EXPERIMENTAL INVESTIGATIONS

ON

PSYCHIC FORCE.

BY

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EXPERIMENTAL INVESTIGATION OF A NEW FORCE.

TWELVE months ago in this journal* I wrote an article, in which, after expressing in the most emphatic manner my belief in the occurrence, under certain circumstances, of phenomena inexplicable by any known natural laws, I indicated several tests which men of science had a right to demand before giving credence to the genuineness of these phenomena. Among the tests pointed out were, that a "delicately poised balance should be moved under test conditions;" and that some exhibition of power equivalent to so many "foot-pounds" should be "manifested in his laboratory, where the experimentalist could weigh, measure, and submit it to proper tests." I said, too, that I could not promise to enter fully into this subject, owing to the difficulties of obtaining opportunities, and the numerous failures attending the enquiry; moreover, that "the persons in whose presence these phenomena take place are few in number, and opportunities for experimenting with previously arranged apparatus are rarer still."

Opportunities having since offered for pursuing the investigation, I have gladly availed myself of them for applying to these phenomena careful scientific testing experiments, and I have thus arrived at certain definite results which I think it right should be published. These experiments appear conclusively to establish the existence of a new force, in some unknown manner connected with the human organisation, which for convenience may be called the Psychic Force.

Of all the persons endowed with a powerful development of this Psychic Force, and who have been termed "mediums" upon quite another theory of its origin, Mr. Daniel Dunglas Home is the most remarkable, and it is mainly owing to

* See Quarterly Journal of Science, vol. vii., p. 316, July, 1870.

the many opportunities I have had of carrying on my investigation in his presence that I am enabled to affirm so conclusively the existence of this Force. The experiments I have tried have been very numerous, but owing to our imperfect knowledge of the conditions which favour or oppose the manifestations of this force, to the apparently capricious manner in which it is exerted, and to the fact that Mr. Home himself is subject to unaccountable ebbs and flows of the force, it has but seldom happened that a result obtained on one occasion could be subsequently confirmed and tested with apparatus specially contrived for the purpose.

Among the remarkable phenomena which occur under Mr. Home's influence, the most striking, as well as the most easily tested with scientific accuracy, are—(1) the alteration in the weight of bodies, and (2) the playing of tunes upon musical instruments (generally an accordion, for convenience of portability) without direct human intervention, under conditions rendering contact or connection with the keys impossible. Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality. Still, desiring to place the matter beyond the shadow of doubt, I invited Mr. Home on several occasions to come to my own house, where, in the presence of a few scientific enquirers, these phenomena could be submitted to crucial experiments.

The meetings took place in the evening, in a large room lighted by gas. The apparatus prepared for the purpose of testing the movements of the accordion, consisted of a cage, formed of two wooden hoops, respectively 1 foot 10 inches and 2 feet diameter, connected together by 12 narrow laths, each 1 foot 10 inches long, so as to form a drum-shaped frame, open at the top and bottom; round this 50 yards of insulated copper wire were wound in 24 rounds, each being rather less than an inch from its neighbour. These horizontal strands of wire were then netted together firmly with string, so as to form meshes rather less than 2 inches long by 1 inch high. The height of this cage was such that it would just slip under my dining table, but be too close to the top to allow of the hand being introduced into the interior, or to admit of a foot being pushed underneath it. In another room were two Grove's cells, wires being led from them into the dining-room for connection, if desirable, with the wire surrounding the cage.

The accordion was a new one, having been purchased by

myself for the purpose of these experiments at Wheatstone's, in Conduit Street. Mr. Home had neither handled nor seen the instrument before the commencement of the test experiments.

In another part of the room an apparatus was fitted up for experimenting on the alteration in the weight of a body. It consisted of a mahogany board, 36 inches long by 9½ inches wide and 1 inch thick. At each end a strip of mahogany 1½ inches wide was screwed on, forming feet. One end of the board rested on a firm table, whilst the other end was supported by a spring balance hanging from a substantial tripod stand. The balance was fitted with a self-registering index, in such a manner that it would record the maximum weight indicated by the pointer. The apparatus was adjusted so that the mahogany board was horizontal, its foot resting flat on the support. In this position its weight was 3 lbs., as marked by the pointer of the balance.

Before Mr. Home entered the room, the apparatus had been arranged in position, and he had not even the object of some parts of it explained before sitting down. It may, perhaps, be worth while to add, for the purpose of anticipating some critical remarks which are likely to be made, that in the afternoon I called for Mr. Home at his apartments, and when there he suggested that, as he had to change his dress, perhaps I should not object to continue our conversation in his bedroom. I am, therefore, enabled to state positively, that no machinery, apparatus, or contrivance of any sort was secreted about his person.

The investigators present on the test occasion were an eminent physicist, high in the ranks of the Royal Society, whom I will call Dr. A. B.; a well-known Serjeant-at-Law, whom I will call Serjeant C. D.; my brother; and my chemical assistant.*

Mr. Home sat in a low easy chair at the side of the table. In front of him under the table was the aforesaid cage, one of his legs being on each side of it. I sat close to him on his left, and another observer sat close to him on his right,

* It argues ill for the boasted freedom of opinion among scientific men, that they have so long refused to institute a scientific investigation into the existence and nature of facts asserted by so many competent and credible witnesses, and which they are freely invited to examine when and where they please. For my own part I too much value the pursuit of truth, and the discovery of any new fact in nature, to avoid enquiry because it appears to clash with prevailing opinions. But as I have no right to assume that others are equally willing to do this, I refrain from mentioning the names of my friends without their permission.

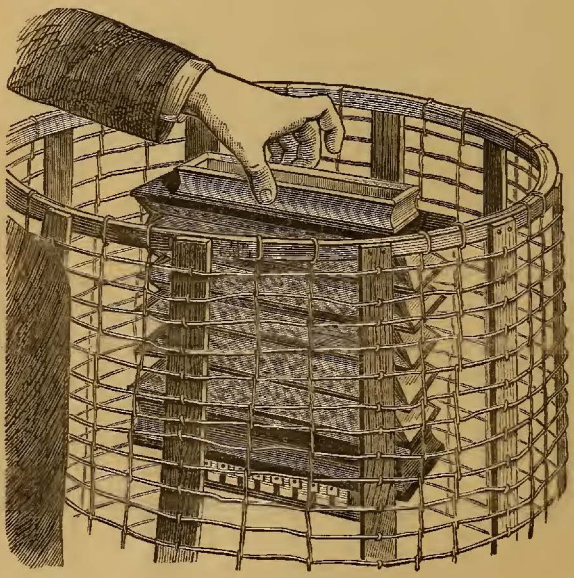
the rest of the party being seated at convenient distances round the table.

For the greater part of the evening, particularly when anything of importance was proceeding, the observers on each side of Mr. Home kept their feet respectively on his feet, so as to be able to detect his slightest movement.

The temperature of the room varied from 68° to 70° F.

Mr. Home took the accordion between the thumb and middle finger of one hand at the opposite end to the keys (see woodcut, Fig. 1), (to save repetition this will be subsequently called "in the usual manner.") Having

Fig. 1.



previously opened the bass key myself, and the cage being drawn from under the table so as just to allow the accordion to be passed in with its keys downwards, it was pushed back as close as Mr. Home's arm would permit, but without hiding his hand from those next to him (see Fig 2). Very soon the accordion was seen by those on each side to be waving about in a somewhat curious manner; then sounds came from it, and finally several notes were played in succession. Whilst this was going on, my assistant went under the table, and reported that the accordion was expanding and contracting; at the same time it was seen that the

hand of Mr. Home by which it was held was quite still, his other hand resting on the table.

Presently the accordion was seen by those on either side of Mr. Home to move about, oscillating and going round and round the cage, and playing at the same time. Dr. A. B. now looked under the table, and said that Mr. Home's hand appeared quite still whilst the accordion was moving about emitting distinct sounds.

Mr. Home still holding the accordion in the usual manner in the cage, his feet being held by those next him,

Fig. 2.

and his other hand resting on the table, we heard distinct and separate notes sounded in succession, and then a simple air was played. As such a result could only have been produced by the various keys of the instrument being acted upon in harmonious succession, this was considered by those present to be a crucial experiment. But the sequel was still more striking, for Mr. Home then removed his hand altogether from the accordion, taking it quite out of the cage, and placed it in the hand of the person next to him. The instrument then continued to play, no person touching it and no hand being near it.

I was now desirous of trying what would be the effect of passing the battery current round the insulated wire of the cage, and my assistant accordingly made the connection with the wires from the two Grove's cells. Mr. Home again held the instrument inside the cage in the same manner as before, when it immediately sounded and moved about vigorously. But whether the electric current passing round the cage assisted the manifestion of force inside, it is impossible to say.

The accordion was now again taken without any visible touch from Mr. Home's hand, which he removed from it entirely and placed upon the table, where it was taken by the person next to him, and seen, as now were both his hands, by all present. I and two of the others present saw the accordion distinctly floating about inside the cage with no visible support. This was repeated a second time, after a short interval. Mr. Home presently re-inserted his hand in the cage and again took hold of the accordion. It then commenced to play, at first chords and runs, and afterwards a well-known sweet and plaintive melody, which it executed perfectly in a very beautiful manner. Whilst this tune was being played, I grasped Mr. Home's arm, below the elbow, and gently slid my hand down it until I touched the top of the accordion. He was not moving a muscle. His other hand was on the table, visible to all, and his feet were under the feet of those next to him.

Having met with such striking results in the experiments with the accordion in the cage, we turned to the balance apparatus already described. Mr. Home placed the tips of his fingers lightly on the extreme end of the mahogany board which was resting on the support, whilst Dr. A. B. and myself sat, one on each side of it, watching for any effect which might be produced. Almost immediately the pointer of the balance was seen to descend. After a few seconds it rose again. This movement was repeated several times, as if by successive waves of the Psychic Force. The end of the board was observed to oscillate slowly up and down during the experiment.

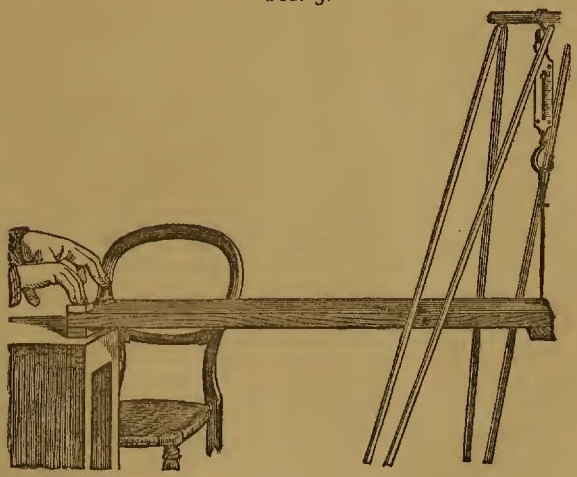
Mr. Home now of his own accord took a small hand-bell and a little card match-box, which happened to be near, and placed one under each hand, to satisfy us, as he said, that he was not producing the downward pressure (see Fig. 3). The very slow oscillation of the spring balance became more marked, and Dr. A. B., watching the index, said that he saw it descend to $6\frac{1}{2}$ lbs. The normal weight of the board as so suspended being 3 lbs., the additional downward pull

was therefore $3\frac{1}{2}$ lbs. On looking immediately afterwards at the automatic register, we saw that the index had at one time descended as low as 9 lbs., showing a maximum pull of 6 lbs. upon a board whose normal weight was 3 lbs.

In order to see whether it was possible to produce much effect on the spring balance by pressure at the place where Mr. Home's fingers had been, I stepped upon the table and stood on one foot at the end of the board. Dr. A. B., who was observing the index of the balance, said that the whole weight of my body (140 lbs.) so applied only sunk the index $1\frac{1}{2}$ lbs., or 2 lbs. when I jerked up and down. Mr. Home had been sitting in a low easy-chair, and could not, therefore, had he tried his utmost, have exerted any material influence on these results. I need scarcely add that his feet as well as his hands were closely guarded by all in the room.

This experiment appears to me more striking, if possible, than the one with the accordion. As will be seen on referring to the cut (Fig. 3), the board was arranged perfectly horizontally, and it was particularly noticed that Mr.

Fig. 3.



Home's fingers were not at any time advanced more than $1\frac{1}{2}$ inches from the extreme end, as shown by a pencil-mark, which, with Dr. A. B.'s acquiescence, I made at the time. Now, the wooden foot being also $1\frac{1}{2}$ inches wide, and resting flat on the table, it is evident that no amount of pressure exerted within this space of $1\frac{1}{2}$ inches could produce any action on the balance. Again, it is also evident that when

the end furthest from Mr. Home sank, the board would turn on the further edge of this foot as on a fulcrum. The arrangement was consequently that of a see-saw, 36 inches in length, the fulcrum being 1½ inches from one end; were he therefore to have exerted a downward pressure, it would have been in opposition to the force which was causing the other end of the board to move down.

The slight downward pressure shown by the balance when I stood on the board was owing probably to my foot extending beyond this fulcrum.

I have now given a plain unvarnished statement of the facts from copious notes written at the time the occurrences were taking place, and copied out in full immediately after. Indeed, it would be fatal to the object I have in view—that of urging the scientific investigation of these phenomena—were I to exaggerate ever so little; for although to my readers Dr. A. B. is at present represented by incorporeal initials, to me the letters represent a power in the scientific world that would certainly convict me if I were to prove an untrustworthy narrator.

I confess I am surprised and pained at the timidity or apathy shown by scientific men in reference to this subject. Some little time ago, when an opportunity for examination was first presented to me, I invited the co-operation of some scientific friends in a systematic investigation; but I soon found that to obtain a scientific committee for the investigation of this class of facts was out of the question, and that I must be content to rely on my own endeavours, aided by the co-operation from time to time of the few scientific and learned friends who were willing to join in the inquiry. I still feel that it would be better were such a committee of known men to be formed, who would meet Mr. Home in a fair and unbiassed manner, and I would gladly assist in its formation; but the difficulties in the way are great.

A committee of scientific men met Mr. Home some months ago at St. Petersburg. They had one meeting only, which was attended with negative results; and on the strength of this they published a report highly unfavourable to Mr. Home. The explanation of this failure, *which is all they have accused him of*, appears to me quite simple. Whatever the nature of Mr. Home's power, it is very variable, and at times entirely absent. It is obvious that the Russian experiment was tried when the force was at a minimum. The same thing has frequently happened within my own experience. A party of scientific men met Mr. Home at my house, and the results were as negative as those at

St. Petersburg. Instead, however, of throwing up the inquiry, we patiently repeated the trial a second and a third time, when we met with results which were positive.

These conclusions have not been arrived at hastily or on insufficient evidence. Although space will allow only the publication of the details of one trial, it must be clearly understood that for some time past I have been making similar experiments and with like results. The meeting on the occasion here described was for the purpose of confirming previous observations by the application of crucial tests, with carefully arranged apparatus, and in the presence of irreproachable witnesses.

Respecting the cause of these phenomena, the nature of the force to which, to avoid periphrasis, I have ventured to give the name of *Psychic*, and the correlation existing between that and the other forces of nature, it would be wrong to hazard the most vague hypothesis. Indeed, in enquiries connected so intimately with rare physiological and psychological conditions, it is the duty of the enquirer to abstain altogether from framing theories until he has accumulated a sufficient number of facts to form a substantial basis upon which to reason. In the presence of strange phenomena as yet unexplored and unexplained following each other in such rapid succession, I confess it is difficult to avoid clothing their record in language of a sensational character. But, to be successful, an enquiry of this kind must be undertaken by the philosopher without prejudice and without sentiment. Romantic and superstitious ideas should be entirely banished, and the steps of his investigation should be guided by intellect as cold and passionless as the instruments he uses. Having once satisfied himself that he is on the track of a new truth, that single object should animate him to pursue it, without regarding whether the facts which occur before his eyes are "naturally possible or impossible."

Since this article was in type, the Author has been favoured with the following letters from Dr. Huggins and Mr. Serjeant Cox—the Dr. A. B. and Serjeant C. D. therein referred to :—

Upper Tulse Hill, S.W.,
June 9, 1871.

DEAR MR. CROOKES,—Your proof appears to me to contain a correct statement of what took place in my presence at your house. My position at the table did not permit me to

be a witness to the withdrawal of Mr. Home's hand from the accordion, but such was stated to be the case at the time by yourself and by the person sitting on the other side of Mr. Home.

The experiments appear to me to show the importance of further investigation, but I wish it to be understood that I express no opinion as to the cause of the phenomena which took place. Yours very truly,

WILLIAM HUGGINS.

WM. CROOKES, Esq., F.R.S.

36, Russell Square,
June 8, 1871.

MY DEAR SIR,—Having been present, for the purpose of scrutiny, at the trial of the experiments reported in this paper, I readily bear my testimony to the perfect accuracy of your description of them, and to the care and caution with which the various crucial tests were applied.

The results appear to me conclusively to establish the important fact, that there is a force proceeding from the nerve-system capable of imparting motion and weight to solid bodies within the sphere of its influence.

I noticed that the force was exhibited in tremulous pulsations, and not in the form of steady continuous pressure, the indicator rising and falling incessantly throughout the experiment. This fact seems to me of great significance, as tending to confirm the opinion that assigns its source to the nerve organisation, and it goes far to establish Dr. Richardson's important discovery of a nerve atmosphere of various intensity enveloping the human structure.

Your experiments completely confirm the conclusion at which the Investigation Committee of the Dialectical Society arrived, after more than forty meetings for trial and test.

Allow me to add that I can find no evidence even tending to prove that this force is other than a force proceeding from, or directly dependent upon, the human organisation, and therefore, like all other forces of nature, wholly within the province of that strictly scientific investigation to which you have been the first to subject it.

Psychology is a branch of science as yet almost entirely unexplored, and to the neglect of it is probably to be attributed the seemingly strange fact that the existence of this nerve-force should have remained so long untested, unexamined, and almost unrecognised.

Now that it is proved by mechanical tests to be a fact in nature (and if a fact, it is impossible to exaggerate its importance to physiology and the light it must throw upon the obscure laws of life, of mind, and the science of medicine) it cannot fail to command the immediate and most earnest examination and discussion by physiologists and by all who take an interest in that knowledge of "man," which has been truly termed "the noblest study of mankind." To avoid the appearance of any foregone conclusion, I would recommend the adoption for it of some appropriate name, and I venture to suggest that the force be termed the *Psychic Force;* the persons in whom it is manifested in extraordinary power *Psychics*; and the science relating to it *Psychism*, as being a branch of *Psychology*.

Permit me, also, to propose the early formation of a *Psychological Society*, purposely for the promotion, by means of experiment, papers, and discussion, of the study of that hitherto neglected Science.—I am, &c.,

EDWD. WM. COX.

To W. CROOKES, Esq., F.R.S.

[From the "Quarterly Journal of Science," October 1, 1871.]

SOME FURTHER EXPERIMENTS ON PSYCHIC FORCE.

"I am attacked by two very opposite sects,—the scientists and the know-nothings. Both laugh at me—calling me 'the frogs' dancing master.' Yet I know that I have discovered one of the greatest forces in nature."—GALVANI.

IT was my intention to have allowed a longer time to elapse before again writing on the subject of "Psychic Force" in this journal. My reason for this resolve was not so much owing to want of new matter and fresh results,—on the contrary, I have much that is new in the way of experimental evidence in support of my previous conclusions,—but I felt some reluctance to impose on the readers of the "Quarterly Journal of Science" a subject which they might view with little favour. When the editor of a scientific journal is also an experimental investigator, or a student of any special branch of knowledge, there is a natural tendency on his part to unduly exalt the importance of that which is occupying his thoughts at the time; and thus the journal which he conducts is in danger of losing breadth of basis, of becoming the advocate of certain opinions, or of being coloured by special modes of thought.

The manner in which the experimental investigation described in the last "Quarterly Journal" has been received, removes any doubt I might entertain on this score. The very numerous communications which have been addressed to the office of this journal show that another paper on the same subject will not be distasteful to a large number of those who did me the honour to read my former article; whilst it appears to be generally assumed that I should take an early opportunity to reply to some of the criticisms provoked by the remarkable character of the experimental results which I described.

Many of the objections made to my former experiments are answered by the series about to be related. Most of the criticisms to which I have been subjected have been perfectly fair and courteous, and these I shall endeavour to meet in the fullest possible manner. Some critics, however, have fallen into the error of regarding me as an advocate for certain *opinions*, which they choose to ascribe to me, though in truth my single purpose has been to state fairly and to offer no opinion. Having evolved men of straw from their own imagination, they proceed vigorously to slay them,

under the impression that they are annihilating me. Others,—and I am glad to say they are very few,—have gone so far as to question my veracity:—"Mr. Crookes must get better witnesses before he can be believed!" Accustomed as I am to have my word believed without witnesses, this is an argument which I cannot condescend to answer. All who know me and read my articles will, I hope, take it for granted that the *facts* I lay before them are correct, and that the experiments were honestly performed, with the single object of eliciting *the truth*.

It is edifying to compare some of the present criticisms with those that were written twelve months ago. When I first stated in this journal that I was about to investigate the phenomena of so-called spiritualism, the announcement called forth universal expressions of approval. One said that my "statements deserved respectful consideration;" another, expressed "profound satisfaction that the subject was about to be investigated by a man so thoroughly qualified as," &c.; a third was "gratified to learn that the matter is now receiving the attention of cool and clear-headed men of recognised position in science;" a fourth asserted that "no one could doubt Mr. Crookes's ability to conduct the investigation with rigid philosophical impartiality;" and a fifth was good enough to tell its readers that "if men like Mr. Crookes grapple with the subject, taking nothing for granted until it is proved, we shall soon know how much to believe."

These remarks, however, were written too hastily. It was taken for granted by the writers that the results of my experiments would be in accordance with their preconceptions. What they really desired was not *the truth*, but an additional witness in favour of their own foregone conclusion. When they found that the facts which that investigation established could not be made to fit those opinions, why,—"so much the worse for the facts." They try to creep out of their own confident recommendations of the enquiry by declaring that "Mr. Home is a clever conjuror, who has duped us all." "Mr. Crookes might, with equal propriety, examine the performances of an Indian juggler." "Mr. Crookes must get better witnesses before he can be believed." "The thing is too absurd to be treated seriously." "It is impossible, and therefore can't be."* "The observers have all been biologised (!) and fancy they saw things occur which really never took place," &c., &c.

* The quotation occurs to me—"I never said it was possible, I only said it was true."

These remarks imply a curious oblivion of the very functions which the scientific enquirer has to fulfil. I am scarcely surprised when the objectors say that I have been deceived merely because they are unconvinced without personal investigation, since the same unscientific course of *à priori* argument has been opposed to all great discoveries. When I am told that what I describe cannot be explained in accordance with preconceived ideas of the laws of nature, the objector really begs the very question at issue and resorts to a mode of reasoning which brings science to a standstill. The argument runs in a vicious circle: we must not assert a fact till we know that it is in accordance with the laws of nature, while our only knowledge of the laws of nature must be based on an extensive observation of facts. If a new fact seems to oppose what is called a law of nature, it does not prove the asserted fact to be false, but only that we have not yet ascertained all the laws of nature, or not learned them correctly.

In his opening address before the British Association at Edinburgh this year, Sir William Thomson said, "Science is bound by the everlasting law of honour to face fearlessly every problem which can fairly be presented to it." My object in thus placing on record the results of a very remarkable series of experiments is to present such a problem, which, according to Sir W. Thomson, "Science is bound by the everlasting law of honour to face fearlessly." It will not do merely to deny its existence, or try to sneer it down. Remember, I hazard no hypothesis or theory whatever; I merely vouch for certain facts, my only object being—the *truth*. Doubt, but do not deny; point out, by the severest criticism, what are considered fallacies in my experimental tests, and suggest more conclusive trials; but do not let us hastily call our senses lying witnesses merely because they testify against preconceptions. I say to my critics, Try the experiments; investigate with care and patience as I have done. If, having examined, you discover imposture or delusion, proclaim it and say how it was done. But, if you find it be a fact, avow it fearlessly, as "by the everlasting law of honour" you are bound to do.

I may at once answer one objection which has been made in several quarters, viz., that my results would carry more weight had they been tried a greater number of times, and with other persons besides Mr. Home. The fact is, I have been working at the subject for two years, and have found nine or ten different persons who possess psychic power in

more or less degree; but its development in Mr. D. D. Home is so powerful, that, having satisfied myself by careful experiments that the phenomena observed were genuine, I have, merely as a matter of convenience, carried on my experiments with him, in preference to working with others in whom the power existed in a less striking degree. Most of the experiments I am about to describe, however, have been tried with another person other than Mr. Home, and in his absence.

Before proceeding to relate my new experiments, I desire to say a few words respecting those already described. The objection has been raised that announcements of such magnitude should not be made on the strength of one or two experiments hastily performed. I reply that the conclusions were not arrived at hastily, nor on the results of two or three experiments only. In my former paper ("Quarterly Journal of Science," page 340), I remarked:—"Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality." Before fitting up special apparatus for these experiments, I had seen, on five separate occasions, objects, varying in weight from 25 to 100 lbs., temporarily influenced in such a manner, that I, and others present, could with difficulty lift them from the floor. Wishing to ascertain whether this was a physical fact, or merely due to a variation in the power of our own strength under the influence of imagination, I tested with a weighing machine the phenomenon on two subsequent occasions when I had an opportunity of meeting Mr. Home at the house of a friend. On the first occasion, the increase of weight was from 8 lbs. normally, to 36 lbs., 48 lbs., and 46 lbs. in three successive experiments tried under strict scrutiny. On the second occasion, tried about a fortnight after, in the presence of other observers, I found the increase of weight to be from 8 lbs. to 23 lbs., 43 lbs., and 27 lbs., in three successive trials, varying the conditions. As I had the entire management of the above-mentioned experimental trials, employed an instrument of great accuracy, and took every care to exclude the possibility of the results being influenced by trickery, I was not unprepared for a satisfactory result when the fact was properly tested in my own laboratory. The meeting on the occasion formerly described was, therefore, for the purpose of confirming my previous observations by the application of crucial tests, with carefully arranged apparatus of a still more delicate nature.

That this is a legitimate subject for scientific inquiry scarcely needs assertion. Faraday himself did not consider it beneath his dignity to examine similar phenomena; and, in a letter to Sir Emerson Tennent, written in 1861 on the occasion of a proposed experimental inquiry into the phenomena occurring in Mr. Home's presence, he wrote:—"Is he (Mr. Home) willing to investigate as a philosopher, and, as such, to have no concealments, no darkness, to be open in communication, and to aid inquiry all that he can? . . . . Does he consider the effects natural or supernatural? If they be the glimpses of natural action not yet reduced to law, ought it not to be the duty of everyone who has the least influence in such actions personally to develope them, and to aid others in their development, by the utmost openness and assistance, and by the application of every critical method, either mental or experimental, which the mind of man can devise?"

If circumstances had not prevented Faraday from meeting Mr. Home, I have no doubt he would have witnessed phenomena similar to those I am about to describe, and he could not have failed to see that they offered "glimpses of natural action not yet reduced to law."

I have already alluded to the publication of the ill-success encountered by the members of the St. Petersburg Committee. Had the results been satisfactory, it must be fairly assumed that the members would have been equally ready to publish a report of their success.

I am informed by my friend Professor Boutlerow,* that during the last winter, he tried almost the same experiments as those here detailed, and with still more striking results. The normal tension on the dynamometer being 100 lbs., it was increased to about 150 lbs., Mr. Home's hands being placed in contact with the apparatus in such a manner that any exertion of power on his part would diminish, instead of increase, the tension.

In 1854, Count Agénor de Gasparin published a book,† giving full details of a large series of physical experiments which he had tried with some private friends in whom this force was found to be strongly developed. His experiments were very numerous and were carried on under the strictest test conditions. The fact of motion of heavy bodies without mechanical contact was demonstrated over and over

* Professor of Chemistry at the University of St. Petersburg; author of a work on Chemistry, entitled "Lehrbuch der Organischen Chemie;" Leipzig, 1868.

† Science *versus* Spiritualism. Paris, 1854. New York, 1857.

again. Careful experiments were made to measure the force both of gravitation and of levitation thus communicated to the substances under trial, and an ingenious plan was adopted by which Count de Gasparin was enabled to obtain a rough numerical estimate of the power of the psychic force in each individual. The author finally arrived at the conclusion that all these phenomena are to be accounted for by the action of natural causes, and do not require the supposition of miracles nor the intervention of spirits or diabolical influences. He considers it as a fact fully established by his experiments, that the will, in certain states of the organism, can act at a distance on inert matter, and most of his work is devoted to ascertaining the laws and conditions under which this action manifests itself.

In 1855, M. Thury, a Professor at the Academy of Geneva, published a work,* in which he passed in review Count de Gasparin's experiments, and entered into full details of researches he had been simultaneously carrying on. Here, also, the trials were made with private friends, and were conducted with all the care which a scientific man could bring to bear on the subject. Space will not allow me to quote the valuable numerical results obtained by M. Thury, but from the following headings of some of his chapters, it will be seen that the enquiry was not conducted superficially :—Facts which Establish the Reality of the New Phenomenon ; Mechanical Action rendered Impossible; Movements effected without Contact ; The Causes ; Conditions requisite for the Production and Action of the Force ; Conditions for the Action with Respect to the Operators ; The Will ; Is a Plurality of Operators Necessary ? Preliminary Requisites ; Mental Condition of the Operators ; Meteorological Conditions; Conditions with Respect to the Instruments Operated upon ; Conditions relative to the Mode of Action of the Operators on the Instruments ; Action of Substances interposed ; Production and Transmission of the Force ; Examination of the Assigned Causes ; Fraud ; Unconscious Muscular Action produced in a particular Nervous State ; Electricity ; Nervo-magnetism; M. de Gasparin's Theory of a Special Fluid ; General Question as to the Action of Mind on Matter. 1st Proposition : In the ordinary conditions of the body the will only acts directly within the sphere of the organism. 2nd Proposition : Within the organism itself there are a series of mediate acts. 3rd Proposition : The substance on which the mind

* Geneva; Librairie Allemande de J. Kessmann. 1855.

acts directly—the *psychode*—is only susceptible of very simple modification under the influence of the mind; Explanations which are based on the Intervention of Spirits. M. Thury refutes all these explanations, and considers the effects due to a peculiar substance, fluid, or agent, pervading, in a manner similar to the luminiferous ether of the scientist, all matter, nervous, organic, or inorganic—which he terms *psychode*. He enters into full discussion as to the properties of this state or form of matter, and proposes the term *ectenic force* (ἐκτένια, extension), for the power exerted when the mind acts at a distance through the influence of the psychode.*

There is likewise another case on record in which similar test experiments were tried, with like results, by a thoroughly competent observer. The late Dr. Robert Hare, in one of his works,† gives an engraving of an apparatus very similar to my own, by which the young man with whom he was experimenting was prevented from having any other communication with the apparatus except through water; yet, under these circumstances, the spring balance indicated the exertion of a force equal to 18 lbs. The details of this experiment were communicated by Dr. Hare to the American Association for the Advancement of Science, at the meeting in August, 1855.

The references I now give afford an answer to the statement that these results must be verified by others. They have been verified over and over again. Indeed, my own experiments may be regarded merely as verifications of results already obtained and published by eminent scientific men in this and other countries.‡

But I was not content with this. I felt that having the opportunity of showing these phenomena to others, I might

* Professor Thury's ectenic and my psychic force are evidently equivalent terms. Had I seen his work three months ago I should have adopted his term. The suggestion of a similar hypothetical nervous fluid has now reached us from another and totally different source, expounded with distinct views, and couched in the language of one of the most important professions—I allude to the theory of a nervous atmosphere advanced by Dr. Benjamin W. Richardson, M.D., F.R.S., in the "Medical Times," No. 1088, May 6, 1871.

† "Experimental Investigation;" By Robert Hare, M.D., Emeritus Professor of Chemistry in the University of Pennsylvania, &c. New York: Partridge and Britton, 1858.

‡ The Report of the Dialectical Society on Spiritualism will appear in a few days, and it will be seen that the Investigation Committee, though commencing their experiments with the entire conviction that they should expose an imposture, have ended by affirming that they are convinced of the existence of a force emanating from the human organisation, by which motion may be imparted to heavy substances, and audible sounds made on solid bodies without muscular contact; they also state that this force is often directed by some intelligence.

at a future time be blamed did I not, once for all, take the very best means of bringing them before the notice of the scientific world. Accordingly I forwarded an account of my experiments to the Royal Society on June 15, 1871, and addressed myself to the two secretaries of the Royal Society, Professor Sharpey and Professor Stokes, inviting them to my house to meet Mr. Home, at the same time asking them to be prepared for negative results, and to come a second, or, if necessary, a third time, before forming a judgment.

Dr. Sharpey politely declined the invitation.

Professor Stokes replied that he thought there was a fallacy in my apparatus, and concluded by saying—

"The facts you mentioned in the paper were certainly at first sight very strange, but still possible modes of explanation occurred to me which were not precluded by what I read in the paper. If I have time when I go to London I will endeavour to call at your house. I don't want to meet anyone; my object being to scrutinise the apparatus, not to witness the effects."

To this I replied on June 20th; the following extracts are taken from my answer:—

"I am now fitting up apparatus in which contact is made through water only, in such a way that transmission of mechanical movement to the board is impossible; and I am also arranging an experiment in which Mr. Home will not touch the apparatus at all. This will only work when the power is very strong; but last night I tried an experiment of this kind, and obtained a considerable increase of tension on the spring balance when Mr. Home's hands were three inches off. With him the power is so great that I can work with large and crude materials, and measure the force in pounds. But I propose to make a delicate apparatus, with a mirror and reflected ray of light, to show fractions of grains. Then I hope to find this force is not confined to a few, but is, like the magnetic state, universal. The subject shall have a 'most scrupulously searching physical scrutiny,' and whatever results I obtain shall be published. I consider it my duty to send first to the Royal Society, for by so doing I deliberately stake my reputation on the truth of what I send. But will the Society (or the Committee*) accept my facts as facts, or will they require vouchers for my integrity? If my statements of fact are taken as correct, and only my interpretation or arrangements of apparatus objected to, then it would seem to be right to give me an opportunity of answering these objections before finally deciding. The other supposition—that my facts are incorrect—I cannot admit the discussion of till I am definitely assured that such is entertained.

"Mr. Home is coming here on Wednesday and Friday evenings; if you can come on either or both occasions at 8 p.m., I shall be glad to see you, or if you only wish to scrutinise the apparatus, I will be here at any time you like to name."

On the 28th of June another paper was sent to the Royal Society. Two days after, Professor Stokes wrote a letter, from which I quote:—

"As I was otherwise engaged so as not to be able conveniently to go to your house, I may as well mention the possible sources of error which occurred to me with reference to your first apparatus. I don't suppose they all exist;

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* Alluding to a rumoured rejection of my paper by the Committee of the Royal Society.

but it is evidently, as you yourself would freely admit, for the *assertor* of a new force to remove all sources of reasonable objection.

"The breadth of the foot of the board was, I think, 1½ or 2 inches, and the bell placed on it was, perhaps, 2 or 3 inches broad. (I can't carry the exact figures in my head.) Join the left edge* of the top of the bell, *a*, with the right hand edge, *b*, of the base of the bell, and let *e f* be the joining line. Then we may suppose the fingers to have pressed in any direction short of the limiting line *e f*. Also as the board was rigid, the fulcrum for aught we know may have been at *c*. From *c* let fall a perpendicular *c m* on the line, *e f*. Then the pressure of the finger may have acted at the distance, *c m*, from the fulcrum. Also, as the base lay flat on the table and both were rigid, for aught we know an infinitesimal, and therefore imperceptible, tilt communicated to the table at the time of trying the experiment may have shifted the fulcrum from the edge *d* to the edge *c*, so that the weight of the hand may have acted by an arm longer than before by *c d*, which would have contributed to the result.

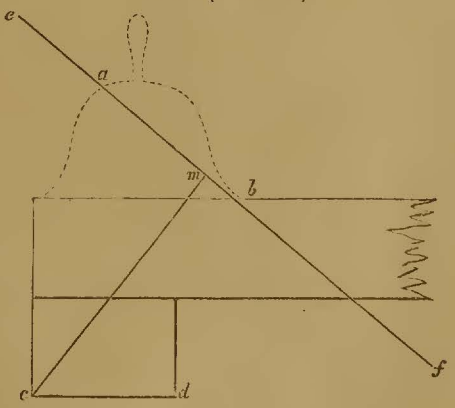
"In your second paper the uncertainty as to the broad bearing is removed. But when the hand was dipped into the water the pressure on the base of the glass vessel (after a little time if the connecting hole be narrow) is increased by the weight of the water displaced, and that would of course depress the balance.

"I don't think much of mere tremors, for it would require very elaborate appliances to prove that they were not due to a passing train or omnibus or to a tremor in the body of one of the company. . . . . What do you wish to be done with the papers?"

To this I replied as follows, on July 1st:—

"In your letter of the 30th ult., just received, you are quite right in saying that I would freely admit that 'the *assertor* of a new force should remove all sources of reasonable objection.' In your previous letter of the 19th June, you write with equal fairness, that 'your opinion is that you (the R. S.) ought not to refuse to admit evidence of the existence of a hitherto unsuspected

FIG. 1 (half-scale).



force; but that before printing anything on such a subject, you ought to require a most scrupulously searching physical scrutiny of the evidence adduced in favour of the existence of such a force.'

---

* The diagram referred to here is shown, drawn to scale, in my answer further on. The experiment under discussion is the one figured and described in the last number of the "Quarterly Journal of Science," page 345.

"You have now been good enough to explain to me in detail what the fallacy is which you think exists in my first experiments, and what you consider to be the possible sources of error in my subsequent trials.

"On re-drawing the diagram you give in your letter, Fig. 1, to the full size, supplying the deficient data, viz., the position of the shoulder, *a*, and the point, *b*, your line *c m* appears to be about 2·9 inches long; and, as you assume that the fulcrum shall be at *c*, the lever becomes one of the third order, the two forces acting respectively at $p = 2{\cdot}9$ inches, and at $q = 36$ inches from *c*. What power, P, must be exerted at p to overcome a resistance or weight, Q, of 6 lbs. at the end of the lever, q?

$$Pp = Qq.$$

$$\text{Hence, } P \times 2{\cdot}9 = Q \times 36.$$

$$\therefore P = 74{\cdot}5 \text{ lbs.}$$

Therefore, it would have required a force of 74·5 lbs. to have been exerted by Mr. Home to have produced the results, even if all your suppositions are granted; and, considering that he was sitting in a low easy chair, and four pairs of sharp, suspicious eyes were watching to see that he exerted no force at all, but kept the tips of his fingers lightly on the instrument, it is sufficiently evident that an exertion of this pressure was impossible. A few pounds vertical pressure was all he could have effected.

"Again, you are not justified in assuming that the fulcrum was at *c*. Granting that 'an infinitesimal and therefore imperceptible tilt' might, at the very first movement, have thrown it from *d* to *c*, it is evident that the movement would at once throw it forward again from *c* to *d*. To have failed to have done so, the tilt must have been so obvious as to have been detected at once.

"But, as I said in my last paper, I prefer to appeal to new experiments rather than argue about old ones, and hence my employment of the water for transmitting the force. The depth of water in the copper hemisphere was only 1⅛ inches, whilst the glass vessel was 9 inches in diameter.* I have just tried the experiment of immersing my hand to the very utmost in the copper vessel (Mr. Home only dipped in the tips of his fingers) and the rise of the level of the water is not sufficient to produce any movement whatever on the index of the balance, the friction of the apparatus being enough to absorb the ounce or two thus added to the weight. In my more delicate apparatus, this increase of hydrostatic pressure produces a decided movement of the spot of light, but this difficulty I shall overcome by placing the water vessel over the fulcrum, or on the short side of it.

"You say 'you don't think much of mere tremors,' as if in the other experiments described in my second paper the movements of the apparatus were only of this kind. This is not the case; the quivering of the apparatus always took place before the index moved, and the upward and downward motion of the board and index was of a very slow and deliberate character, occupying several seconds for each rise and fall; a tremor produced by passing vehicles is a very different thing from a steady vertical pull of from 4 to 8 lbs., lasting for several seconds.

"You say the session is now over, and ask what I wish to be done with the papers.

"Three years ago (June 27th, 1868), I sent a paper to the Society, 'On the Measurement of the Luminous Intensity of Light,' just after the session closed. It was not read till December 17th. My *wish* would be for a similar course to be adopted in the present instance, although I am scarcely sanguine enough to expect that so much notice will be taken of these communications. So many scientific men are now examining into these strange phenomena (including many Fellows of the Society), that it cannot be many years before the subject will be brought before the scientific world in a way that will enforce attention. I confess that, in sending in these papers to the Society, I have been actuated more by the desire of being the first scientific experimenter

* For a description of this apparatus, see p. 484.

who has ventured to take such a course, than by any particular desire that they should meet with immediate attention. I owe to the Society the first intimation of important scientific results, and these I shall continue to send, '*pour prendre date*,' if for no other reason."

"The Spectator" of July 22nd contained an editorial note, in which it is asserted that my paper was declined by the committee :—

"The Royal Society, they say, was quite open to communications advocating the existence of a force in nature as yet unknown, if such communications contained scientific evidence adequate to establish its probability; but that, looking to the inherent improbability of the case as stated by Mr. Crookes, and the *entire want of scientific precision* in the evidence adduced by him, the paper was not regarded as one deserving the attention of the Royal Society."

This paragraph not only states that my papers were declined, but proceeds to state the grounds of their rejection. The fact is, that a quorum of the committee of papers not having been present, the question was deferred to the next session in November, and on inquiry at Burlington House, I am informed by the Assistant-Secretary of the Royal Society that my papers, with others, are still awaiting the decision of the committee. Consequently the statement of a rejection was not only premature, but purely imaginary.

It appears, however, that there were some grounds for this statement, for in "The Spectator" of July 29th, 1871, the editor replies as follows :—

"Our note was not founded on any mere rumour. The words we used contained an exact copy of the words conveyed to us as used, not, as we inadvertently stated by the committee, but by one of the secretaries, Professor Stokes, who in the absence of a quorum, exercised *pro tempore* the usual discretionary authority in regard to papers offered."

I am unable to explain how it is that Professor Stokes's statements to me and to the editor of "The Spectator" bear so different an interpretation, or why a weekly newspaper was chosen for first conveying to me a decision of the committee of papers of the Royal Society.

At the urgent request of gentlemen on the committee of section A, I communicated a paper consisting of about sixteen closely-written pages to the British Association, in which I recounted some of the experiments described in the present paper. Section A referred the paper to a committee to decide whether it should be read. Professor Stokes afterwards handed to me the following document :—

"*Report on Mr. Crookes's Paper.*

"August 7, 1871.

"The paper having been placed in my hands about ten o'clock, and a decision wanted in writing by a quarter to eleven, I have been obliged to be hasty.

"The subject *seems to be investigated in a philosophical spirit*, and I do not see the explanation of the result of the first class of experiments, while at

the same time I am not prepared to give in my adhesion without a thorough sifting by more individuals than one. I don't see much use discussing the thing in the sections, crowded as we already are: but if a small number of persons in whom the public would feel confidence choose to volunteer to act as members of a committee for investigating the subject, I don't see any objection to appointing such committee. I have heard too much of the tricks of Spiritualists to make me willing to give my time to such a committee myself.

"G. G. Stokes."

Whilst I cannot but regret that a physicist of such eminence as Professor Stokes should "be hasty," in deciding on the merits of a paper which it is physically impossible he could have even once read through, I am glad to find that he no longer continues to speak of the "entire want of scientific precision in the evidence adduced" by me, but rather admits that "the subject seems to be investigated in a philosophical spirit."

In submitting these experiments, it will not seem strange that I should consider them final until rebutted by arguments also drawn from facts, and that I should seek to know on what grounds contra-statements are founded. Professor Allen Thomson, at the recent meeting of the British Association, remarked that no course of inquiry into the matter before us "can deserve the name of study or investigation." And why not? On the other hand, Professor Challis, of Cambridge, writes, "In short, the testimony has been so abundant and consentaneous, that either the facts must be admitted to be such as are reported, or the possibility of certifying facts by human testimony must be given up." It is certainly not too much to suppose that Dr. Thomson had some grounds for his statement; and, indeed, "I have," he owns, "been fully convinced of this (the fallacies of spiritualistic demonstration) by repeated examinations;" but where are the results of his investigations to be found? They must be very conclusive to warrant him in the use of such expressions as "a few men of acknowledged reputation in some departments of science have surrendered their judgments to these foolish dreams, *otherwise* appearing to be within the bounds of sanity." If Dr. Thomson's dogmatic denial arises from the mere strangeness of the facts I have set forth, what can he think of the address of the President for this year. Surely the conception of a nerve-force is no more difficult than that "of the inner mechanism of the atom;" and again, any investigation, be it worthy the name or not, bearing on a matter in which eminent men have avowed their belief, which takes a leading rank among the social questions of the day, and which numbers its adherents by millions, is surely as full of merit, and as instructive to all, as hypothetical

inquiries into "interatomic atmospheres" and "gyrating interatomic atoms." Professor Huxley has observed, "If there is one thing clear about the progress of modern science, it is the tendency to reduce all scientific problems, except those that are purely mathematical, to problems in molecular physics—that is to say, to attractions, repulsions, motions, and co-ordination of the ultimate particles of matter! Yet these ultimate particles, molecules, or atoms, are creatures of the imagination, and as pure assumptions as the spirits of the spiritualist." But perhaps Dr. Allen Thomson's respect for mathematics is so great that he is blind to actuality. It does not speak well for modern scientific philosophy that, after the startling revelations of the spectroscope during the last decade, investigations should be scouted because they pertain to an ulterior state of things of which at present we have little idea. That I have furnished no dynamic equivalent of psychic force, or given no formulæ for the variable intensity of Mr. Home's power, is certainly no argument whatever against the existence of such a force. Men thought before the syllogism was invented, and, strange as it may seem to some minds, force existed before its demonstration in mathematical formulæ.

As an answer to Professor Balfour Stewart's rather bold conjecture, that Mr. Home possesses great electro-biological power (whatever that may mean), by which he influences those present, I point to the curves illustrating this paper; however susceptible the *persons* in the room might have been to that assumed influence, it will hardly be contended that Mr. Home biologised the recording instruments.

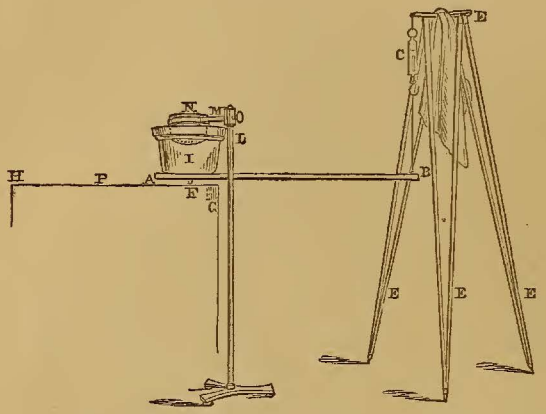
I will not occupy more time with personal matters, or with explanations forced from me in self-defence against uncourteous commentaries based on unjust misrepresentations; but I will proceed to describe the experiments, most of which, I may remark, might have been witnessed by Professor Stokes and Professor Sharpey, had they accepted the invitations I gave them.

On trying these experiments for the first time, I thought that actual contact between Mr. Home's hands and the suspended body whose weight was to be altered was essential to the exhibition of the force; but I found afterwards that this was not a necessary condition, and I therefore arranged my apparatus in the following manner:—

The accompanying cuts (Figs. 2, 3, 4) explain the arrangement. Fig. 2 is a general view, and Figs. 3 and 4 show the essential parts more in detail. The reference

letters are the same in each illustration. A B is a mahogany board, 36 inches long by 9½ inches wide, and 1 inch thick. It is suspended at the end, B, by a spring balance, C,

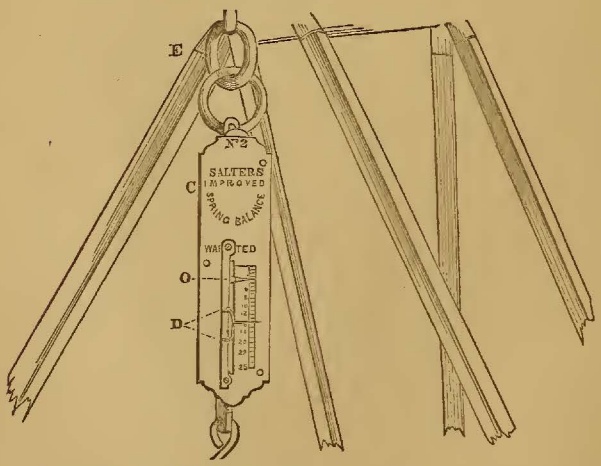
Fig. 2.



furnished with an automatic register, D. The balance is suspended from a very firm tripod support, E.

The following piece of apparatus is not shown in the figures. To the moving index, O, of the spring balance, a

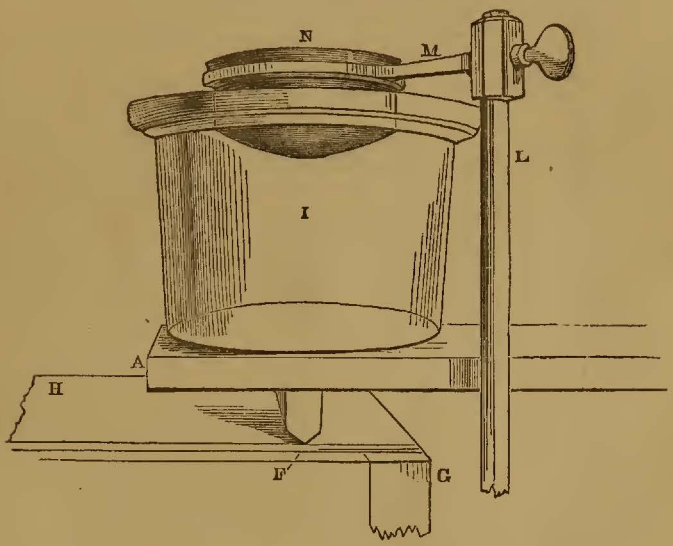
Fig. 3.



fine steel point is soldered, projecting horizontally outwards. In front of the balance, and firmly fastened to it, is a

grooved frame carrying a flat box similar to the dark box of a photographic camera. This box is made to travel by clock-work horizontally in front of the moving index, and it contains a sheet of plate-glass which has been smoked over a flame. The projecting steel point impresses a mark on this smoked surface. If the balance is at rest, and the clock set going, the result is a perfectly straight horizontal line. If the clock is stopped and weights are placed on the

Fig. 4.



end B of the board, the result is a vertical line, whose length depends on the weight applied. If, whilst the clock draws the plate along, the weight of the board (or the tension on the balance) varies, the result is a curved line, from which the tension in grains at any moment during the continuance of the experiments can be calculated.

The instrument was capable of registering a diminution of the force of gravitation as well as an increase; registrations of such a diminution were frequently obtained. To avoid complication, however, I will only here refer to results in which an increase of gravitation was experienced.

The end B of the board being supported by the spring balance, the end A is supported on a wooden strip, F, screwed across its lower side and cut to a knife edge (see Fig. 4). This fulcrum rests on a firm and heavy wooden

and, G H. On the board, exactly over* the fulcrum, is placed a large glass vessel filled with water, I. L is a massive iron stand, furnished with an arm and a ring, M N, in which rests a hemispherical copper vessel perforated with several holes at the bottom.

The iron stand is 2 inches from the board A B, and the arm and copper vessel, M N, are so adjusted that the latter dips into the water $1\frac{1}{2}$ inches, being $5\frac{1}{2}$ inches from the bottom of I, and 2 inches from its circumference. Shaking or striking the arm M, or the vessel N, produces no appreciable mechanical effect on the board, A B, capable of affecting the balance. Dipping the hand to the fullest extent into the water in N does not produce the least appreciable action on the balance.

As the mechanical transmission of power is by this means entirely cut off between the copper vessel and the board A B, the power of muscular control is thereby completely eliminated.

For convenience I will divide the experiments into groups 1, 2, 3, &c., and I have selected one special instance in each to describe in detail. Nothing, however, is mentioned which has not been repeated more than once, and in some cases verified, in Mr. Home's absence, with another person possessing similar powers.

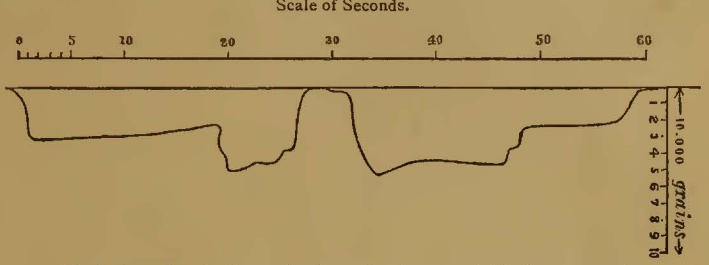
There was always ample light in the room where the experiments were conducted (my own dining-room) to see all that took place.

Experiment I.—The apparatus having been properly adjusted before Mr. Home entered the room, he was brought in, and asked to place his fingers in the water in the copper vessel, N. He stood up and dipped the tips of the fingers of his right hand in the water, his other hand and his feet being held. When he said he felt a power, force, or influence, proceeding from his hand, I set the clock going, and almost immediately the end B of the board was seen to descend slowly and remain down for about 10 seconds; it then descended a little further, and afterwards rose to its normal height. It then descended again, rose suddenly, gradually sunk for 17 seconds, and finally rose to its normal height, where it remained till the experiment was concluded. The lowest point marked on the glass was equivalent to a direct pull of about 5000 grains. The accompanying figure (5) is a copy of the curve traced on the glass.

* In my first experiments with this apparatus, referred to in Professor Stokes's letter and my answer (page 479), the glass vessel was not quite over the fulcrum, but was nearer B.

Experiment II.—Contact through water having proved to be as effectual as actual mechanical contact, I wished to see if the power or force could affect the weight, either through other portions of the apparatus or through the air. The glass vessel and iron stand, &c., were therefore removed,

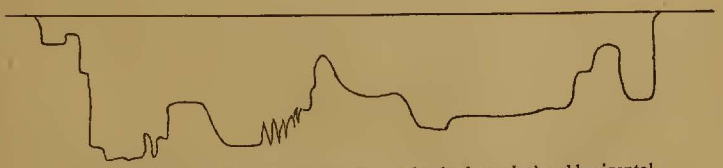
FIG. 5.



The horizontal scale of seconds shows the time occupied in the movements, the experiment lasting one minute. The vertical scale shows the tension in grains exerted on the balance at any moment.

as an unnecessary complication, and Mr. Home's hands were placed on the stand of the apparatus at P (Fig. 2). A gentleman present put his hand on Mr. Home's hands, and his foot on both Mr. Home's feet, and I also watched him closely all the time. At the proper moment the clock was

FIG. 6.

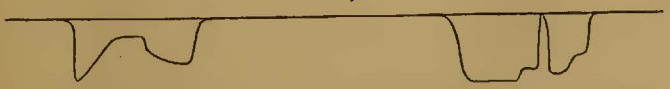


In this and the two following figures the scales, both vertical and horizontal, are the same as in Fig. 5.

again set going; the board descended and rose in an irregular manner, the result being a curved tracing on the glass, of which Fig. 6 is a copy.

Experiment III.—Mr. Home was now placed one foot from the board A B, on one side of it. His hands and feet

FIG. 7.



were firmly grasped by a bystander, and another tracing, of which Fig. 7 is a copy, was taken on the moving glass plate.

Experiment IV.—(Tried on an occasion when the power was stronger than on the previous occasions). Mr. Home was now placed 3 feet from the apparatus, his hands and feet being tightly held. The clock was set going when he gave the word, and the end B of the board soon descended, and again rose in an irregular manner, as shown in Fig. 8.

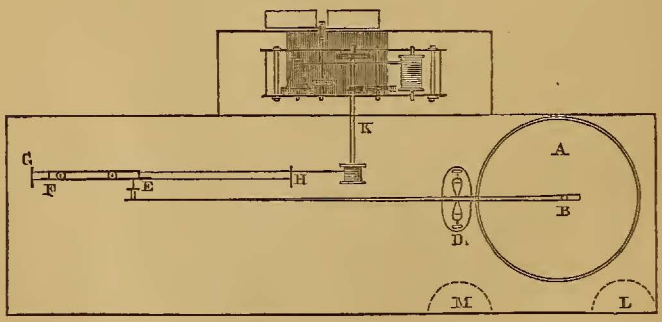
The following series of experiments were tried with more delicate apparatus, and with another person, a lady, Mr. Home being absent. As the lady is non-professional, I do

FIG. 8.

not mention her name. She has, however, consented to meet any scientific men whom I may introduce for purposes of investigation.

A piece of thin parchment, A, Figs. 9 and 10, is stretched tightly across a circular hoop of wood. B C is a light lever turning on D. At the end B is a vertical needle point touching the membrane A, and at C is another needle point, projecting

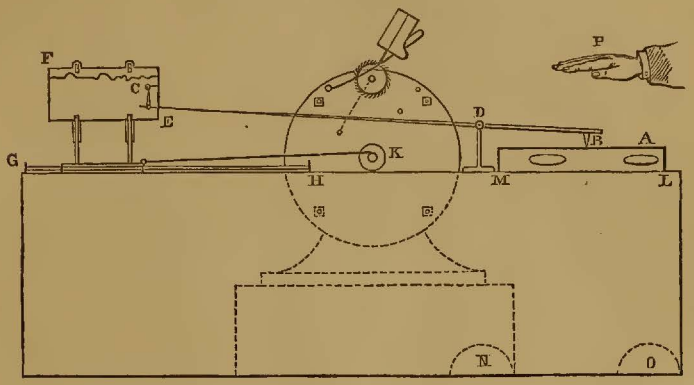
FIG. 9. (Plan.)



horizontally and touching a smoked glass plate, E F. This glass plate is drawn along in the direction H G by clock-work, K. The end B of the lever is weighted so that it shall quickly follow the movements of the centre of the disc, A. These movements are transmitted and recorded on the glass plate E F, by means of the lever and needle

point C. Holes are cut in the side of the hoop to allow a free passage of air to the under side of the membrane. The apparatus was well tested beforehand by myself and others, to see that no shaking or jar on the table or support would interfere with the results: the line traced by the point C

FIG. 10. (Section.)



on the smoked glass was perfectly straight in spite of all our attempts to influence the lever by shaking the stand or stamping on the floor.

Experiment V.—Without having the object of the instrument explained to her, the lady was brought into the room and asked to place her fingers on the wooden stand at the points L M, Fig. 9. I then placed my hands over hers to enable me to detect any conscious or unconscious movement on her part. Presently percussive noises were heard on the parchment resembling the dropping of grains of sand on its surface. At each percussion a fragment of graphite which I had placed on the membrane was seen to be projected upwards about 1-50th of an inch, and the end C of the lever moved slightly up and down. Sometimes the sounds were as rapid as those from an induction-coil, whilst at others they were more than a second apart. Five or six tracings were taken, and in all cases a movement of the end C of the lever was seen to have occurred with each vibration of the membrane.

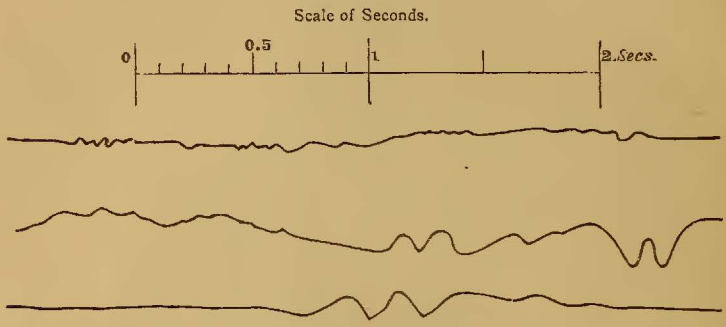
In some cases the lady's hands were not so near the membrane as L M, but were at N O, Fig. 10.

The accompanying Fig. 11 gives tracings taken from the plates used on these occasions.

Experiment VI.—Having met with these results in Mr. Home's absence, I was anxious to see what action would be produced on the instrument in his presence.

Accordingly I asked him to try, but without explaining the instrument to him.

FIG. 11.



I grasped Mr. Home's right arm above the wrist and held his hand over the membrane, about 10 inches from its surface, in the position shown at P, Fig. 10. His other hand was held by a friend. After remaining in this position for about half a minute, Mr. Home said he felt some influence passing. I then set the clock going, and we all saw the index, C, moving up and down. The movements were much slower than in the former case, and were almost entirely unaccompanied by the percussive vibrations then noticed.

Figs. 12 and 13 show the curves produced on the glass on two of these occasions.

Figs. 11, 12, 13 are magnified.

These experiments *confirm beyond doubt* the conclusions at which I arrived in my former paper, namely, the existence of a force associated, in some manner not yet explained, with the human organisation, by which force, increased weight is capable of being imparted to solid bodies without physical contact. In the case of Mr. Home, the development of this force varies enormously, not only from week to week, but from hour to hour; on some occasions the force is inappreciable by my tests for an hour or more, and then suddenly reappears in great strength. It is capable of acting at a distance from Mr. Home (not unfrequently as far as two or three feet), but is always strongest close to him.

Being firmly convinced that there could be no manifestation of one form of force without the corresponding expenditure of some other form of force, I for a long time searched

in vain for evidence of any force or power being used up in the production of these results.

Now, however, having seen more of Mr. Home, I think I perceive what it is that this psychic force uses up for its development. In employing the terms *vital force*, or *nervous energy*, I am aware that I am employing words which convey very different significations to many investigators; but after witnessing the painful state of nervous and bodily prostration in which some of these experiments have left

FIG. 12.

Mr. Home—after seeing him lying in an almost fainting condition on the floor, pale and speechless—I could scarcely doubt that the evolution of psychic force is accompanied by a corresponding drain on vital force.

I have ventured to give this new force the name of *Psychic Force*, because of its manifest relationship to certain psychological conditions, and because I was most desirous to avoid the foregone conclusions implied in the title under

FIG. 13.

which it has hitherto been claimed as belonging to a province beyond the range of experiment and argument. But having found that it is within the province of purely scientific research, it is entitled to be known by a scientific name, and I do not think a more appropriate one could have been selected.

To witness exhibitions of this force it is not necessary to have access to known psychics. The force itself is probably possessed by all human beings, although the individuals endowed with an extraordinary amount of it are doubtless

few. Within the last twelve months I have met in private families five or six persons possessing a sufficiently vigorous development to make me feel confident that similar results might be produced through their means to those here recorded, provided the experimentalist worked with more delicate apparatus, capable of indicating a fraction of a grain instead of recording pounds and ounces only.

As far as my other occupations will permit, I purpose to continue the experiments in various forms, and I will report from time to time their results. In the meanwhile I trust that others will be induced to pursue the investigation in its scientific form. It should, however, be understood that, equally with all other scientific experiments, these researches must be conducted in strict compliance with the conditions under which the force is developed. As it is an indispensable condition of experiments with frictional electricity that the atmosphere should be free from excess of moisture, and that no conducting medium should touch the instrument while the force is being generated, so certain conditions are found to be essential to the production and operation of the Psychic force, and unless these precautions are observed the experiments will fail. I am emphatic on this point, because unreasonable objections have sometimes been made to the Psychic Force that it is not developed under adverse conditions dictated by the experimentalist, who, nevertheless, would object to conditions being imposed upon himself in the exhibition of any of his own scientific results. But I may add, that the conditions required are very few, very reasonable, and in no way obstruct the most perfect observation and the application of the most rigid and accurate tests.

Just before going to press I have received from my friend Professor Morton an advance sheet of the "Journal of the Franklin Institute," containing some remarks on my last paper by Mr. Coleman Sellers, a leading scientific engineer of the United States. The essence of his cricitism is contained in the following quotation:—

"On page 341" (of the Quarterly Journal of Science) "we have given a mahogany board '36 inches long by 9½ inches wide, and 1 inch thick,' with 'at each end a strip of mahogany 1½ inches wide screwed on, forming feet.' This board was so placed as to rest with one end on the table, the other suspended by a spring balance, and, so suspended, it recorded a weight of 3 pounds; *i.e.*, a *mahogany* board of the above dimensions is shown to weigh 6 pounds—3 pounds on the balance and 3 pounds on the table. A mechanic used to handling wood wonders how this may be. He looks through his

limited library and finds that scientific men tell him that such a board should weigh about 13¼ pounds. Did Mr. Crookes make this board himself? or did Mr. Home furnish it as one of his pieces of apparatus? . . . . It would have been more satisfactory if Mr. Crookes had stated, in regard to this board, who made it. . . . Let it be discovered that the 6 pound mahogany board was furnished by Mr. Home and the experiments will not be so convincing."

My experiments must indeed be convincing if so accomplished a mechanician as Mr. Coleman Sellers can find no worse fault with them than is expressed in the comments I have quoted. He writes in so matter-of-fact a manner, and deals so plausibly with dimensions and weights, that most persons would take it for granted that I really *had* committed the egregious blunder he points out.

Will it be believed, therefore, that my mahogany board does *weigh only* 6 *pounds?* Four separate balances in my own house tell me so, and my greengrocer confirms the fact.

It is easy to perceive into what errors a "mechanic" may fall when he relies for practical knowledge on his "limited library" instead of appealing to actual experiment.

I am sorry I cannot inform Mr. Sellers who made my mahogany board. It has been in my possession about sixteen years; it was originally cut off a length in a wood-yard; it became the stand of a spectrum camera, and as such is described with a cut in the "Journal of the Photographic Society" for January 21, 1856 (vol. ii., p. 293). It has since done temporary duty in the arrangement of various pieces of apparatus in my physical laboratory, and was selected for these particular experiments owing to its shape being more convenient than that of other available pieces of wood.

But is it seriously expected that I should answer such a question as "Did Mr. Home furnish the board?" Will not my critics give me credit for the possession of some amount of common sense? And can they not imagine that obvious precautions, which occur to them as soon as they sit down to pick holes in my experiments, are not unlikely to have also occurred to me in the course of prolonged and patient investigation?

The answer to this as to all other like objections is, Prove it to be an error by showing where the error lies, or, if a trick, by showing *how* the trick is performed. Try the experiment fully and fairly. If then fraud be found, expose it; if it be a truth, proclaim it. This is the only scientific procedure, and this it is that I purpose steadily to pursue.