

CORRESPONDENCE

UPON

DR. CARPENTER'S ASSERTED REFUTATION

OF

MR. CROOKES'S EXPERIMENTAL PROOF OF THE EXISTENCE
OF A HITHERTO UNDETECTED FORCE.

DR. W. B. CARPENTER, F.R.S., introduced into a Public Lecture an experiment which he alleged to be that upon which I had relied for proof of the existence of a hitherto undetected force. It was not my experiment, but an unjustifiable misrepresentation of it. Called upon to apologise for the wrong he had thus publicly done to me, Dr. Carpenter threw the responsibility from himself upon others whom he stated to have been his informants. I print the Correspondence, and leave it to the judgment of the scientific world.

WILLIAM CROOKES.

20. MORNINGTON ROAD, N.W.
February 21st, 1872.

PROFESSOR G. G. STOKES, SEC. R.S.

DEAR SIR,

My attention has been called to some statements publicly made by Dr. W. B. Carpenter, F.R.S., who gives you as the authority for some serious misrepresentations respecting myself.

On Friday, 19th January, 1872, Dr. Carpenter in a Lecture at the Vestry Hall, Chelsea, said:—

“There was one fact of this kind in connection with Psychic Force which he would grapple with. Mr. William Crookes had sent a paper to the Royal Society last summer, containing investigations into what he called a new force. It was returned to him by the Secretary, with a letter telling him that the Society would not refuse to receive papers upon the subject, but that some kind of scientific evidence ought to be given. Mr. Crookes afterwards sent in a second series of experiments. The Secretary did not like to refuse this paper on his own responsibility, so it came before the Council of the Royal Society; it was a most unusual thing for the Council to refuse a paper sent in by a member. Mr. Crookes's second paper came before the Council a month ago, and a Committee of two was appointed to examine it. They gave in their Report to the Council yesterday, and it was unanimously resolved that the paper be returned to him, as in the opinion of the Royal Society it was good for nothing. Anybody who had a pair of scales in the house could make an experiment to prove the fallacy of one of the points in Mr. Crookes's paper.”

Dr. Carpenter here exhibited an experiment intended to show (and which some of his audience must have believed really did show) that I was ignorant of the merest rudiments of mechanics, and was deluded by an experiment the fallacy of which an intelligent schoolboy could have pointed out. He exhibited a glass of water poised against an equal weight upon a balance, and showed that by dipping a finger in water—that is, by *pressing* with a force exactly equal to the weight of the water displaced by the immersed finger—he increased the weight on that side of the balance. Now, unless the audience were intended to believe that I was ignorant of this childishly simple fact, and, further, that it completely accounted for the result of my experiment, for what purpose was this experiment shown?

A gentleman present who had read an account of my researches subsequently wrote to Dr. Carpenter, protesting against this misleading experiment being put forward as fairly representing what I had tried. In his reply to this protest, Dr. Carpenter says:—

“So far from having been labouring to prejudice Mr. Crookes at the Royal Society, I did not even know of his having sent in a second paper until after it had been rejected by the Council. This rejection took place on Thursday afternoon, and I heard of it and the grounds of it from Professor Stokes and Sir Charles Wheatstone at the evening meeting. What I stated as to Mr. Crookes's experiment with the balance was *on*

*their authority,** as I shall be prepared to prove if the correctness of that statement is impugned."

Now, as a member of that Committee which decided on the rejection of my papers, you, of course, are aware that Dr. Carpenter's balance experiment wholly misrepresents my experiment. My illustrations showed you that the vessel of water was placed over the centre of the fulcrum. You had likewise read what I wrote in my last paper, that "immersing the hand to the utmost in the water, does not raise the level of the water sufficient to produce any movement whatever of the index of the balance."

From the construction of the instrument, as shown by the several drawings and photographs, and fully described in words, you would also have seen that not only was it impossible for any such effect to have taken place, but that the single experiment in which I employed water contact was one I had specially devised for the purpose of getting over some untenable objections raised by yourself against one of my early experiments.

My papers, as well as the illustrations accompanying them, therefore distinctly prove that I could not have made the blunder which Dr. Carpenter told a public audience I had committed; and as Dr. Carpenter, being pressed on the subject, now endeavours to shift the burden of misrepresentation on to your shoulders, I shall feel obliged by your informing me if you really did make the statement which he attributes to you.

I remain,

Truly yours,

(Signed) WILLIAM CROOKES.

A similar letter was sent to Sir Charles Wheatstone. In due time I received the following replies:—

ATHENÆUM CLUB, Pall Mall.
February 28th, 1872.

DEAR SIR,

The conversation between Sir Charles Wheatstone, Dr. Carpenter, and myself, to which you allude in your letter of the 21st inst., has wholly passed out of my memory. It attracted no particular attention on my part, as I had no conception that a mere casual conversation in the tea-room of the Royal Society was going to be reproduced, with greater or less accuracy, at a public meeting. I can only speak with confidence of what I could or could not have said from the clear recollection I have of what I then knew.

You may recollect that in writing to you on the subject of your first paper, I stated as my own opinion, that the mere fact that a paper professed to establish the existence of a hitherto unrecognised force was no reason why a scientific Society should refuse to accept it, but *was* a reason why the experiments should be subjected to the most rigorous scrutiny. This position you accepted as perfectly fair and reasonable. I also pointed out conceivable modes of explaining the results of some of the experiments you described, by referring them to the action of perfectly well known causes. I did not maintain that the

* The italics are Dr. Carpenter's.

results were *actually produced* in the particular way I suggested, but only that they might reasonably be *conceived to have been so produced*, so that a person professing to establish the existence of a new force was bound to make his demonstration free from such objections.

Among other things, I pointed out that the glass vessel of water which you employed in one of your experiments rested on the board at some distance from the fulcrum; and that, consequently, when the hand was dipped into the water contained in the copper basin which, resting on a firm independent support, dipped into the water contained in the glass vessel, with which its interior was in communication by a hole, if time were given for the water to run through, the pressure on the base of the glass vessel would be increased by the weight of the water displaced by the hand, and consequently the spring balance would be affected.

Whether in the letter you wrote me in reply this particular point was noticed I do not at the moment recollect, nor does it signify, for in your second and third papers, one or both, I noticed particularly that you modified your experiment by placing the glass vessel with its middle over the fulcrum, and tested by direct experiment whether the insertion of the hand in the water in the copper vessel had any sensible effect on the balance.

These modifications I noticed particularly, as they had been made, as I presumed, expressly to meet certain objections which I had raised. It is quite impossible, therefore, that in my conversation with Dr. CARPENTER, after your papers were ordered to be returned to you, I could have represented them to him as open to this objection. I *may* have talked to him on this subject (I don't know that I *did*), when your first paper alone had appeared; and, if so, it is conceivable that he may have confounded two conversations held, one several months ago, the other quite recently.

I wish to make one remark before I conclude. The question brought before the Committee of Papers of the Royal Society with reference to your papers was simply whether they should be accepted or declined. The decision of the Committee, as entered on the Minutes, was simply "declined." What estimate of the value of your papers each individual voter may have formed—what considerations mainly may have influenced him in giving his vote—are questions which he alone can answer; so that no one, as I conceive, has a right to add to the formal decision his notion of the grounds of it.

I am, Dear Sir,

Yours sincerely,

G. G. STOKES.

WILLIAM CROOKES, Esq., F.R.S.

19, PARK CRESCENT,
PORTLAND PLACE, N.W.
March 14, 1872.

DEAR SIR,

I did not state to Dr. Carpenter that the water experiment *disproved* the existence of your hypothetical psychic force; what I did say was to the effect that no argument in its favour could be deduced from the experiment which you put forward so prominently.

You say, page 20 of your first pamphlet, "I am now fitting up an apparatus in which contact is made through water in such way that transmission of mechanical movement to the board is impossible;" and

again, at page 28, "As the mechanical transmission of power is by this means entirely cut off between the copper vessel and the board, the power of muscular control is thereby completely eliminated." In both these sentences you explain why you employed the interposition of water, and you state nothing from which I can infer that you had any other reason for doing so. It is further evident that in the experiments first communicated to Professor Stokes, the vessel of water was not placed directly over the fulcrum of the lever; for you say (page 28) "In my first experiments with this apparatus, referred to in Professor Stokes's letter and my answer, the glass vessel was not over the fulcrum, but nearer B." That under such circumstances a mechanical pressure is exerted on the lever when the hand is dipped in the water is an undoubted fact; whether it produces the effect in question or not depends on the sensibility of the apparatus and the placing of the vessel. A displacement of 3 cubic inches of water would exert a pressure which, if directly applied to your machine, would be equal to 6816 grains; the extreme pressure of your imaginary psychic force being, according to your own statement, 5000 grains. The fluctuation of the pressure in your experiment would naturally follow from the varying quantity of water displaced owing to the unsteadiness of the hand in the liquid.

From the above it appears to me that your experiment with the water vessel does not offer an iota of proof in favour of your doctrine of psychic force, or any disproof of the effect not being mechanical; though it might easily lead persons unacquainted with hydrostatic laws to infer that no mechanical pressure could be communicated under such circumstances.

I cannot see what part you intended the water to play when you subsequently placed the vessel over the dead point, and it appears to me contrary to all analogy that a force acting according to physical laws should produce the motion of a lever by acting on its fulcrum.

Yours faithfully,

C. WHEATSTONE.

W. CROOKES, Esq.

PS.—I enclose a note which I have received from Dr. Carpenter.

UNIVERSITY OF LONDON,
BURLINGTON GARDENS, W.
Feb. 28, 1872.

DEAR SIR CHARLES,

If you should be communicating with Mr. Crookes on the subject as to which you spoke to me, it may be as well that you should let him know what was *my* understanding of the matter, as derived from yourself and Professor Stokes, and what was the account I gave of it in my Lecture.

I understood from you that Mr. Crookes had adduced the descent of a balanced vessel of water, on the immersion of Mr. Home's fingers into it, as a proof of the exertion of some force which could not be mechanical, and which must therefore be a *new* force, call it psychic, spiritual, or what you please. And I showed my audience that the immersion of the fingers into a tumbler of water so balanced would produce its descent simply by hydrostatic pressure; from which I drew the inference that Mr. Crookes's experiment gave no proof whatever of the existence of any force not known to us.

If I have in any way misunderstood your account of Mr. C.'s experiment, and have thereby done him injustice in my representation of it, I shall be quite ready to make any correction that you (as a mutual friend) may consider to be called for.

Believe me,

Yours faithfully,

WILLIAM B. CARPENTER.

Sir CHAS. WHEATSTONE.

20, MORNINGTON ROAD, N.W.

March 27th, 1872.

DEAR SIR CHARLES,

You must allow me to protest against the experiments given in my Royal Society paper of September 27th, 1871, being ignored and the discussion being made to turn on a less decisive experiment referred to in an earlier paper. The experiments of September 27th, 1871, are those referred to by Dr. Carpenter, and reported on by Professor Stokes and by yourself. That there is no doubt of this being the case is evident from Dr. Carpenter's language at Chelsea and elsewhere:—

"Mr. William Crookes had sent a paper to the Royal Society last summer [June 14th and June 28th, 1871] containing investigations into what he called a new force. It was returned to him by the Secretary. Mr. Crookes afterwards sent in a second series of experiments [September 27th, 1871.] The Secretary did not like to refuse this paper on his own responsibility, so it came before the Council of the Royal Society. . . . Mr. Crookes's second paper came before the Council a month ago, and a Committee of two was appointed to examine it. They gave in their Report to the Council yesterday [January, 18th, 1872], and it was unanimously resolved that the paper be returned to him, as in the opinion of the Royal Society it was good for nothing."

"This rejection took place on Thursday afternoon [January 18th, 1872], and I heard of it and the grounds of it from Professor Stokes and Sir Charles Wheatstone at the evening meeting. What I stated as to Mr. Crookes's experiment with the balance was *on their authority*."

Dr. Carpenter here explicitly refers to the experiments given in my paper of September 27th, 1871, and not only says that you mentioned to him the grounds of the rejection of that paper on the very day it occurred, but that you described to him one of the experiments given in it.

I must therefore object to having the discussion drawn from the point at issue, from the testing experiment in question presented to the Royal Society, to an imperfect form of the same experiment which was merely referred to in a paper published elsewhere.

From my pamphlet reprinted from the "*Quarterly Journal of Science*" for October 1st, 1871 (page 28), you quote the following words:—

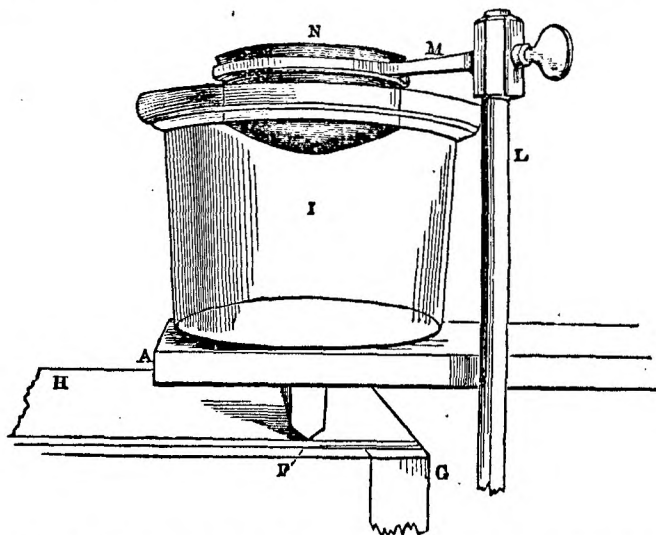
"As the mechanical transmission of power is by this means entirely cut off between the copper vessel and the board, the power of muscular control is thereby completely eliminated."

You also quote a foot-note in which I refer to an early and imperfect form of the experiment, and you thereupon comment on these passages, speak of well-known hydrostatic laws, and give calculations, as if my

published experiments in question really afforded any grounds for severe remarks.

It is much to be regretted that you should have selected from my pamphlet two passages occurring on page 28, and should have omitted to read the few lines which connect these passages; otherwise it must have been apparent to you that your self-evident exposition of a well-known hydrostatic law had no bearing on the case in point.

Let me supply the deficiency. The following paragraph, from page 28 of my pamphlet, fills up the gap between the two passages you quote:—



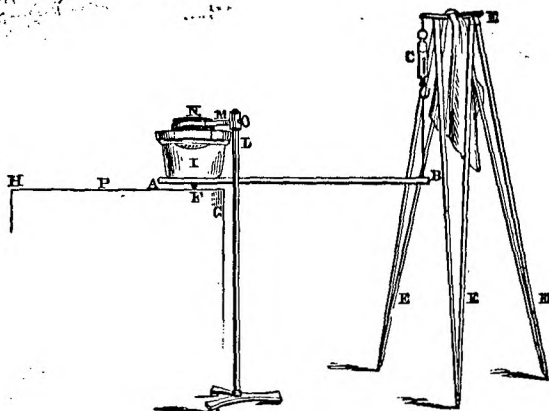
"On the board, exactly over the fulcrum, is placed a large glass vessel filled with water, *i.* *L* is a massive iron stand furnished with an arm and a ring, *MN*, in which rests a hemispherical copper vessel, perforated with several holes in the bottom. The iron stand is 2 inches from the board, *AB*, and the arm and copper vessel, *MN*, 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 *AB* 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 *AB*, the power of muscular control is thereby eliminated."

I venture to think that had you read the above connecting link between your two quotations from my pamphlet, or had even noticed the parts I have italicised, you would not have written,—

"That under such circumstances a mechanical pressure is exerted on the lever when the hand is dipped in the water is an undoubted fact; whether it produces the effect in question or not

depends on the sensibility of the apparatus and the placing of the vessel. A displacement of 3 cubic inches of water would exert a pressure which, if directly applied to your machine, would be equal to .6816 grains; the extreme pressure of your imaginary psychic force being, according to your own statement, 5000 grains."

I have preferred to quote from the reprint of my paper in the "Quarterly Journal of Science" for October 1st, 1871, as your citations appear to show that you have derived your information from it; but in my Royal Society communication of September 27th, 1871—the paper to which Dr. Carpenter and yourself referred—the same experiment is described in almost identical words, and is, moreover, illustrated with photographs of the apparatus.



But why refer only to the water-contact experiment? The true explanation is the one which will reconcile all the indisputable facts. How does the well-known hydrostatic law account for Experiment 2 on p. 29, in which the vessel of water was removed? Or Experiment 3, in which the force acted through a space of of 1 foot? Or Experiment 4, in which the force acted at a distance of 3 feet? Or Experiments 5 and 6, in which another kind of apparatus was used, and the force likewise acted at a distance?

The only sentence in your letter bearing in any way on my actual experiment is the last one, in which you say:—

"I cannot see what part you intended the water to play when you subsequently placed the vessel over the dead point, and it appears to me contrary to all analogy that a force acting according to physical laws should produce the motion of a lever by acting on its fulcrum."

In this I entirely agree. I too cannot see the part the water played; nor can I trace the analogy between the psychic force and a force acting according to known physical laws. Yet the facts recorded in my papers are true for all that.

I remain,

Truly yours,

WILLIAM CROOKES.

Sir CHARLES WHEATSTONE, F.R.S., &c.