.

The former is in every way unsatisfactory; it is unprecedented and quite fails to throw any real light on the subject. The latter is a mechanical contradiction in terms.

None the less, it is the second alternative which I think we should adopt. Most, if not all, scientific discoveries have originated in the observation of facts which at first sight have appeared incompatible, and have remained so until the unifying clue has been forthcoming.

I believe that the whole essence of the problem before us in this case lies in the reconciliation of these contradictory properties of the cantilever.

When we have solved the secret of its rigidity—or even got so far as to imagine any means whereby that rigidity could be obtained—I believe we shall hold in our hands the key which will in time unlock most of the closed doors which at present confront us in the investigation of Psychic forces.

At the present time I do not think that we are in a position even to adumbrate the final solution of the

mystery, and in this vague and somewhat unsatisfactory state I must be content to leave the subject.

But before we can hope to solve any problem we must first be assured of the phenomena which give rise to it, and then narrow the investigation down to the minimum number of specific points by the elimination of all irrelevant details and untenable possibilities.

It is in these preliminary steps that I hope the preceding paragraphs may be of some slight value.