ANOTHER LESSON FROM THE RADIOMETER.

THE great aim of the writer who criticises or popularises scientific discoveries should be accuracy. He who misrepresents, and then refutes, not what has been really advanced by the author, but what has been foisted upon him, lays himself open to the gravest censure. The business of the critic is to investigate, to digest, and then to describe, briefly perhaps, but so as not to lead astray. If there are before him errors of fact, let them be pointed out; if false conclusions, let them be refuted. No mistake in doctrine or method, in matter or manner, should be passed over. On the other hand the critic should remember that experimental research is necessarily and slowly progressive, and that the early provisional hypothesis has to be modified, adjusted, perhaps altogether abandoned, in deference to later observations. We do not censure the dawn for not being full daylight, nor should an author's more advanced researches be used to condemn and to discredit his first gropings after truth.

In an 'age of research' it is of consequence, too, that the work of critical examination should be entrusted to competent hands. And who should interpret to the public the results of the investigator? There is but one answer to this question. The only fully competent authority is a specialist versed in the department he undertakes to criticise. He only is adequately alive to what has previously been done, and can best estimate the difficulties that beset a complicated inquiry. He alone can pronounce most authoritatively on the validity of the methods employed, can appreciate the solutions arrived at, and can point out the collateral issues opened up. There should be specialists and specialists, and a specialist trained in one department is rarely fitted to pronounce upon the work of a specialist in another and totally distinct department.

The April number of the *Nineteenth Century* contains an article bearing the signature of Dr. W. B. Carpenter and ostensibly treating of the 'Radiometer and its Lessons.' The description of the instrument itself and of its reception in scientific circles contains little perhaps openly or strikingly erroneous, but unfortunately Dr. Carpenter has endeavoured to combine matters which have no possible con-

nection with the Radiometer; moreover, the omissions and inaccuracies which occur in his historical review of my experiments and published researches on the subject would seem to deprive his inferences and conclusions of any value which they otherwise might have possessed.

We are told [1] that when the theory of the Radiometer was under discussion at the Royal Society Professor Stokes confined himself to the statement 'that such mechanical action must lie *outside* the Undulatory Theory, which deals only with light *as* light—*i.e.* as producing visual phenomena.' The four last words are added by Dr. Carpenter to the observations of Professor Stokes. That the undulatory theory gives no account of the phenomena of light, save so far as they are connected with the vision of man and animals, is, to say the least, a startling revelation.

Again, we read [2] that when the movement of the Radiometer was discussed 'it was noticed by several as anomalous, that the *black* should be the "driving" side of the disks, since it might have been anticipated that the mechanical action of light would manifest itself in pushing away the surface from which its rays are reflected, and that the surface into which they are absorbed would move towards the source from which the rays emanate.' Dr. Carpenter here omits the explanation of this apparent anomaly given by me at the Royal Society, and accepted as satisfactory by the eminent physicists present, to the effect that the rays falling on the white surface are simply reflected off without doing any work; but the rays falling on the black surface are absorbed, and their energy, disappearing in its original form, reappears as mechanical motion.

Dr. Carpenter next affirms [3] that I committed myself 'explicitly to the doctrine that the Radiometer is driven by *light*.' Later on I am accused of showing some lingering unwillingness to surrender this position; and I am then gravely censured for not knowing that *heat* causes the movement of the Radiometer. Now what are the facts? Let my own written words speak for me.

In 1873 I published [4] the description of an experiment proving that *every ray* of the spectrum produced repulsion: the maximum action being in the extreme red. In March 1875, I wrote [5]—

> Although I most frequently speak of repulsion by *heat*, it must be clearly understood that these results are not confined to the heating rays of the spectrum, but that any ray, from the ultra red to the ultra violet, will produce repulsion in a vacuum.

So much for my earlier ideas on this subject. What are my later views regarding which, according to my critic, I still show 'some lingering unwillingness to surrender my position'?

[1] P. 244. [2] P. 244. [3] P. 245.
[4] *Phil. Trans.* vol. clxiv. p. 518. [5] *Ibid.* vol. clxv. p. 526.

In 1876 I wrote [6]—

> Is the effect due to *heat* or *light*? I cannot answer this question. The terms heat and light are not definite enough. The physicist has no test for light independent of heat. Light and colour are physiological accidents, due to the fact that a small portion near the middle of the spectrum happens to be capable of affecting the retina of the human eye. There is no real distinction between heat and light; all we can take account of is difference of wave-length.

After describing experiments with a pure solar spectrum, and giving numerical values for the motion-producing powers of the various coloured rays, I continue:—

> A comparison of these figures with those usually given in text-books to represent the distribution of heat in the spectrum will be a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

Dr. Carpenter then accuses me of attributing the movement of the Radiometer to *light*. The very contrary is the case. I have always sought to guard against this misconception, insisting that every ray of the spectrum, visible or invisible, must cause motion. Hence I called the instrument the Radiometer—ray-measurer. Those who most persistently deny that light occasions the movements curiously enough continue to use the term '*light*-mill.'

Dr. Carpenter introduces an account of an experiment I showed at the Royal Institution, on the evening of the 11th of February, 1876, with the words—'This he called "weighing a beam of light."' Now, my actual words at the lecture were [7]—

> I want to ascertain the amount of pressure which radiation exerts on a blackened surface. I will put a ray of light on the pan of a balance, and give you its weight in grains. *For I think in this Institution and before this audience I may be allowed a Scientific Use of the Imagination, and may speak of weighing that which is not affected by gravitation.*

The italicised words render it evident that I was only speaking figuratively; and not, as Dr. Carpenter wishes to make it appear, that I conceived light to be a material substance.

Another misstatement follows on the next page, where Dr. Carpenter pronounces it 'as pure an assumption on Mr. Crookes's part to affirm that the *mechanical* action exerted by two flames of different kinds would measure their relative *illuminating* powers, as it would have been to say that their heating action would be proportional to their illuminating action, which we know perfectly well *not* to be the case,—the gas flame, as every one knows, having a much greater heating power than the candle flame, in proportion to the light it gives.'

[6] *Phil. Trans.* vol. clxvi. pp. 360, 361, 362.

[7] *Proceedings of the Royal Institution*, February 11, 1876, and *Quarterly Journal of Science*, April, 1876, p. 250.

Once again Dr. Carpenter omits part of my explanation. I will assume that he has read a portion of the description of the photometric experiment he criticises, given by me in the *Proceedings of the Royal Society*, No. 167, 1876. Why did he not read the next sentence, beginning?—

> By interposing screens of water or plates of alum, and so practically cutting off all the dark heat, the actual luminosity is measured.

Or perhaps he gained his information from my Royal Institution Lecture.[8] In this case he must have read the following remarks:—

> Before this instrument can be used as a photometer or light measurer, means must be taken to cut off from it all those rays coming from the candle or gas which are not actually luminous. A reference to the spectrum diagram (fig. 5) will show that at each end of the coloured rays there is a large space inactive, as far as the eye is concerned, but active in respect to the production of motion—strongly so at the red end, less strong at the violet end. Before the instrument can be used to measure luminosity, these rays must be cut off. We buy gas for the light that it gives, not for the heat it evolves on burning, and it would therefore never do to measure the heat and pay for it as light.

Dr. Carpenter either failed to remember this explicit statement, or overlooked it.

In referring to the kinetic theory of gases as explaining the movement of the Radiometer, Dr. Carpenter seems to imply that the question is altogether settled. He might however have found that this view is by no means universally accepted. That the movement of the Radiometer is due not to any direct action of the solar (or other) rays, but to their effect upon residual gases, is now indeed, owing to my more recent researches, a matter of demonstration. That some such explanation was in my mind at the time of my first publication of the phenomena, as one of the probable causes of the repulsion resulting from radiation, is shown by the following quotations:—

> I object to the term *perfect* as applied to any vacuum at present known.
>
> That the residual gas in an air-pump vacuum is capable of exerting considerable mechanical action, may be assumed by the phenomena attending the passage of meteorites through the upper regions of the atmosphere, their friction against the air at an average height of 65 miles above the earth's surface raising them to incandescence.[9]
>
> Whether the ethereal waves actually strike the substance moved, or whether at that mysterious boundary surface separating solid from gaseous matter there are intermediary layers of condensed gas which, taking up the blow, pass it on to the layer beneath, are problems the solution of which must be left to further research.[10]

[8] *Loc. cit.*
[9] *Phil. Trans.* December 11, 1873, vol. clxiv. pp. 507, 524.
[10] *Phil Mag.* August, 1874.

My experiment on the movement of the glass case of a Radiometer[11] is termed 'an ingenious arrangement first devised by Dr. Schuster and subsequently improved on by Mr. Crookes;' whereas, during the discussion which followed the reading of Dr. Schuster's paper at the Royal Society on the 23rd of March, 1876, I mentioned an experiment which I had tried some time before, bearing on his observations. I afterwards tried my own experiment in a modified form; and as the results were very decided and appeared calculated to throw light on many disputed points in the theory of these obscure actions, I described the experiment, and showed the apparatus at work, at the next meeting of the Royal Society. Without wishing in the least to detract from the merits of Dr. Schuster, I may add that our two experiments are entirely different as to mode of arrangement and simplicity of exhibition. They doubtless both prove the same thing—the existence of a reactionary force between the moving fly and the glass case; but whilst Dr. Schuster's experiment requires special arrangement of lime light, lantern, reflecting mirrors, torsion threads, &c., and then temporarily demonstrates only the reactionary force, my experiment merely requires a radiometer floating in a basin of water, and a small magnet to fix the fly, when the case rotates steadily and continuously.

Whilst Dr. Carpenter was trying to prove from my papers that I was committed to a wrong theory which I was reluctant to abandon, how could he avoid reading the following sentences?

> Throughout the course of these investigations I have endeavoured to remain unfettered by the hasty adoption of a theory, which, in the early stages of an inquiry, must almost of necessity be erroneous. Some minds are so constituted that they seem impelled to form a theory on the slightest experimental basis. There is then great danger of their becoming advocates, and unconsciously favouring facts which seem to prove their preconceived ideas and neglecting others which might oppose their views. This is unfortunate, for the mind should always be free to exercise the judicial function, and give impartial weight to every phenomenon which is brought before it. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of any theory which has yet come to my mind.[12]
>
> As there is much discussion at present respecting the cause of these movements, and as some misunderstanding seems to prevail as to my own views on the theory of the repulsion resulting from radiation, I wish to take this opportunity of removing the impression that I hold opinions which are in antagonism to some strongly urged explanations of these actions. I have on five or six occasions specially stated that I wish to keep free from theories.[13]

Why, also, may I ask, has Dr. Carpenter when speaking of the Radiometer attributed to me the words 'new force' and 'new mode of force'? They are not my words. From which of my papers did he quote them?

[11] *Proceedings of the Royal Society*, No. 168, 1876.
[12] *Quarterly Journal of Science*, July, 1875.
[13] *Proceedings of the Royal Society*, No. 168, 1876.

The time for a mere popular preliminary sketch of the Radiometer is gone by; that for its thorough and exhaustive appreciation has not yet come. Dr. Carpenter gives neither, but devotes the remaining part of his paper to exhibiting as a solitary 'lesson' the contrast assumed to exist between Mr. Crookes the physicist, investigating the phenomena of the Radiometer, and Mr. Crookes the 'spiritualist,' examining the manifestations of 'psychic force.' To use his own language, he brings 'into contrast with the admirable series of scientific investigations which led up to that invention, his [Mr. Crookes's] thoroughly unscientific course in relation to another doctrine of which he has put himself forward as the champion.'

In order to prove what he terms the 'duality' of my mental constitution, Dr. Carpenter contrasts my researches on the Radiometer with some experiments I made six years ago when I attempted to solve the mystery of the phenomena called spiritual, and he describes the apparatus I devised to test the alteration of the weight of suspended bodies in Mr. Home's presence, by mere contact and without pressure. In a lecture delivered at Chelsea, on the 19th of January, 1872, Dr. Carpenter referred to this experiment; and whether his description was accurate will be seen by an extract from a letter by Mr. A. R. Wallace dated February 15, 1872:—

> In the report of Dr. Carpenter's lecture at Chelsea there occurs a passage so extraordinary and so entirely misleading that I must beg you, in the interests of truth, to allow me to make a few remarks upon it. Dr. Carpenter is stated to have said that he would grapple with Mr. Crookes's 'Psychic Force;' and, in attempting to do so, exhibited an experiment intending to show (and which his audience must have believed really did show) that Mr. Crookes 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. Dr. Carpenter, it is said, exhibited a glass of water poised against an equal weight upon a balance, and showed that by dipping a finger in the water—that is, by *pressing* with a force exactly equal to the weight of the water displaced by the immersed finger—you increased the weight on that side of the balance. Now, unless the audience were intended to believe that Mr. Crookes was ignorant of this childishly simple fact, and, further, that it completely accounted for the result of his experiment, for what purpose was this experiment shown? Yet if this *is* what it was intended to prove, then it becomes absolutely certain that Dr. Carpenter could never have read Mr. Crookes's account of his experiments given in October last in the *Quarterly Journal of Science* (for he would certainly not wilfully misrepresent the experiment), and was therefore in complete ignorance of what he was attempting to disprove. For, will it be believed, Mr. Crookes expressly states that '*dipping the hand to the fullest extent into the water does not produce the least appreciable action on the balance*,' the reason of which is sufficiently clear, for his woodcut shows, and his description tells us, that the vessel of water was not placed on the scale of a balance at all, but on a board *exactly over its fulcrum* or point of support at one end, while the distant end was suspended from a balance. Yet this balance showed a force of more than *one pound* exerted on it, when Mr. Home merely dipped the tips of the fingers of one hand in the water!

I have no wish in this article to discuss Mr. Home's psychic

powers. I simply wish to ask, has Dr. Carpenter described my test arrangements correctly, and were these arrangements scientifically devised and employed, or were they, as Dr. Carpenter maintains, absurd, and even childish? Dr. Carpenter says in the *Nineteenth Century*:

> Nothing would have been easier than for Mr. Crookes, on the one hand, to have carefully watched Mr. Home, to have precisely imitated his whole procedure, and to have done his best to depress the board to the same degree by his own muscular effort; and, on the other hand, to have devised an 'indicator' for *downward* pressure by which it could be at once determined whether Mr. Home could depress the lever-board without such muscular effort. . . . The *fact* was simply that the lever-board went down when Mr. Home's hands were laid upon it; and the testimony of Mr. Crookes and his friends was quite sufficient to justify others in accepting it as such. On the other hand, Mr. Crookes's assertion that the lever-board went down in obedience to some other force than that of Mr. Home's muscular pressure was *not* a fact, but an inference drawn by Mr. Crookes; and this inference he had no scientific right to draw until he had assured himself by every conceivable test that Mr. Home *did* not and *could* not so depress it.

Dr. Carpenter also says that I have never published any proof obtained from these test experiments, although explicitly challenged to do so in the *Quarterly Review*, October, 1871.

Dr. Carpenter must surely know that the experiment to which he takes exception was merely the first of a series. Had he described the later experiments in full, the public could not have failed to perceive that the test which I am blamed for not trying would have been utterly useless. I will therefore briefly describe these experiments, which are given in detail and illustrated with woodcuts in the *Quarterly Journal of Science* for July and October, 1871. To meet the *foreseen* objection that while the attention of the observers was otherwise engaged, Mr. Home might possibly slide his fingers along the board and thus obtain leverage, I placed a vessel of water, *with its centre exactly over the fulcrum* of the board the extremity of which was attached to the weighing-machine. To prevent Mr. Home touching the bottom of the vessel, and to lessen the possibility of 'rhythmical agitation,' a copper basin with several perforations in its bottom was supported on a retort stand, so as to dip into the water in the first vessel. Into this basin Mr. Home plunged his fingers. By this arrangement it was rendered impossible for Mr. Home to obtain leverage—that is, if the doctrine still holds good that water transmits pressure equally in all directions. Further, the copper vessel acting as a breakwater, any rhythmical agitation set up by Mr. Home would be much enfeebled before reaching the bottom of the outer vessel. Yet, in spite of these precautions, the depressions of the board were substantially the same as when Mr. Home placed his fingers on the wood. But I went still further. I caused Mr. Home to place his hands not on the board at all, but on the table on which the fulcrum rested, first near the end of the board

and then at distances gradually increasing to three feet. Still the balance recorded great variations of pressure. Finally, while Mr. Home placed his hands in the position just mentioned, witnesses held both his hands and his feet. The result was still the same, the balance indicating ebbs and flows of pressure. I submit, therefore, that Dr. Carpenter's test would have been here a mere waste of time, and that I was fully justified in its omission. Indeed, it was as unnecessary as a determination of Mr. Home's 'downward pressure' on the chair on which he was sitting, or on his boots when standing.

One most significant conclusion which might be drawn, and which must surely suggest itself to every man of science who reads the history of the Radiometer, is the importance of *residual phenomena*. It is well known to chemists that of late years new elementary bodies, new interesting compounds, have often been discovered in residual products, in slags, flue-dusts, and waste of various kinds. In like manner if we carefully scrutinise the processes either of the laboratory or of nature, we may occasionally detect some slight anomaly, some excess or deficiency of action, some unanticipated phenomenon, which we cannot account for, and which, were received theories correct and sufficient, ought not to occur. Such residual phenomena are hints which may lead the man of disciplined mind and of finished manipulative skill to the discovery of new elements, of new laws, possibly even of new forces. Upon undrilled men these possibilities are simply thrown away. The untrained physicist or chemist fails to catch these suggestive glimpses. If they appear under his hands, he ignores them as the miners of old did the ores of cobalt and nickel. That in the experiments undertaken to determine the atomic weight of thallium I should at once detect a slight anomaly in the action of my very delicate balance, should consider it worthy of the most minute and protracted investigation, and should follow up the clue for so many years, is surely sufficient to refute the charge of imperfect training advanced by Dr. Carpenter at the close of his article in the *Nineteenth Century*. The moral might have been pointed with additional force by a reference to my discovery of thallium itself, which was likewise the result of the careful and systematic examination of a chemical residue, in which, when a mere boy, I had detected a chemical anomaly, and noted it for further investigation.

This great lesson—the importance of residual phenomena—must be pronounced of the highest moment to the student, and interesting, surely, even to the multitude. Yet Dr. Carpenter, addressing a highly cultivated class of readers, overlooks it altogether! He gives, indeed, an account of the 'origin of these researches,' and pronounces it 'rather singular,' but the moral he desires to point is of a totally different nature.

As I have said, Dr. Carpenter can draw but one lesson from the

analysis of my scientific researches, and he insists that it is criminal to be '"possessed" of any ideas, or class of ideas, that the common sense of educated mankind pronounces to be irrational.'[14] But the 'common sense of educated mankind' at one time denied the circulation of the blood, and pronounced the earth to be the immovable centre of the universe. At the present day it upholds errors and absurdities innumerable, and 'common sense' has been well characterised as the name under which men deify their own ignorance. Are scientific men never to step over a rigid line, to refrain from investigation because it would clash with common-sense ideas? How far should we have advanced in knowledge if scientific men had never made known new discoveries, never published the results of their researches for fear of outraging this 'common sense of educated mankind'? Take the very subject which suggests the text for Dr. Carpenter's article. Can the wildest dreams of the spiritualist ask credence to anything more repugnant to 'common sense' than the hypotheses imagined by science, and now held to account for the movements of the Radiometer? In the glass bulb which has been exhausted to such a degree that 'common sense' would pronounce it to be quite empty, we must conceive there are innumerable smooth elastic spheres, the molecules of the residual gas, dashing about in apparent confusion, with sixty times the velocity of an express train, and hitting each other millions of times in a second. Will the 'common sense of educated mankind' consider this rational doctrine? Again, both inside this empty space and outside it, between the reader and the paper before him, between the earth and the sun, occupying all the interplanetary space further than the eye can reach or indeed the mind can conceive, there is assumed to be a *something* indefinitely more elastic and immeasurably more solid than tempered steel, a medium in which suns and worlds move without resistance. Is not such a doctrine utterly incredible to the 'common sense of educated mankind'? Yet the kinetic theory of gases and the undulatory theory of light are accepted as true by nine-tenths of the scientific men of the present day; and doubtless in the processes of scientific evolution in the coming times many a discovery will be brought to light to give a sharp shock to 'the common sense of educated mankind.'

[14] P. 256.

WILLIAM CROOKES.