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XLVI. Certain dimensional properties of matter in the gaseous state. An answer to Mr. George Francis Fitzgerald

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proposition of Gen. Schubert we think it undesirable to pass in complete silence over one point, which, though it does not belong directly to the subject, yet to avoid misunderstanding demands some explanation. We refer to the statement, frequently occurring both in General Schubert's essay in the *Astronomische Nachrichten* and in his communication to the Academy, that it is to Airy that the English arc owes its preeminent position as marking an epoch in geodesy, through the application by him of the before-mentioned corrections to individual latitudes. This statement seems to be entirely without foundation; for in the account of the English arc we find nothing which can be regarded as in favour of this statement, but rather the contrary. The only occasion on which Airy's name occurs in that work in connection with the investigations of local attraction is in the mention of his ingenious speculation by which he seeks to explain the phenomenon that the Himalayas exert no sensible influence upon the plumb-line at the neighbouring stations of the Indian arc. This speculation alone should have sufficed to prove that Airy did not approve of the application generally and unconditionally of such corrections. I have moreover had the opportunity, partly by letter and partly by oral communication, of learning what are Airy's views on this point; and think myself entitled to say that that distinguished philosopher is in agreement with me in the opinion that such correction of latitudes in general must be regarded as opposed to the geodetical purpose, while at the same time he certainly does not ignore the bearing which such investigations must have upon geological studies. If there is any thing in the said English work which could suggest the thought that Airy was directly concerned in it, it would be the careful, circumspect, and, in a word, masterly treatment of the geodetic material; but for the credit of this too, Airy, as I know from his own lips, waives all claim: it belongs exclusively to the authors named upon the title-page —to the present Director of the Ordnance Survey, Sir Henry James, and to his distinguished Assistant, Captain Clarke.

XLVI. *Certain Dimensional Properties of Matter in the Gaseous State. An Answer to* Mr. George Francis Fitzgerald. *By* Professor Osborne Reynolds, *F.R.S.**

In the February number of the Philosophical Magazine there appeared a paper by Mr. Fitzgerald, in which he criticised my paper "On certain Dimensional Properties of Matter in the Gaseous State," Philosophical Transactions

* Communicated by the Author.

of the Royal Society, 1879. Mr. Fitzgerald courteously put his remarks in the form of questions, expressing the hope that I would answer them. I was prevented by other work from preparing any thing in time for insertion in the April number; but I now ask your space for a few remarks.

The objections taken by Mr. Fitzgerald to my work may be summed up as three:—

(1) That by dividing space into eight regions I have adopted a method which is at once inelegant and unnecessarily elaborate.

(2) That I have omitted terms which, if retained, would have altered the results.

(3) That I have changed my views and adopted the theory which I had previously combated.

To all these accusations I would most emphatically plead *not guilty*. And I would further suggest, in explanation of Mr. Fitzgerald's difficulty, (1) that he has not paid equal attention to all parts of my paper, but has rather confined his attention to those parts which relate to the phenomena of impulsion, in which he seems to be especially interested, and that thus he has failed to see that, in order to obtain any results whatever for transpiration, the division of space into regions is necessary; and (2) that in his anxiety to find a different result in the case of impulsion from that which I had obtained, he has failed to perceive that the terms which I have neglected, and of which he instances one as disproving my conclusion, are of a distinctly smaller order of magnitude than those which appear in my result.

As regards, then, the charge of inelegance, I am sure that Mr. Fitzgerald would not for one moment have urged it had he not thought that the particular step to which he objects might be replaced by some other known method. One might as well abuse David because he used a stone and sling, as object to the inelegance of a mathematical method by which alone true results have been obtained. Of course I do not for one moment defend my method as being elegant, nor should I have noticed this remark were it not that, taken together with the more definite criticism to the same effect, it shows conclusively that Mr. Fitzgerald has failed to notice the gist of the greater portion of my paper—that he has failed to notice one of the most important terms in the equation of transpiration and the manner in which this term enters. In the paragraph beginning at the bottom of page 104 he says, "With the symbols and notation I have no fault to find; but I must enter a protest against his elaborate and totally unnecessary *division of space into eight regions*. He might have perfectly well calculated equations (43) to (47) without rendering a difficult subject tenfold as elaborate as was necessary." And then he goes on to show how I might have obtained equations for the aggregate results at one integration. Clearly, then, he has seen no object in my division of space into regions, and is at a loss to account for it except as mere clumsiness in the integrations. Had he, however, looked closer, or even been careful to be accurate in his statement, he would have seen that the two equations (44), which are among those to which he refers, only apply to the partial groups for which u is respectively positive and negative, and that they contain a term which apparently disappears if the respective members of the two equations be added; and he would have seen that the same thing is true of equations (45)*, which hold only for groups for which v is respectively positive and negative, and from which two terms disappear when the results are added. Now these terms, which are the first and second, are sufficiently obvious in the partial equations, whereas they do not appear at all if the integration be extended to both groups; and if Mr. Fitzgerald had followed the next articles (83) and (84), he would have seen why these terms are important. To ignore these two articles is to ignore the method by which the results for transpiration are obtained; and these results were the main purpose of the preliminary work in the paper.

To obtain any results at all for transpiration, it is necessary to divide space into two regions, or else to consider the mean range s as function of the position of the point and discontinuous at the solid boundaries; and by the latter method the determination of the form of the function requires that space should be divided. The results depend entirely on the terms which, when s is constant, disappear in the complete integration, but which, if different arbitrary values are assigned to s for the different regions, do not cancel when the partial integrals are added. No result whatever is obtained by complete integration if s be constant; and although Mr. Fitzgerald does not seem to have noticed it, the late Professor Maxwell fol-

* The partial equations (45):—

$$\sigma_y^{v+}(\mathrm{M}u)=\frac{\rho\alpha\mathrm{U}}{2\sqrt{\pi}}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{U}}{dy}-\frac{s}{2\pi}\frac{d\rho\alpha^2}{dx}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{V}}{dx},$$

$$\sigma_y^{v-}(\mathrm{M}u)=-\frac{\rho\alpha\mathrm{U}}{2\sqrt{\pi}}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{U}}{dy}+\frac{s}{2\pi}\frac{d\rho\alpha^2}{dx}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{V}}{dx}.$$

The equation obtained by complete integration:—

$$\sigma_y(\mathrm{M}u)=-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{U}}{dy}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha\mathrm{V}}{dx}.$$

lowed me in dividing space into two regions at the bounding surfaces, calling the two groups the *absorbed and evaporated gas*. But without the use of arbitrary coefficients he had no means of dealing with the variable condition of his gas, except by assuming that the same distribution holds in both groups at all points. To meet this assumption (which, he points out at the top of page 253 *, is improbable) he had further to assume a highly complex and improbable condition of surface; and the result is that the equation he obtains (77) is short of the most important term. This term is that which gives the result when the tubes are small compared with s; and as this is the only case in which the results are appreciable, when Maxwell came to apply his equation to an actual case there was no sensible result.

In the first instance, I also began by considering space as divided only at the bounding surface, and, assuming the distribution in the two groups the same, integrated for the complete space; and the result I then obtained was precisely the same in form as that subsequently obtained by Maxwell. These results correspond with the experimental results for a tube whose diameter is large compared with s—called by Graham *transpiration*; but they do not at all correspond with the law which Graham found to hold when he used a fine graphite plug, and which I have shown to hold also with coarse stucco plugs when the gas is sufficiently rare, viz. that the times of transpiration of equal volumes of different gases are proportional to the square roots of the atomic weights. Graham had considered this law as depending on the fineness of the pores of the plug, and had suggested that the action then resembled that of effusion through a small aperture in a thin plate, rather than transpiration through a tube of uniform bore; and this is the assumption which Maxwell falls back upon to account for the difference between his calculated results and those of experiment. That I did not do the same was owing to my having, by reasoning *ab initio*, after the manner explained in the analogy of the batteries, in the very first instance found that the law of the square roots of the atomic weights must hold in a tube whenever the gas was so rare that the molecules ranged from side to side without encounter, and to my having proved by experiment that both laws might be obtained with the same plug by changing the density of the gas. It was thus clear to me that some term had been omitted in my equation; and after a long search it was found that, though the term vanished in the complete integral, it appeared in the partial integrals when space was divided into regions, and that, as the values

* "On Stresses in Rarefied Gases," Appendix, p. 249, Phil. Trans. 1879.

of s were obviously different in the different regions, the assumptions on which the complete integral had been obtained were clearly at fault. The further division into eight regions was not only for the sake of symmetry, but that all the other terms which enter into the partial integrals might be examined, and as being necessary in particular cases—as, for instance, in that of a round tube, which is also treated of in the paper.

Having thus shown that, however elaborate and inelegant, the division of space into regions is essential, it is unnecessary to defend it on other grounds. But I may remark, by the way, that such a division does tend greatly to simplify the consideration of motion. This, I think, is proved by the universal adoption of north, east, south, west, zenith, and nadir.

I have dwelt at considerable length on the foregoing point, as the misconception of this point is fundamental to all Mr. Fitzgerald's criticism. The rest I may answer shortly.

With regard to Professor Maxwell's remarks on my paper, and his own work on the same problem, of course the sad circumstance of his death occurring, so that this was about the last work he did, renders it very difficult to approach the subject; but with reference to what I have already said, and in explanation of the apparently imperfect idea at which he arrived as to the scope and purpose of my method, it may be stated that, before writing his own paper, Professor Maxwell had only seen my paper in manuscript in the condition in which it was first sent in to the Royal Society, when the preliminary part was very much compressed, and, as I fear, somewhat vaguely stated, besides being founded on different assumptions from the present. Without entering further upon this now, I may refer to a letter which I addressed to Prof. Stokes after seeing an early copy of Prof. Maxwell's paper, and before I was aware of his illness, which letter was subsequently published in the Proceedings of the Royal Society for April 1880, p. 300.

Mr. Fitzgerald has asked me for an explanation of the system on which certain terms are retained and others neglected. This is difficult to give in a few words; but I was under the impression that it is sufficiently explained in the paper. It seems to me that the difficulty which Mr. Fitzgerald has found must have arisen from his having adopted the hitherto vague way of looking at the mean path of a particle (or in this case the mean range) as a small quantity, without strictly inquiring as compared with what it is small. In my paper, s is nowhere to be regarded as small except in cases where it comes into direct comparison with some definitely larger quantity. The small factors are $\frac{U}{\alpha}$, $\frac{s}{\alpha}\frac{d\alpha}{dx}$, and $\frac{s}{\alpha}\frac{dU}{dx}$;

the squares of such quantities being consistently neglected. Such factors as $\frac{s^2}{\alpha}\frac{d^2\alpha}{dx^2}$ and variations of higher order are zero in the case of transpiration, but in the case of impulsion they are of the same order as the results. But the retention of such terms in equations (42) to (48), or in the fundamental theorem, would only give rise in the results to such terms as $\frac{s^3}{r\alpha}\frac{d^2\alpha}{dx^2}$; so that as long as s is small compared with r no error can have arisen from the neglect of these terms. And this is the only case to which these results have been applied, the extreme case where s is large compared with r having been dealt with by a special method which gives rigorous results. In the first instance, all terms of the second order such as $\frac{s^2}{\alpha}\frac{d^2\alpha}{dx^2}$ were retained; and it was only after it was found that these did not in any way affect the results as a first approximation that they were neglected. The terms I have neglected are, as far as I perceive, the same as those neglected by Professor Maxwell; and such was the care taken in this matter (which is of fundamental importance) that I am very confident that there is no mistake. On the other hand, it is difficult for me to see how Mr. Fitzgerald can have failed to see that the residual term, which he instances as showing that I am wrong in saying that my equations show that there is no force in the case of parallel flow, is distinctly of the second order of small quantities. But even to this term he has no right; for in order to obtain results to such an order the variations of s would have to be considered. It seems that Mr. Fitzgerald is of opinion that the parallel flow of heat does cause stresses in the gas, and that he has been trying to find that I have not disproved the possibility of such stresses. If he confines his attention to stresses of the same order of magnitude as those now shown to exist in the case of converging or diverging flow, he will find that both Professor Maxwell and I have proved the impossibility of their existence; but if he goes, as he appears unwittingly to have done, to a higher order of small quantities, then I have nothing to say, except that he has no inconsiderable task before him.

Lastly, as regards the charge of having changed my views and having adopted a theory which is practically the same as that which I had been previously combating, I can only say that against no theory have I said a word of which I do not maintain the truth. I have never asserted that the variation of pressure in the direction of the flow of heat, which I have consistently maintained to be necessary to the production of the phenomena of impulsion, may not be attended by a difference of pressure in different directions; and, of course, I have known that such must be the case since the time that I have seen and proved by experiment that this direct variation of the pressure depends on the convergence of the lines of flow, which was before the letter referred to appeared in 'Nature.' But what I have consistently maintained is, that a difference of pressure in different directions (*i. e.* parallel and normal to the hot and cold surface) will not explain the experimental results; and this was the theory advanced in opposition to mine, and which Mr. Fitzgerald still seems inclined to defend.

I am asked to mention the result which is referred to in art. 54. I can only point to every phenomenon of the radiometer; for there the gas between the hot and cold surfaces always maintains a greater pressure on the hot than on the cold plate—a result which is fully explained in art. 129, as the consequence of the divergence of the lines of flow from the hot plate and their convergence onto the cold plate, shown in fig. 13. If Mr. Fitzgerald will only study the phenomena, he will see that it is he who has misapprehended the entire problem. He says a difference of pressure in different directions might tend to cause the plates to recede from each other. Obviously it would; but then there is not the slightest evidence that the plates do so tend to recede, while they actually move in the same direction, the cold plate following the hot. Hence no force merely causing them to separate can explain the phenomena. I have pointed this out over and over again, and now, so far from having changed my views, I have to go over the same ground again. I will take a simple case—a light mill with two equal radial vanes in the same plane, and on opposite sides of the pivot, one black and one white. Let the light be placed exactly opposite the vanes, and let the vanes be at rest. Also let the surface of the vessel and the gas be generally at the mean temperature of the vanes. If, then, the force were only such as tends to separate the hot and cold surfaces, there would be exactly the same force between the comparatively hot black vane and the colder glass as between the comparatively hotter glass and the colder white vane; for there are the same differences of temperature; and therefore the forces on the two vanes would tend to turn the mill in opposite directions, and the mill would remain at rest, instead of whirling round as it actually does. That the flow of heat caused the surfaces to follow each other was proved from the first by the experiments; and that there is no force causing the surfaces to separate of the same order of magnitude as the force which causes them to follow is now proved by the kinetic theory.

I think that now Mr. Fitzgerald will reconsider his protest against § 53; for while maintaining, on the one hand, a theory fundamentally different from that in my paper, he can hardly maintain, on the other, that there are no such theories, and that they have not found supporters. But, in truth, the remark in art. 53 was not applied to the theory which Mr. Fitzgerald seems to be supporting; and as I am sure that he is not prepared to maintain that the phenomena of the radiometer take place in an *absolute vacuum*, or are due to the *same cause as gravitation*, I am sure that he will not wish to stand sponsor to all the thoories set forth since 1874.

In conclusion, I would say one word in acknowledgment of those remarks in Mr. Fitzgerald's paper that were the reverse of critical, and to confess that it is a matter of no small satisfaction to have found a reader of Mr. Fitzgerald's knowledge and acumen.

Owens College,
March 24, 1881.

XLVII. *An Integrating-Machine.*

By C. V. Boys, *Assoc. Royal School of Mines.**

[Plate VIII.]

ALL the integrating-machines hitherto made of which I can find any record may be classed under two heads:—one, of which Amsler's beautiful instrument is the sole representative, depending on the revolution of a disk which partly rolls and partly slides on the paper; the other, comprising all the remaining machines, depending on the varying diameters of the parts of a rolling system. As this subject has been treated so recently by Mr. Merrifield in his "Report on the Present State of Knowledge of the Application of Quadratures and Interpolation to Actual Data," read at the meeting of the British Association at Swansea, 1880, in which he briefly describes previous machines and refers to the papers in which a full description may be found, I do not think it advisable to say more concerning them, except that none of them do their work by the method of the mathematician, but in their own way. The machine, however, which I have the honour of bringing before the notice of the Physical Society is an exact mechanical translation of the mathematical method of integrating $y\,dx$, and thus forms a third type of instrument.

The mathematical rule may be described in words as follows:—Required the area between a curve, the axis of x, and two ordinates. It is necessary to draw a new curve such that

* Communicated by the Physical Society, having been read at the Meeting on February 26.