

# EXPERIMENTAL RESEARCHES

IN

## CHEMISTRY AND PHYSICS.

BY

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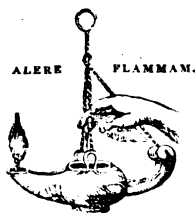
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## PREFACE.

THE reasons which induce me to gather together in this Volume the various physical and chemical papers scattered in the Philosophical Transactions and elsewhere, are the same as those which caused the 'Experimental Researches in Electricity' to be collected into one Series. As investigations, several of them are very imperfect; but it was thought a duty to print them just as they were, that they might be referred to as safely for facts, opinions, and dates, as the original papers. The correction of certain phrases and typographical errors, and the addition of some matter here and there with its proper date, is not considered as interfering with this intention.

MICHAEL FARADAY.

October, 1858.



# CONTENTS.

	Page
Analysis of native caustic lime .....	1
Escape of gases through capillary tubes .....	5
Passage of gases through tubes .....	6
Combustion of the diamond .....	11
Apparatus for the combustion of the diamond .....	11
Oxide of silver in ammonia.....	13
Combinations of ammonia with chlorides .....	18
— Sounds produced by flame in tubes .....	21
— Action of boracic acid on turmeric .....	27
— Change of vegetable colours as an alkaline property .....	29
— Action of salts on turmeric paper .....	31
— Decomposition of chloride of silver by hydrogen and by zinc .....	31
— Two new compounds of chlorine and carbon, &c.....	33
— New compound of chlorine and carbon ( <i>Phillips</i> and <i>Faraday</i> ) ..	53
— Vapour of mercury at common temperatures.....	57
— Alloys of steel ( <i>Stodart</i> and <i>Faraday</i> ) .....	57
— Hydriodide of carbon .....	81
— Hydrate of chlorine .....	81
— >Fluid chlorine .....	85
— — Condensation of several gases into liquids .....	89
— — Liquefaction and solidification of bodies generally existing as gases ..	96
— — Historical statement—liquefaction of gases .....	124
— — History of the condensation of gases .....	135
— — Change of musket balls in Shrapnell shells .....	141
— — Action of gunpowder on lead .....	142
— — Purple tint of plate-glass affected by light .....	142
— — Cases of the formation of ammonia .....	143
— — Substitution of tubes for bottles .....	152
— — Composition of crystals of sulphate of soda .....	153
— — New compounds of carbon and hydrogen .....	154
— — Pure caoutchouc .....	174
— — Mutual action of sulphuric acid and naphthaline .....	182

	Page
Existence of a limit to vaporization . . . . .	199
Limits of vaporization . . . . .	205
Fluidity of sulphur at common temperatures . . . . .	212
Fluidity of sulphur and phosphorus at common temperatures . . . . .	213
Perspective aerial light and shadow . . . . .	215
Confinement of dry gases over mercury . . . . .	217
Decomposition of hydrocarbons by expansion . . . . .	219
Transference of heat by change of capacity in gas . . . . .	221
Labarraque's disinfecting soda liquid . . . . .	222
Anhydrous sulphate of soda . . . . .	230
Manufacture of optical glass . . . . .	231
- Peculiar class of optical deceptions . . . . .	291
Sound during the conduction of heat . . . . .	311
Peculiar class of acoustical figures . . . . .	314
Preparation of the lungs for holding the breath . . . . .	358
Ventilation of lighthouse lamps . . . . .	362
Thoughts on ray-vibrations . . . . .	366
On ice and freezing water . . . . .	372
On regelation . . . . .	377
Table-turning . . . . .	382
Relations of gold and other metals to light . . . . .	391
Conservation of force . . . . .	443
Lecture on mental education . . . . .	463
INDEX . . . . .	493

perature of snow in a thawing state. The experiment, however, would require much consideration in every point of view, and much care before it could be considered as telling anything beyond the temperature of ice-cold water.

On the other hand, if a spherical cup of ice could be prepared containing water within, to which no heat could pass except by conduction through the ice itself, that water ought to be a little colder than the ice cup around it:—also if a mixture of snow and water were pressed together, the temperature should rise whenever regelation occurred, being an effect in the contrary direction to that which Prof. J. Thomson contemplates; and such a mixture, as a whole, ought to be warmer than the water in the ice sphere mentioned above. No doubt nice experiment will hereafter enable us to criticise such results as these, and separating the true from the untrue, will establish the correct theory of regelation.

*September, 1858.*

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*On Table-turning\*.*

*To the Editor of the Times.*

SIR,—I have recently been engaged in the investigation of table-turning. I should be sorry that you should suppose I thought this necessary on my own account, for my conclusion respecting its nature was soon arrived at, and is not changed; but I have been so often misquoted, and applications to me for an opinion are so numerous, that I hoped, if I enabled myself by experiment to give a strong one, you would consent to convey it to all persons interested in the matter. The effect produced by table-turners has been referred to electricity, to magnetism, to attraction, to some unknown or hitherto unrecognized physical power able to affect inanimate bodies—to the revolution of the earth, and even to diabolical or supernatural agency. The natural philosopher can investigate all these supposed causes but the last; that must, to him, be too much connected with credulity or superstition to require any attention on his part. The investigation would be too long in description to obtain a place in your columns. I therefore purpose asking admission

\* Times, June 30, 1853.

for that into the 'Athenæum' of next Saturday, and propose here to give the general result. Believing that the first cause assigned—namely, a quasi involuntary muscular action (for the effect is with many subject to the wish or will)—was the true cause; the first point was to prevent the mind of the turner having an undue influence over the effects produced in relation to the nature of the substances employed. A bundle of plates, consisting of sand-paper, millboard, glue, glass, plastic clay, tinfoil, cardboard, gutta-percha, vulcanized caoutchouc, wood, and resinous cement, was therefore made up and tied together, and being placed on a table, under the hand of a turner, did not prevent the transmission of the power; the table turned or moved exactly as if the bundle had been away, to the full satisfaction of all present. The experiment was repeated, with various substances and persons, and at various times, with constant success; and henceforth no objection could be taken to the use of these substances in the construction of apparatus. The next point was to determine the place and source of motion, *i. e.* whether the table moved the hand, or the hand moved the table; and for this purpose indicators were constructed. One of these consisted of a light lever, having its fulcrum on the table, its short arm attached to a pin fixed on a cardboard, which could slip on the surface of the table, and its long arm projecting as an index of motion. It is evident that if the experimenter willed the table to move towards the left, and it did so move before the hands, placed at the time on the cardboard, then the index would move to the left also, the fulcrum going with the table. If the hands involuntarily moved towards the left without the table, the index would go towards the right; and, if neither table nor hands moved, the index would itself remain immoveable. The result was, that when the parties saw the index it remained verysteady; when it was hidden from them, or they looked away from it, it wavered about, though they believed that they always pressed directly downwards; and, when the table did not move, there was still a resultant of hand force in the direction in which it was wished the table should move, which, however, was exercised quite unwittingly by the party operating. This resultant it is which, in the course of the waiting time, while the fingers and hands become stiff, numb, and insensible by continued



pressure, grows up to an amount sufficient to move the table or the substances pressed upon. But the most valuable effect of this test-apparatus (which was afterwards made more perfect and independent of the table) is the corrective power it possesses over the mind of the table-turner. As soon as the index is placed before the most earnest, and they perceive—as in my presence they have always done—that it tells truly whether they are pressing downwards only or obliquely, then all effects of table-turning cease, even though the parties persevere, earnestly desiring motion, till they become weary and worn out. No prompting or checking of the hands is needed—the power is gone; and this only because the parties are made conscious of what they are really doing mechanically, and so are unable unwittingly to deceive themselves. I know that some may say that it is the cardboard next the fingers which moves first, and that it both drags the table, and also the table-turner with it. All I have to reply is, that the cardboard may in practice be reduced to a thin sheet of paper weighing only a few grains, or to a piece of goldbeaters' skin, or even the end of the lever, and (in principle) to the very cuticle of the finger itself. Then the results that follow are too absurd to be admitted: the table becomes an incumbrance, and a person holding out the fingers in the air, either naked or tipped with goldbeaters' skin or cardboard, ought to be drawn about the room, &c.; but I refrain from considering imaginary yet consequent results which have nothing philosophical or real in them. I have been happy thus far in meeting with the most honourable and candid though most sanguine persons, and I believe the mental check which I propose will be available in the hands of all who desire truly to investigate the philosophy of the subject, and, being content to resign expectation, wish only to be led by the facts and the truth of nature. As I am unable, even at present, to answer all the letters that come to me regarding this matter, perhaps you will allow me to prevent any increase by saying that my apparatus may be seen at the shop of the philosophical instrument maker—Newman, 122 Regent-street. Permit me to say, before concluding, that I have been greatly startled by the revelation which this purely physical subject has made of the condition of the public mind. No doubt there are many persons who have formed a right judgment or used a cautious reserve, for I know several such,

and public communications have shown it to be so; but their number is almost as nothing to the great body who have believed and borne testimony, as I think, in the cause of error. I do not here refer to the distinction of those who agree with me and those who differ. By the great body, I mean such as reject all consideration of the equality of cause and effect, who refer the results to electricity and magnetism, yet know nothing of the laws of these forces,—or to attraction, yet show no phenomena of pure attractive power,—or to the rotation of the earth, as if the earth revolved round the leg of a table,—or to some unrecognized physical force, without inquiring whether the known forces are not sufficient,—or who even refer them to diabolical or supernatural agency, rather than suspend their judgment, or acknowledge to themselves that they are not learned enough in these matters to decide on the nature of the action. I think the system of education that could leave the mental condition of the public body in the state in which this subject has found it, must have been greatly deficient in some very important principle.

I am, Sir, your very obedient Servant,  
*Royal Institution, June 28, 1853.* M. FARADAY.

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*Experimental Investigation of Table-Moving\*.*

THE object which I had in view in this inquiry was not to satisfy myself, for my conclusion had been formed already on the evidence of those who had turned tables; but that I might be enabled to give a strong opinion, founded on facts, to the many who applied to me for it. Yet the proof which I sought for, and the method followed in the inquiry, were precisely of the same nature as those which I should adopt in any other physical investigation. The parties with whom I have worked were very honourable, very clear in their intentions, successful table-movers, very desirous of succeeding in establishing the existence of a peculiar power, thoroughly candid, and very effectual. It is with me a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power. They say, the table draws their hands;

\* *Athenæum*, July 2, 1853.

that it moves first, and they have to follow it,—that sometimes it even moves from under their hands. With some the table will move to the right or left according as they wish or will it, —with others the direction of the first motion is uncertain:—but all agree that the table moves the hands and not the hands the table. Though I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action, still I had no doubt of the influence of expectation upon their minds, and through that upon the success or failure of their efforts. The first point, therefore, was to remove all objections due to expectation, having relation to the substances which I might desire to use:—so, plates of the most different bodies, electrically speaking,—namely, sand-paper, millboard, glue, glass, moist clay, tinfoil, cardboard, gutta percha, vulcanized rubber, wood, &c.,—were made into a bundle and placed on a table under the hands of a turner. The table turned. Other bundles of other plates were submitted to different persons at other times,—and the tables turned. Henceforth, therefore, these substances may be used in the construction of apparatus. Neither during their use nor at other times could the slightest trace of electrical or magnetic effects be obtained. At the same trials it was readily ascertained that one person could produce the effect; and that the motion was not necessarily circular, but might be in a straight line. No form of experiment or mode of observation that I could devise gave me the slightest indication of any peculiar natural force. No attractions, or repulsions, or signs of tangential power, appeared,—nor anything which could be referred to other than the mere mechanical pressure exerted inadvertently by the turner. I therefore proceeded to analyse this pressure, or that part of it exerted in a horizontal direction:—doing so, in the first instance, unawares to the party. A soft cement, consisting of wax and turpentine, or wax and pomatum, was prepared. Four or five pieces of smooth slippery cardboard were attached one over the other by little pellets of the cement, and the lower of these to a piece of sand-paper resting on the table; the edges of these sheets overlapped slightly, and on the under surface a pencil line was drawn over the laps so as to indicate position. The upper cardboard was larger than the rest, so as to cover the whole from sight. Then the table-turner placed the hands

upon the upper card,—and we waited for the result. Now, the cement was strong enough to offer considerable resistance to mechanical motion, and also to retain the cards in any new position which they might acquire,—and yet weak enough to give way slowly to a continued force. When at last the tables, cards, and hands all moved to the left together, and so a true result was obtained, I took up the pack. On examination it was easy to see by the displacement of the parts of the line, that the hand had moved further than the table, and that the latter had lagged behind ;—that the hand, in fact, had pushed the upper card to the left, and that the under cards and the table had followed and been dragged by it. In other similar cases when the table had not moved, still the upper card was found to have moved, showing that the hand had carried it in the expected direction. It was evident, therefore, that the table had not drawn the hand and person round, nor had it moved simultaneously with the hand. The hand had left all things under it behind, and the table evidently tended continually to keep the hand back.

The next step was to arrange an index which should show whether the table moved first, or the hand moved before the table, or both moved or remained at rest together. At first this was done by placing an upright pin fixed on a leaden foot upon the table, and using that as the fulcrum of a light lever. The latter was made of a slip of foolscap paper, and the short arm, about  $\frac{1}{4}$  of an inch in length, was attached to a pin proceeding from the edge of a slipping card placed on the table, and prepared to receive the hands of the table-turner. The other arm, of  $11\frac{1}{2}$  inches long, served for the index of motion. A coin laid on the table marked the normal position of the card and index. At first the slipping card was attached to the table by the soft cement, and the index was either screened from the turner, or the latter looked away: then, before the table moved, the index showed that the hand was giving a resultant pressure in the expected direction. The effect was never carried far enough to move the table, for the motion of the index corrected the judgment of the experimenter, who became aware that, inadvertently, a side force had been exerted. The card was now set free from the table, *i. e.* the cement was removed. This, of course, could not

interfere with any of the results expected by the table-turner, for both the bundle of plates spoken of and single cards had been freely moved on the tables before; but now that the index was there, witnessing to the eye, and through it to the mind, of the table-turner, not the slightest tendency to motion either of the card or of the table occurred. Indeed, whether the card was left free or was attached to the table, all motion or tendency to motion was gone. In one particular case there was relative motion between the table and the hands: I believe that the hands moved in one direction; the table-turner was persuaded that the table moved from under the hand in the other direction:—a gauge, standing upon the floor, and pointing to the table, was therefore set up on that and some future occasions,—and then, neither motion of the hand nor of the table occurred.

A more perfect lever apparatus was then constructed in the following manner:—Two thin boards,  $9\frac{1}{2}$  inches by 7 inches, were provided; a board, 9 inches by 5 inches, was glued to the middle of the underside of one of these (to be called the table-board), so as to raise the edges free from the table; being placed on the table, near and parallel to its side, an upright pin was fixed close to the further edge of the board, at the middle, to serve as the fulcrum for the indicating lever. Then four glass rods, 7 inches long and  $\frac{1}{4}$  in diameter, were placed as rollers on different parts of this table-board, and the upper board placed on them; the rods permitted any required amount of pressure on the boards, with a free motion of the upper on the lower to the right and left. At the part corresponding to the pin in the lower board, a piece was cut out of the upper board, and a pin attached there, which, being bent downwards, entered the hole in the end of the short arm of the index lever: this part of the lever was of cardboard; the indicating prolongation was a straight hay-stalk 15 inches long. In order to restrain the motion of the upper board on the lower, two vulcanized rubber rings were passed round both, at the parts not resting on the table: these, whilst they tied the boards together, acted also as springs,—and whilst they allowed the first feeblest tendency to motion to be seen by the index, exerted, before the upper board had moved a quarter of an inch, sufficient power in pulling the upper board back from either side, to resist a

strong lateral action of the hand. All being thus arranged, except that the lever was away, the two boards were tied together with string, running parallel to the vulcanized rubber springs, so as to be immoveable in relation to each other. They were then placed on the table, and a table-turner sat down to them:—the table very shortly moved in due order, showing that the apparatus offered no impediment to the action. A like apparatus, with metal rollers, produced the same result under the hands of another person. The index was now put into its place and the string loosened, so that the springs should come into play. It was soon seen, with the party that could will the motion in either direction (from whom the index was purposely hidden), that the hands were gradually creeping up in the direction before agreed upon, though the party certainly thought they were pressing downwards only. When shown that it was so, they were truly surprised; but when they lifted up their hands and immediately saw the index return to its normal position, they were convinced. When they looked at the index and could see for themselves whether they were pressing truly downwards, or obliquely so as to produce a resultant in the right- or left-handed direction, then such an effect never took place. Several tried, for a long while together, and with the best will in the world; but no motion, right or left, of the table, or hand, or anything else occurred.—[Then occurs a passage from the ‘Times,’ already printed at pp. 383, 384.]

Another form of index was applied thus:—a circular hole was cut in the middle of the upper board, and a piece of cartridge paper pasted under it on the lower surface of the board; a thin slice of cork was fixed on the upper surface of the lower board corresponding to the cartridge paper; the interval between them might be a quarter of an inch or less. A needle was fixed into the end of one of the index hay-stalks, and when all was in place the needle point was passed through the cartridge paper and pressed slightly into the cork beneath, so as to stand upright: then any motion of the hand, or hand-board, was instantly rendered evident by the deflection of the perpendicular hay-stalk to the right or left.

I think the apparatus I have described may be useful to many who really wish to know the truth of nature, and would prefer that truth to a mistaken conclusion; desired, perhaps

only because it seems to be new or strange. Persons do not know how difficult it is to press directly downward, or in any given direction against a fixed obstacle, or even to *know only* whether they are doing so or not; unless they have some indicator, which, by visible motion or otherwise, shall instruct them: and this is more especially the case when the muscles of the fingers and hand have been cramped and rendered either tingling, or insensible, or cold by long-continued pressure. If a finger be pressed constantly into the corner of a window-frame for ten minutes or more, and then, continuing the pressure, the mind be directed to judge whether the force at a given moment is all horizontal, or all downward, or how much is in one direction and how much in the other, it will find great difficulty in deciding; and will at last become altogether uncertain: at least such is my case. I know that a similar result occurs with others; for I have had two boards arranged, separated, not by rollers, but by plugs of vulcanized rubber, and with the vertical index: when a person with his hands on the upper board is requested to press only downwards, and the index is hidden from his sight, it moves to the right, to the left, to him and from him, and in all horizontal directions; so utterly unable is he strictly to fulfil his intention without a visible and correcting indicator. Now, such is the use of the instrument with the horizontal index and rollers: the mind is instructed, and the involuntary or *quasi* involuntary motion is checked in the commencement, and therefore never rises up to the degree needful to move the table, or even permanently the index itself. No one can suppose that looking at the index can in any way interfere with the transfer of electricity or any other power from the hand to the board under it or to the table. If the board tends to move, it may do so, the index does not confine it; and if the table tends to move, there is no reason why it should not. If both were influenced by any power to move together, they may do so,—as they did indeed when the apparatus was tied, and the mind and muscles left unwatched and unchecked.

I must bring this long description to a close. I am a little ashamed of it, for I think, in the present age, and in this part of the world, it ought not to have been required. Nevertheless, I hope it may be useful. There are many whom I do not

expect to convince ; but I may be allowed to say that I cannot undertake to answer such objections as may be made. I state my own convictions as an experimental philosopher, and find it no more necessary to enter into controversy on this point than on any other in science, as the nature of matter, or inertia, or the magnetization of light, on which I may differ from others. The world will decide sooner or later in all such cases, and I have no doubt very soon and correctly in the present instance. Those who may wish to see the particular construction of the test apparatus which I have employed, may have the opportunity at Mr. Newman's, 122 Regent Street. Further, I may say, I have sought earnestly for cases of lifting by attraction, and indications of attraction in any form, but have gained no traces of such effects. Finally, I beg to direct attention to the discourse delivered by Dr. Carpenter at the Royal Institution on the 12th of March, 1852, entitled, "On the Influence of Suggestion in modifying and directing Muscular Movement, independently of Volition;" which, especially in the latter part, should be considered in reference to table-moving by all who are interested in the subject.

*Royal Institution, June 27.*

M. FARADAY.

THE BAKERIAN LECTURE.—*Experimental Relations of Gold  
(and other Metals) to Light\*.*

[Received November 15, 1856,—Read February 5, 1857.]

THAT wonderful production of the human mind, the undulatory theory of light, with the phenomena for which it strives to account, seems to me, who am only an experimentalist, to stand midway between what we may conceive to be the coarser mechanical actions of matter, with their explanatory philosophy, and that other branch, which includes, or should include, the physical idea of forces acting at a distance; and admitting for the time the existence of the ether, I have often struggled to perceive how far that medium might account for or mingle in with such actions generally; and to what extent experimental trials might be devised, which, with their results and consequences, might contradict, confirm, enlarge, or modify the idea we form of it, always with the hope that the corrected or instructed idea would

\* Philosophical Transactions, 1857, p. 145.