SCIENTIFIC PAPERS.

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PREFACE.

THIS volume contains, in addition to a further selection from my scientific papers, a few articles reprinted from the last edition of the *Encyclopaedia Britannica*; and an Introductory Lecture to my Ordinary Class, devoted mainly to the question of how Natural Philosophy ought, as well as how it ought not, to be taught. For permission to reprint these I am indebted to the courtesy of Messrs A. & C. Black, and of Messrs Isbister, respectively.

I have been assured by competent judges that my remarks on Science Teaching, as it is too commonly conducted, are not only in no sense exaggerated, but are *even now* as appropriate and as much needed as they seemed to me twenty years ago.

To the short article on Quaternions I was inclined to attach special importance, of course solely from the historical point of view; for (in consequence of my profound admiration for Hamilton's genius) I had spared neither time nor trouble in the attempt to make it at once accurate and as complete as the very limited space at my disposal allowed. Yet, as will be seen from the short note now appended to the article, the claims of Hamilton to entire originality in the matter have once more been challenged:—on *this* occasion in behalf of Gauss. [It is noteworthy that Hamilton himself seems to have had at one time a notion that, if he *had* been anticipated, it could have been only by that very remarkable man. But he expresses himself as having been completely reassured on the subject, by a pupil of Gauss who was acquainted with the drift of his teacher's unpublished researches. See Hamilton's *Life*, Vol. III. pp. 311—12, 326.]

It is therefore with much regret that I allow this volume to be issued before full materials are available for the final settlement of such an important question in scientific history. But it is reasonable to conclude that the so-called anticipations had at least no very intimate connection with a subject at once so novel and so unique as Quaternions. For Gauss, though he survived their (hitherto supposed) date of birth for about twelve years, certainly seems to have made no (public) claim in the matter.

The arrangement of the contents is, as nearly as possible, that adopted in the former volume:—all papers on one large subject, such as the Kinetic Theory of Gases, Impact, the Linear and Vector Function, the Path of a Rotating Spherical Projectile, &c., being brought into groups in relative sequence. I have reprinted only the later of my papers on the Kinetic Gas Theory. The earlier were numerous, but fragmentary, and a great part of their contents (often in an improved form) had been embodied in the later ones.

I have again to thank Drs Knott and Peddie for their valuable help in reading the proofs.

It is intended that a third volume shall contain some later papers together with a complete list (including those not re-published) and a general Index.

P. G. TAIT.

COLLEGE, EDINBURGH,

January 15*th*, 1900.

CONTENTS.

LXI.

REPORT ON SOME OF THE PHYSICAL PROPERTIES OF FRESH WATER AND OF SEA-WATER.

[*From the* "Physics and Chemistry" *of the Voyage of H.M.S. Challenger;* Vol. II. Part IV., 1888.]

INTRODUCTION.

As I had taken advantage of the instruments employed for the determination of the *Pressure Errors of the Challenger Thermometers*[1] to make some other physical investigations at pressures of several hundred atmospheres, Dr Murray requested me to repeat on a larger scale such of these as have a bearing on the objects of the Challenger's voyage. The results of the inquiry are given in the following paper. The circumstances of the experiments, whether favourable to accuracy or not, are detailed with a minuteness sufficient to show to what extent of approximation these results may be trusted. My object has been rather to attempt to settle large questions about which there exists great diversity of opinion, based upon irreconcilable experimental results, than to attain a very high degree of accuracy. My apparatus was thoroughly competent to effect the first, but could not without serious change (such as greatly to affect its strength) have been made available for the second purpose. The results of Grassi, Amaury and Descamps, Wertheim, Pagliani and Vincentini, &c., as to the compressibility of water at low pressures, differ from one another in a most distracting manner; and the all but universal opinion at present seems to be that, for at least five or six hundred atmospheres, there is little or no change in the compressibility, the explicit statement of Perkins notwithstanding. My experiments have all been made with a view to direct application in problems connected with the Challenger work, and therefore at pressures of at least 150 atmospheres, so that I have only incidentally and indirectly attacked the first of these questions; but I hope that no doubt can now remain as to the proper answer to the second. The study of the compressibility of various strong solutions of common salt has, I believe, been carried out for the first time under high pressures; and the effect of pressure on the maximum-density point of water has been approximated to by three different experimental methods, one of which is direct.

[1] *Narr. Chall. Exp.*, vol. II., App. A., 1882. (*Anté*, No. LX.)

CONTENTS.

COMPRESSIBILITY OF WATER, GLASS, AND MERCURY.

I. General Account of the Investigation.

I will first give a general account of the subjects treated, of the mode of conducting the experiments, and of the difficulties which I have more or less completely overcome in the course of several years' work. The reader will then be in a position to follow the full details of each branch of the inquiry.

The experiments were for the most part carried on in the large Fraser gun fully described and figured in my previous Report[1]. But it was found to be impracticable to maintain this huge mass of metal at any steady temperature, except that of the air of the cellar in which it is placed. The great thickness of the College walls, aided by the comparative mildness of recent winters, thus limited till the beginning of the present year the available range of temperature for this instrument to that from 3° C. to about 12° C. As I did not consider this nearly sufficient, and as comparative experiments at the higher and lower of these temperatures could only be made at intervals of about six months, I procured (in May 1887) a much less unwieldy apparatus. It was made entirely of steel, so as to be of as small mass as possible, with the necessary capacity and strength: and could at pleasure be used at the temperature of the air, or be wholly immersed in a large bath of melting ice. As this apparatus was mounted, not in a cellar but, in a room sixty feet above the ground and facing the south, it enabled me to obtain a temperature range of 0° C. to 19° C., with which I was obliged to content myself. A great drawback to the use of this apparatus was found in the smallness of its capacity. Not only was I limited to the use of *two*, instead of six or seven, piezometers at a time; but the pressure could not be got up so slowly and smoothly as with the large apparatus, and (what was still worse) it could not be let off so slowly. In spite of these and other difficulties, to be detailed later, I think it will be found that the observations made with this apparatus are not markedly inferior in value to those made with the great gun.

In the piezometers I have adhered to the old and somewhat rude method of recording by means of indices containing a small piece of steel, and maintained in their positions (till the mercury reaches them and after it has left them) by means of attached hairs. These indices are liable to two kinds of deceptive displacement, upwards or downwards, by the current produced at each stroke of the pump, or by that produced during the expansion on relief of pressure. The first could almost always be avoided, even in the smaller apparatus, provided the pressure was raised

[1] Pressure Errors of the Challenger Thermometers. *Anté*, No. LX.

with sufficient *steadiness*, and the index brought down to the mercury at starting. But the instantaneous reaction, partly elastic, partly due to cooling, and on rare occasions due to leakage of the pump or at the plug, after a rash stroke of the pump, sometimes left the index a little *above* the mercury just before the next stroke. If another rash stroke followed, the index might be carried still farther above the point reached by the mercury. Practically, however, there is little fear of my estimates of compression having been exaggerated by this process. They are much more likely to have been slightly diminished by a somewhat sudden fall of pressure which, in spite of every care, occasionally took place at the very commencement of the relief. Once or twice the experiments were entirely vitiated by this cause; but, as we had recorded the sudden outrush *before* the plug had been removed in order to take out the piezometers, we were fully warranted in rejecting the readings taken on such an occasion:—and we *invariably* did so, whether they agreed with the less suspicious results or not.

Another and very puzzling source of uncertainty in the use of these indices depends on the fact that the amount of pressure required to move them varies from one part of the tube to another, sometimes even (from day to day) in the same part of the tube:—and the index thus records the final position of the top of the mercury column *in different phases of distortion* on different occasions. The effect of this will be to make all the determinations of compression *too small*, and it will be more perceptible the smaller the compression measured. And in sea-water, and still more in strong salt-solutions, the surface-tension of the mercury changes (a slight deposit of calomel (?) being produced), while the elasticity of the hairs also is much affected. But, by multiplying the experiments, it has been found possible to obtain what appears a fairly trustworthy set of mean values by this process.

I discarded the use of the silvering process, which I had employed in my earlier experiments[1], partly because I found that the mercury column was liable to break, especially when sea-water was used, partly from the great labour and loss of time which the constant resilvering and refilling of the piezometers would have involved. This process has also the special disadvantage that the substance operated on is not necessarily the same in successive repetitions of the experiment.

And the electrical process[2] which I devised for recording the accomplishment of a definite amount of compression could not be employed, because it was impossible to lead insulated wires into either of my compression-chambers. This was much to be regretted, as I know of no method but this by which we can be absolutely certain of the temperature at which the operation is conducted.

My next difficulty was in the measurement of pressure. In my former Report I have pointed out the untrustworthiness of the Bourdon gauges, and the uncertainty of the unit of my external gauge. This gauge was amply sufficient for all the purposes of my investigation of the errors of the Challenger thermometers, where the inevitable error of a deep-sea reading formed, according to the depth, from 5 to 20 per cent. of the pressure error; but, besides the uncertainty as to its unit, it was on so small a scale that an error of 1 per cent. in the reading, mainly due to

[1] *Proc. Roy. Soc. Edin.*, vol. XII. pp. 223, 224, 1883.

[2] *Appendix* A to this Report.

capillary effects at the surface of the mercury column, was quite possible when the pressure did not exceed 150 atmospheres. Fortunately I was informed of the great improvement made by Amagat on the principle of the old *Manomètre Desgoffes*,—an improvement which has made it an instrument of precision instead of an ingenious scientific toy. M. Amagat was so kind as to superintend the construction of one of his instruments for me (it will be a surprise to very many professors of physics in this country to hear that the whole work was executed in his laboratory), and to graduate it by comparison with his well-known nitrogen gauge. My measurements of pressure are therefore only *one* remove from Amagat's 1000 feet column of mercury.

The change of temperature produced by compression of water is one of the most formidable difficulties I have encountered. During the compression the contents of the piezometer, as well as the surrounding water, constantly change in temperature; and the amount of change depends not only on the initial temperature of the water, but also on the rapidity with which the pressure is raised. It was impossible to ascertain exactly what was the true temperature of the water in the piezometer at the instant when the pressure was greatest, and a change of even 0°·1 C. involves a displacement of the hair index, which is quite easily detected even by comparatively rude measurement. Any very great nicety of measurement was thus obviously superfluous. My readings, therefore, were all made directly by applying to the tube of the piezometer a light but very accurate scale. The zero of this scale was adjusted to the level of the upper surface of the mercury of each piezometer the instant it was removed from the water-vessel, in which it was lifted from the pressure-chamber, and the position of the index was afterwards read at leisure. As the same scale was employed in the calibration of the piezometer tubes, its unit is, of course, of no consequence. The expansibility of water at atmospheric pressure is so small, at least up to 8° C., that no perceptible displacement of the mercury can have been introduced before the zero of the scale was adjusted to it. The effects of the raising of temperature by heating are two: a direct increase of the volume (provided the temperature be above the maximum-density point, and the pressure be kept constant), and a diminution of compressibility (provided the temperature be under the minimum compressibility point). These conspire to diminish the amount of compression produced by a given pressure. At 15° C., or so, the first of these is, in the range of my experiments, the more serious of the two, especially in the case of the solutions of common salt.

The water in the compression apparatus, even when the large one was used, slowly changed in temperature from one group of experiments to the next:—sometimes perceptibly during the successive stages of one group. The effect of this source of error was easily eliminated by means of the rough results of a plotting of the uncorrected experimental data. From this the effect of a small change of temperature on the compressibility at any assigned temperature was determined with accuracy far more than sufficient to enable me to calculate the requisite correction. This correction was therefore applied to all the experimental data of each group, for which the temperature differed from that at the commencement of the group. The corrected numbers were employed in the second and more complete graphical calculation. I endeavoured to raise the pressure in each experiment as nearly as possible by 1, 2, or 3 tons weight per square inch:—having convinced myself by many trials that this was the most convenient plan. The cure for any (slight) excess or defect of pressure was at once supplied by the graphical method employed in the reductions, in which the pressures were laid down as abscissæ, and the corresponding average compressibilities per atmosphere as ordinates.

When this work has been fully carried out, we have still only the *apparent* compressibility of the water or salt-solution. The correction for the compressibility of glass, which is by no means a negligible quantity,—being in fact about 5 per cent. of that of water at 0° C.,—involves a more formidable measurement than the other; but I think I have executed it, for two different temperatures, within some 2 per cent. or so. The resulting values of the true compressibility of water may therefore err, on this account, by 0·1 per cent. This is considerably less than the probable error of the determinations of apparent compressibility, so that it is far more than sufficient. With a view to this part of the work the piezometers, whether for water or for mercury, were all constructed from narrow and wide tubes of the same glass, obtained from one melting in Messrs Ford's Works, Edinburgh; while solid rods of the same were also obtained for the application of Buchanan's method[1].

My results are not strictly comparable with any that, to my knowledge, have yet been published, except, of course, those which I gave in 1883 and 1884. The reason is that the lowest pressure which I applied (about 150 atmospheres, or nearly one ton weight per square inch) is far greater than the highest employed by other experimenters, at least for a consecutive series of pressures. I must except, however, the results of Perkins and some remarkable recent determinations made by Amagat[2]. Perkins' results are entirely valueless as to the *actual* compressions, because his pressure unit is obviously very far from correct. They show, however, at one definite temperature, the rate at which the compressibility diminishes as the pressure is raised. Amagat's work, on the other hand, though of the highest order, is not yet completed by the determination of the correction for the compression of the piezometer.

The extension of my formulæ to very low pressures, though it agrees in a remarkable manner with some of the best of accepted results, such as those of Buchanan and of Pagliani and Vincentini, is purely conjectural, and may therefore possibly involve error, but not one of the least consequence to any inquiries connected with the problems to which the Challenger work was directed.

The piezometers, which had been for three years employed on water and on sea-water, were, during the end of last summer, refilled with solutions of common salt of very different strengths, prepared in the laboratory of Dr Crum Brown. The determinations of compressibility were made at three temperatures only, those which could be steadily maintained, viz. 0° C., 10° C., and about 19° C., the two latter being the temperature of the room, the former obtained by the use of an ice-bath. Here great rapidity of adjustment of the scale to the mercury was requisite, even in the experiments made near 0° C., for the salt solutions (especially the nearly saturated one)

[1] *Trans. Roy. Soc. Edin.*, vol. XXIX. pp. 589–598, 1880.

[2] *Comptes Rendus*, tom. CIII., 1886, and tom. CIV., 1887.

show considerable expansibility at that temperature. In these salt solutions, however, the hair indices behave very irregularly; so that this part of my work is much inferior in exactitude to the rest.

Besides the determinations briefly described above, there will be found in this Report a number of experimental results connected with the effect of pressure on the temperature of water and on the temperature of the maximum density of water. Though I afterwards found that the question was not a new one, I was completely unaware of the fact when some experiments, which I made in 1881 on the heat developed by compressing water, gave results which seemed to be inexplicable except on the hypothesis that the maximum-density point is lowered by pressure. Hence I have added a description of these experiments, since greatly extended by parties of my students.

And I have appended other and more direct determinations of the change of the maximum-density point. I also give, after Canton, but with better data than his, an estimate of the amount by which the depth of the sea is altered by compression. Also some corresponding inquiries for the more complex conditions introduced by the consideration of the maximum-density point, &c.

An Appendix contains all the theoretical calculations, the results of which are made use of in the text; as well as some speculations, not devoid of interest, which have arisen in the course of the inquiry.

II. Some former Determinations.

There seems now to be no doubt that Canton (in 1762) was the first to establish the fact of the compressibility of water. But he did far more; he measured its apparent amount at each of three temperatures with remarkable accuracy, and thus discovered (in 1764) the curiously important additional fact that it diminishes when the temperature is raised. As his papers, or at all events the second of them, seem to have fallen entirely out of notice[1], and as they are exceedingly brief and clear, I think it well to reproduce some passages textually from the *Philosophical Transactions* of the dates given above.

"Having procured a small glass tube of about two feet in length, with a ball at one end of it of an inch and a quarter in diameter; I filled the ball and part of the tube with mercury; and, keeping it, with a Fahrenheit's thermometer, in water which was frequently stirred, it was brought exactly to the heat of 50 degrees; and the place where the mercury stood in the tube, which was about $6\frac{1}{2}$ inches above the ball, was carefully marked. I then raised the mercury, by heat, to the top of the tube, and sealed the tube hermetically; and when the mercury was brought to the same degree of heat as before, it stood in the tube $\frac{32}{100}$ of an inch higher than the mark.

[1] Perhaps the reason may be, in part, that by a printer's error the title of Canton's first paper is given (in the Index to vol. LII. of the *Phil. Trans.*) as "Experiments to prove that Water is not compressible."

"The same ball, and part of the tube being filled with water exhausted of air, instead of the mercury, and the place where the water stood in the tube when it came to rest in the heat of 50 degrees, being marked, which was about 6 inches above the ball; the water was then raised by heat till it filled the tube; which being sealed again, and the water brought to the heat of 50 degrees as before, it stood in the tube $\frac{43}{100}$ of an inch above the mark.

"Now the weight of the atmosphere (or about 73 pounds avoirdupois) pressing on the outside of the ball and not on the inside, will squeeze it into less compass[1]. And by this compression of the ball, the mercury and the water will be equally raised in the tube; but the water is found, by the experiments above related, to rise $\frac{11}{100}$ of an inch more than the mercury; and therefore the water must expand, so much, more than the mercury, by removing the weight of the atmosphere.

"In order to determine how much water is compressed by this, or a greater weight, I took a glass ball of about an inch and $\frac{6}{10}$ in diameter which was joined to a cylindrical tube of 4 inches and $\frac{2}{10}$ in length, and in diameter about $\frac{1}{100}$ of an inch; and by weighing the quantity of mercury that exactly filled the ball, and also the quantity that filled the whole length of the tube; I found that the mercury in $\frac{23}{100}$ of an inch of the tube was the 100,000 part of that contained in the ball; and with the edge of a file, I divided the tube accordingly.

"This being done, I filled the ball and part of the tube with water exhausted of air; and left the tube open, that the ball, whether in rarefied or condensed air, might always be equally pressed within and without, and therefore not altered in its dimensions. Now by placing this ball and tube under the receiver of an air-pump, I could see the degree of expansion of the water, answering to any degree of rarefaction of the air; and by putting it into a glass receiver of a condensing engine, I could see the degree of compression of the water, answering to any degree of condensation of the air. But great care must be taken, in making these experiments, that the heat of the glass ball be not altered, either by the coming on of moisture, or its going off by evaporation; which may easily be prevented by keeping the ball under water, or by using oil only in working the pump and condenser.

"In this manner I have found by repeated trials, when the heat of the air has been about 50 degrees, and the mercury at a mean height in the barometer, that the water will expand and rise in the tube, by removing the weight of the atmosphere, 4 divisions and $\frac{6}{10}$; or one part in 21,740; and will be as much compressed under the weight of an additional atmosphere. Therefore the compression of water by twice the weight of the atmosphere, is one part in 10,870 of its whole bulk[2].

[1] "See an account of experiments made with glass balls by Mr Hooke (afterwards Dr Hooke) in Dr Birch's *History of the Royal Society*, vol. I. p. 127."

[2] "If the compressibility of the water was owing to *any air* that it might still be supposed to contain, it is evident that *more* air must make it *more compressible;* I therefore let into the ball a bubble of air that measured near $\frac{6}{10}$ of an inch in diameter, which the water absorbed in about four days; but I found upon trial that the water was not *more* compressed, by twice the weight of the atmosphere, than before."

"The compression of the glass in this experiment, by the equal and contrary forces acting within and without the ball, is not sensible: for the compression of water in two balls, appears to be exactly the same, when the glass of one is more than twice the thickness of the glass of the other. And the weight of an

"The famous Florentine Experiment, which so many philosophical writers have mentioned as a proof of the incompressibility of water, will not, when carefully considered, appear sufficient for that purpose: for in forcing any part of the water contained in a hollow globe of gold through its pores by pressure, the figure of the gold must be altered; and consequently, the internal space containing the water, diminished; but it was impossible for the gentlemen of the Academy del Cimento to determine, that the water which was forced into the pores and through the gold, was exactly equal to the diminution of the internal space by the pressure."

"By similar experiments made since, it appears that water has the remarkable property of being more compressible in winter than in summer; which is contrary to what I have observed both in spirit of wine and oil of olives: these fluids are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold. Water and spirit of wine I have several times examined, both by the air-pump and condenser, in opposite seasons of the year: and, when Fahrenheit's thermometer has been at 34 degrees, I have found the water to be compressed by the mean weight of the atmosphere 49 parts in a million of its whole bulk, and the spirit of wine 60 parts; but when the thermometer has been at 64 degrees, the same weight would compress the water no more than 44 parts in a million, and the spirit of wine no less than 71 of the same parts. In making these experiments, the glass ball containing the fluid to be compressed must be kept under water, that the heat of it may not be altered during the operation.

"The compression by the weight of the atmosphere, and the specific gravity of each of the following fluids, (which are all I have yet tried,) were found when the barometer was at $29\frac{1}{2}$ inches, and the thermometer at 50 degrees.

		Millionth parts.	Specific gravity.
Compression of	Spirit of Wine,	66	846
„	Oil of Olives,	48	918
„	Rain-Water,	46	1000
„	Sea-Water,	40	1028
„	Mercury,	3	13595

These fluids are not only compressible, but also elastic: for if the weight by which they are naturally compressed be diminished, they expand; and if that by which they are compressed in the condenser be removed, they take up the same room as at first. That this does not arise from the elasticity of any air the fluids contain, is evident; because their expansion, by removing the weight of the atmosphere, is not greater than their compression by an equal additional weight: whereas air will expand twice as much by removing half the weight of the atmosphere, as it will be compressed by adding the whole weight of the atmosphere.

"It may also be worth observing, that the compression of these fluids, by the same weight are not in the inverse ratio of their densities or specific gravities, as might be supposed. The compression of spirit of wine, for instance, being compared

atmosphere, which I found would compress mercury in one of these balls but $\frac{1}{3}$ part of a division of the tube, compresses water in the same ball 4 divisions and $\frac{6}{10}$."

with that of rain-water, is *greater* than in this proportion, and the compression of sea-water is *less*."

With the exception of the mistake as to the non-effect of compressibility of glass, and its consequences (a mistake into which Örsted and many others have fallen long since Canton's day), the above is almost exact. The argument from the fact that thick and thin vessels give the same result is unfounded; but the discovery of the fact itself shows how accurate the experiments must have been. The formula (A) below (Section VII.), if extended to $p = 0$, gives for the value of the apparent compressibility of water at 10° C. (50° F.), which is what Canton really measured, the number

$$0{\cdot}0000461,$$

exactly the same as that given by him 126 years ago!

The next really great step in this inquiry was taken by Perkins in 1826. He showed beyond the possibility of doubt that in water at 10° C. the compressibility diminishes as the pressure is increased, quickly at first, afterwards more and more slowly[1]. This was contested by Örsted, who found no change of compressibility up to 70 atmospheres. Many other apparently authoritative statements have since been made to the same effect. Unfortunately Perkins' estimates of pressure are very inaccurate, so that no *numerical* data of any value can be obtained from his paper.

Colladon[2] is sometimes referred to as an authority on the compression of liquids. But, referring to Canton, he states that there is no difference in the compressibility of water at 0° C. and at 10° C. His words are: "Nous avons trouvé que l'eau a la même compressibilité à 0° et à + 10°. Nous avons déjà fait observer les causes d'erreur qui ont dû altérer les résultats des expériences de Canton." There can be no doubt whatever that there is a difference of 6 per cent., which is what Canton gives!

In Regnault's experiments[3] pressure was applied alternately to the outside and to the inside of the piezometer, and then simultaneously to both. From the first *Appendix* to my *Report on the Pressure-Errors*, &c., it will be seen that the three measurements of changed content thus obtained are not independent, the third giving the algebraic sum of the first two; so that, unless we had an absolutely incompressible liquid to deal with, we could not employ them to determine the elastic constants of the piezometer. For the compression of the liquid contents is added to the quantity measured, in the second and third of the experiments. Thus Regnault had to fall back on the measurement of Young's modulus, in order to obtain an additional datum. In place of this, Jamin afterwards suggested the measurement of the change of external volume of the piezometer; and this process was carried out by Amaury and Descamps. But there are great objections to the employment of external, or internal, pressure alone in such very delicate inquiries. For, unless the bulbs be truly spherical, or cylindrical, and the walls of perfectly uniform thickness

[1] The carefully drawn plate which illustrates his paper is one of the very best early examples of the use of the graphic method. *Phil. Trans.*, vol. CVI. p. 541, 1826.

[2] *Mém. Inst. Savans Étrang.*, tom. V. p. 296, 1838.

[3] *Mém. Acad. Sci. Paris*, tom. XXI. pp. 1 *et seq.*, 1847.

and of perfectly uniform material, the theoretical conditions will not be fulfilled:—and the errors may easily be of the same order as is the quantity to be measured.

Finding that he could not obtain good results with glass vessels, Regnault employed spherical shells of brass and of copper. With these he obtained, for the compressibility of water, the value

0·000048 per atm.

for pressures from one to ten atmospheres. The temperature, unfortunately, is not specially stated.

Grassi[1], working with Regnault's apparatus, made a number of determinations of compressibility of different liquids, all for small ranges of pressure.

The following are some of his results for water:—

Temperature.	Compressibility per atm.
0°·0 C.	0·0000503
1°·5	515
4°·1	499
10°·8	480
18°·0	462
25°·0	456
34°·5	453
53°·0	441

These numbers cannot be even approximately represented by any simple formula; mainly in consequence of the maximum compressibility which, they appear to show, lies somewhere about 1°·5 C. No other experimenter seems to have found any trace of this maximum.

Grassi assigns, for sea-water at 17°·5 C., 0·94 of the compressibility of pure water, and gives

0·00000295

as the compressibility of mercury. He also states that the compressibility of salt solutions increases with rise of temperature. These are not in accordance with my results. But, as he further states that alcohol, chloroform, and ether *increase* in compressibility with rise of pressure (a result soon after shown by Amagat to be completely erroneous), little confidence can be placed in any of his determinations.

A very complete series of measurements of the compressibility of water (for low pressures) through the whole range of temperature from 0° C. to 100° C., has been made by Pagliani and Vincentini[2]. Unfortunately, in their experiments, pressure was applied to the inside only of the piezometer, so that their indicated results have to be diminished by from 40 to 50 per cent. The effects of heat on the elasticity of glass are, however, carefully determined, a matter of absolute necessity when so large a range of temperature is involved. The absolute compressibility of water at 0° C.

[1] *Ann. de Chimie*, sér. 3, tom. XXXI. p. 437, 1851.

[2] *Sulla Compressibilità dei Liquidi*, Torino, 1884.

is assumed from Grassi. The following are some of their results, showing a much larger temperature effect than that obtained by Grassi:—

Temperature.	Compressibility per atm.
0°·0 C.	0·0000503
2°·4	496
15°·9	450
49°·3	403
61°·0	389
66°·2	389
77°·4	398
99°·2	409

Thus water appears to have its minimum compressibility (for low pressures) about 63° C.

My own earlier determinations[1] will be given more fully below (Section VI.). I may here quote one or two, premising that they were given with a caution (not required, as it happens), that the pressure unit of my external gauge was somewhat uncertain. They are *true*, not *average*, compressibilities. See *Appendix* B.

At 12°·0 C.

		Ratio
Fresh water	$0{\cdot}00720\ (1 - 0{\cdot}034p)$	1 : 0·925
Sea-water	$0{\cdot}00666\ (1 - 0{\cdot}034p)$	

At 15°·5 C.

		Ratio
Fresh water	$0{\cdot}00698\ (1 - 0{\cdot}05p)$	1 : 0·924
Sea-water	$0{\cdot}00645\ (1 - 0{\cdot}05p)$	

In all of these the unit of pressure is one ton-weight per square inch (152·3 atm.). The diminution of compressibility with increased pressure was evident from the commencement of the investigations. I assumed, throughout, for the compressibility of glass

0·000386 per ton,

which, as will be seen below, is a little too small.

By direct comparison with Amagat's manometer, I have found that the pressure unit of my external gauge is too small, but only by about 0·5 per cent. This very slight underestimate of course does not account for the smallness of the pressure term of the first expression above. As will be seen later, the true cause is probably to be traced to the smallness of the piezometers which I used in my first investigations, and to the fact that their stems were cut off "square" and *dipped* into mercury. Allowing for this, it will be seen that the above estimates of compressibility agree very fairly, in other respects, with those which I have since obtained. The sea-water employed in the comparison with fresh water was collected about a mile and a half off the coast at Portobello, and was therefore somewhat less dense (and more compressible) than the average of ocean-water. In my later experiments, to be detailed

[1] *Proc. Roy. Soc. Edin.* 1883 and 1884.

below, the sea-water operated on was taken at a point outside the Firth of Forth, considerably beyond the Isle of May.

As stated in my Report on the Pressure Errors, &c., the unit of my external gauge was determined by the help of Amagat's data for the compression of air. As the piezometer containing the air had to be enclosed in the large gun, the record was obtained by silvering the interior of the narrow tube into which the air was finally compressed:—and the heating of the air by compression, as well as the uncertainty of the allowance for the curvature of the mercury, alone would easily account for the underestimate. Besides, it is to be remembered that the reading of the external gauge for 152 atm. is only about 22 mm.; so that a slight variation of surface-curvature of the mercury would of itself explain a considerable part of the half per cent. deficit. It is, however, a matter of no consequence whatever, as regards the conclusions of that Report.

Buchanan, in the paper already cited, gives for the compressibility of water at 2°·5 C. the value 0·0000516; and at 12°·5 C., 0·0000483. The empirical formula, which is one of the main results of this Report (Section VII. below), extended to $p = 0$, gives 0·0000511 and 0·0000480 respectively. The agreement is very remarkable.

Amagat's[1] investigations, which were carried out by means of the electric indicator already alluded to (which informs the experimenter of the instant at which *a given amount of compression* is reached), have been extended to pressures of nearly 20 tons weight on the square inch (3000 atm.). As a preliminary statement he gives the average apparent compression (per atmosphere) of water at 17°·6 C. as follows:—

From	1	to	262	atm.	0·0000429,
„	262	to	805	„	0·0000379,
„	805	to	1334	„	0·0000332.

And he states that, at 3000 atmospheres, water (at this temperature) has lost about 1/10 of its original bulk. But Amagat has not yet published any determination of the compressibility of his glass, so that the *amount* of compression shown by his experiments cannot be compared with the results of this paper. The rate of diminution of compressibility with increased pressure, however, can be (very roughly) approximated to; and Amagat appears to make it somewhat less than I do. He operated on distilled water, thoroughly deprived of air. My experiments were made on cistern water, boiled for as short a time as possible. The analogies given in the present paper appear to show that this difference of substance operated on may perhaps suffice completely to explain the difference between our results.

I am indebted to a footnote in the recent great work of Mohn[2] for a hint which has led me to one of the most singular calculations as to the compressibility of water which I have met with. As it is given in a volume[3] whose very *raison d'être* is supposed to be the minutest attainable accuracy in physical determinations, I consulted it with eagerness. The reader may imagine the disappointment with which I

[1] *Comptes Rendus*, tom. CIII. p. 429, 1886, and tom. CIV. p. 1159, 1887.

[2] Den Norske Nordhavs-Exped., Nordhavets Dybder, &c., Christiania, 1887.

[3] *Travaux et Mémoires du Bureau International des Poids et Mesures*, tom. II. p. D30, Paris, 1883.

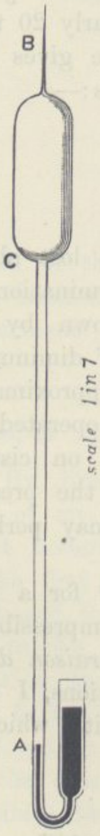
found that, as regards compressibility of water, its main feature is the amazing empirical formula,—

$$501{\cdot}53 - 1{\cdot}58995t - 0{\cdot}003141113t^2\,!$$

This formula represents a parabola which is everywhere convex upwards, and thus cannot possibly be consistent with the existence of a minimum compressibility. Instead of representing the results of new experiments, it is based on data extracted from the old and very dubious results of Grassi (two data being wrongly quoted), Descamps, and Wertheim, which differ in the wildest way from one another. What method of calculation has been employed upon this chaotic group we are not told. The result is a smug little table (D. IX.), in which no single entry can be looked upon as trustworthy! Plate II. fig. 1, shows some of the materials, as well as the final extract or quintessence derived from them.

III. The Piezometers—Reckoning of Log. Factors—Compressibility of Mercury.

The annexed sketch shows the form of piezometer employed. Six of these instruments, three filled with fresh water and three with sea-water, were simultaneously exposed to pressure. The upper end of the bulb at B was drawn out into a very fine tube, so that the instruments could be opened and refilled several times without appreciable change of internal volume. They were contained in a tall copper vessel which was let down into the pressure cylinder, and which kept them (after removal from it) surrounded by a large quantity of the press water till they could be taken out and measured one by one; each, after measurement, being at once replaced in the vessel. Large supplies of water were kept in tin vessels close to the pressure apparatus; and the temperatures of the contents of all were observed from time to time with a Kew Standard.



The stems, AC, of the piezometers were usually from 30 to 40 cm. in length, and the volumes of the cylindrical bulbs, CB, were each (roughly) adjusted to the bore of the stem, so that the whole displacement of the indices in the various vessels should be nearly the same for the same pressure. At A, on each stem, below the working portion, the special mark of the instrument was made in dots of black enamel (*e.g.* ∴, .., ⁝, &c.), so that it could be instantly recognised, and affixed to the record of the index in the laboratory book. Above this enamel mark a short millimetre scale was etched on the glass for the purpose of recording the volume of the water contents at each temperature before pressure was applied. The factor by which the displacement of the index has to be multiplied, in order to find the whole compression, varies (slightly) with the initial bulk of the water-contents. This, in its turn, depends on the temperature at which the experiment is made. Practically, it was found that no correction of this kind need be made in experiments on fresh water between 0° and 8° C., but for higher temperatures it rapidly came into play. In the case of the stronger salt-solutions it was always required.

As an example of the general dimensions of the piezometers, I print here the details of a rough preliminary measurement of one only; and employ these merely to exhibit the nature of the calculation for the compressibility of the contents.

Measurements for (:).

21/12/86. At temperature 3° C. (:) filled with Portobello sea-water gave for

413	of gauge (about	150 atm.)	131·2	of displacement for index
834	„ „	300 „	256	
1254	„ „	450 „	373·6	

Before pressure, mercury 20 mm. from enamel.

This experiment is selected because its data were taken for the approximate lengths of the columns of mercury used to calibrate the stem of (:).

22/6/87.

	Length of col. of mercury in stem.		Weight, mercury and dish.
End 18 mm. from enamel		130·8 mm.	12·567 grm.
„ 45 „		130·8 „	Dish 9·387 „
„ 72 „		130·9 „	
„ 100 „		130·9 „	Hg. 3·180 „
„ 140 „		131·1 „	

Another column of Hg.:—

End 18 mm. from enamel	261 mm.	15·712 grm.
„ 36 „	261·1 „	9·387 „
„ 57 „	261·1 „	
„ 75 „	261·1 „	Hg. 6·325 „
„ 94 „	261·3 „	

Again another:—

End 18 mm. from enamel	372·6 mm.	18·407 grm.
„ 43 „	372·4 „	Dish 9·387 „
		Hg. 9·020 „

Weight of dish with Hg. filling bulb and stem to

599 mm. from enamel,	517·63 „
Weight of dish,	37·69 „
Hg. in piezometer, less 599 of stem,	479·94 „
Hg. in 599 of stem,	14·56 „
Whole content to enamel,	494·50 „
„ 20 from enamel,	494·0 „

The calculations are as follows,—the Gauge log will be explained in Section IV.:—the formula is given in *Appendix* C, and the mantissæ only are written:—

log 494 = ·69373
log 130·8 = ·11661

(Sum) ·81034
log 3·18 = ·50243

(Difference) ·69209
Gauge log ·43856

(Sum) ·13065 = log factor for pressures near 150 atm.

·69373	·69373
·41664	·57124
·11037	·26497
·80106	·95521
·69069	·69024
·43856	·43856
·12925 for 300 atm.	·12880 for 450 atm.

Hence apparent average compressibility of Portobello sea-water per atm. at 3° C. as given by (:) on 21/12/86 is,

For first ton.......................... ·11793 = log 131·2
·61595 = log 413

·50198
log factor ·13065

·63263 Antilog = ·00004292

first two tons .... ·40824
·92117

·48707
·12925

·61632 Antilog = ·00004134

first three tons............ ·57240
·09829

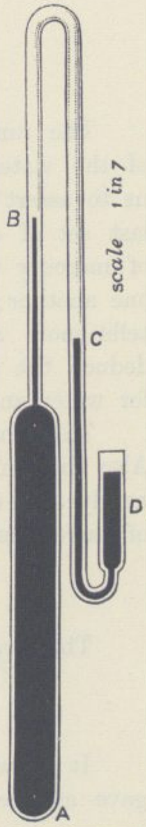
·47411
·12880

·60291 Antilog = ·00004008

A few larger instruments were made for very accurate comparisons of fresh water and sea-water at about 1 ton weight per square inch, and at different temperatures.

The mercury contents of their bulbs, &c., were over 1000 grm. The content of 250 mm. of stem in mercury was about 7 grm.; and the log factor, for pressures about 150 atm., nearly = 0·8.

For the compressibility of mercury, the annexed form of piezometer was employed, as in this case the recording index could not be put in contact with the liquid to be compressed. The bulb A and stem to B contain mercury, and so does the U-tube CD. Between B and C there is a column of water, whose length is carefully determined. The recording index rests on the mercury column at C. Thus, obviously, its displacement is due to



Compression of mercury AB + Compression of water BC − Compression of vol. of glass vessel from A to C.

The measurements of this apparatus are:—

MERCURY PIEZOMETER. 25/7/87.

Hg. and vessel	1100	grm.
Vessel	37·7	„
Weight of mercury whose compression is measured...	1062·3	„
Hg. and dish	14·412	„
Dish	9·386	„
Weight of mercury in 210 mm. of tube BC	5·026	„
Length of water column BC	286	mm.

The observations made with this apparatus were as follows, the results calculated being added, enclosed in square brackets:—

22/6/86.	Kew Standard, 12°·75.		24/6/86.	K. S. 12°·4.
	Alteration of Index, 17 mm.			Index, 17
	Gauge pressure, 811			Pressure, 833
	[Apparent compressibility, 0·00000102]			[0·00000098]

25/6/86.	K. S. 12°·3.			
	Index,	18·5	26·0	26·0
	Pressure,	834	1252	1257
		[0·00000109]	[102]	[101]

23/7/87. K. S. 1°·2.

Index,	7·3	17·3	25
Pressure,	436	865	1264
	[0·00000074]	[94]	[93]

25/7/87. K. S. 16°·5.

Index,	9	16·6	25
Pressure,	459	866	1271
	[0·00000093]	[92]	[95]

The range of temperature is quite sufficient to allow a change of compressibility of the water column to be noted; but the experiments unfortunately do not enable us to assert anything as to a change in that of mercury; though, were it not for the last set of experiments, there would appear to be a decided *increase* of compressibility of mercury with rise of temperature. The experiments are only fairly consistent with one another; but this was noted at the time as the fault of the index, which, of course, tells more as the quantity measured is less. It may be as well to show how to deduce the compressibility of mercury from them at once, assuming the requisite data for water and for glass from subsequent parts of the Report.

Take, for instance, the first result of 25/6/86. 834 of gauge is about 305 atmospheres. Also shortening of 286 mm. of water column (in glass) at 12°·3 C. by 305 atm. = 3·7 mm. nearly:—so that the compressed mercury apparently loses about the content of 14·8 mm. of narrow tube = bulk of 0·354 grm. Hg.

$$\text{Apparent compressibility} = \frac{0{\cdot}354}{305 \times 1062{\cdot}3} = 0{\cdot}00000109.$$

The average of all the normal experiments gives 0·000001 very nearly.

Add compressibility of glass = 0·0000026,
Compressibility of mercury = 0·0000036.

It is well to remember that though Grassi, working with Regnault's apparatus, gave as the compressibility of mercury

0·00000295,

which Amaury and Descamps afterwards reduced to

0·00000187,

the master[1] himself had previously assigned the value

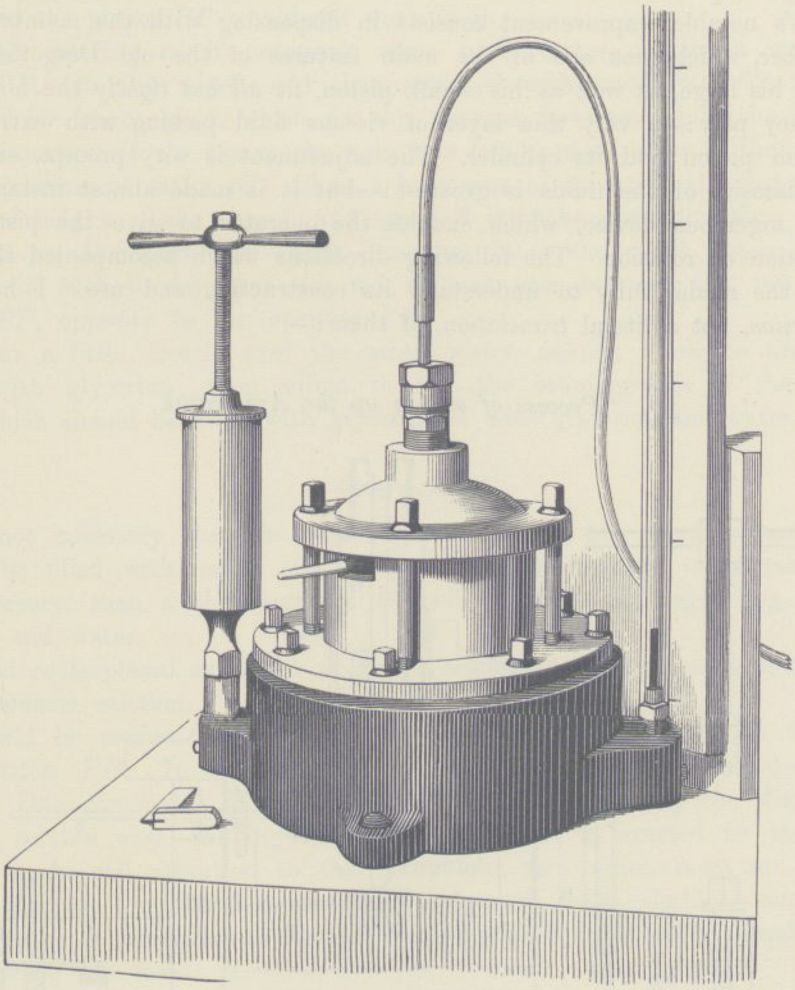
0·00000352.

Had Grassi's result been correct, I should have got only about half the displacements observed; had that of Amaury and Descamps been correct, the apparent compressibility would have had the *opposite sign* to that I obtained, so that the index would not have been displaced. In such a case the construction of the instrument might have been much simplified, for the index would have been placed in contact with the mercury at *B*, and the bent part of the tube would have been unnecessary.

[1] Relation des Expériences, &c., *Mém. Acad. Sci. Paris*, tom. XXI. p. 461, 1847.

IV. Amagat's Manomètre à Pistons libres.

The annexed sketch of the instrument (in which the large divisions shown on the manometric scale correspond to decimetres), with the section given below, will enable



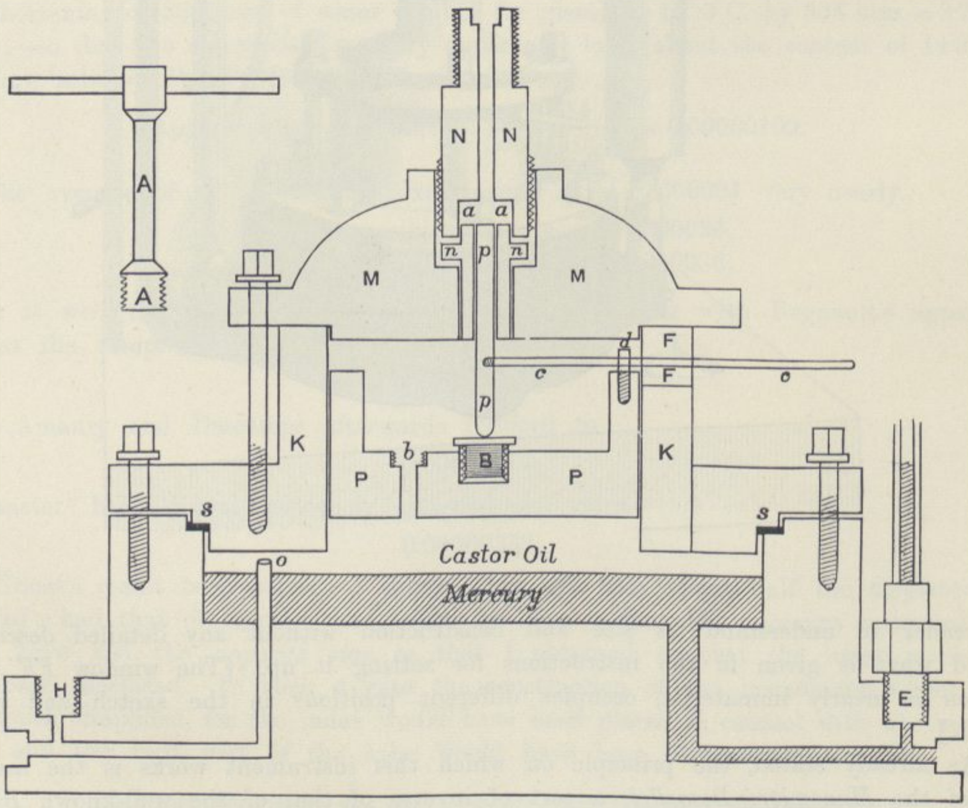
the reader to understand its size and construction without any detailed description beyond what is given in the instructions for setting it up. [The window *FF*, whose position is nearly immaterial, occupies different positions in the sketch and in the section.]

As already stated, the principle on which this instrument works is the same as that of the *Manomètre Desgoffes*, a sort of inverse of that of the well-known *Bramah*

Press. In the British instrument pistons of very different sectional area are subjected to the same pressure (that of one mass of liquid), and the total thrust on each is, of course, proportional to its section. In the French instrument the pistons are subjected to *equal* total thrusts, being exposed respectively to fluid pressures which are inversely proportional to their sections. The British instrument is employed for the purpose of overcoming great resistances by means of moderate forces; the French, for that of measuring great pressures in terms of small and easily measurable pressures.

Amagat's notable improvement consists in dispensing with the membrane, or sheet of india-rubber, which was one of the main features of the old Desgoffes manometer, and making his large, as well as his small, piston, fit *all but tightly* the hollow cylinders in which they play:—a very thin layer of viscous fluid passing with extreme slowness between each piston and its cylinder. The adjustment is very prompt, even in winter when the viscosity of the fluids is greatest:—but it is made almost instantaneous by a simple but ingenious device, which enables the operator to give the pistons a simultaneous motion of rotation. The following directions which accompanied the instrument will enable the reader fully to understand its construction and use. I have given an accurate *version*, not a literal *translation*, of them:—

"*Process of setting up the Apparatus.*



"1. Screw in, at E, the manometer tube, and at H the regulating pump.

"2. Pour in the layer of mercury, and on it that of castor oil. Fill the pump with glycerine, and insert its piston, taking care to exclude air-bubbles.

"3. Insert the gun-metal part K. Its bearing (at s) on the rim of the cast-iron base-piece must not be made with leather, but with a ring of india-rubber, or of very uniform cardboard. The fixing down of this part, by means of the (six) screws, must be done with great exactness:—otherwise (thick as it is) it might suffer a very slight distortion, and the piston PP would not work in it.

"4. After pouring in, if necessary, some more castor oil, insert *very cautiously* the piston PP, carefully wiped, and then anointed with castor oil. To put it in, it is to be held by means of A, which, for this purpose, is screwed into the middle of it. During the insertion of the piston the hole b is left open to allow of the escape of air and (possible) excess of castor oil. Close b by means of its screw, the piston being held at the desired height. Take out A, and screw B into the piston in place of it.

"5. Put on the part MM—after inserting in it the small piston pp, with its cylinder nn—in such a way that the rod cc may pass between the two studs d on the piston PP, opposite to the opening FF.

"6. Pour a little treacle over the small piston at aa; screw on the piece NN, and fill it with glycerine; then adjust to NN the coupling-tube of the compression apparatus, which should be filled with glycerine or with glycerine and water.

" *Observations.*

"It is not necessary that the whole space between the mercury and the piston PP should be filled with castor oil. A layer of glycerine and water may be placed over the mercury, then a thin layer of the oil. In fact, the regulating pump is full of glycerine and water.

"The rod cc is placed as shown to give a simultaneous rotation to the two pistons, so as to overcome stiction.

"It should be moved slowly, and in such a way as to exert no vertical force upon the piston PP. It ought to be pushed by a vertical straight-edge, moved horizontally. One can judge of the delicacy of the apparatus by the displacement of the mercury column when the slightest vertical pressure is exerted on the rod.

"I will again call attention to the scrupulous care which must be bestowed on the pistons and on the cylinders in which they work:—the slightest scratch, due to dust, would make it necessary to retouch these surfaces; and after several retouchings they will become too loose.

"The manometer tube, which is to be cemented into the iron piece which screws into E, should be chosen of small enough diameter to prevent sensible change of level of the mercury in the reservoir, and yet not so narrow as to prevent free motion of the mercury.

"*Important Remark.*—During the successive operations the large piston should always, by means of the regulating pump, be kept at such a height that the rod cc shall not come in contact with the wall of the opening FF, and not high enough to make the wide lower part of the small piston come against the piece M (this, of course, when the smaller of the two upper pistons is used:—that whose lower part is thickened).

"There are two pistons *pp* for this manometer. The ratio of the section of the larger to that of *PP* is 1/61·838, and the reading per atmosphere is 12·290 mm.

"For the smaller, the ratio of the sections is 1/277·75, and the reading per atmosphere is 2·736 mm.

"The former serves for the measurement of lower pressures, up to the point at which the oil passes visibly round the large piston. For higher pressures the latter must be used.

"The treacle must be changed from time to time; first, because, after a while, some of it passes the small piston; second, because it gradually dissolves in the glycerine, and at last becomes hardened round the small piston, so as to make the friction too great. The small piston and its cylinder should occasionally be cleaned with the greatest care, and anointed with neats-foot oil."

In all my later experiments I have used exclusively the smaller of the two small pistons. The scale which I fitted to the manometer tube was a long strip of French plotting paper. It had shrunk slightly, so that 752·5 divisions corresponded to 750 mm. Neglecting the difference in the values of gravity at Lyons and at Edinburgh, the number of scale divisions per atmosphere is $2{\cdot}736 \times 752{\cdot}5/750$; and its logarithm, *i.e.* the Gauge Log. above spoken of, is ·43856.

V. Compressibility of Glass.

Buchanan's process, already referred to, consists simply in measuring the fractional change of length of a glass rod exposed to hydrostatic pressure, and trebling the linear compressibility thus determined. The only difficulty it presents is that of directly measuring the length of the rod while it is under pressure. I employed a couple of reading microscopes, with screw-travelling adjustment, fixed to the ends of a massive block of well-seasoned wood. This block was placed over the tube containing the glass rod, but quite independently,—the two distinct parts of the apparatus being supported separately on the asphalt floor of a large cellar. No tremors were perceptible except when carriages passed rapidly along the wooden pavement of the street, and even then they were not of much consequence.

The ends of the tube containing the rod must, of course, be made of glass, or some other transparent material. In the first apparatus which I used, tubes of soda-water-bottle glass were employed, their bore being about 0·2 inch, and the thickness of the walls about 0·3 inch. The image of the small enamel bead at the end of the glass rod was very much distorted when seen through this tube, but the definition was greatly improved by laying on it a concavo-plane cylindrical lens (which fitted the external curvature), with a single drop of oil between them. I found, by trial, that, had it been necessary to correct for the internal curvature also, the employment of winter-green (or *Gaultheria*) oil as the compressing liquid would have effected the purpose completely:—the refractive index being almost exactly the same as that of the green glass.

As the construction and mode of support of this apparatus did not enable us completely to get rid of air from its interior, there were occasional explosions of a somewhat violent character when the glass tubes gave way; and the operators who were not otherwise protected (as by the microscopes, for instance) were obliged to hold pieces of thick plate glass before their eyes during the getting up of pressure. The explosions not only shattered the thick glass tube into small fragments, but smashed the ends of the experimental glass rod, so that a great deal of time was lost after each. Only on one occasion did we reach a pressure of 300 atm., and an explosion occurred before the measurement was accurately made. On these accounts, after four days experimenting (the first being merely preliminary), we gave up working with this apparatus:—and the results obtained by means of it cannot be regarded as wholly satisfactory, though they agreed very well with one another.

As a sudden shock might have injured the Amagat gauge, all the pressures were measured by the old external gauge, whose unit is now determined with accuracy. Hence the readings are in tons-weight per square inch (152·3 atm.), which are below called "tons" as in the vernacular of engineers. Three of us at least were engaged in each experiment, one to apply and measure the pressure, and one at each microscope. Pressure, in each group of experiments, was applied and let off six or seven times in succession, readings of the two microscopes being taken before, during, and after each application of pressure. To get rid of the possible effects of personal equation, the observers at the microscopes changed places after each group of experiments (sometimes after two groups), so that they read alternately displacements to the right and to the left.

The values of the screw-threads were carefully verified upon one of the subdivisions of the scale which was employed to measure the length of the experimental rod; these subdivisions having been since tested among themselves by means of a small but very accurate dividing-engine of Bianchi's make.

These experiments were made in July 1887, when the day temperature of the room was nearly 20° C. In the last two groups the compression tube was surrounded *in great part* by a jacket containing water and pounded ice. We had no means of ascertaining the average temperature of the glass rod, but it cannot have been more than some 5 or 6 degrees above 0° C. This was done merely to ascertain whether glass becomes less compressible or no as the temperature is lowered, not the *amount* of change. The question appears to be answered in the affirmative.

Early in the present year Mr Buchanan kindly lent me his own apparatus, which is in three respects superior to mine. (1) A longer glass rod can be operated on. (2) The air can be entirely got rid of from the interior, so that when the glass tubes give way there is no explosion. (3) The glass tubes are considerably narrower in bore (though with equal proportionate thickness), and consequently stronger. I used my own pump and external gauge, but the necessary coupling pieces were easily procured; and the reading-microscopes were fastened to a longer block of seasoned wood than before. These experiments have been made near one temperature only, but it is about the middle of the range of temperatures in my experiments on water and sea-water.

It is not necessary to print the details of the experiments in full. I give below part of a page of the laboratory book for a single day's work, to show how far the experiments of one group agree with one another. I purposely choose one in which the glass rod was somewhat displaced in the apparatus during the course of the measurements:—

23/2/88. Kew Standard, 9°·1 C.

(Length of glass rod, 75·75 inches.)

External Gauge (Lindsay).		Right Microscope (Nagel).	Left Microscope (Peddie).	Contraction and Elongation.
		in.	in.	
41·5		0·4570	0·3377	
63·5	22 = 1 ton	475	3	0·0099
41·5		570	7	0·0099
41·5		0·4571	0·3377	
63·5	22	473	3	0·0102
41·5		572	6	0·0102
41·5		0·4572	0·3376	
63·5	22	473	2	0·0103
41·5		572	6	0·0103
		(Peddie.)	(Nagel.)	
42		0·4566	0·3380	
64	22	469	77	0·0100
42		574	73	0·0101
42		0·4575	0·3373	
64	22	475	68	0·0105
42		574	73	0·0104
42		0·4574	0·3374	
64	22	475	70	0·0103
42		574	73	0·0102
			Mean, .	0·0102

The mean thus obtained coincided very closely with the mean of all the experiments. Hence the average linear compressibility per atmosphere for the first ton is, at 9°·1 C.,

$$\frac{0{\cdot}0102}{152{\cdot}3 \times 75{\cdot}75} = 0{\cdot}000000884,$$

whence the compressibility of glass is

$$0{\cdot}00000265.$$

The two series of experiments agreed fairly with one another, and appeared to show an increase of compressibility with rise of temperature, and a diminution with rise of pressure, but these are not made certain. Considerably greater ranges, both of pressure and of temperature, are necessary to settle such questions.

As I cannot trust to a unit or two in the last place (*i.e.* the seventh place of decimals) my results for the apparent compressibility of water, and as an error of reading of the external gauge may easily amount to 1 per cent. of the whole ton applied, I have taken from the above experiments the number 0·0000026 as expressing with sufficient accuracy the compressibility of the glass of the piezometers *throughout* the range of temperature 0° to 15° C., and of pressure from 150 to 450 atm. This number is simply to be *added* to all the values of apparent compressibility. Had I pushed the pressures farther than 450 atm., this correction would have required reduction, as shown in *Appendix* D.

VI. Résumé of my own Experiments on Compression of Water and of Sea-Water.

The following details are, where not otherwise stated, taken from my laboratory books. I was led to make these experiments by the non-success of an attempt to determine the exact unit of the external gauge (described in my former Report). Not being aware of the great discovery of Canton (in fact, having always been accustomed to speak of *the* compressibility of water as 1/20,000 per atm.), I imagined that I could verify my gauge by comparing, on a water piezometer, the effects of a pressure measured by the gauge with those produced by a measured depth of sea-water, without any reference to the temperatures at which measurements were made; provided, of course, that these were not very different. The result is described in the following extract[1]:—

"To test by an independent process the accuracy of the unit of my pressure gauge, on which the estimated corrections for the Challenger deep-sea thermometers depend, it was arranged that H.M.S. 'Triton' should visit during the autumn a region in which soundings of at least a mile and a half could be had. A set of manometers, filled with pure water, and recording by the washing away of part of a very thin film of silver, were employed. They were all previously tested, up to about 2½ tons weight per square inch, in my large apparatus. As I was otherwise engaged, Professor Chrystal and Mr Murray kindly undertook the deep-sea observations; and I have recently begun the work of reducing them.

"The first rough reductions seemed to show that my pressure unit must be somewhere about 20 per cent. too small. As this was the all but unanimous verdict of fifteen separate instruments, the survivors of two dozen sent out, I immediately repeated the test of my unit by means of Amagat's observed values of the volume of air at very high pressures. The result was to confirm, within 1 per cent., the accuracy of the former estimate of the unit of my gauge. I then had the manometers resilvered, and again tested in the compression apparatus. The results were now only about 5 per cent. different from those obtained in the 'Triton.' There could be no essential difference between the two sets of home experiments, except that the first set was made in July, the second in November,—while the temperatures at which the greatest compressions were reached in the 'Triton' were at least 3° C. lower than those in the latter set. Hence it seems absolutely certain that

[1] *Proc. Roy. Soc. Edin.*, vol. xii. pp. 45, 46, 1882.

water becomes considerably more compressible as its temperature is lowered, at least as far as 3° C. (the 'Triton' temperature). This seems to be connected with the lowering by pressure of the maximum density point of water[1], and I intend to work it out. It is clear that in future trials of such manometers some liquid less anomalous than water must be employed.

"Another preliminary result, by no means so marked as the above, and possibly to be explained away, is that by doubling (at any one temperature) a high pressure we obtain somewhat less than double the compression. This, however, may be due to the special construction of the manometer, which renders the exact determination of the fiducial point almost impossible."

In the winter of 1882 and the succeeding spring, I spent a great deal of time in trying to get definite results from the records of the "Triton" trials, and in making further experiments on those of the specially prepared piezometers which had not been broken or left at the bottom of the sea. But this work led to no result on which I could rely. I then directly attacked the problem of the compressibility of water at different temperatures and pressures, having once more verified the unit of my pressure gauge by comparison with Amagat's data for air. Results for one temperature were published, as below, in the *Proc. Roy. Soc. Edin.*, vol. XII. pp. 223, 224, 1883. [The mercury content of the bulbs of the new piezometers was about 200 grm., and that of 100 mm. of stem about 2·6 grm.]

"The apparatus employed was of a very simple character, similar to that which was used last autumn in the 'Triton.'

"It consisted of a narrow and a wide glass tube, forming as it were the stem and bulb of a large air-thermometer. The stem was made of the most uniform tube which could be procured, and was very accurately gauged; and the weight of the content of the bulb in mercury was determined. Thus the fraction of the whole content, corresponding to that of one millimetre of the tube, was found.

"This apparatus had the interior of the narrow tube very carefully silvered; and while the whole, filled with the liquid to be examined, was at the temperature of the water in the compression apparatus, the open end was inserted into a small vessel containing clean mercury. Four instruments of this kind were used, all made of the same kind of glass. [They were numbered, as in the headings of the columns below, 1, 2, 3, 4, respectively. 20/6/88.]

"The following are the calculated apparent average changes of volume per ton weight of pressure per square inch (*i.e.* about 150 atmospheres):—

FRESH WATER, at 12° C.

Pressure	1	2	3	4	Mean.
1	0·00670	*	665	666	0·00667
2	0·00657	*	646	656	0·00653
2·5	0·00651	650	640	648	0·00647
3	0·00641	633	636	636	0·00636

NOTE.—The first two experiments with No. 2 failed in consequence of a defect in the silvering.

[1] [The reason for this remark will be seen in the second extract in Section XII. below. 20/6/88.]

The compressibility of glass was not directly determined. It may be taken as approximately 0·000386 per ton weight per square inch.

"From these data, which are fairly consistent with one another, we find the following value of the *true* compressibility of water per ton, the unit for pressure (p) being 1 ton-weight per square inch, and the temperature 12° C.,

$$0{\cdot}0072\,(1 - 0{\cdot}034\,p)\,;$$

showing a steady falling off from Hooke's Law.

SEA-WATER, at 12° C.

Pressure	1	2	3	4	Mean.
1	0·00606	611	615	627	0·00615
2	0·00595	607	598	601	0·00600
2·5	0·00600	600	594	590	0·00594
3	0·00588	593	586	586	0·00588

NOTE.—The sea-water employed was collected about 1½ miles off the coast at Portobello.

These give, with the same correction for glass as before, the expression

$$0{\cdot}00666\,(1 - 0{\cdot}034\,p).$$

Hence the relative compressibilities of sea and fresh water are about

$$0{\cdot}925\,;$$

while the rate of diminution by increase of pressure is sensibly the same (3½ per cent. per ton weight per square inch) for both.

"With the same apparatus I examined alcohol, of sp. gr. 0·83 at 20° C.

ALCOHOL, at 12° C.

Pressure	1	2	3	4	Mean.
1	0·01202	1193	*	*	0·01200
2·5	0·01040	1052	1050	1056	0·01049
3	0·01043	1050	1043	1058	0·01048

These experiments were not so satisfactory as those with water. There are peculiar difficulties with the silver film. I therefore make no definite conclusion till I have an opportunity of repeating them."

It will be observed that the diminution of compressibility as the pressure is raised is here brought out unequivocally for all the three liquids examined.

In the course of another year I had managed to obtain similar results for a range of temperature of about 9° C. They were described in *Proc. Roy. Soc. Edin.*, vol. XII. pp. 757, 758, 1884, as follows:—

"I had hoped to be able, during the winter, to extend my observations to temperatures near the freezing point, but the lowest temperature reached by the large compression apparatus was 6°·3 C.; while the highest is (at present) about 15° C.

From so small a range nothing can be expected as to the temperature effect on the compressibility of water, further than an approximation to its values through that range.

"The following table gives the mean values of the average compression per ton weight per square inch:—

Pressure in Tons	1	2	2½	3	3½	4
6°·3 C.	0·00704	692	684	672	...	...
7°·6	...	682	...	670	660	...
11°·3	684	670	...	654	...	...
13°·1	...	666	...	648	...	637
15°·2	673	654	...	633	...	...

"These are all *fairly* represented by the expression

$$0·00743 - 0·000038t - 0·00015p,$$

where t is the temperature centigrade, and p the pressure in tons weight per square inch. This, of course, cannot be the true formula, but it is sufficient for ordinary purposes within the limits of temperature and pressure above stated. It represents the value of

$$\frac{v_0 - v}{pv_0}.$$

"With a new set of compression apparatus, very much larger and more sensitive than those employed in the above research, I have just obtained the following mean values for the single temperature 15°·5 C.:—

Pressure in Tons	1	1½	2	3
Fresh water . .	0·00678	663	657	638
Sea-water . .	0·00627	618	609	593

"These are the values of $\frac{v_0 - v}{pv_0}$, and they give, for the true compressibility $\left(-\frac{1}{v}\frac{dv}{dp}\right)$* at any pressure, and temperature 15°·5 C., the formulæ,

Fresh water . . . . . . 0·00698 (1 − 0·05p)
Sea-water . . . . 0·00645 (1 − 0·05p)

"The ratio is 0·925, *i.e.* the compressibility of sea-water at the above temperature is only 92·5 per cent. of that of fresh water."

The new and larger piezometers referred to were made when Mr Murray requested me to write this Report. They are those whose form and dimensions have been detailed in Section III. above. The former piezometers had no capsule containing mercury, but had the stem simply cut off flat at the end, and when filled with water were merely dipped in mercury. I had felt that to this was probably due

* [See *Appendix* B to this Report.]

the fact that my experiments gave a value of the compressibility at 0° C. somewhat smaller than that usually accepted. It will be seen that the very first data given by the new instruments at once tended to set this matter right. For while the formula representing the results of the smaller instruments gave the compression of water at 15°·5 C. as 0·00678 for one ton weight per square inch, that for those of the new instruments gave 0·00698, *i.e.* about 1/34th more, which is much nearer to the result of my later experiments.

For two winters after this period the apparatus was kept in working order in the hope that I might be enabled to employ temperatures between 6° and 0° C. But a single day's work at 1°·7 C., and a few days at temperatures between 3° and 5° C. were all I got. Hence the reason for procuring the smaller compression apparatus, as stated in Section I. But, as yet, my measurements of pressure were not satisfactory.

In the spring of 1886 I obtained the Amagat gauge, and after a careful comparative trial determined to employ exclusively the lesser of the two small pistons. Some time was spent upon a comparison of the indications of this instrument with those of the external gauge, with the result that single indications of the latter could not be trusted within about 1 per cent., though the mean of a number of observations was occasionally very close to the truth. I therefore put aside all the compression observations already made, and commenced afresh with the same piezometers as before, and with the Amagat gauge exclusively.

In the summer of 1886 I obtained a long series of determinations at about 11°·8 C., and others at 14°·2 and 15° C. In December of the same year I worked for a long time between 3° and 3°·5 C. All of these were with the large Fraser gun.

In June 1887, with the new compression apparatus, I secured numerous determinations at 0°·4 C.

In July the piezometers were filled with solutions of salt of various strengths, and examined at temperatures near 19° C. and 1° C. In November these were again examined, this time in the large gun at about 9° C.; and the piezometers were again filled, some with fresh water and some with sea-water.

During the winter complete series of observations in the large gun were obtained at about 7°, 5°, 3°·2, 2°·3, 1°·1; and, finally (on March 16, 1888), at 0°·5 C.

The piezometers were, once more, filled with the salt solutions, as I considered that I had obtained sufficient data for fresh water and for sea-water; except in the one important particular of the exact values of the *ratio* of their compressibilities at one or two definite temperatures and pressures.

These were finally obtained in May and June 1888, with piezometers considerably larger and more delicate than the former set.

VII. Final Results and Empirical Formulæ for Fresh Water.

Although my readings and calculations were throughout carried to four significant figures, I soon found that (for reasons already sufficiently given in Section I.) only three of these could be trusted even in the average of a number of successive experiments, and that the third might occasionally (especially with sea-water) err by an entire unit or two; at most $\frac{1}{2}$ per cent. of the whole quantity measured. Of course, now and then there occurred results so inconsistent with the rest as to indicate, without any doubt, a displacement of the index by upward or (more frequently) downward currents.

This was made obvious by comparison of the indications of any one piezometer in successive experiments at the same temperature and pressure; but it was even more easily seen in the relative behaviour of a number of piezometers which were simultaneously exposed to exactly the same temperature and pressure several times in succession. A single page of my laboratory book, taken at random, sufficiently illustrates this. To avoid confusion, I give the records of two of the ordinary instruments (with fresh water) alone, leaving out the records of those with sea-water, and I insert [in brackets] the pressures and the average apparent compressibilities calculated from the data. The water employed was that of the ordinary supply of Edinburgh, and was boiled, for a short time only, to expel air:—

23/7/86.

I.	E. G.	A. G.	2 c.		[Pressure 0·983 tons]
	25·0	8		∴ 136·2	[4333]
	46·4	419	28·0	.. —	—
	25·0	8			
		K. S. (in gun) 14°·9 C.			
II.					
	25·1	8			[0·993]
	47·0	423	28·0	∴ 137·7	[4339]
	25·1	8		.. 122·5	[4342]
		K. S. 15°			
III.					
	25·1	8			[1·992]
	68·1	841	56·0	∴ 269·0	[4218]
	25·1	8		.. 256·6	[4214]
		K. S. 15°			
IV.					
	25·2	8			[2·0]
	68·4	844	56·0	∴ 269·8	[4216]
	25·2	8		.. 258·1	[4224]
V.					
	25·2	8			[2·997]
	90·0	1261	85·0	∴ 393·7	[4092]
	25·5	8		.. 376·9	[4116]
		K. S. 15°			

VI.

25·6	8			[3·002]
90·0	1263	85·0	∴ 394·4	[4093]
25·5	8		.. 376·9	[4110]

The left-hand column gives the readings of the external gauge, the next those of Amagat's gauge, before, during, and after the application of pressure. The third gives the pressure as read by one of the internal gauges described in my previous Report. The fourth column gives the readings of the two piezometers selected; the fifth the pressure (in tons) for each experiment, and the compressibility calculated. The latter numbers are multiplied by 10^8.

Notice that, in the first experiment (..) failed to give a reading. Also in the fifth and sixth the indications of the two instruments do not agree very closely. The character of the results, however, points apparently to an error in gauging one or other of the instruments. It was the unavoidable occurrence of defects of these kinds that led me to make so many determinations at each temperature and pressure selected. The above specimen contains less than 1 per cent. of my results for fresh water, and I obtained at least as many reduced observations on sea-water.

To obtain an approximate formula for the full reduction of the observations, I first made a graphic representation, on a large scale, of the results for different pressures at each of four temperatures, adding the compressibility of glass as given in Section VI. above. From this I easily found that the average compressibility for 2 tons pressure (at any one temperature) is somewhat *less* than half the sum of those for 1 and for 3 tons. Thus the average compressibility through any range of pressure falls off more and more slowly as that range is greater. And, within the limits of my experiments, I found that this relation between pressure and average compressibility could be fairly well represented by a portion of a rectangular hyperbola, with asymptotes coincident with and perpendicular to the axis of pressure. Hence at any one temperature (within the range I was enabled to work in), if v_0 be the volume of fresh water at one atmosphere, v that under an *additional* pressure p, we have

$$\frac{v_0 - v}{pv_0} = \frac{A}{\Pi + p}$$

very nearly, A and Π being quantities to be found.

I had two special reasons (besides, of course, its adaptability to the plotted curve) for selecting this *form* of expression. *First*, it cannot increase or diminish indefinitely for increasing positive values of p, and is therefore much to be preferred in a question of this kind to the common mode of representation by ascending powers of the variable, such as two or more terms of

$$B_0 + B_1 p + B_2 p^2 + \&c.,$$

or the absolutely indefensible expression, too often seen in inquiries connected with this and similar questions,

$$C_0 + C_1 p^{\frac{m}{n}} + \&c.$$

Second, it becomes zero when p is infinite, as it ought certainly to do in this physical problem. It appeared also to suggest a theoretical interpretation. But I will say no more about this for the present, as it is simply a matter of speculation. See the latter part of Section X., below. But there is a grave objection to this form of expression, in the fact that small percentage changes in the data involve large percentage changes in A and Π, though not in the ratio A/Π. This objection, however, does not apply to the use of it in the calculations preliminary to the full reduction, as in them it is A/Π only which is required.

Next, on calculating from my data the values of A and Π for different temperatures, I found that, within the recognised limits of errors of the observations, Π might be treated as sensibly constant. Thus I was enabled easily to make graphic representations of the average compressibility at each pressure, in terms of temperature. Again I obtained curves which could, for a first trial at least, be treated as small portions of rectangular hyperbolas, with the axis of temperature as one asymptote. Hence

$$A = \frac{B}{T+t},$$

where T is a constant; and B also may for a time be treated as constant.

Thus I arrived at the empirical expression

$$\frac{B}{(\Pi+p)(T+t)}$$

whose simplicity is remarkable, and which lends itself very readily to calculation. As I required it for a temporary purpose only, I found values of the constants by a tentative process; which led to the result

$$\frac{0{\cdot}28}{(36+p)(150+t)}.$$

This gives the *average compressibility per atmosphere* throughout the range of additional pressure p, the latter being measured in tons' weight per square inch.

The following brief table shows with what approximation the (unreduced) experimental results (multiplied by 10^7) are represented by this formula. The *nearest* integer is taken in the third place:—

	1 ton.			2 tons.			3 tons.		
Temp.	Obs.	Calc.	D.	Obs.	Calc.	D.	Obs.	Calc.	D.
0°·4	503	503	0	489	490	−1	477	477	0
3°·2	492	494	−2	479	481	−2	466	469	−3
11°·8	467	468	−1	454	455	−1	441	444	−3
15°·0	459	459	0	448	447	+1	436	435	+1

The agreement is tolerably close, so that the empirical formula may be used, without any great error, in the hydrostatic equations, so long as the temperatures and pressures concerned are such as commonly occur in lakes.

But the columns of differences show that the *form* of the formula is not suitable. The pressure factor seems appropriate, but it is clear that, at any one pressure, the curve representing the compression in terms of the temperature has greater curvature than the formula assigns. Still the formula amply suffices for the reduction of the observations of any one group when the pressures or temperatures were not precisely the same in all. It was, however, not much required, for the pressure could be adjusted with considerable accuracy, and (especially when the large gun was used) the changes of temperature were very slow.

The next step was to enter, as shown in Plate II. fig. 3, *all* the results obtained from the various piezometers at each definite temperature and pressure, with the view of selecting the most probable value. The amount of discordance was in all cases very much the same as that shown in the plate for the series of experiments at two tons' pressure and the one temperature 5° C. It will be observed that the extreme limits of divergence from the mean are not more than about two units in the third significant place. For a pressure of one ton this corresponds to about half a millimetre in the position of the indices, so that after what has been said about their peculiarities of behaviour it may obviously be treated as unavoidable error. Thus the ordinary process of taking means is applicable, unless the observations themselves show some peculiarity which forbids the use of this method.

All the results of observations made up to June 1887 (with the help of the Amagat gauge) having been treated in this way, the following mean values of *apparent* average compressibility (multiplied by 10^8) were deduced from them :—

Apparent Compressibility of Cistern Water, boiled for a short time.

Temp. C.	1 ton.	2 tons.	3 tons.
0°·4	4770	4617	4510
3°·2	4670	4527	4402
3°·4	4671	4521	4395
11°·8	4415	4276	4163
14°·2	4330	4220	4115
14°·4	4344	4217	4105
15°·0	4338	4219	4102

[I think it extremely probable that the small irregularities among the last three numbers in each pressure column may be due to want of uniformity of temperature throughout the column of water in the pressure chamber. The day-temperature of the cellar is, in summer, always a good deal above that at night, so that in the forenoon (when the experiments were made) the gun and its contents were steadily growing warmer. Thus the column of water was not at a uniform temperature. The assumed temperature was the mean of the readings before the vessel containing the piezometers was inserted, and after it was taken out. While it was in the chamber, the contents could not be properly stirred except by raising and depressing the vessel itself.]

The points thus determined were laid down (marked with a ∗) as in Plate I., and smooth curves were drawn *liberâ manu* among them. From these curves the following values were taken at intervals at 5° for the sake of ease of calculation, 260 being added to each for the compressibility of glass:—

	0°	5°	10°	15°
1 ton	5044	4874	4723	4594
2 tons	4898	4733	4584	4466
3 tons	4776	4608	4468	4360

The fact that water has a temperature of minimum compressibility led me to try to represent these numbers by a separate parabolic formula for each pressure. The following were easily found:—

$$\left.\begin{array}{l} 504 - 3{\cdot}60t + 0{\cdot}04t^2 \\ 490 - 3{\cdot}65t + 0{\cdot}05t^2 \\ 478 - 3{\cdot}70t + 0{\cdot}06t^2 \end{array}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(A)},$$

for 1, 2, and 3 tons respectively. [The terms independent of t belong to the formula $520 - 17p + p^2$. This will be made use of in future sections.] The utmost difference between the results of these formulæ and the numbers from which they were obtained is less than 1/10th per cent. No closer approximation could be desired, much less expected, especially when we consider the way in which the * points (on which the whole depends) were themselves obtained. These are represented as follows:—

0°·4		3°·2		11°·8		14°·4		15°·0	
Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
503	502·5	493	493	467·5	467·2	460·4	460·5	459·8	459
487·7	488·5	478·7	479	453·6	453·9	447·7	447·8	447·9	446·5
477	476·5	466·2	466·8	442·3	442·7	436·5	437·1	436·2	436

In one instance only does the difference reach unit in the third significant place. [It must be remembered that all these numbers commence with the fifth digit after the decimal point.]

In spite of some remarks above as to uncertainty about temperature, I am convinced that the mode of experimenting employed is calculated to insure considerably greater accuracy in the comparison of compressibilities at different temperatures for any one pressure, than in that of compressibilities for different pressures at any one temperature. The displacement of the indices by the expanding water is likely to be more serious the higher the pressure, as the difficulty of effecting the relief quietly is much greater. Probably all the values for the higher pressures are a little too small for this reason.

The results given above are represented with a fair degree of accuracy by the simple formula

$$\frac{0{\cdot}001863}{36+p}\left(1 - \frac{3t}{400} + \frac{t^2}{10{,}000}\right),$$

which will amply suffice for ordinary purposes. In this form, however, some small but highly expressive and apparently important features of the formulæ (A) for the separate pressures are, of course, lost. The statement above, as to the greater uncertainty of the values the higher the pressure, renders it probable that, in the pressure factor in this formula, both the constants ought to be somewhat larger. It is clear that very small changes in the relative values of the compressions for 1, 2, and 3 tons would make great changes in these constants. In fact, an error of 1 per cent. at 3 tons involves an error of some twenty per cent., nearly, in each of the constants of the pressure factor.

Again, this last formula would give, *for all pressures*, minimum compressibility at about 37° C.; while the former three give 45° C. at 1 ton, 36°·5 at 2, and 30°·8 at 3 tons:—these minima being 423, 423·4, and 421 respectively.

If we venture to extend the formulæ (A) to atmospheric pressure, we are led to

$$520 - 3{\cdot}55t + 0{\cdot}03t^2.$$

I have already shown[1] that this is in close accordance with Buchanan's results at 2°·5 and 12°·5 C. Buchanan's pressure unit is thoroughly trustworthy; for it was determined by letting down the piezometer, with a Challenger thermometer attached, to a measured depth in the ocean. It would thus appear that the extension of my formulæ to low pressures is justified by the result to which it leads.

This formula gives 415 for the minimum compressibility of water at low pressures, the corresponding temperature being about 60° C. This accords remarkably with the determination made by Pagliani and Vincentini, who discovered it, and placed it at 63° C.

On Plate II. I have exhibited graphically a number of known determinations of the compressibility of water for very low pressures at different temperatures. The line marked *Hypothetical* is drawn from the formula above, the authors of the others are named in the plate. It will be seen at a glance that, if Pagliani and Vincentini had taken Grassi's value of the compressibility of water at 1°·5 C., instead of that at 0° C., as their single assumption, their curve would have coincided *almost exactly* with my Hypothetical curve!

So far matters seemed to have gone smoothly enough. But when I came to reduce the observations made *since* June 1887, I found that they gave a result differing, slightly indeed but in a consistently characteristic manner, from that already given. The processes of reduction were carried out precisely as before; and the points determined by the second series of observations are inserted in Plate I., marked with a ⊙. Curves drawn through them as before are now seen to be *parallel* to the former curves, but not coincident with them. And the amount of deviation steadily diminishes from the lowest to the highest pressure. These curves, of course, are very closely represented by the formulæ (A) above, provided the first terms be made 499, 488, 477, respectively, *i.e.* provided 5, 2, and 1 be subtracted from the numbers for 1, 2, and 3 tons respectively. Thus, while the amount of the compressibility is reduced, it is made to depend on temperature precisely as before, but the way in which it depends on pressure is altered. The rate of diminution of compressibility with increase of pressure

[1] See p. 13, above.

is now made constant at any one temperature, instead of becoming slowly less as the pressure is increased. This is incompatible with the results of all of the first series of experiments. The total amount of the compressibility is likewise diminished, by 1 per cent. at 1 ton, by 0·4 per cent. at 2 tons, and by 0·2 per cent. at 3 tons.

Small as these differences are, their regularity struck me as very remarkable, and as pointing definitely to some difference of conditions between the two sets of experiments. Now there were undoubtedly many circumstances in which the series of experiments differed:—

First. The observers were not the same. All the readings in the first series were made by myself; but (in consequence of an accident which prevented me from working in the cellar) I was unable to take part in the second series, and the readings for it were all made by Mr Dickson. Thus there may be a difference, of personal equation, in the mode of applying the scale to the stem of the piezometer, or in the final adjustment of the manometer. Such an explanation is quite in accordance with the results, as a constant difference of reading would tell most when the whole quantity measured is least, *i.e.* at the lowest pressure. But a difference of a full millimetre in the piezometer readings may be dismissed as extremely improbable.

Second. It is possible that, during the second series of experiments, less care may have been taken than in the first series to let off the pressure with extreme slowness. Thus the indices may have been slightly washed down, and the record of compression rendered too small. Even with the greatest care, this undoubtedly occurred in some, at least, of the experiments of the first series; and the screw-tap may have been altered for the worse during the second series.

Third. It is recorded in the laboratory book that, during the second series of observations (which were made for the most part in the exceptionally cold weather of last spring) the oil and treacle in the manometer had become very viscous, so that it was difficult to make the pistons rotate. As artificial cooling, of the pressure apparatus alone, was employed in the first series, this objection does not apply to it. A constant zero error of 4 mm. only in the gauge would fully explain the discrepancy. And there was another cause which may have tended to produce this result, viz. the oxidation of the mercury in the manometric column, which had soiled the interior of the lower part of the tube, and thus made it very difficult to read the zero.

Fourth. The piezometers had been twice refilled, and of course slightly altered in content, between the two series, and the hair-indices had necessarily been changed. The former cause could have produced no measurable effect; but if the indices were *all* somewhat stiffer to move in the second series than in the first, the discrepance might be fully accounted for.

Fifth. Between the two series all the piezometers had, for several months, been filled with strong salt-solutions. Imperfect washing out of these solutions may have had the effect of rendering the second series a set of experiments on water very slightly salt.

Sixth. To make my observations applicable to natural phenomena, I purposely did not employ distilled water. The ordinary water supply of Edinburgh is of very fair quality, and I took care that it should not be boiled longer than was absolutely necessary to prevent air-bubbles from forming in the piezometers. But it comes from different sources, and is supplied as a mixture containing these in proportions which vary from time to time. From this cause also the substance operated upon may have been slightly different in the two series of experiments.

As will be seen in next section, I have obtained direct proof that the first series of observations is to be preferred to the second,—though I have not been able to ascertain definitely which of the above causes may have been most efficient in producing the discrepancy.

It will be observed that this discussion has nothing to do with the important question, Does the compressibility of water diminish from the very first as the pressure increases, as was asserted by Perkins? The first and rudest of my experiments sufficed to answer this definitely in the affirmative; though the contrary opinion has been confidently advanced, and is very generally held to this day.

The discussion deals with a much more refined and difficult question, viz. Is the diminution of average compressibility simply proportional to the pressure for the first few hundred atmospheres, or does the compressibility fall off more slowly than that proportion would indicate, as the pressure is raised?

VIII. Reductions, Results, and Formulæ for Sea-Water.

As already stated, three of the six piezometers employed were filled with fresh water and three with sea-water, so that simultaneous observations were made on the two substances. The accordance among the various observations made with sea-water, at any one temperature and pressure, was not so good as it was with fresh water; especially when the smaller compression apparatus was used. There is some curious action of salt upon the hairs attached to the indices, which has the effect of rendering them too loose, however stiffly they may originally have fitted the tube. Treating the observations of the first series exactly as described in the preceding section, I obtained the points marked * in Plate I. Drawing smooth curves through these, I obtained parabolic formulæ for the apparent compressibility. These gave the following results when compared with the data from observation:—

Apparent Compressibility of Sea-Water.

	1 ton.		2 tons.		3 tons.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
0°·4	435	435	420	420	410	410
3°·0	427	427	413	413	402·5	403
11°·8	404	404	392	392	383·5	384
14°·2	398	399	389	388	380	380
15°·0	398	397	387	387	378	378

Adding the correction for glass, the formulæ became, for 1, 2, and 3 tons respectively—

$$\left.\begin{array}{l} 462 \quad - 3{\cdot}20t + 0{\cdot}04t^2 \\ 447{\cdot}5 - 3{\cdot}05t + 0{\cdot}05t^2 \\ 437{\cdot}5 - 2{\cdot}95t + 0{\cdot}05t^2 \end{array}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(B)},$$

which may be compared with (A) for fresh water; and which may be approximately expressed in the form (very nearly correct for $p = 2$)—

$$\frac{0{\cdot}00179}{38+p}\left(1 - \frac{t}{150} + \frac{t^2}{10{,}000}\right),$$

with sufficient accuracy for most purposes of calculation.

Of course it is easy to deduce from formulæ (B) the points of minimum compressibility, etc., for different pressures; but the data are scarcely accurate enough to warrant such a proceeding. We may, however, extend the formulæ tentatively to the case of very low pressures, for which we obtain

$$481 - 3{\cdot}4t + 0{\cdot}03t^2.$$

[The term independent of t in the formulæ (B) is of the form

$$481 - 21{\cdot}25p + 2{\cdot}25p^2.]$$

The second series of observations gave, when reduced, the points marked ⊙ on the plate. The curves which I have drawn, and which evidently suit them very closely, are *parallel* respectively to the curves drawn through the * points. The interval between them is throughout about 7 for 1 ton, 4 for 2 tons, and 3 for 3 tons, which must be subtracted from the first terms of (B) respectively. The corresponding intervals for the fresh water curves in the two series were 5, 2, 1. The differences of corresponding intervals between the sets of curves are 2, 2, 2; the same for all the groups of four curves each.

This seems to throw light on the question raised in last section, and to show that the main cause of the discrepancy between the first and second series of observations is not due to a difference in the substance operated on. The constant *difference* of the differences is due to such a cause, being at once traceable to the fact that the sea-water put into some of the piezometers for the second series of experiments was taken from the same Winchester quart bottle as was that with which they had been filled two years before. During these two years the sea-water had probably, by evaporation, become slightly stronger, and, therefore, less compressible. The change of compressibility is less than 0·5 per cent. of the whole, and is therefore practically (as it is in the third significant figure) the same for all three pressures. If we now look back to the suggested explanations in last section, we see that the above remarks entirely dispose of the fifth and sixth so far as fresh water is concerned, though the sixth, in a modified form, has to do in part with the discrepancy between the two series of observations on sea-water.

To decide between the two series I made a new set of observations, employing the two piezometers of large capacity spoken of at the end of Section III. These are called M_1 and M_2. On the first day of experimenting M_1 held sea-water from a Winchester quart filled at the same time with the first, but which had remained unopened. M_2 had fresh water. On the second day M_2 held sea-water, and M_1 fresh water. The object of this was to discover, if such existed, errors in the calibration of the piezometers, and then to eliminate them by a process akin to that of weighing with a false balance.

One of the ordinary piezometers (∵), filled with fresh water, was associated with the others as a check. I quote the results of one experiment only, made on the second day:—

5/6/88				[0·997 ton]
5	9°·4	M_1	310·9	[4465]
422		M_2	234·7	[4080]
5		∵	126·0	[4463]

Thus we have the following comparison of estimates of true average compressibility for the first additional ton:—

		Fresh Water.	Sea-Water.
9°·4	1st Series	474	434
	2nd „	469	427
	New „	473	434

A few of the experiments were not thoroughly decisive; none were in favour of the second series. This seems (so far as the first ton is concerned) to settle the question in favour of the first series.

The formulæ (A) and (B) may therefore, for one ton at least, be regarded as approximations to the truth, probably about as close as the apparatus and the method employed are capable of furnishing.

They show that the ratio of compressibilities of sea-water and fresh water varies but little from

$$0·92$$

throughout a range of temperature from 0° to 15° C.

[The doubts as to the behaviour of the indices, which have been more than once alluded to above, have just led me to make a series of experiments (at one temperature but at different pressures) by the help of the silvering process. The results with fresh water were not much more concordant than when the hair-indices were used. When means were taken, exactly as before, it was found that the results for 1 ton were almost identical with the former. For 2 tons the average value was usually *greater* than before by a unit (and in some cases two units) in the third place. For 3 tons it was also greater, but now by one or two (and sometimes three) units. Hence it is probable that the hair-indices do behave as I suspected, but that the effect is small,—not at the worst (*i.e.* at the highest pressure) more than about 0·5 per cent. of the mean value found. With sea-water there was a complex reaction, which made it difficult to read the indications of the silver film. The ratio of the true compressibilities of sea-water and fresh water was now found to be about 0·925, the value which I gave from my earliest experiments. 30/6/88.]

Dr Gibson has furnished me with the following data regarding specimens of sea-water taken from two of the Winchester quarts filled off the Isle of May. One of these had remained unopened; the other had been often opened, and not closed with special care. These correspond (at least closely) to the materials used in the first and second series of experiments respectively:—

DENSITY.

Percentage of Cl.	0° C.	6° C.	12° C.
1·8649	1·027286	1·026745	1·025834
1·9094	1·027941	1·027405	1·026462

Taking the reciprocals in the last three columns, we have

VOLUME.

0° C.	6°	12°
0·973439	0·973951	0·974816
0·972818	0·973326	0·974220

Expressing these volumes as parabolic functions of the temperature, we find, for the maximum density points, $-5^\circ{\cdot}7$ and $-4^\circ{\cdot}9$ respectively.

IX. COMPRESSIBILITY, EXPANSIBILITY, ETC., OF SOLUTIONS OF COMMON SALT.

This part of the inquiry was a natural extension of the observations on sea-water, but it was also in part suggested by the fact that an admixture of salt with water produces effects very similar to those of pressure. Thus it appeared to me that an investigation of the compressibility of brines of various strengths might throw some light on the nature of solution; and also on the question of the internal pressure of liquids, which (in some theories of capillary forces) is regarded as a very large quantity.

The solutions experimented on contained, roughly, 4, 9, 13·4, and 17·6 per cent. of common salt. The piezometers used for the experiments already described were filled with these solutions in July 1887; one, for comparison, being left full of fresh water. I obtained a large number of results at temperatures about 1°, 9°, and 19° C., and at 1, 2, and 3 tons weight per square inch. Unfortunately these were still more discordant than those made with sea-water; so much so, in fact, that an error of 1 or occasionally even 2 per cent. was not by any means uncommon. However, by plotting all the observations exactly as described in the two last sections, I found that they could be *fairly* represented by the curves shown in Plate I. In most cases two at least of the three points for each curve were fairly determinate; one of these being, in all cases, within a degree or so of 10° C. For this was obtained by experiments in the large gun, where the difficulty of relieving the pressure without jerks is much less than in the smaller apparatus. Of the *general* accuracy of these curves I have no doubt. Thus, for instance, it is certain that the compressibility at any one temperature and pressure diminishes rapidly as the percentage of salt increases. And the rate at which the compressibility (for any one range of pressure) diminishes as temperature increases, becomes rapidly less as the solution is stronger. My observations do not enable me to settle the more delicate question of the variation of the rate at which the compressibility (at any one temperature) falls off with increase of pressure in the various solutions. For the limits of error in the various determinations, especially with the more nearly saturated solutions, are quite sufficient to mask an effect of this kind unless it were considerable. An attempt, however, will be made in next Section.

There is little to be gained by putting the results of the inquiry in a tabular form; for they can be obtained from the plate quite as accurately as is warranted by the limits of uncertainty of the experiments. See p. 44.

I am indebted to Dr Gibson for the following determinations, which have a high value of their own as showing the connection between the strength of a salt-solution and its expansibility:—

	Density.		
Percentage of NaCl.	0° C.	6° C.	12° C.
3·8845	1·029664	1·028979	1·027935
8·8078	1·067589	1·066144	1·064485
13·3610	1·101300	1·099341	1·097244
17·6358	1·138467	1·136040	1·133565

From Dr Gibson's numbers, with the help of a table of reciprocals, we have the following data as to volume instead of density:—

Percentage of NaCl.	0° C.	6°	12°
3·88	·97119	·97184	·97282
8·81	·93669	·93796	·93942
13·36	·90802	·90963	·91137
17·63	·87837	·88025	·88217

Next, to find the maximum density for each solution, and the corresponding temperature, we must represent these volumes by parabolic functions of t. Thus the first three numbers are closely represented by

$$y = 0·97083 + \frac{0·00011}{24}(9+t)^2,$$

so that the first solution has its maximum density (1·030) at $-9°$ C., and its coefficient of expansion is

$$0·0000093\,(9+t).$$

Such formulæ, of course, must be taken for no more than embodiments of the data, and any application of them considerably beyond the temperature limits 0°—12° C. is purely hypothetical.

For the second solution—

$$y = 0{\cdot}93306 + \frac{0{\cdot}0000951}{36}(37{\cdot}2 + t)^2,$$

so that (under the reservation just made) the maximum density is 1·0717, at $-37°{\cdot}2$, and the coefficient of expansion is

$$0{\cdot}0000056\,(37{\cdot}2 + t).$$

For the third—

$$y = 0{\cdot}89884 + 0{\cdot}0000018\,(72 + t)^2.$$

The maximum density is 1·1125, at $-72°$ C.; and the expansibility

$$0{\cdot}000004\,(72 + t).$$

The numbers for the volume of the fourth solution are so nearly in arithmetical progression that we can hardly use them to approximate, even roughly, to the position of the maximum density point, or the corresponding density. The expansibility has practically (from 0° to 12° C.) the constant value

$$0{\cdot}00036.$$

Thus we have for the various salt solutions:—

Percentage NaCl.	Max. Density Point.	Max. Density.	Density at 0° C.	Expansibility.
0	+4°	1	0·99986	$-0{\cdot}000068\left(1 - \frac{t}{4}\right)$
3·88	−9°	1·030	1·02966	$+0{\cdot}000084\left(1 + \frac{t}{9}\right)$
8·81	−37°	1·0717	1·06759	$0{\cdot}00021\left(1 + \frac{t}{37}\right)$
13·36	−72°	1·1125	1·10130	$0{\cdot}00029\left(1 + \frac{t}{72}\right)$
17·63	...	...	1·13847	0·00036

As a good illustration of the analogy at the beginning of this section, let us deal for a moment with fresh water at such a pressure that its maximum density point is −9° C., that of the first of the salt solutions. It will be seen later that the requisite pressure is about 4 tons. At that pressure (A) gives

$$468 - 3{\cdot}75t + 0{\cdot}07t^2.$$

Hence as the unit of volume at 1 atm. and 4° C. becomes 1·000136 at 1 atm. and 0° C., it is reduced at 4 tons and 0° C. to

$$(1{\cdot}000136)\left(1 - \frac{609 \times 468}{10^7}\right) = 1 - 0{\cdot}0284,$$

so that the density has become

$$1{\cdot}0292.$$

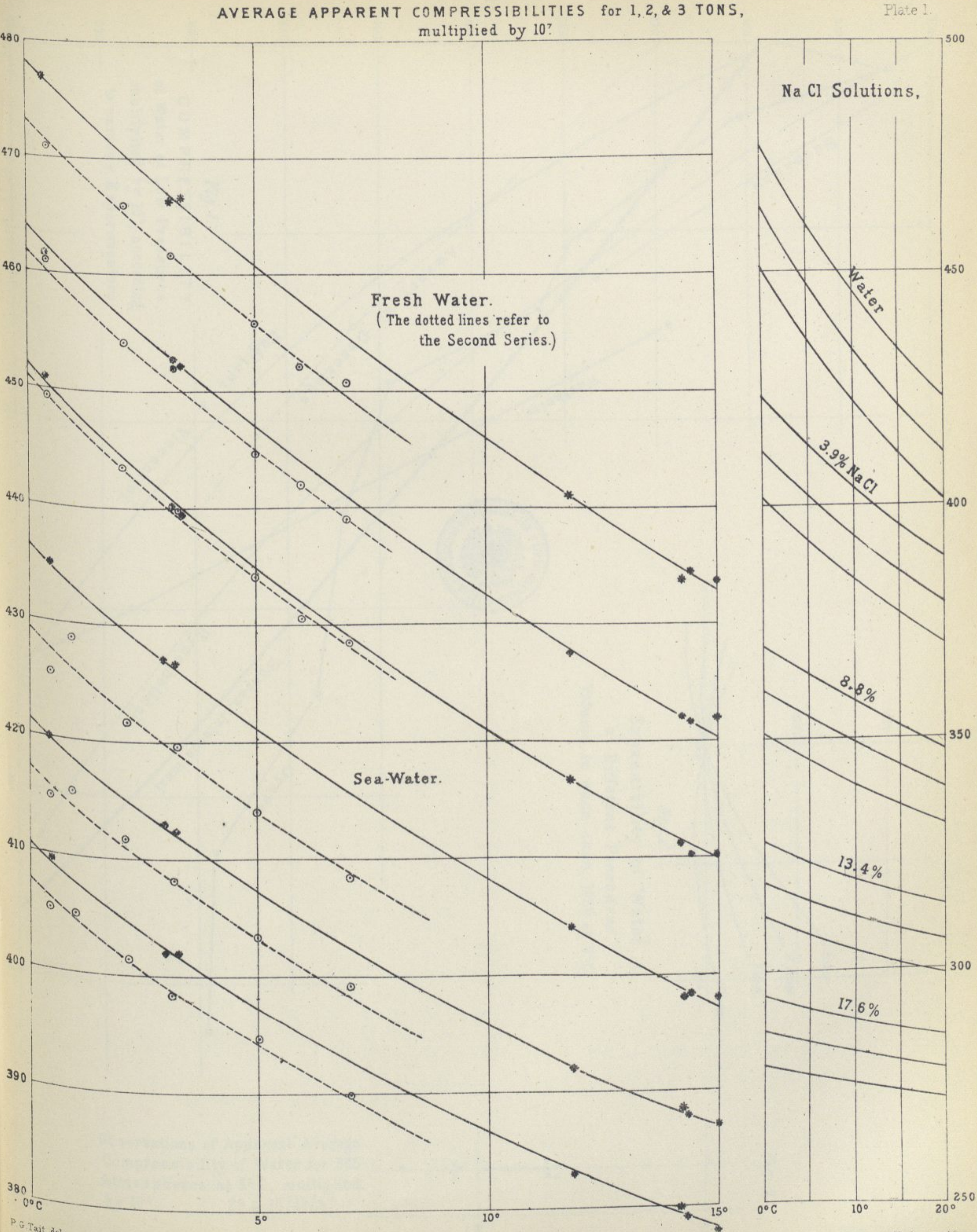
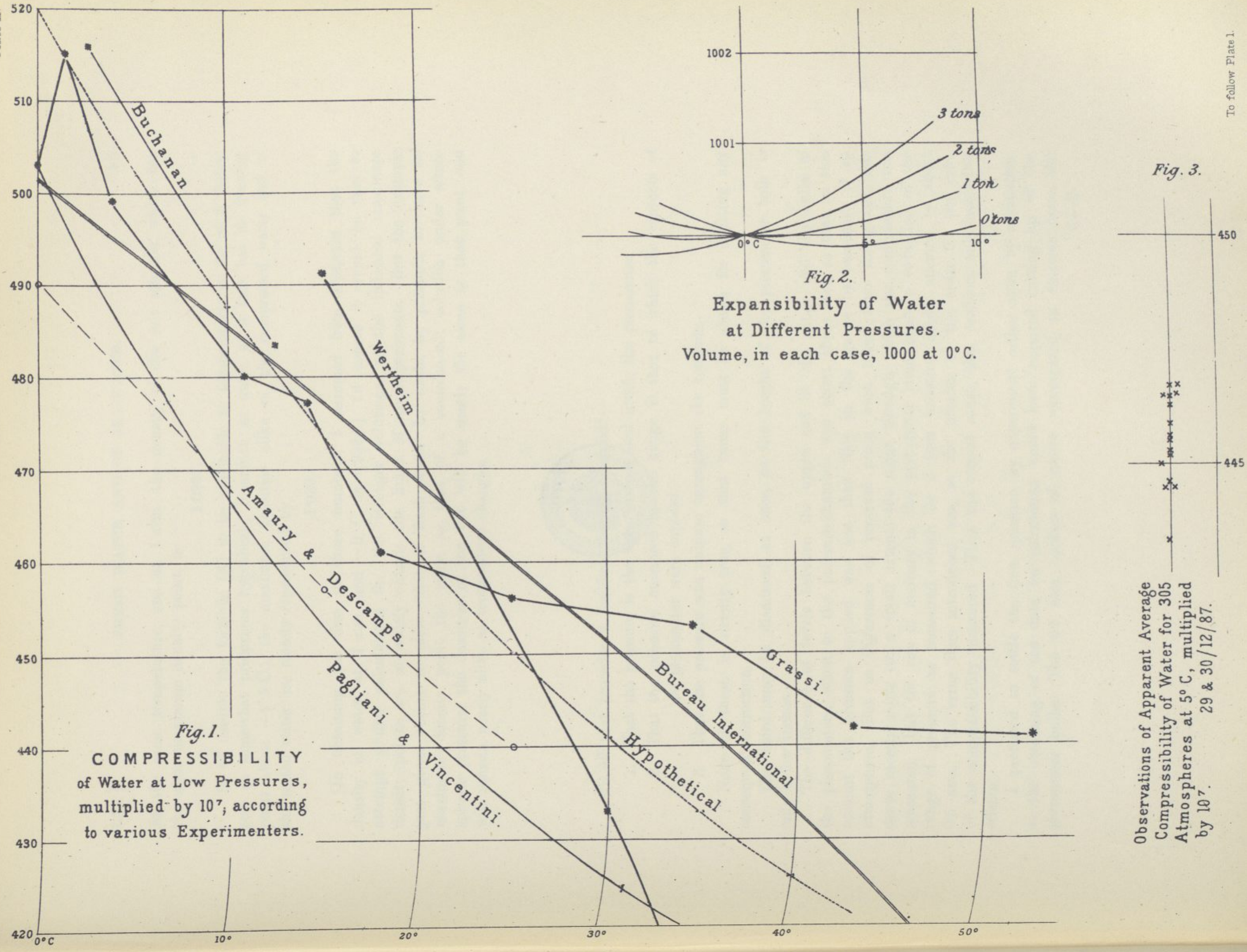

AVERAGE APPARENT COMPRESSIBILITIES for 1, 2, & 3 TONS,
multiplied by 10⁷
Fresh Water.
(The dotted lines refer to the Second Series.)
Sea-Water.
Na Cl Solutions,
Water
3.9% Na Cl
8.8%
13.4%
17.6%
480
470
460
450
440
430
420
410
400
390
380
0° C
5°
10°
15°
500
450
400
350
300
250
20°
P. G. Tait del.

Plate II.

To follow Plate 1.



At the same temperature, and at 1 atm., the density of the salt solution, which has the same maximum density point, is

$$1{\cdot}0297.$$

If we assume the formulæ (A) to be applicable to temperatures so far as 9° below zero (a somewhat precarious hypothesis, inasmuch as water at 4 tons has its freezing point about $-4^\circ{\cdot}5$ C.), the maximum densities alike of the compressed water and of the salted water are closely represented by

$$1{\cdot}030.$$

[In obtaining the first of these numbers, I assumed from Despretz that the density of water at 1 atm. and -9° C. is 0·9984.] Of course it would be vain to attempt similar calculations for the stronger solutions, as the indicated maximum density points are so widely outside the limits of my experiments. But the example just given seems to show that if fresh water be made, by pressure, to have its maximum density point the same as that of a common-salt solution under atmospheric pressure, the densities of the two will be nearly the same at that point, and will remain nearly alike as temperature changes.

NOTE.

In all that precedes it has been tacitly assumed:—

1. That the pressure is the same outside and inside the piezometer.
2. That the pressure measured by the gauge is that to which the contents of the piezometer were exposed.
3. That the pressure was uniform throughout the contents.

None of these is strictly true, so that cause must be shown for omitting any consequent correction.

The third may be dismissed at once, as the height of the piezometer bulb is only a few inches.

The difference of levels between the upper end of the gauge and the bulbs of the piezometers, when in the pressure-chamber, was about three feet, so that on this account the pressure applied was less than that in the gauge by one-tenth of an atmosphere. But as *differences* of pressure alone were taken from the gauge, this cause merely *shifts* (to a small extent) the range through which the compression was measured. But the rise of mercury in the piezometer stem made a reduction of the range of pressure as measured, which for 3 tons pressure might amount to about 0·5 atm. The error thus introduced was, at the utmost, of the order 0·1 per cent. of the compressibility measured. Thus the second cause, also, produces only negligible effects.

I preferred to settle the first question by experiment rather than by calculation, as the obtaining of the data for calculation would have required cutting up of the piezometer bulbs. The 0·5 atm. spoken of above represented, in extreme cases, the excess of external over internal pressure in the piezometers. By direct experiment on two of the instruments themselves, it was found that their internal volume was diminished at most 0·00002 of the whole by 0·6 atm. of external pressure. This would involve as a correction the adding of 0·1 per cent. only to the results at 3 tons, so that it also is well within the limits of error of the measurements above.

ASSOCIATED PHYSICAL QUESTIONS.

X. Theoretical Speculations.

If instead of the percentage of NaCl in the solutions we tabulate the amount of NaCl to 100 of water, and along with it the compressibility at zero, we have—

s = amount of NaCl to 100 of water.	Average compressibility at 0° C. × 10^7. For first ton.	First 2 tons.	First 3 tons.
0·0	503	490	477
4·0	449	438	428
9·6	396	386	378
15·4	354	345	338
21·4	321	313	306

The relation between these numbers is very fairly represented by the formula—

$$\text{Average compressibility for first } p \text{ tons} = \frac{0·00186}{36+p+s}.$$

It is remarkable that if we put $t=0$ in the formula of Section VII., we have—

$$\text{Average compressibility of fresh water for first } p+s \text{ tons} = \frac{0·00186}{36+p+s}$$

which presents an exceedingly striking resemblance to that last written.

Though these formulæ are only approximate, we may assume the true constants to be at least nearly the same in both, and make the following statement as a sort of *memoria technica* in this subject:—

At 0° C. the average compressibility, for p tons, of a solution of s lbs. of common salt in 100 lbs. of water, is nearly equal to the average compressibility of fresh water for the first $p+s$ tons of additional pressure.

The numerical coincidence above is, of course, accidental; because the formulæ are taken for the special temperature 0° C., and the special unit of pressure 1 ton weight per square inch.

But a coincidence of a much more striking character, and one which does not depend upon special choice of units, is suggested by the common *form* of the expressions compared.

It appears from the Kinetic Theory of Gases, in which the particles are treated as hard spheres, whose coefficient of restitution is 1, and which exert no action on one another except at impact, that the pressure and volume of the group at any one temperature are connected by a relation approximately of the form

$$p(v-\alpha) = \text{constant}.$$

The quantity α obviously denotes the ultimate volume, *i.e.* that to which the group would be reduced if the pressure were infinite.

I have pointed out[1] that this expression coincides almost exactly with the results of Amagat's experiments on the compression of hydrogen. The introduction of an attractive force between the particles, sensible only when they are at a mutual distance of the order of their diameters, merely alters the constants in this expression. Let us see what interpretation it will bear if, for a moment, we suppose it roughly to represent the state of things in water.

The average compressibility of such a group of particles, between the pressures ϖ and $\varpi + p$, viz.,

$$\frac{v_0 - v}{pv_0}$$

where v_0 is the volume at ϖ, and v that at $\varpi + p$, is easily shown to be

$$\frac{1-\dfrac{\alpha}{v_0}}{\varpi + p}.$$

Compare this with the empirical expression above for the compressibility of water say at 0° C. (per ton weight on the square inch)—

$$\frac{152{\cdot}3 \times 0{\cdot}00186}{36+p} = \frac{0{\cdot}283}{36+p}$$

and we see that they agree exactly in form. If, then, the results of the kinetic theory be even roughly applicable to the case of a liquid, we may look upon the 36 in this expression as the number of tons weight per square inch by which the internal pressure of water exceeds the external pressure. And the corresponding empirical expression for the compressibility of a solution of common salt may be interpreted as showing that the addition of salt to water increases the internal pressure by an amount simply proportional to the quantity of salt added.

That liquids have very great internal pressure has been conjectured from the results of Laplace's and other theories of capillarity, in which the results are derived statically from the hypothesis of molecular forces exerted intensely between contiguous portions of the liquid, but insensibly between portions at sensible distances apart. A very interesting partial verification of this proposition was given by Berthelot[2] in 1850. By an ingenious process he subjected water to external *tension*, and found that it could support at least fifty atmospheres. The calculation was made on the hypothesis that a moderate negative pressure increases the volume of water as much as an equal positive pressure diminishes it.

[1] *Trans. Roy. Soc. Edin.*, vol. XXXIII. p. 90, 1886.

[2] *Ann. de Chimie*, tom. XXX. p. 232.

I was led to the conclusion that the internal pressure of a liquid must be greatly superior to the external, as a consequence of the remarkable results of Andrews' experiments on carbonic acid, and of the comments made on them by J. Thomson and Clerk-Maxwell[1]. It was Prof. E. Wiedemann who, while making an abstract of my paper (*Appendix* E) for the *Beiblätter zu den Ann. d. Physik,* first called my attention to Berthelot's experiment.

In *Appendix* F a short account of Laplace's calculations is given, and it is shown that the work required to carry unit volume of water, from the interior to a distance from the surface greater than the range of molecular forces, is

$$2K \times 1 \text{ cub. inch},$$

where K is the internal molecular pressure per square inch. The speculation above would make this work

$$72 \text{ inch-tons.}$$

But, in work units, the heat required to vaporize 1 cub. inch of water at 0° C. is

$$\frac{62 \cdot 5}{1728} 606 \times 1390 \text{ foot-pounds,}$$

or

$$163 \text{ inch-tons.}$$

The two quantities are at least of the same order of magnitude, and it is to be remembered that what has been taken out in the one case is very small particles of *water*; in the other, particles of *vapour*. This raises another extremely difficult question, viz.,—What fraction of the whole latent heat is required to convert water, in excessively small drops, into vapour?

The comparison above, if it be well founded, would seem to show that the utmost reduction of volume which water at 0° C. can suffer by increase of pressure is 0·283; *i.e.* that water can be compressed to somewhat less than 3/4ths of its original bulk, but not further.

Of course the whole of this speculation is of the roughest character, for two reasons. The kinetic gas formula has been proved only for cases in which the whole volume of the particles is small compared with the space they occupy. The compression formula is only an approximation, and was obtained for the range of pressures from 150 to 450 atmospheres; while we have extended its application to much higher pressures.

XI. Equilibrium of a Vertical Column of Water.

In Canton's second paper we have the following interesting statement:—

"The weight of 32½ feet of sea-water is equal to the mean weight of the atmosphere: and, as far as trial has yet been made, every additional weight equal to that of the atmosphere, compresses a quantity of sea-water 40 millionth parts; now if this constantly holds, the sea, where it is two miles deep, is compressed by its own weight 69 feet 2 inches; and the water at the bottom is compressed 13 parts in 1000."

[1] *Theory of Heat*, chap. VI., London, 1871.

Either Canton overestimated the density of sea-water or he underestimated the amount of an atmosphere, for undoubtedly 33 feet is a much closer approximation to the column of sea-water which produces 1 atmosphere of pressure. He does not give his process of calculation, but it was probably something like this:—The pressure increases uniformly from the top to the bottom (neglecting the small effect due to change of density produced by compression), and everywhere produces a contraction proportional to its own value. Hence the whole contraction is equal to that which would have been produced if the pressure had, at all depths, its mean value, *i.e.* that due to half the whole depth. This process, with Canton's numbers, gives nearly his numerical results.

If, then, a be the depth, and ρ_0 the original density, $g\rho_0 a/2$ is the mean pressure. If e be the compressibility, the whole contraction of a column, originally of length a, is $eg\rho_0 a^2/2$. Now, a mile of sea-water gives nearly 160 atmospheres of pressure, so that the loss of depth of a mile of sea (supposed at 10° C. throughout) is

$$160 \times 0{\cdot}000045 \times 5280/2 = 19 \text{ feet, nearly.}$$

For other depths it varies as the square of the depth; so that for two miles it is 76 feet, and for six miles 684 feet nearly.

This, however, is an overestimate, because we have not taken account of Perkins' discovery of the diminution of compressibility as the pressure increases. The investigation for this case is given in *Appendix* G, where the change of depth is shown to be

$$eg\rho_0 a^2/2\left(1 - \frac{2\varpi}{3\Pi} + \frac{\varpi^2}{2\Pi^2} - \dots\right),$$

ϖ being the pressure at the bottom in tons weight per square inch, and Π (by Section VIII.) being 38 in the same units.

For six miles of sea this is, in feet—

$$684\left(1 - \frac{2}{19} + \frac{1}{80} - \&\text{c.}\right) = 620 \text{ nearly.}$$

In the *Appendix* referred to I have given a specimen of the hydrostatic problems to which this investigation leads. Any assigned temperature distribution, if not essentially unstable, can be approximately treated. But the up- or down-rushes which result from instability are hopelessly beyond the powers of mathematics.

One remark of a curious character may be added, viz. that in a very tall column of water (salt or fresh), at the same temperature throughout, the equilibrium might be rendered unstable in consequence of the heat developed by a sudden large increase of pressure. For, as will be seen later, the expansibility of water is notably increased by pressure; and thus the lower parts of the column will become hotter, and less compressible, than the upper. This effect is not produced in a tall column of air, for the expansibility is practically unaltered by pressure. And the opposite effect is produced in bodies like alcohol, &c., where the compressibility steadily increases with rise of temperature.

XII. Change of Temperature produced by Compression.

The thermal effects of a sudden increase or relaxation of pressure formed an important element in my examination of the Challenger thermometers, and were practically the origin of this inquiry; one of the most unexpected of the results I obtained being the very considerable compression-change of temperature of the vulcanite slabs on which the thermometers are mounted. Thomson's formula for this heating effect, in terms of the pressure applied, and of the specific heat and expansibility of the body compressed, is given in *Appendix* C to my former Report. My first direct experiment on the subject was described as follows[1]:—

"When...the bulb of one of the thermometers was surrounded by a shell of lard upwards of half an inch thick, the total effect produced by a pressure of 3½ tons weight was 5° F.; while for the same pressure, without the lard, the effect was only 1°·8 F. The temperature of the water in the compression apparatus was 43° F., so that the temperature effect due to the compression of water was less than 0°·2 F."

On May 16 of the same year I read a second note on the subject, from which I extract the following[2]:—

"I have examined for a number of substances the rise of temperature produced by a sudden application of great pressure, and the corresponding fall of temperature when the pressure was very suddenly relaxed. The copper-iron circuit is, however, too little sensitive for very accurate measurements; as, from the nature of the apparatus, the wires must be so thin as to have considerable resistance, and the thermo-electric power of the combination is not large....I content myself, for the present, with a general statement of the results for cork and for vulcanized india-rubber, which are apparently typical of two classes of solids quite distinct from one another in their behaviour.

"In the case of india-rubber the rise of temperature was found to be about 1°·3 F. for each ton-weight of pressure per square inch; and the fall in relaxation was almost exactly the same.

"With cork each additional ton of pressure gave less rise of temperature than the preceding ton; and the fall on relaxation of pressure was, for one or two tons, only about half the rise. For higher pressures its ratio to the rise became greater. Two tons gave a rise of about 1°·6 F., and a fall of 0°·9 F.

"With the same arrangement, the fall of temperature in water suddenly relieved from pressure at a temperature of 60° F. was found to be for

One ton-weight per square inch	0°·25 F.
Two „ „	0°·56 „
Three „ „	0°·93 „
Four „ „	1°·35 „

"These numbers give the averages of groups of fairly concordant results. I employed cooling exclusively in these experiments, because one of the valves of my

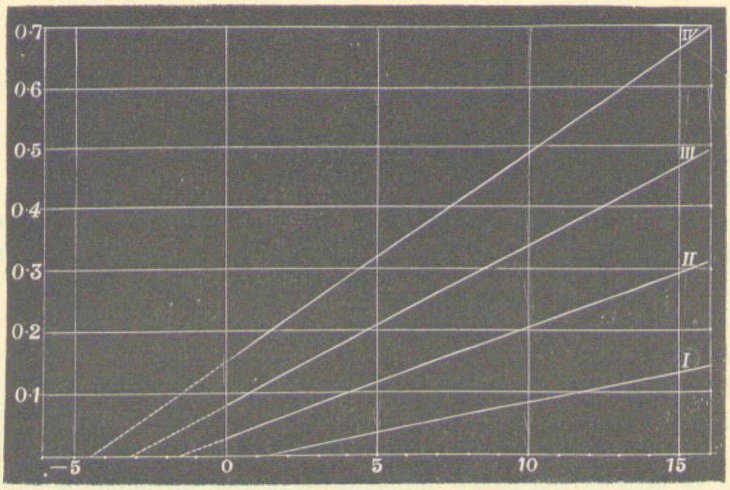
[1] *Proc. Roy. Soc. Edin.*, vol. XI. p. 51, 1881. [2] *Proc. Roy. Soc. Edin.*, vol. XI. pp. 217, 218, 1881.

pump was out of order, and the pressure could not be raised at a uniform rate. The effects obtained for successive tons of pressure are thus, roughly, 0°·25, 0°·31, 0°·37, and 0°·42 F.

"If these results may be trusted, they probably indicate a lowering of the maximum-density point of water by pressure[1]."

In the next extract it will be seen that I deduced from these data a lowering of the maximum-density point amounting to about 3° C. per ton.

The experiments on water were carried further in the following year by Professors Marshall and Michie Smith, and Mr Omond[2]. The second of their papers contains



the annexed graphic representation of the results, which is alluded to in the following extract. The final result of these experiments, as assigned by the authors, was a probable lowering of the maximum-density point of water by 5° C. for one ton pressure. To this paper I added the following note (*l.c.* p. 813):—

"If we assume the lowering of the temperature of maximum-density to be proportional to the pressure, which is the simplest and most natural hypothesis, we may write

$$t_0' = t_0 - Bp,$$

where p is in tons weight per square inch.

"Now Thomson's thermo-dynamic result is of the form

$$\delta t = A\,(t - t_0')\,\delta p.$$

"This becomes, with our assumption,

$$\delta t = A\,(t - t_0 + Bp)\,\delta p.$$

"As the left-hand member is always very small, no sensible error will result from integrating on the assumption that t is constant on the right (except when the

[1] [See footnote to p. 26.] [2] *Proc. Roy. Soc. Edin.*, vol. XI. pp. 626 and 809, 1882.

quantity in brackets is very small, and then the error is of no consequence). Integrating, therefore, on the approximate hypothesis that A and B may be treated as constants, we have for the whole change of temperature produced by a finite pressure p—

$$\Delta t = A\,(t - t_0)\,p + \tfrac{1}{2}ABp^2.$$

"I have found that all the four lines in the diagram given [from Messrs Marshall, Smith, and Omond, on last page, where y is the heating effect of p tons at temperature t] can be represented, with a fair approach to accuracy, by the formula

$$y = 0{\cdot}0095\,(t-4)\,p + 0{\cdot}017p^2,$$

where p has the values 1, 2, 3, 4 respectively. Hence, comparing with the theoretical formula, we have the values

$$A = 0{\cdot}0095, \quad B = 3^\circ{\cdot}6 \text{ C.}$$

"B expresses the lowering of the maximum-density point for each ton weight of pressure per square inch.

"It seems, however, that all the observations give considerably too small a change of temperature; for the part due to the first power of the pressure is from 30 to 40 per cent. less than that assigned by Thomson's formula and his numerical data. One obvious cause of this is the small quantity of water in the compression apparatus, compared with the large mass of metal in contact with it. This would tend to diminish all the results, whether heating or cooling; and the more so the more deliberately the experiments were performed. Another cause is the heating (by compression) of the *external* mercury in the pressure gauge. Thus the pressures are always overestimated; the more so the more rapidly the experiments are conducted. A third cause, which may also have some effect, is the time required by the thermo-electric junction to assume the exact temperature of the surrounding liquid.

"Be this, however, as it may, the following table shows the nature of the agreement between the results of my original experiments [*ante*, p. 48] and the data derived from the present investigations. The gauge and the compression apparatus were the same as in my experiments of last year; the galvanometer, the thermo-electric junctions, and the observers were all different. The column MSO gives the whole heating or cooling effect at 15°·5 C., calculated for different pressures from the results of the investigation by Professor Marshall and his coadjutors. The column T contains the results of my direct experiments at that temperature:—

p (tons)	MSO	T	Thomson.
1	0·131 C.	0·139 C.	0·177 C.
2	0·294	0·311	0·355
3	0·465	0·516	0·533
4	0·665	0·750	0·711

"It will be noticed that there is, again, a fair agreement; though the results are, as a rule, lower than those calculated from Thomson's formula. My own agree most nearly with Thomson's formula, probably because they were very rapidly conducted. As they stand, they give about 3° C. for the effect of 1 ton on the maximum-density point. It is to be observed that if we could get the requisite corrections for conduction and for compression of mercury, their introduction would increase (as in fact is necessary) the constant A above, but would have comparatively little effect on the value of B, which is the quantity really sought."

The experiments on other substances were carried out for me by Messrs Creelman and Crocket, from whose important paper[1] I extract the following results, which have some connection with the subjects of this and of my former *Report*:—

Cork, at 15° C.			"Challenger" Vulcanite, at 16° C.		
Pressure.	Rise per ton.	Fall per ton.	Pressure.	Rise per ton.	Fall per ton.
1	0°·75	0°·51	1	0°·33	0°·33
2	0°·65	0°·45	2	0°·31	0°·33
3	0°·59	0°·42	3	0°·28	0°·32
Glass, at 15° C.			India-rubber, at 15° C.		
1	0°·12	0°·12	1	0°·74	0°·79
2	0°·13	0°·14	2	0°·70	0°·79
3	0°·13	0°·14	3	0°·70	0°·80
Gutta Percha, at 16° C.			Beeswax, at 15° C.		
1	0°·65	0°·67	1	0°·83	0°·83
2	0°·60	0°·64	2	0°·79	0°·86
3	0°·58	0°·63	3	0°·78	0°·89
Solid Paraffin, at 14° C.			Marine Glue, at 15°·5 C.		
1	0°·56	0°·57	1	0°·91	0°·98
2	0°·56	0°·59	2	0°·85	0°·90
3	0°·54	0°·61	3	0°·82	0°·91
Chloroform, at 17° C.			Sulphuric Ether, at 21° C.		
1	1°·44	1°·45	1	1°·8	1°·9
2	1°·34	1°·45	2	1°·74	1°·8
3	1°·31	1°·47	3	1°·7	1°·7

As was to be expected from the fact that the getting up of pressure requires a short time, while the relief is practically instantaneous, the heating effect is generally a little smaller than the cooling effect for the same change of pressure.

These experimenters thus completely confirmed my statements as to the curiously exceptional behaviour of cork, but they found no other substance, in the long list of those which they examined, which behaves in a similar manner.

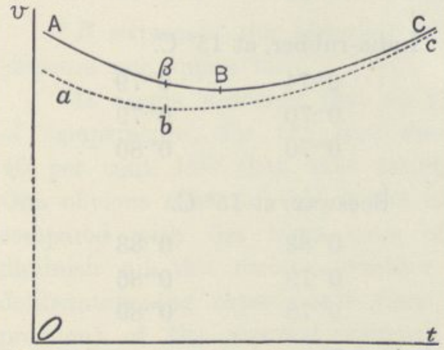
It is to be remarked that as, in all the experiments described or cited in this section, the temperature-changes were measured by a thermo-electric junction which was itself exposed to the high pressures employed, there may be error due to the compression of the materials forming the junction. The wires were, for several reasons,

[1] *Proc. Roy. Soc. Edin.*, vol. XIII. p. 311, 1885.

very thin; so that the error, if any, is not due to changes of temperature in them, but to (possible) change of relative thermo-electric position, due to pressure. This is a very insidious source of error, and it is not easy to see how to avoid it.

XIII. Effect of Pressure on the Maximum-Density Point.

Though the lowering of the maximum-density point of water by pressure is an immediate consequence of Canton's discovery, that the compressibility diminishes as the temperature is raised, it seems to have been first pointed out, so lately as 1875, by Puschl[1]. I was quite unaware of his work, and of that of Van der Waals[2], when (as shown in Section XII. above) I was led to the same conclusion by the differences between theory and experiment, as to the heat developed by compression of water.



This can very easily be shown as follows. Let the (vertical) ordinates of the curve ABC represent the volume of water at 1 atm., the abscissæ the corresponding temperatures, B the maximum-density point. Let the dotted curve abc represent the same for a greater pressure, say two atmospheres. Then, by Canton's result, the vertical distance between these curves (the difference between corresponding ordinates) diminishes continuously from A to C; so long, at least, as the temperature at C is under that of minimum compressibility. Hence the inclination of abc to the axis of temperatures is everywhere greater than that of the corresponding part of ABC. Thus the minimum, b, of the dotted curve (where its tangent is horizontal) must correspond to a point, β, in the full curve, where the inclination is *negative*—*i.e.* a point at a lower temperature than B.

To calculate the amount of this lowering, by the process indicated, we must know the form of the curve abc. This, in its turn, can be calculated from a knowledge of the form of ABC, and of the relation between compressibility and temperature. Both of the authors named took their data as to the latter matter from the experiments of Grassi; and, as was therefore to be expected, gave results wide of the truth. Puschl calculates a lowering of 1° C. by 87·6 atm., which is certainly too small; Van der Waals, 0°·78 C. by 10·5 atm., as certainly much too large.

To obtain a good estimate in this way is by no means easy, for authorities are not quite agreed as to the form of the curve ABC. If we calculate from the datum of Despretz, which has been verified by Rossetti[3], namely,—

$$\frac{\text{vol. at } 0^\circ \text{ C.}}{\text{vol. at } 4^\circ \text{ C.}} = 1{\cdot}000136,$$

[1] *Sitzungsb. d. math.-naturw. Cl. d. k. Akad. d. Wiss. Wien*, Bd. LXXII. p. 283, 1875.
[2] *Archives Néerl.*, tom. XII. p. 457, Haarlem, 1877.
[3] *Pogg. Ann.*, *Ergänzungsband*, v. p. 260, 1871.

we obtain for the volume of water at 1 atm., in terms of temperature,

$$1 + 0{\cdot}0000085\,(t-4)^2 \qquad \text{.................................(1).}$$

[This refers only to the part AB of the curve, which is what we want. There seems general agreement that the curve is not symmetrical about the ordinate at B.] Now, by (A), the factor for reduction of volume by 1 ton of additional pressure is

$$1 - 0{\cdot}007676 + 0{\cdot}000055t - 0{\cdot}00000061t^2 \qquad \text{.................................(2).}$$

The product of these factors, (1) and (2), is a minimum when

$$0{\cdot}000017\,(t-4) = -\,0{\cdot}000055 + 0{\cdot}00000122t;$$

or
$$t = 4 - \frac{501}{158} = 4 - 3{\cdot}17.$$

Thus, according to these data, the maximum-density point is lowered by $3^\circ{\cdot}17$ C. per ton of pressure. It will be observed that this is not much less than the result I calculated from the data of Professor Marshall and his comrades, but it agrees almost exactly with that which I derived from my own.

The following description of the results of my earlier attempts to solve this question *directly*, is taken from the *Proc. Roy. Soc. Edin.*, vol. XII. pp. 226–228, 1883:—

"I determined to try a direct process analogous to that of Hope, for the purpose of ascertaining the maximum-density point at different pressures. The experiments presented great difficulties, because (for Hope's method) the vessel containing the water must have a considerable cross section; and thus I could not use my smaller compression apparatus, which was constructed expressly to admit of measurements of temperature by thermo-electric processes. I had therefore to work with the huge Fraser gun employed for the Challenger work, and to use the protected thermometers (which are very sluggish) for the measurement of temperatures. It was also necessary to work with the gun at the temperature of the air,—it would be almost impossible to keep it steadily at a much lower temperature,—so that I had to work in water at about 12° C.

"The process employed was very simple. A tall cylindrical jar full of water had two Challenger thermometers (stripped of their vulcanite mounting) at the bottom, and was more than half-filled with fragments of table-ice floating on the water, and confined by wire-gauze at the top. This was lowered into the water of the gun, and pressure was applied.

"It is evident that *if there were no conduction of heat* through the walls of the cylinder, and if the ice lasted long enough under the steadily maintained pressure, the thermometers would ultimately show, by their recording minimum indices, the maximum-density point corresponding to the pressure employed:—always provided that that temperature is not lower than the melting point of ice at the given pressure.

"Unfortunately, all the more suitable bad conductors of heat are either bodies like wood (which is crushed out of shape at once under the pressures employed) or like tallow, &c. (which become notably raised in temperature by compression). I was therefore obliged to use glass. The experiments were made on successive days, three each day, with three different cylindrical jars. These had all the same height and the same internal diameter. The first was of tinned iron; the second of glass about $\frac{1}{6}$ inch thick; the third, of glass nearly an inch thick, was procured specially for this work.

"With the external temperature 12°·2 C., the following were the results of $1\frac{1}{2}$ tons pressure per square inch, continued in each case for 20 minutes (some unmelted ice remaining on each occasion). The indications are those of two different Challenger thermometers, corrected for index-error by direct comparison with a Kew standard:—

Tin Cylinder.	Thin Glass	Thick Glass.
4° C.	2°·67	0°·83
4°	2°·61	0°·83

The coincidence of the first numbers with the ordinary maximum-density point of water is, of course, mere chance. When no pressure was applied, but everything else was the same, the result was—

Tin.	Thin.	Thick.
5°·7 C.	5°	4°

It is clear that the former set of numbers points to a temperature of maximum density, somewhere about 0° C., under $1\frac{1}{2}$ tons pressure per square inch. But still the mode of working is very imperfect.

"I then thought of trying a *double* cylindrical jar, the thin one above mentioned being enclosed in a larger one which surrounded it all round, and below, at the distance of about $\frac{3}{4}$ inch. Both vessels were filled with water, with broken ice floating on it, and had Challenger thermometers at the bottom. By this arrangement I hoped to get over the difficulty due to the temperature of the gun, by having the inner vessel enclosed in water which would be lowered in temperature to about 3° C. by the application of pressure. The device proved quite successful. The result of $1\frac{1}{2}$ tons pressure per square inch maintained for 20 minutes, some ice being still left in each vessel, was from a number of closely concordant trials—

Temperature in outer vessel . . . 1°·7 C.

Temperature in inner vessel . . . 0°·3 C.

The direct pressure correction for the thermometers is only about − 0°·1 C., and has therefore been neglected.

"The close agreement of this result with that obtained (under similar pressure conditions) in the thick glass vessel leaves no doubt that the lowering of the maximum-density point is somewhat under 4° C. for $1\frac{1}{2}$ tons, or 2°·7 C. for 1 ton per square inch. It is curious how closely this agrees with the result of my indirect experiments."

Further work of the same kind led me to the conclusion that even the double vessel had not sufficiently protected the contents from conducted heat, and to state in my *Heat* (p. 95, 1884) that "a pressure of 50 atmospheres lowers the maximum-density point by 1° C."

During the next two years I made several repetitions of these experiments, with the help of thermometers protected on the Challenger plan, but very much more sensitive. These experiments were not so satisfactory as those just described. The new thermometers caused a great deal of trouble by the uncertainty of their indications, which I finally traced to the fact that the paraffin oil which they contained passed, in small quantities, from one end of the mercury column to the other. I was occupied with an attempt to obtain more suitable instruments, when the arrival of the Amagat gauge turned my attention to other matters.

So far as I can judge from the results of the three different methods which I have employed, the lowering of the maximum-density point of water by 1 ton of pressure is very nearly, though perhaps a little in excess of, 3° C.

It is peculiarly interesting to find that Amagat, by yet another process,—viz. finding two temperatures not far apart at which water, at a given pressure, has the same volume,—has lately obtained a closely coinciding result. He says: "À 200 atm. (chiffres ronds) le maximum de densité de l'eau a rétrogradé vers zéro et l'a presque atteint; il paraît situé entre zéro et 0°·5 (un demi-degré)[1]." This makes the effect of 1 ton slightly *less* than 3° C.

As the freezing point is lowered, according to J. Thomson's discovery, by about 1°·13 only per ton of additional pressure,—and has a start of but 4°,—the maximum-density point will overtake it at about − 2°·4, under a pressure of 2·14 tons.

The diagram 2 of Plate II. shows the consequences of the pressure-shifting of the maximum-density point in a very clear manner,—especially in its bearing on the expansibility of water at any one temperature but at different pressures. The curves in the diagram are for atmospheric pressure, and for additional pressures of 1, 2 and 3 tons respectively. They are traced roughly by the help of Despretz's tables of expansibility at atmospheric pressure, and the compression data of the present Report. The quantity of water taken in each case is that which, at 0° and under the particular pressure, has unit volume. Thus all the curves pass through the same point on the axis of volumes. How, in consequence of the gradual lowering of the maximum-density point, the expansibility at zero, which is negative at atmospheric pressure, and even at 1 ton of additional pressure, becomes positive and then rapidly greater as the pressure is raised, is seen at a glance.

I have to state, in conclusion, that my chief coadjutors in the experimental work have been Mr H. N. Dickson and my mechanical assistant Mr T. Lindsay. Mr Dickson also reduced all the observations, about half of them having been done in duplicate by myself.

In the compression of glass I had the assistance of Mr A. Nagel, and occasionally of Dr Peddie.

Mr A. C. Mitchell assisted me in the graphic work, and checked the calculations in the text.

[1] *Comptes Rendus*, tom. CIV. p. 1160, 1887.

I have already acknowledged the density determinations and analyses of sea-water and salt solutions made by Dr Gibson.

And I have again been greatly indebted to the very skilful glass-working of Mr Kemp.

[7/9/88.—The following analysis of the glass of my piezometers is given by Mr T. F. Barbour, working in Dr Crum Brown's Laboratory:—

$$
\begin{aligned}
SiO_2 &= 61{\cdot}20\\
PbO &= 20{\cdot}94\\
Al_2O_3 + Fe_2O_3 &= 0{\cdot}82\\
CaO &= 2{\cdot}20\\
MgO &= 0{\cdot}26\\
K_2O &= 1{\cdot}93\\
Na_2O &= 11{\cdot}72.]
\end{aligned}
$$

ADDENDUM (8/8/88).

The reader has already seen that I have, more than once in the course of the inquiry, found myself reproducing the results of others. A few days ago I showed the proof-sheets of this Report to Dr H. du Bois, who happened to visit my laboratory, and was informed by him that one of Van der Waals' papers (he did not know which, but thought it was a recent one) contains an elaborate study of the molecular pressure in liquids. I had been under the impression, strongly forced on me by the reception which my speculations (Appendix E., below) met with both at home and abroad, that Laplace's views had gone entirely out of fashion;—having made, perhaps, their final appearance in Miller's *Hydrostatics*, where I first became acquainted with them about 1850. In Van der Waals' memoir "On the Continuity of the Gaseous and Liquid States," which I have just rapidly perused in a German translation, the author expresses himself somewhat to the following effect: If I here give values of K for some bodies, I do it not from the conviction that they are satisfactory, but because I think it important to make a commencement in a matter where our ignorance is so complete that not even a single opinion, based on probable grounds, has yet been expressed about it.

Van der Waals gives, as the value of K in water, 10,500 atmospheres; and, in a subsequent paper, 10,700 atm.; while the value given in the text above is about half, viz. 5480 atm. So far as I can see, he does not state how these values were obtained, though he gives the data and the calculations for other liquids. It is to be presumed, however, that his result for water was obtained, like those for ether and alcohol, from Cagniard de la Tour's data as to any two of the critical temperature, volume, and pressure. Van der Waals forms, by a very ingenious process, a general equation of the isothermals of a fluid, in which there are but two disposable constants. This is a cubic in v, whose three roots are real and equal at the critical point. Thus the critical temperature, volume, and pressure can all be expressed in terms of the two constants, so that one relation exists among them. Two being given, the equation of the isothermals can be formed, and from it K can be at once found.

My process, as explained above, was very different. I formed the equation of the isothermal of water at 0° C. from the empirical formula for the average compressibility under large additional pressures; and by comparing this, and the corresponding equation for various salt solutions, with an elementary formula of the Kinetic theory of gases, I was led to interpret, as the internal pressure, a numerical quantity which appears in the equations.

I have left the passages, in the text and Appendix alike, which refer to this subject in the form in which they stood before I became acquainted with Van der Waals' work. I have not sufficiently studied his memoir to be able as yet to form a definite opinion whether the difficulty (connected with the non-hydrostatic nature of the pressure in surface films) which is raised in Appendix E. can, or cannot, be satisfactorily met by Van der Waals' methods. Anyhow, the isothermals spoken of in that Appendix are totally different from those given by Van der Waals' equation, inasmuch as the whole pressure, and not merely the external pressure, is introduced graphically in my proposed construction.

SUMMARY OF RESULTS.

It is explained in the preceding pages that the pressures employed in the experiments ranged from 150 to 450 atm., so that results given below for higher or lower pressures [and enclosed in square brackets] are extrapolated. A similar remark applies to temperature, the range experimentally treated for water and for sea-water being only 0° to 15° C. Also it has been stated that the recording indices are liable to be washed down the tube, to a small extent, during the relief of pressure, so that the results given are probably a little too *small*.

Compressibility of Mercury, per atmosphere, . . .	0·0000036
„ „ Glass, ...	0·0000026

Average compressibility of fresh water:—

[At low pressures	$520 . 10^{-7}$	$- 355 . 10^{-9}t$	$+ 3 . 10^{-9}t^2$]
For 1 ton = 152·3 atm.	504	360	4
2 „ = 304·6 „	490	365	5
3 „ = 456·9 „	478	370	6

The term independent of t (the compressibility at 0° C.) is of the form

$$10^{-7}(520 - 17p + p^2),$$

where the unit of p is 152·3 atm. (one ton-weight per sq. in.). This must not be extended in application much beyond $p = 3$, for there is no warrant, experimental or other, for the minimum which it would give at $p = 8·5$.

The point of minimum compressibility of fresh water is probably about 60° C. at atmospheric pressure, but is lowered by increase of pressure.

As an *approximation* through the whole range of the experiments we have the formula:—

$$\frac{0·00186}{36 + p}\left(1 - \frac{3t}{400} + \frac{t^2}{10,000}\right);$$

while the following formula exactly represents the average of all the experimental results at each temperature and pressure:—

$$10^{-7}(520-17p+p^2)-10^{-9}(355+5p)t+10^{-9}(3+p)t^2.$$

Average compressibility of sea-water (about 0·92 of that of fresh water):—

[At low pressures	$481 . 10^{-7}$	$- 340 . 10^{-9}t$	$+ 3 . 10^{-9}t^2$]
For 1 ton	462	320	4
2 „	447·5	305	5
3 „	437·5	295	5

Term independent of t:—

$$10^{-7}(481-21{\cdot}25p+2{\cdot}25p^2).$$

Approximate formula:—

$$\frac{0{\cdot}00179}{38+p}\left(1-\frac{t}{150}+\frac{t^2}{10{,}000}\right).$$

Minimum compressibility point, probably about 56° C. at atmospheric pressure, is lowered by increase of pressure.

Average compressibility of solutions of NaCl for the first p tons of additional pressure, at 0° C.:—

$$\frac{0{\cdot}00186}{36+p+s}$$

where s of NaCl is dissolved in 100 of water.

Note the remarkable resemblance between this and the formula for the average compressibility of fresh water at 0° C. and $p+s$ tons of additional pressure.

[Various parts of the investigation seem to favour Laplace's view that there is a large molecular pressure in liquids. In the text it has been suggested, in accordance with a formula of the Kinetic Theory of Gases, that in water this may amount to about 36 tons-weight on the square inch. In a similar way it would appear that the molecular pressure in salt solutions is greater than that in water by an amount directly proportional to the quantity of salt added.]

Six miles of sea, at 10° C. throughout, are reduced in depth 620 feet by compression. At 0° C. the amount would be about 663 feet, or a furlong. (This quantity varies nearly as the square of the depth.) Hence the pressure at a depth of 6 miles is nearly 1000 atmospheres.

The maximum-density point of water is lowered about 3° C. by 150 atm. of additional pressure.

From the heat developed by compression of water I obtained a lowering of 3° C. per ton-weight per square inch.

From the ratio of the volumes of water (under atmospheric pressure) at 0° C. and 4° C., given by Despretz, combined with my results as to the compressibility, I found 3°·17 C.:—and by direct experiment (a modified form of that of Hope) 2°·7 C. The circumstances of this experiment make it certain that the last result is too small.

Thus, at ordinary temperatures, the expansibility of water is increased by the application of pressure.

In consequence, the heat developed by sudden compression of water at temperatures above 4° C. increases in a higher ratio than the pressure applied; and water under 4° C. may be heated by the sudden application of sufficient pressure.

The maximum density coincides with the freezing-point at $-2°\cdot4$ C., under a pressure of 2·14 tons.

APPENDIX A.

ON AN IMPROVED METHOD OF MEASURING COMPRESSIBILITY[1].

"WHEN the compressibility of a liquid or gas is measured at very high pressures, the compression vessel has to be enclosed in a strong cylinder of metal, and thus it must be made, in some way, self-registering. I first used indices, prevented from slipping by means of hairs. Sir W. Thomson's devices for sounding, at small depths, by the compression of air, in which he used various physical and chemical processes for recording purposes, led me to devise and employ a thin silver film which was washed off by a column of mercury. Much of my work connected with the Challenger Thermometers was done by the help of this process. Till quite recently I was unaware that it had been devised and employed by Cailletet in 1873, only that his films were of gold.

"But the use of all these methods is very laborious, for the whole apparatus has to be opened *for each individual reading*. Hence it struck me that, instead of measuring the compression produced by a given pressure, we should try to measure the pressure required to produce a given compression. I saw that this could be at once effected by the simplest electric methods; *provided that glass, into which a fine platinum wire is fused, were capable of resisting very high pressures without cracking or leaking at the junctions.* This, on trial, was found to be the case.

"We have, therefore, only to fuse a number of platinum wires, at intervals, into the compression tube, and very carefully calibrate it with a column of mercury which is brought into contact with each of the wires successively. Then if thin wires, each resisting say about an ohm, be interposed between the pairs of successive platinum wires, we have a series whose resistance is diminished by one ohm each time the mercury, forced in by the pump, comes in contact with another of the wires. Connect the mercury with one pole of a cell, the highest of the platinum wires with the other, leading the wires out between two stout leather washers; interpose a galvanometer in the circuit, and the arrangement is complete. The observer himself works the pump, keeping an eye on the pressure gauge, and on the spot of light reflected by the mirror of the galvanometer. The moment he sees a change of deflection he reads the gauge. It is convenient that the external apparatus should be made to leak slightly; for thus a *series* of measures may be made, in a minute or two, for the contact with each of the platinum wires. Then we pass to the next in succession."

M. Amagat[2] remarks on the use of this method as follows:—"Le liquide du piézomètre, et le liquide transmettant la pression dans lequel il est plongé (glycérine), s'échauffent considérablement par la pression; cette circonstance rend les expériences très longues: il faut

[1] *Proc. Roy. Soc. Edin.*, vol. XIII. pp. 2, 3, 1884. [2] *Comptes Rendus*, tom. CIII. p. 431, 1886.

un temps considérable pour équilibrer la masse qui est peu conductrice; il faut répéter les lectures jusqu'à ce que l'indication du manomètre devienne constante au moment du contact. Les séries faites par pressions décroissantes produisent le même effet en sens inverse; on prend la moyenne des résultats, dont la concordance montre que l'ensemble de la méthode ne laisse réellement presque rien à désirer.

"On voit par là quelles grossières erreurs ont pu être commises avec les autres artifices employés jusqu'ici pour la mesure des volumes dans des conditions analogues."

It must be remembered that M. Amagat is speaking of experiments in which pressures rising to 3000 atmospheres were employed.

APPENDIX B.

RELATION BETWEEN TRUE AND AVERAGE COMPRESSIBILITY.

THE average compressibility per ton for the first p tons of additional pressure is

$$\frac{v_0 - v}{pv_0};$$

where v_0 is the initial volume, and v is the volume at p additional tons.

The true compressibility at p additional tons is

$$-\frac{dv}{v\,dp}.$$

Hence, if one of these quantities is given as a function of p, it may be desirable to find the corresponding expression for the other. The simplest example, that on p. 28, will suffice to show the principle of the calculation. Let

$$\frac{v_0 - v}{pv_0} = e(1 - fp) \quad \text{.........(1)};$$

where e is, in general, a much smaller quantity than f. We have

$$\frac{v}{v_0} = 1 - ep + efp^2,$$

whence $$-\frac{dv}{v\,dp} = \frac{e(1-2fp)}{1-ep+efp^2} = e\{1-(2f-e)p+\ldots\} \quad \text{.........(2)},$$

where the expansion may be easily carried further if required.

If the terms in the second and higher powers of p are to be neglected, (1) and (2) as written show at once how to convert from true to average compressibility, or *vice versâ*.

APPENDIX C.

Calculation of Log. Factors.

Let W be the weight of mercury which would take the place of the liquid in the piezometer, w that of the mercury which fills a length l of the stem. Then a compression read as x on the stem is

$$\frac{x}{l}\,\frac{w}{W}.$$

This assumes the stem to be uniform; in general it must be corrected from the results of the calibration:—unless, as in the example given on p. 15 of the text, l be chosen very nearly equal to x, as found by trial for each value of the pressure.

Also if y be the reading of the gauge, and if a on the gauge correspond to an atmosphere, the pressure is

$$\frac{y}{a}\text{ atm.}$$

Hence the average apparent compressibility per atmosphere is

$$\frac{x}{y}\cdot\frac{wa}{lW}.$$

Its logarithm is $\qquad \log x - \log y + (\log w - \log W - \log l) + \log a.$

The last four terms, of which $\log a$ is the "gauge log," form the log factor as given in the text.

APPENDIX D.

Note on the Correction for the Compressibility of the Piezometer.

The usual correction neglects the fact that when the compressibility of the liquid is different from that of the walls, the liquid under pressure does not occupy the *same part* of the vessel as before pressure.

Let V be the volume of the part of the vessel occupied by liquid; a that of the tube between the two positions of the index, both measured at 1 atmosphere; e, ϵ, the average absolute compressibility of liquid and vessel per ton for the first p additional tons. Equate to one another the volume of the liquid, and the volume of the part of the vessel into which it is forced, both at additional pressure p. We have thus—

$$V(1-ep) = (V-a)(1-\epsilon p),$$

whence
$$e = \epsilon\left(1-\frac{a}{V}\right)+\frac{a}{pV}.$$

As $\frac{a}{V}$ is usually small, this equation is treated as equivalent to

$$e = \epsilon + \frac{a}{pV},$$

i.e. the absolute compressibility of the liquid is equal to its apparent compressibility, added to the absolute compressibility of the envelop.

One curious consequence of the exact equation is that, if the compressibilities were both constant, or were known to change in a given ratio by pressure, it would be possible (theoretically at least) to measure absolute compressibilities by piezometer experiments alone, without employing a substance whose absolute compressibility is determined by an independent process. For the additional term in the exact equation makes the coefficients of e and ϵ numerically different; whereas in the approximate equation they are equal, but with opposite signs, and therefore can give $e - \epsilon$ only.

In my experiments described above, a/V rarely exceeds 0·02, so that this correction amounts to (0·02 × 26 in 500, or) 5 units in the fourth significant place; and thus *just* escapes having to be taken account of. When 4 places are sought at lower pressures than 3 tons, or 3 places at pressures of 4 tons and upwards, it must be taken account of.

APPENDIX E.

On the Relations between Liquid and Vapour.

In connection with the present research a number of side issues have presented themselves, some of which come fairly within the scope of the Report. I commence by reprinting two Notes, read on January 19 and February 2, 1885, to the Royal Society of Edinburgh[1]:—

ON THE NECESSITY FOR A CONDENSATION-NUCLEUS.

"The magnificent researches of Andrews on the isothermals of carbonic acid formed, as it were, a nucleus in a supersaturated solution, round which an immediate crystallization started, and has since been rapidly increasing.

"They gave the clue to the explanation of the paradoxical result of Regnault, that hydrogen is less compressible and other gases more compressible, under moderate pressure, than Boyle's Law indicates; and to that of the companion result of Natterer that, at very high pressures, all gases are less compressible than that law requires. Thus they furnished the materials for an immense step in connection with the behaviour of fluids *above* their critical points.

"But they threw at least an equal amount of light on the liquid-vapour question, *i.e.* the behaviour of fluids at temperatures *under* their critical points. In Andrews' experiments there was a commencement, and a completion, of liquefaction; each at a common definite pressure, but of course at very different volumes, for each particular temperature.

"In 1871 Professor J. Thomson communicated to the Royal Society a remarkable paper on the *abrupt* change from vapour to liquid, or the opposite, indicated by these experiments. He called special attention to the necessity for a 'start,' as it were, in order that these

[1] *Proc. Roy. Soc. Edin.*, vol. XIII. pp. 78 and 91, 1885.

changes might be effected. [It is to this point that the present Note is mainly directed, but I go on with a brief analysis of Thomson's work.] He pointed out that there were numerous experiments proving that water could be heated, under certain conditions, far above its boiling point without evaporating; and that, probably, steam might be condensed isothermally to supersaturation without condensing. Hence he was led to suggest an isothermal of continued curvature, instead of the broken line given by Andrews, as representing the *continuous* passage of a fluid from the state of vapour to that of liquid; the whole mass being supposed to be, at each stage of the process, in the same molecular state.

"In Clerk-Maxwell's *Treatise on Heat*, this idea of J. Thomson's was developed, in connection with a remarkable speculation of W. Thomson[1], on the pressure of vapour as depending on the curvature of the liquid surface in contact with it. This completely accounts for the deposition of vapour when a proper nucleus is present. Maxwell showed that it could also account for the 'singing' of a kettle, and for the growth of the larger drops in a cloud at the expense of the smaller ones.

"The main objection to J. Thomson's suggested isothermal curve of transition is that, as Maxwell points out, it contains a region in which pressure and volume increase or diminish simultaneously. This necessarily involves instability, inasmuch as, for definite values of pressure at constant temperature within a certain range in which vapour and liquid can be in equilibrium, Thomson's hypothesis leads to three different values of volume: two of which are stable; but the intermediate one essentially unstable. According to Maxwell, the extremities of this triple region correspond to pressures, at which, regarded from the view of steady increase or diminution of pressures, either the vapour condenses suddenly into liquid, or the liquid suddenly bursts into vapour.

"If this were the case, no nucleus would be *absolutely* requisite for the formation either of liquid from vapour or of vapour from liquid. All that would be required, in either case, would be the proper increase or diminution of pressure;—temperature being kept unaltered. The latent heat of vapour, which we know to become less as the critical point is gradually arrived at, would thus be given off in the explosive passage from vapour to liquid. It is difficult to see, on this theory, how it can be explosively taken in on the sudden passage from liquid to vapour.

"Aitken's experiments tend to show, what J. Thomson only speculatively announced, that possibly vapour may not be condensed (in the absence of a nucleus), when compressed isothermally, even at ranges far beyond the *maximum* of pressure indicated in Thomson's figures. Hence it would appear that the range of instability is much less than that given by Thomson's figures, and may (perhaps) be looked on as a vanishing quantity; the corresponding part of the isothermal being a finite line parallel to the axis of pressures, corresponding to the sudden absorption or giving out of latent heat."

ON EVAPORATION AND CONDENSATION.

"While I was communicating my Note on the *Necessity for a Condensation Nucleus* at the last meeting of the Society, an idea occurred to me which germinated (on my way home) to such an extent that I sent it off by letter to Professor J. Thomson that same night.

"J. Thomson's idea, which I had been discussing, was to preserve, if possible, physical (as well as geometrical) *continuity* in the isothermal of the liquid-vapour state, by keeping

[1] *Proc. Roy. Soc. Edin.*, vol. VII. p. 63, 1870.

the *whole* mass of fluid in one state throughout. He secured geometrical, but not physical, continuity. For, as Clerk-Maxwell showed, one part of his curve makes pressure and volume increase simultaneously, a condition essentially unstable. The idea which occurred to me was, while preserving geometrical continuity, to get rid of the region of physical instability, *not* (as I had suggested in my former Note) by retaining Thomson's proposed finite maximum and minimum of pressure in the isothermal, while bringing them infinitely close together so far as volume is concerned, and thus restricting the unstable part of the isothermal to a finite line parallel to the pressure axis; but, *by making both the maximum and minimum infinite.* Geometrical continuity, of course, exists across an asymptote parallel to the axis of pressures; so that, from this point of view, there is nothing to object to. On the other hand, there is essentially physical discontinuity, in the form of an impassable barrier between the vaporous and liquid states, so long at least as the substance is considered as homogeneous throughout.

"It appeared to me that here lies the true solution of the difficulty. As we are dealing with a fluid mass essentially homogeneous throughout, it is clear that we are not concerned with cases in which there is a molecular surface-film.

"Suppose, then, a fluid mass, somehow maintained at a constant temperature (lower than its critical point), and so extensive that its boundaries may be regarded as everywhere infinitely distant, what will be the form of its isothermal in terms of pressure and volume?

"Two prominent experimental facts help us to an answer.

"*First.* We know that the interior of a mass of liquid mercury can be subjected to hydrostatic *tension* of considerable amount without rupture. The isothermal must, in this case, *cross* the line of volumes; and the limit of the tension would, in ordinary language, be called the cohesion of the liquid. I am not aware that this result has been obtained with water free from air; but possibly the experiment has not been satisfactorily made. The common experiment in which a rough measure is obtained of the force necessary to tear a glass plate from the surface of water is vitiated by the instability of the concave molecular film formed.

"*Second.* Aitken has asserted, as a conclusion from the results of direct experiment, that even immensely supersaturated aqueous vapour will not condense without the presence of a nucleus. This may be a solid body of finite size, a drop of water, or fine dust particles.

"Both of these facts fit perfectly in to the hypothesis, that the isothermal in question has an asymptote parallel to the axis of pressure; the vapour requiring (in the absence of a nucleus) practically infinite pressure to reduce it, without change of state or of temperature, to a certain finite volume; while the liquid, also without change of state or temperature, may by sufficient hydrostatic *tension* be made to expand almost to the same limit of volume.

"This limiting volume depends, of course, on the temperature of the isothermal; rising with it up to the critical point.

"The physical, not geometrical, discontinuity is of course to be attributed to the latent heat of vaporisation. The study of the adiabatics, as modified by this hypothesis, gives rise to some curious results.

"It is clear that the experimental realisation of the parts of the here suggested curve near to the asymptote, on either side, will be a matter of great difficulty for any substance. But valuable information may perhaps be obtained from the indications of a sensitive thermo-electric junction immersed in mercury at the top of a column which does not descend in a barometer tube of considerably more than 30 inches long, when the tube is suddenly placed at a large angle with the vertical; or from those of a similar junction immersed in water, when it has a concave surface of great curvature from which the atmospheric pressure is removed.

"Nothing of what is said above will necessarily apply when we have vapour and liquid in presence of one another, or when we consider a small portion of either in the immediate neighbourhood of another body. For then we are dealing with a state of stress which cannot, like hydrostatic pressure or tension, be characterized (so far as we know) by a single number. The stress in these molecular films is probably one of tension in all directions parallel to the film, and of pressure in a direction perpendicular to it. Thus it is impossible to represent such a state properly on the ordinary indicator diagram. This question is still further complicated by the possibility that the difference between the internal pressures, in a liquid and its vapour in thermal equilibrium, may be a very large quantity."

As soon as I heard of Berthelot's experiment, I had it successfully repeated in my laboratory; and I considered that it afforded very strong confirmation of the hypothesis advanced in the last preceding extract.

But since I have been led to believe that there is probably truth in Laplace's statement as to the very great molecular pressure in liquids, I have still further modified the speculation. I now propose to take away the new asymptote, and make the two branches of the isothermal join one another by what is practically a part of that asymptote:—thus making the liquid and the vaporous stages continuous with one another by means of a portion very nearly straight and parallel to the pressure axis. Somewhere on this will be found one of the points of inflection of the isothermal, the other being at a somewhat smaller volume, and at a pressure which is moderate for temperatures close to, but under, the "critical point," but commences to increase with immense rapidity as the temperature of the isothermal is lowered. *All* the isothermals will now present the same general features, dependent on the existence of two asymptotes and two points of inflection, whether they be above or below the critical point; but their form will be modified in different senses above and below it. The portion of the curve which is convex upwards will be nearly horizontal at the critical point, and will become steeper both above and below it; but pressure and volume will nowhere increase together. This suggestion, of course, like that in the second extract above, is essentially confined to the case of a fluid mass which is supposed to have no boundaries; for their introduction at once raises the complex difficulties connected with the surface-skin. Thus it will be seen that the conviction that water has large molecular pressure has led me back to what is very nearly the first of the two hypotheses I proposed.

A practical application of some of the principles just discussed is described in the following little paper:—

ON AN APPLICATION OF THE ATMOMETER[1].

"The Atmometer is merely a hollow ball of unglazed clay, to which a glass tube is luted. The whole is filled with boiled water, and inverted so that the open end of the tube stands in a dish of mercury. The water evaporates from the outer surface of the clay (at a rate depending partly on the temperature, partly on the dryness of the air), and in consequence the mercury rises in the tube. In recent experiments this rise of mercury has been carried to nearly 25 inches during dry weather. But it can be carried much farther by artificially drying the air round the bulb. The curvature of the capillary surfaces

[1] *Proc. Roy. Soc. Edin.*, vol. XIII. pp. 116, 117, 1885.

in the pores of the clay, which supports such a column of mercury, must be somewhere about 14,000 (the unit being an inch). These surfaces are therefore, according to the curious result of Sir W. Thomson (*Proc. Roy. Soc. Edin.*, p. 63, 1870), specially fitted to absorb moisture. And I found, by inverting over the bulb of the instrument a large beaker lined with moist filter-paper, that the arrangement can be made extremely sensitive. The mercury surface is seen to become flattened the moment the beaker is applied, and a few minutes suffice to give a large descent, provided the section of the tube be small, compared with the surface of the ball.

"I propose to employ the instrument in this peculiarly sensitive state for the purpose of estimating the amount of moisture in the air, when there is considerable humidity; but in its old form when the air is very dry. For this purpose the end of the tube of the atmometer is to be connected, by a flexible tube, with a cylindrical glass vessel, both containing mercury. When a determination is to be made in moist air, the cylindrical vessel is to be lowered till the difference of levels of the mercury amounts to (say) 25 inches, and the diminution of this difference in a definite time is to be carefully measured, the atmospheric temperature being observed. On the other hand, if the air be dry, the difference of levels is to be made *nil*, or even negative, at starting, in order to promote evaporation. From these data, along with the constant of the instrument (which must be determined for each clay ball by special experiments), the amount of vapour in the air is readily calculated. Other modes of observation with this instrument readily suggest themselves, and trials, such as it is proposed to make at the Ben Nevis Observatory during summer, can alone decide which should be preferred."

APPENDIX F.

The Molecular Pressure in a Liquid.

Laplace's result, so far as concerns the question raised in the text, may be stated thus. If $MM'\phi(r)$ be the molecular force between masses M, M' of the liquid, at distance r, the whole attraction on unit mass, at a distance x within the surface, is

$$X = 2\pi\rho \int_x^\infty r\,dr \int_r^\infty \phi(r)\,dr,$$

where ρ is the density of the liquid. The density is supposed constant, even in the surface-skin. As we are not concerned with what are commonly called capillary forces, the surface is supposed to be plane.

The pressure, p, is found from the ordinary hydrostatic equation

$$\frac{dp}{dx} = \rho X.$$

Hence the pressure in the interior of the liquid is

$$K = \rho \int_0^a X\,dx,$$

where a is the limit at which the molecular force ceases to be sensible.

But the expression for K is numerically the work required to carry unit volume of the liquid from the interior, through the skin, to the surface. It is easy to see that the further work, required to carry it wholly out of the range of the molecular forces, has precisely the same value. Thus the whole work required to carry, particle by particle, a cubic inch of the liquid from the interior to a finite distance from its surface is

$$2K \times 1 \text{ cub. in.}$$

This investigation assumes ρ to be constant throughout the liquid, and thus ignores the (almost certain) changes of density in the various layers of the surface-skin; so that its conclusions, even when the question is regarded as a purely statical one, are necessarily subject to serious modification. With our present knowledge of the nature of heat, we cannot regard this mode of treatment as in any sense satisfactory.

APPENDIX G.

Equilibrium of a Column of Water.

First, suppose the temperature to be the same throughout. Let a be the whole depth, ρ_0 the density, on the supposition that gravity does not act. Then, if ρ be the density at the distance ξ from the bottom, when gravity acts, we have by the hydrostatic equation

$$\frac{dp}{d\xi} = -g\rho = -g\rho_0 \frac{1}{1 - \dfrac{Ap}{\Pi + p}},$$

if we adopt the rough formula of Section VII. for the compressibility. The integral is

$$p(1-A) + A\Pi \log(\Pi + p) = C - g\rho_0\xi.$$

Now the conditions are—

(1) $\xi = \xi_0$ (the altered depth), $p = 0$;

(2) $\xi = 0$, $p = g\rho_0 a = \varpi$ suppose.

So that
$$\xi_0 = a(1-A) + \frac{A\Pi}{g\rho_0} \log \frac{\Pi + g\rho_0 a}{\Pi}$$

$$= a(1-A) + \frac{A\Pi a}{\varpi} \log\left(1 + \frac{\varpi}{\Pi}\right).$$

Since, even in the deepest sea, ϖ/Π is not greater than 1/6, we may expand the logarithm in ascending powers of this fraction. We thus obtain

$$\xi_0 = a - aA\left\{1 - \frac{\Pi}{\varpi}\left(\frac{\varpi}{\Pi} - \frac{\varpi^2}{2\Pi^2} + \frac{\varpi^3}{3\Pi^3} - \dots\right)\right\}$$

$$= a - aA\left\{\frac{\varpi}{2\Pi} - \frac{\varpi^2}{3\Pi^2} + \dots\right\}.$$

The second term is the diminution of depth required. We may write it, with change of sign, as

$$\frac{A}{2\Pi} g\rho_0 a^2 \left(1 - \frac{2\varpi}{3\Pi} + \frac{\varpi^2}{2\Pi^2} - \&c.\right).$$

As the factor A/Π stands for what is called e in the text, the first term is the result given in the text; and the others show how it is modified by taking account of the diminished compressibility at the higher pressures.

Of course we might have employed the more exact formulæ, (A) or (B) as the case may be, but for all practical applications the rough formula suffices.

It might be interesting to study the effect on the mean level of a lake due to the indirect as well as the direct results of change of temperature. Heating of the water throughout, if there be a case of the kind, would increase the depth not only in consequence of expansion (provided the temperature were nowhere under the maximum-density point), but also in consequence of the diminution of compressibility which it produces. Thus there would be an efficient cause of variation of depth with the seasons, altogether independent of the ordinary questions of supply from various sources and loss by evaporation.

If the temperature be not constant for all depths, ρ_0, ρ, and A are functions of ξ. Substituting their values in the hydrostatic equation, we must integrate it and determine the constant by the same conditions as before.

The condition for stable equilibrium is merely that $d\rho/d\xi$ shall not be anywhere positive. Until some definite problem is proposed, no more can be done with the equation.

[29/10/88.—At Dr Murray's request I have calculated, from the data given in his paper: "On the Height of the Land, and the Depth of the Ocean" (*Scottish Geographical Magazine*, vol. IV. pp. 1—41, 1888), that the whole depression of the ocean level, due to compression, is about

116 feet only.

If water ceased to be compressible, the effect would be to submerge some 2,000,000 square miles of land, about 4 per cent. of the whole.]

LXII.

OPTICAL NOTES.

[*Proceedings of the Royal Society of Edinburgh*, 16 *January*, 1881.]

1. *On a Singular Phenomenon produced by some old Window-Panes.*

A FIGURE, illustrating the action of a cylindrical lens, which was inserted in a recent page of these *Proceedings*, has reminded me of my explanation of a phenomenon which I have repeatedly seen for more than twenty years in the College. When sunlight enters my apparatus-room through a vertical chink between the edge of the blind and the window-frame, the line of light formed on the wall or floor shows a well-marked *kink*. Similar phenomena, though not usually so well marked, are often seen in old houses, when the sun shines through the chinks of a Venetian blind. They are obviously due to inequalities (bull's-eyes) in the glass which was used more than a generation ago for window-panes. It is evident that the focal length of successive annuli of such a piece of glass, treated as a lens, increases from the central portion to the circumference, where it becomes infinite. For an approximate study of its behaviour we may assume that the focal length of an annulus of radius r is $b^2/(a-r)$, where a is the extreme radius, at which the sides of the pane become parallel. Suppose sunlight, passing through a narrow slit, to fall on such a lens at a distance e from its centre, and to be received on a screen at a distance c from the lens. It is easy to see that the polar equation of the illuminated curve on the screen is (the pole being in the axis of the lens)

$$\rho = -\frac{e \sec\theta}{b^2}(ac - b^2 - ce\sec\theta).$$

This curve can be readily traced by points for various values of the constants. In fact, if r be the radius vector of a straight line, the vector of any one of these curves (drawn in the same direction) is proportional to $r(A-r)$, and the curve can therefore be constructed from a straight line and a circle. Here the value of A is $(ac-b^2)/c$; *i.e.*, it is a fourth proportional to c, a, and the distance of the screen from the focus of the central portion of the lens. When A is small compared with the least value of r, the curve has a point resembling a cusp, but as A increases the kink appears. This is easily observed by gradually increasing the distance of the screen from the lens; and the traced curves present forms which are precisely of the general character of those observed.

2. *On the Nature of the Vibrations in Common Light.*

One of the few really unsatisfactory passages in Airy's well-known "Tract" on the *Undulatory Theory of Optics* is that which discusses the nature of common light. To explain the production of Newton's rings in homogeneous light to the number of several thousands, it is necessary that at least several thousand successive waves should be almost exactly similar to one another. On the other hand, we cannot suppose the vibrations (which will in general be elliptic) to be similar to one another for more than a small fraction of a second; if they were so, we should see colour phenomena in doubly refracting plates by the aid of an analysing Nicol only.

And, moreover, the nature of the vibration can have no *periodic* changes of a kind whose period amounts to a moderate fraction of a second. Nor can it have a slow *progressive* change. Either of these would lead to its resolution into rays of *different* wave-lengths. Airy suggests, as consistent with observation, some thousand waves polarized in one plane followed by a similar number polarized in a plane at right angles to the first. But no physical reason can be assigned for such an hypothesis.

The difficulty, however, disappears if we consider the question from the modern statistical point of view, as it is applied for instance in the kinetic theory of gases. We may consider first a *space* average taken for the result due to each separate vibrating particle near the surface of a luminous body. When we remember that, for homogeneous light, of mean wave-length, a million vibrations occupy only about one five hundred millionth of a second; it is easy to see that the resultant vibration at any point may not sensibly vary for a million or so of successive waves, though the contributions from individual particles may very greatly change. But when we consider the *time average* of about a hundred millions of groups of a million waves each, all entering the eye so as to be simultaneously perceptible,—in consequence of the duration of visual impressions,—we see that the chances in favour of a deviation from apparently absolute uniformity are so large that, though possible, such uniformity is not to be expected for more than a very small fraction of a second. The improbability of its occurrence for a single second is of the same nature as that of the possible, but never realised, momentary occurrence of a cubic inch of the air in a room filled with oxygen or with nitrogen alone.

[*Added; May* 1, 1882.—I am indebted to Professor Stokes for a reference to his paper "On the Composition and Resolution of Streams of Polarized Light from Different Sources" (*Camb. Phil. Trans.*, 1852), in which the nature of common light is very fully investigated. I find I was not singular in my ignorance of the contents of this paper, as the subject has quite recently been proposed as a Prize Question by a foreign Society.]

LXIII.

ON A METHOD OF INVESTIGATING EXPERIMENTALLY THE ABSORPTION OF RADIANT HEAT BY GASES.

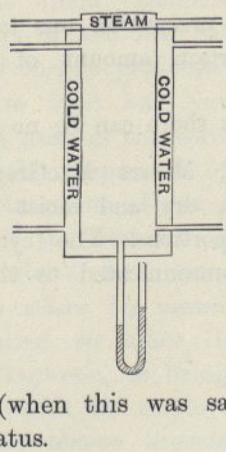
(*Read by Sir W. Thomson at the B. A. Meeting at Southampton.*)

[*Nature, October* 26, 1882.]

THERE are grave objections, which have been only partially overcome, to almost all the processes hitherto employed for testing the diathermancy of vapours. These arise chiefly from condensation on some part of the apparatus. Thus when rock-salt is used, an absorbent surface-layer may be formed; and, when the pile is used without a plate of salt, the effect of radiant heat may be to cool it (the pile) by the evaporation of such a surface film. The use of intermittent radiation is liable to the same objection.

Some time ago it occurred to me that *this* part of the difficulty might be got rid of by dispensing with the pile, and measuring the amount of absorption by its continued effects on the volume and pressure of the gas or vapour itself.

Only preliminary trials have, as yet, been made. They were carried out for me by Prof. MacGregor and Mr Lindsay. Their object was *first* to find whether the



method would work well, *second* (when this was satisfactorily proved) to find the best form and dimensions for the apparatus.

The rough apparatus is merely a double cylinder, placed vertically. Cold water circulates in the jacket, and steam can be blown into the double top. The changes in the pressure of the gas are shown by a manometer U tube at the bottom, which contains a liquid which will not absorb the contents. This apparatus was 4 feet long, with 2 inches internal radius. The results of a number of experiments show that it should be shorter and much wider. The former idea I was not quite prepared for, the latter is obvious.

The effects on the manometer are due to five chief causes:—

1. Heating of the upper layer of gas by contact with lid.
2. Cooling " " " " " sides.
3. Heating of more or less of the column by absorption.
4. Cooling of do. by radiation.
5. " " contact.

(1) and (2) only are present in a perfectly diathermanous gas, and in a perfectly adiathermanous gas or vapour.

All five are present in a partially diathermanous gas or vapour.

The preliminary experiments show that the manometer effect is only *very slightly less* for dry olefiant gas than for dry air, while moist air shows a markedly smaller effect than either of the others.

This is conclusive as to the absorption of low radiant heat by aqueous vapour, but it shows also that the absorption is so small as to take place throughout the whole column.

Even with the present rude apparatus I hope soon to get a very accurate determination of the absorbing power of aqueous vapour, by finding in what proportions olefiant gas must be mixed with air to form an absorbing medium equivalent to saturated air at different temperatures.

I have to acknowledge valuable hints from Prof. Stokes, who, before I told him the results I had obtained (thus knowing merely the *nature* of the experiments) made something much higher than a guess, though somewhat short of a prediction, of the truth.

In these preliminary trials no precaution was taken to exclude *dust.* The results, therefore, are still liable to a certain amount of doubt, as Mr Aitken's beautiful experiments have shown.

The *point* of the method is that there can be no question of surface-layers.

[Since the above was written, Messrs MacGregor and Lindsay have made an extended series of experiments with dry and moist air, and with mixtures of dry air and olefiant gas in different proportions. The cylinder employed was 9 inches in radius. The results will soon be communicated to the Royal Society of Edinburgh.]

LXIV.

1. ON THE LAWS OF MOTION. PART I.

[*Proceedings of the Royal Society of Edinburgh, December* 13, 1882.]

THE substance of part at least of this paper was given in 1876 as an evening lecture to the British Association at its Glasgow meeting. [*Anté*, No. XXXVII.]

While engaged in writing the article "Mechanics" for the *Ency. Brit.*, I had to consider carefully what basis to adopt, and decided that the time had not yet come in which (at least in a semi-popular article) Newton's laws of motion could be modified. The article was therefore based entirely on these laws, with a mere hint towards the end that in all probability they would soon require essential modification. It is well, however, that the question of modification should now be considered; and this should be done, not in a popular essay but, before a scientific society.

The one objection to which, in modern times, that wonderfully complete and compact system is liable, is that it is expressly founded on the conception of what is now called "force" as an agent which "compels" a change of the state of rest or motion of a body. This is part of the first law, and the second law is merely a definite statement of the amount of change produced by a given force.

(Next comes a digression as to what was Newton's expression for what we now mean by the word force, when it is used in the correct signification above.)

There can be no doubt that the proper use of the term *force* in modern science is that which is implied in the statement—Force is whatever changes a body's state of rest or motion. This is part of the first law of motion. Thus we see that force is the English equivalent of Newton's term *vis impressa.* But it is also manifest that, on many occasions, *but only where his meaning admitted of no doubt,* Newton omitted the word *impressa* and used *vis* alone, in the proper sense of force. In other cases he omitted the word *impressa,* as being implied in some other adjective such as *centripeta, gravitans,* &c., which he employed to qualify the word *vis.* Thus (Lemma X.) he says:—*Spatia, quæ corpus urgente quâcunque vi finitâ describit,* &c.

It is needless to multiply examples. But that this is the true state of the case is made absolutely certain by the following:—

Definitio IV. Vis impressa est actio in corpus exercita, ad mutandum ejus statum vel quiescendi vel movendi uniformiter in directum.

Contrast this with the various senses in which the word *vis* is used in the comment which immediately follows, viz.:—

Constitit hæc vis in actione solâ, neque post actionem permanet in corpore. Perseverat enim corpus in statu omni novo per solam vim inertiæ. Est autem vis impressa diversarum originum, ut ex ictu, ex pressione, ex vi centripetâ.

These passages are translated by Motte as below:—

"*Definition IV. An impressed force is an action exerted upon a body, in order to change its state, either of rest, or of moving uniformly forward in a right line.*"

"This force consists in the action only, and remains no longer in the body when the action is over. For a body maintains every new state it acquires, by its *vis inertiæ* only. Impressed forces are of different origins; as from percussion, from pressure, from centripetal force."

The difficulty which Motte here makes for himself, and which he escapes from only by leaving part of the passage in the original Latin, is introduced solely by his use of the word *force* as the equivalent of the Latin *vis*.

If we paraphrase the passage as follows, with attention to Newton's obvious meaning, this difficulty disappears, or rather does not occur:—

"This kind of vis consists in," &c. For the "body continues . . . . by the vis of inertia," &c. However, we may quote two other passages of Newton bearing definitely on this point.

Definitio III. Materiæ vis insita est potentia resistendi, quâ corpus unumquodque, quantum in se est, perseverat in statu suo vel quiescendi vel movendi uniformiter in directum.

It is perfectly clear that, in this passage, the phrase *vis insita* is one idea, not two, and that *vis* cannot here be translated by *force*. Yet Motte has

"The *vis insita*, or innate force of matter, is," &c.

Definitio V. Vis centripeta est, quâ corpora versus punctum aliquod, tanquam ad centrum, undique trahuntur, impelluntur, vel utcumque tendunt.

It is obvious that the qualifying term *centripeta* here includes the idea suggested by *impressa*, defining in fact the direction of the *vis*, and therefore implying that its origin is outside the body.

After what has just been said, no farther comment need be added to show the absurdity of the terms *accelerating force, innate force, impressed force*, &c. All of these have arisen simply from mistranslation. Vis, by itself, is often used for force; but *vis acceleratrix, vis impressa, vis insita*, and other phrases of the kind, must be taken as wholes; and, in them, *vis* does not mean *force*.

The absurdity of translating the word *vis* by *force* comes out still more clearly when we think of the term *vis viva*, or *living force* as it is sometimes called; a name for kinetic energy, which depends on the unit of length in a different way from force. It must be looked upon as one of the most extraordinary instances of Newton's clearness of insight that, at a time when the very terminology of science was only as it were shaping itself, he laid down with such wonderful precision a system absolutely self-consistent.

From the passages just quoted, taken in conjunction with the second law of motion, we see that (as above stated) in Newton's view—

Force is whatever causes (but not, *or tends to cause*) *a change in a body's state of rest or motion.*

Newton gives no sanction to the so-called *statical* ideas of force. Every force, in his view, produces its effect. The effects may be such as to balance or compensate one another; but there is no balancing of forces.

(Next comes a discussion as to the objectivity or subjectivity of force. An abstract of this is given in §§ 288—296 of the article above referred to, and therefore need not be reproduced here.)

But, just as there can be no doubt that force has no objective existence, so there can be no doubt that the introduction of this conception enabled Newton to put his *Axiomata* in their exceedingly simple form. And there would be, even now, no really valid objection to Newton's system (with all its exquisite simplicity and convenience) could we only substitute for the words "force" and "action," &c., in the statement of his laws, words which (like rate or gradient, &c.) do not imply objectivity or causation in the idea expressed. It is not easy to see how such words could be introduced; but assuredly they will be required if Newton's system is to be maintained. The word stress might, even yet, be introduced for this purpose; though, like force, it has come to be regarded as something objective. Were this possible, we might avoid the necessity for any very serious change in the *form* of Newton's system. I intend, on another occasion, to consider this question. How complete Newton's statement is, is most easily seen by considering the so-called "additions" which have been made to it.

The second and third laws, together with the scholium to the latter, expressly include the whole system of "effective forces," &c. for which D'Alembert even now receives in many quarters such extraordinarily exaggerated credit. The "reversed effective force" on a particle revolving uniformly in a circle is nothing but an old friend—"centrifugal force." And even this phantom is still of use, *in skilled hands*, in forming the equations for certain cases of motion.

The chief arguments for and against a modern modification of the laws of motion are therefore as follows—where we must remember that they refer exclusively to the elementary teaching of the subject, and have no application to the case of those who have sufficient knowledge to enable them to avoid the possible dangers of Newton's method :—

I. FOR. Is it wise to teach a student by means of the conception of force, and then as it were to kick down the scaffolding by telling him there is no such thing?

II. AGAINST. Is it wise to give up the use of a system, due to such an altogether exceptional genius as that of Newton, and one which amply suffices for all practical purposes, merely because it owes part of its simplicity and compactness to the introduction of a conception which, though strongly impressed on us by our muscular sense, corresponds to nothing objective?

Everyone must answer these questions for himself, and his answer will probably be determined quite as much by his notions of the usefulness of the study of natural philosophy as by his own idiosyncrasies of thought. To some men physics is an abomination, to others it is something too trivial for the human intellect to waste its energies on. With these we do not reason. To others again all its principles are subjects of intuitive perception. *They* could have foreseen the nature of the physical world, and they *know* that it could not have been otherwise than they suppose it to be. Many minds find delight in the contemplation of the three kinds of lever; others in the ingeniously disguised assumptions in Duchayla's "proof" of the parallelogram of forces; some, perhaps, even in the wonderful pages of *Vis Inertiæ Victa!* The case of these men is only not more hopeless than that of the former classes because it is impossible that it could be so.

But those who desire that their scientific code should be, as far as possible, representative of our real knowledge of objective things, would undoubtedly prefer to that of Newton a system in which there is not an attempt, however successful, to gain simplicity by the introduction of subjective impressions and the corresponding conceptions.

In the present paper simplicity of *principle*, only, is sought for; and the mathematical methods employed are those which appeared (independent altogether of the question of their fitness for a beginner) the shortest and most direct. A second part will be devoted to simplicity of *method* for elementary teaching.

(1) So far as our modern knowledge goes there are but two objective things in the physical world—matter and energy. Energy cannot exist except as associated with matter, and it can be perceived and measured by us only when it is being transferred, by a "dynamical transaction," from one portion of matter to another. In such transferences it is often "transformed"; but no process has ever been devised or observed by which the quantity, either of matter or energy, has been altered.

(2) Hence the true bases of our subject, so far as we yet know, are—

1. Conservation of matter.

2. Conservation of energy.

3. That property (those properties ?) of matter, in virtue of which it is the necessary vehicle, or as the case may be, the storehouse, of energy.

(3) The third of these alone presents any difficulty. So long as energy is obviously kinetic, this property is merely our old friend *inertia.* But the mutual potential energy of two gravitating masses, two electrified bodies, two currents, or two magnets, is certainly associated (at least in part, and in some as yet unknown way) with matter, of a kind not yet subjected to chemical scrutiny, which occupies the region in which these masses, &c., are situated. And, even when the potential energy obviously depends on the strain of a portion of ordinary matter, as in compressed air, a bent spring, a deformed elastic solid, &c., we can, even now, only describe it as due to "molecular action," depending on mechanism of a kind as yet unknown to us, though, in some cases, at least partially guessed at.

(4) The necessity for the explicit assumption of the third principle, and a hint at least of the limits within which it must be extended, appear when we consider the very simplest case of motion, viz., that of a lone particle moving in a region in which its potential energy is the same at every point. For the conservation of energy tells us merely that its *speed* is unaltered. We know, however, that this is only part of the truth: the *velocity* is constant. It will be seen later that this has most important dynamical consequences in various directions.

(The remarkable discussion of this point by Clerk-Maxwell is then referred to, in which it is virtually shown that, were things otherwise, it would be possible for a human mind to have knowledge of *absolute* position and of *absolute* velocity.)

(5) But Maxwell's reasoning is easily seen to apply equally to any component of the velocity. Hence, when we come to the case in which the potential energy depends on the position, the only change in the particle's motion at any instant is a change of the speed in the normal to the equipotential surface on which the particle is at that instant situated. The conservation of energy assigns the amount of this change, and thus the motion is completely determined. In fact, if x be perpendicular to the equipotential surface, the equation

$$\tfrac{1}{2}m(\dot{x}^2+\dot{y}^2+\dot{z}^2)+V=\text{const.}$$

gives

$$m\ddot{x}=-\frac{dV}{dx},$$

for $\dot{y}$ and $\dot{z}$ are independent of x. Generally, in the more expressive language of quaternions,

$$m\ddot{\rho}=-\nabla V.$$

In fact, this problem is precisely the same as was that of the motion of a luminous corpuscle in a non-homogeneous medium, the speed of passing through any point of the medium being assigned.

(6) It is next shown that the above inertia-condition (that the velocity parallel to the equipotential surface is the same for two successive elements of the path) at once leads to a "stationary" value of the sum of the quantities vds for each two successive elements, and therefore for any finite arc, of the path. This is, for a single particle, the *Principle of Least Action*, which is thus seen to be a direct consequence of inertia.

(It is then shown that the results above can be easily extended to a particle which has two degrees of freedom only.)

But it is necessary to remember that, in these cases, we take a partial view of the circumstances; for a lone particle cannot strictly be said to have potential energy, nor can we conceive of a constraint which does not depend upon matter other than that which is constrained. Hence the true statement of such cases requires further investigation.

(7) To pass to the case of a system of free particles we require some quasi-kinematical preliminaries. These are summed up in the following self-evident

proposition:—If with each particle of a system we associate two vectors, *e.g.*, Θ_1, Φ_1, with the mass m_1, &c., we have

$$\Sigma m\Theta\Phi = \Sigma(m).\Theta_0\Phi_0 + \Sigma m\theta\phi,$$

where
$$\Theta = \Theta_0 + \theta,$$
$$\Phi = \Phi_0 + \phi,$$
and
$$\Sigma m\Theta = \Sigma(m).\Theta_0,$$
$$\Sigma m\Phi = \Sigma(m).\Phi_0,$$

so that Θ_0 and Φ_0 are the values of Θ and Φ for the whole mass collected at its centre of inertia; and θ, ϕ, those of the separate particles relative to that centre.

(8) Thus, if $\Theta = \mathrm{P} = \mathrm{P}_0 + \rho$ be the vector of m, $\Phi = \dot\Theta = \dot{\mathrm{P}} = \dot{\mathrm{P}}_0 + \dot\rho$, its velocity, we have

$$\Sigma m\mathrm{P}\dot{\mathrm{P}} = \Sigma(m).\mathrm{P}_0\dot{\mathrm{P}}_0 + \Sigma m\rho\dot\rho,$$

the scalar of which is, in a differentiated form, a well-known property of the centre of inertia. The vector part shows that the sum of the moments of momentum about any axis is equal to that of the whole mass collected at its centre of inertia, together with those of the several particles about a parallel axis through the centre of inertia.

If
$$\Theta = \Phi = \dot{\mathrm{P}},$$
we have
$$\Sigma m\dot{\mathrm{P}}^2 = \Sigma(m).\dot{\mathrm{P}}_0^2 + \Sigma m\dot\rho^2,$$

i.e., the kinetic energy, referred to any point, is equal to that of the mass collected at its centre of inertia, together with that of the separate particles relative to the centre of inertia.

If we integrate this expression, multiplied by dt, between any limits, we obtain a similar theorem with regard to the Action of the system.

Such theorems may be multiplied indefinitely.

(9) From those just given, however, if we take them along with 3 above, we see at once that, provided the particles of the system be all free, while the energy of each is purely kinetic and independent alike of the configuration of the system and of its position in space,

1. The centre of inertia has constant velocity.
2. The vector moment of momentum about it is constant.
3. So is that of the system relative to any uniformly moving point.
4. $\Sigma \int mvds$ is obviously a minimum.

(10) The result of (7) points to an independence between two parts of the motion of a system, *i.e.*, that relative to the centre of inertia, and that of the whole mass supposed concentrated at the centre of inertia. Maxwell's reasoning is

applicable directly to the latter, if the system be self-contained, *i.e.*, if it do not receive energy from, or part with it to, external bodies. Hence we may extend the axiom 3 to the centre of inertia of any such self-contained system, and, as will presently be shown, also to the motion of the system relative to its centre of inertia. This, though not *formally* identical with Newton's Lex III., leads, as we shall see, to exactly the same consequences.

(11) If, for a moment, we confine our attention to a free system consisting of two particles only, we have

$$m_1\dot{\rho}_1 + m_2\dot{\rho}_2 = (m_1 + m_2)\,\alpha,$$

or

$$m_1\ddot{\rho}_1 + m_2\ddot{\rho}_2 = 0 \quad\ldots\ldots(1).$$

This must be consistent with the conservation of energy, which gives

$$\tfrac{1}{2}(m_1\dot{\rho}_1^2 + m_2\dot{\rho}_2^2) = f\,(T(\rho_1 - \rho_2)) \quad\ldots\ldots(2),$$

since the potential energy must depend (so far as *position* goes) on the distance between the particles only. Comparing (1) and (2) we see that we may treat (2) by partial differentiation, so far as the coordinates of m_1 and m_2 are separately concerned. For we thus obtain

$$m_1\ddot{\rho}_1 = \nabla_{\rho_1}.f = f'.\, U(\rho_1 - \rho_2),$$

$$m_2\ddot{\rho}_2 = \nabla_{\rho_2}.f = -f'.\, U(\rho_1 - \rho_2).$$

Each of these, again, is separately consistent with the equation in § 5 for a lone particle. Hence, again, the integral $\int(m_1v_1ds_1 + m_2v_2ds_2)$ has a stationary value.

Hence also, whatever be the origin, provided its velocity be constant,

$$\Sigma m\, V\rho\ddot{\rho} = 0.$$

Thus, even when there is a transformation of the energy of the system, the results of § 9 still hold good. And it is to be observed that if one of the masses, say m_2, is enormously greater than the other, the equation

$$m_1\ddot{\rho}_1 + m_2\ddot{\rho}_2 = 0$$

shows that $\ddot{\rho}_2$ is excessively small, and the visible change of motion is confined to the smaller mass. Carrying this to the limit, we have the case of motion about a (so-called) "fixed centre." In such a case it is clear that though the *momenta* of the two masses relative to their centre of inertia are equal and opposite, the kinetic energy of the greater mass vanishes in comparison with that of the smaller.

These results are then extended to any self-contained system of free particles, and the principle of *Varying Action* follows at once. It is thus seen to be a general expression of the three propositions of § 2 above.

(12) So far as we have yet gone, nothing has been said as to *how* the mutual potential energy of two particles depends on their distance apart. If we suppose it to be enormously increased by a very small increase of distance, we have practically

the case of two particles connected by an inextensible string—as a chain-shot. But from this point of view such cases, like those of connection by an extensible string, fall under the previous categories.

The case of impact of two particles falls under the same rules, so far as motion of the centre of inertia, and moment of momentum about that centre, are concerned. The conservation of energy, in such cases, requires the consideration of the energy spent in permanently disfiguring the impinging bodies, setting them into internal vibration, or heating them. But the first and third of these, at least, are beyond the scope of abstract dynamics.

(13) The same may be said of constraint by a curve or surface, and of loss of energy by friction or resistance of a medium. Thus a constraining curve or surface must be looked upon (like all physical bodies) as deformable, but, if necessary, such that a very small deformation corresponds to a very great expenditure of energy.

(14) To deal with communications of energy from bodies outside the system, all we need do is to *include them in the system.* Treat as before the whole system thus increased, and then consider only the motion of the original parts of the system. This method applies with perfect generality whether the external masses be themselves free, constrained, or resisted.

(15) Another method of applying the same principles is then given. Starting from the *definition* $dA = \Sigma mS\dot{\rho}d\rho$, the kinematical properties of A are developed. Then, by the help of § 2, these are exhibited in their physical translations.

(16) The paper concludes with a brief comparison of the fundamental principles of the science as they have been introduced by Newton, Lagrange, Hamilton, Peirce, Kirchhoff, and Clerk-Maxwell, respectively; and also as they appear in the unique Vortex-system of Thomson.

LXV.

JOHANN BENEDICT LISTING.

[*Nature, February* 1, 1883.]

One of the few remaining links that still continued to connect our time with that in which Gauss had made Göttingen one of the chief intellectual centres of the civilised world has just been broken by the death of Listing.

If a man's services to science were to be judged by the mere number of his published papers, Listing would not stand very high. He published little, and (it would seem) was even indebted to another for the publication of the discovery by which he is most widely known. This is what is called, in Physiological Optics, *Listing's Law.* Stripped of mere technicalities, the law asserts that if a person whose head remains fixed turns his eyes from an object situated directly in front of the face to another, the final position of each eye-ball is such as would have been produced by rotation round an axis perpendicular alike to the ray by which the first object was seen and to that by which the second is seen. "Let us call that line in the retina, upon which the visible horizon is portrayed when we look, with upright head, straight at the visible horizon, the horizon of the retina. Now any ordinary person would naturally suppose that if we, keeping our head in an upright position, turn our eyes so as to look, say, up and to the right, the horizon of the retina would remain parallel to the real horizon. This is, however, not so. If we turn our eyes straight up or straight down, straight to the right or straight to the left, it is so, but not if we look up or down, and also to the right or to the left. In *these* cases there is a certain amount of what Helmholtz calls 'wheel-turning' (*Raddrehung*) of the eye, by which the horizon of the retina is tilted so as to make an angle with the real horizon. The relation of this 'wheel-turning' to the above-described motion of the optic axis is expressed by Listing's law, in a perfectly simple way, a way so simple that it is only by going back to what we might have thought nature should have done, and from that point of view, looking at what the eye really does, and considering the complexity of the problem, that we see the ingenuity of Listing's law, which is simple in the extreme, and seems to agree with fact quite exactly, except in the case of very short-sighted eyes." The physiologists of the time, unable to make out these things for themselves, welcomed the assistance of the mathematician. And so it has always been in Germany. Few are entirely ignorant of the immense accessions which physical science owes to Helmholtz. Yet few are aware that he *became* a mathematician in order that he might be able to carry out properly his physiological researches. What a pregnant comment on the conduct of those "British geologists" who, not many years ago, treated with outspoken contempt Thomson's thermodynamic investigations into the admissible lengths of geological periods!

Passing over about a dozen short notes on various subjects (published chiefly in the Göttingen *Nachrichten*), we come to the two masterpieces, on which (unless, as we hope may prove to be the case, he have left much unpublished matter) Listing's fame must chiefly rest. They seem scarcely to have been noticed in this country, until attention was called to their contents by Clerk-Maxwell.

The first of these appeared in 1847, with the title *Vorstudien zur Topologie.* It formed part of a series, which unfortunately extended to only two volumes, called *Göttinger Studien.* The term Topology was introduced by Listing to distinguish what may be called qualitative geometry from the ordinary geometry in which quantitative relations chiefly are treated. The subject of knots furnishes a typical example of these merely qualitative relations. For, once a knot is made on a cord, and the free ends tied together, its nature remains unchangeable, so long as the continuity of the string is maintained, and is therefore totally independent of the actual or relative dimensions and form of any of its parts. Similarly when two endless cords are linked together. It seems not unlikely, though we can find no proof of it, that Listing was led to such researches by the advice or example of Gauss himself; for Gauss, so long ago as 1833, pointed out their connection with his favourite electromagnetic inquiries.

After a short introductory historical notice, which shows that next to nothing had then been done in his subject, Listing takes up the very interesting questions of Inversion (*Umkehrung*) and Perversion (*Verkehrung*) of a geometrical figure, with specially valuable applications to images as formed by various optical instruments. We cannot enter into details, but we paraphrase one of his examples, which is particularly instructive:—

"A man on the opposite bank of a quiet lake appears in the watery mirror perverted, while in an astronomical telescope he appears inverted. Although both images show the head down and the feet up, it is the dioptric one only which:—if we could examine it:—would, like the original, show the heart on the left side; for the catoptric image would show it on the right side. In type there is a difference between inverted letters and perverted ones. Thus the Roman V becomes, by inversion, the Greek Λ; the Roman R perverted becomes the Russian Я; the Roman L, perverted and inverted, becomes the Greek Γ. Compositors read perverted type without difficulty:—many newspaper readers in England can read inverted type. * * * The numerals on the scale of Gauss' Magnetometer must, in order to appear to the observer in their natural position, be both perverted and inverted, in consequence of the perversion by reflection and the inversion by the telescope."

Listing next takes up helices of various kinds, and discusses the question as to which kind of screws should be called right-handed. His examples are chiefly taken from vegetable spirals, such as those of the tendrils of the convolvulus, the hop, the vine, &c., some from fir-cones, some from snail-shells, others from the "snail" in clock-work. He points out in great detail the confusion which has been introduced in botanical works by the want of a common nomenclature, and finally proposes to found such a nomenclature on the forms of the Greek δ and λ.

The consideration of double-threaded screws, twisted bundles of fibres, &c., leads to the general theory of paradromic winding. From this follow the properties of a large class of knots which form "clear coils." A special example of these, given by Listing for threads, is the well-known juggler's trick of slitting a ring-formed band up the middle, through its whole length, so that instead of separating into two parts, it remains in a continuous ring. For this purpose it is only necessary to give a strip of paper one *half*-twist before pasting the ends together. If three half-twists be given, the paper still remains a continuous band after slitting, but it cannot be opened into a ring, it is in fact a trefoil knot. This remark of Listing's forms the sole basis of a work which recently had a large sale in Vienna:—showing how, in emulation of the celebrated Slade, to tie an irreducible knot on an endless string!

Listing next gives a few examples of the application of his method to knots. It is greatly to be regretted that this part of his paper is so very brief; and that the opportunity to which he deferred farther development seems never to have arrived. The methods he has given are, as is expressly stated by himself, only of limited application. There seems to be little doubt, however, that he was the first to make any really successful attempt to overcome even the preliminary difficulties of this unique and exceedingly perplexing subject.

The paper next gives examples of the curious problem:—Given a figure consisting of lines, what is the smallest number of *continuous* strokes of the pen by which it can be described, no part of a line being gone over more than once? Thus, for instance, the lines bounding the 64 squares of a chess-board can be drawn at 14 separate pen strokes. The solution of all such questions depends at once on the enumeration of the points of the complex figure at which an odd number of lines meet.

Then we have the question of the "area" of the projection of a knotted curve on a plane; that of the number of interlinkings of the orbits of the asteroids; and finally some remarks on hemihedry in crystals. This paper, which is throughout elementary, deserves careful translation into English very much more than do many German writings on which that distinction has been conferred.

We have left little space to notice Listing's greatest work, *Der Census räumlicher Complexe* (Göttingen *Abhandlungen*, 1861). This is the less to be regretted, because, as a whole, it is far too profound to be made popular; and, besides, a fair idea of the nature of its contents can be obtained from the introductory Chapter of Maxwell's great work on Electricity. For there the importance of Listing's Cyclosis, Periphractic Regions, &c., is fully recognised.

One point, however, which Maxwell did not require, we may briefly mention.

In most works on Trigonometry there is given what is called *Euler's Theorem* *about polyhedra*:—viz. that if S be the number of solid angles of a polyhedron (not self-cutting), F the number of its faces, and E the number of its edges, then

$$S+F=E+2.$$

The puzzle with us, when we were beginning mathematics, used to be "What is this mysterious 2, and how came it into the formula?" Listing shows that this is a mere case of a much more general theorem in which corners, edges, faces, *and regions of space*, have a homogeneous numerical relation. Thus the mysterious 2, in Euler's formula, belongs to the two regions of space:—the one enclosed by the polyhedron, the other (the *Amplexum*, as Listing calls it) being the rest of infinite space. The reader, who wishes to have an elementary notion of the higher forms of problems treated by Listing, is advised to investigate the modification which Euler's formula would undergo if the polyhedron were (on the whole) ring-shaped:—as, for instance, an anchor-ring, or a plane slice of a thick cylindrical tube.

LXVI.

LISTING'S *TOPOLOGIE*.

INTRODUCTORY ADDRESS TO THE EDINBURGH MATHEMATICAL SOCIETY, NOVEMBER 9, 1883.

[*Philosophical Magazine, January,* 1884.]

SOME of you may have been puzzled by the advertised title of this Address. But certainly not more puzzled than I was while seeking a title for it.

I intend to speak (necessarily from a very elementary point of view) of those space-relations which are independent of *measure*, though not always of *number*, and of which perhaps the very best instance is afforded by the various convolutions of a knot on an endless string or wire. For, once we have tied a knot, of whatever complexity, on a string and have joined the free ends of the string together, we have an arrangement which, however its apparent form may be altered (as by teazing out, tightening, twisting, or flyping of individual parts), retains, until the string is again cut, certain perfectly definite and characteristic properties altogether independent of the relative lengths of its various convolutions.

Another excellent example is supplied by Crum Brown's chemical *Graphic Formulæ*. These, of course, do not pretend to represent the actual positions of the constituents of a compound molecule, but merely their relative connection.

From this point of view all figures, however distorted by projection &c., are considered to be unchanged. We deal with grouping (as in a *quincunx*), with motion by starts (as in the chess-knight's move), with the necessary relation among numbers of intersections, of areas, and of bounding lines in a plane figure; or among the numbers of corners, edges, faces, and volumes of a complex solid figure, &c.

For this branch of science there is at present no definitely recognized title except that suggested by Listing, which I have therefore been obliged to adopt. *Geometrie der Lage* has now come, like the *Géométrie de Position* of Carnot, to mean something very different from our present subject; and the *Geometria sitûs* of Leibnitz, though intended (as Listing shows) to have specially designated it, turned out, in its inventor's hands, to be almost unconnected with it. The subject is one of very great importance, and has been recognized as such by many of the greatest investigators, including Gauss and others; but each, before and after Listing's time, has made his separate contributions to it without any attempt at establishing a connected account of it as an independent branch of science.

It is time that a distinctive and unobjectionable name were found for it; and once that is secured, there will soon be a crop of *Treatises*. What is wanted is an erudite, not necessarily a very original, mathematician. The materials already to hand are very numerous. But it is not easy (in English at all events) to find a name for it without coining some altogether new word from Latin or Greek roots. *Topology* has a perfectly definite meaning of its own, altogether unconnected with our subject. *Position*, with our mathematicians at least, has come to imply measure. *Situation* is not as yet so definitely associated with measure; for we can speak of a situation to right or left of an object without inquiring *how far off*. So that till a better term is devised, we may call our subject, in our own language, the *Science* (not the *Geometry*, for that implies measure) *of Situation*.

Listing, to whom we owe the first rapid and elementary, though highly suggestive, sketch of this science, as well as a developed investigation of one important branch of it, was in many respects a remarkable man. It is to be hoped that much may be recovered from his posthumous papers; for there can be little doubt that in consequence of his disinclination to publish (a disinclination so strong that his best-known discovery was actually published for him by another), what we know of his work is a mere fragment of the results of his long and active life.

In what follows I shall not confine my illustrations to those given by Listing, though I shall use them freely; but I shall also introduce such as have more prominently forced themselves on my own mind in connection mainly with pure physical subjects. It is nearly a quarter of a century since I ceased to be a Professor of Mathematics; and the branches of that great science which I have since cultivated are especially those which have immediate bearing on Physics. But the subject before us is so extensive that, even with this restriction, there would be ample material, in my own reading, for a whole series of strictly elementary lectures.

I ought not to omit to say, before proceeding to our business, that it is by no means creditable to British science to find that Listing's papers on this subject—the *Vorstudien zur Topologie* (*Göttinger Studien*, 1847), and *Der Census räumlicher Complexe* (Göttingen *Abhandlungen*, 1861)—have not yet been rescued from their most undeserved obscurity, and published in an English dress, especially when so much that is comparatively worthless, or at least not so worthy, has already secured these honours. I was altogether ignorant of the existence of the *Vorstudien* till it was pointed out to me by Clerk-Maxwell, after I had sent him one of my earlier papers on *Knots*; and I had to seek, in the Cambridge University Library, what was perhaps the only then accessible copy.

(1) *Down* and *Up* are at once given us by gravity. They are defined as the

direction in which a stone falls, or in which a plummet hangs, and its reverse. Even below-decks, when the vessel is lying over under a steady breeze, and we "have our sea-legs on," we instinctively keep our bodies vertical, without any thought of setting ourselves perpendicular to the cabin-floor. And this definition holds in every region of space where the earth's attraction is the paramount force. In an imaginary cavity at the earth's centre the terms would cease to have any meaning.

East, in the sense of "*Orient*," is the quarter in which the sun rises; and *this* distinction is correct all over the earth except at the poles, where it has no meaning. But if we were to define *South* as the region in which the sun is seen at midday, our definition would be always wrong if we were placed beyond the tropic of Capricorn, and at particular seasons even if we were merely beyond that of Cancer. Still there is a certain *consensus* of opinion which enables all to understand what is meant by South without the need of any formal definition.

But the distinction between *Right* and *Left* is still more difficult to define. We must employ some such artifice as "A man's right side is that which is turned eastwards, when he lies on his face with his head to the north." For, in the lapse of ages of development, one may perhaps be right in saying, with Molière's physician, "*Nous avons changé tout cela*"; and men's hearts may have migrated by degrees to the other side of their bodies, as does one of the eyes of a growing flounder. Or some hitherto unsuspected superiority of left-handed men may lead to their sole survival; and then the definition of the right hand, as that which the majority of men habitually employ most often, would be false.

I will not speak further of these things, which I have introduced merely to show how difficult it sometimes is to formulate precisely in words what every one in his senses knows perfectly well; and thus to prepare you to expect difficulties of a higher order, even in the very elements of matters not much more recondite.

(2) But there is a very simple method of turning a man's right hand into his left, and *vice versâ*, and of shifting his heart to the right-hand side, without waiting for the (problematical) results of untold ages of development or evolution. We have only to look at him with the assistance of a plane mirror or looking-glass, and these extraordinary transformations are instantly effected. Behind the looking-glass the world and every object in it are *perverted* (*verkehrt*, as Listing calls it). Seen through an astronomical telescope, everything is *inverted* merely (*umgekehrt*). Particular cases of this distinction, which is of very considerable importance, were of course known to the old geometers. For two halves of a circle are congruent; one semicircle has only to be made to rotate through two right angles *in its own plane* to be superposable on the other. But how about the halves of an isosceles triangle formed by the bisector of the angle between the equal sides? They are equal in every respect except congruency; one has to be *turned over* before it can be exactly superposed on the other.

Listing gives many examples of this distinction, of which the following is the simplest:—

Inversion:—(English) V and (Greek) Λ.
Perversion:—(English) R and (Russian) Я.
Inversion and perversion:—(English) L and (Greek) Γ.

He also gives an elaborate discussion of the different relative situations of two dice whose edges are parallel, taking account of the *points* on the various sides.

When we *flype* a glove (as in taking it off when very wet, or as we skin a hare), we perform an operation which (not describable in English by any shorter phrase than "*turning outside in*") changes its character from a right-hand glove to a left. A pair of trousers or a so-called *reversible* water-proof coat is, after this operation has been performed, still a pair of trousers or a coat, but the legs or arms are interchanged; unless the garments, like those of "Paddius à Corko," are buttoned behind[1].

(3) The germ of the whole of this part of the subject lies in the two ways in which we can choose the three rectangular axes of x, y, z; and is intimately connected with the kinematical theory of rotation of a solid.

Thus we can make the body rotate through two right angles about one axis, so that each of the other two is inverted. Such an operation does *not* change their relative situation.

But to invert one only, or all three, of the axes requires that the body should (as it were) be *pulled through itself*, a process perfectly conceivable from the kinematical, but not from the physical, point of view. By *this* process the relative situation of the axes is changed.

When we think of the rotation about the axis of x which shall bring Oy where Oz was, we see that it must be of opposite character in these two cases. And it is a mere matter of convention which of the two systems we shall choose as our normal or positive one.

That which seems of late to have become the more usual is that in which a quadrantal rotation about x (which may be any one of the three) shall change Oy into the former Oz (*i.e.* in the cyclical order x, y, z), when it is applied in the sense in which the earth turns about the *northern* end of its polar axis. Thus we may represent the three axes, in cyclical order, by a northward, an upward, and an eastward line. So that we change any one into its cyclical successor by seizing the positive end of the third, and, as it were, *unscrewing* through a quadrant[2].

The hands of a watch, looked at from the side on which the face is situated, thus move round in the *negative* direction; but if we could see *through* the watch, they would appear to move round in the *positive* direction. This universally employed construction arises probably from watch-dials having been originally made to behave as much as possible like sun-dials, on which the hours follow the apparent daily course of the sun, *i.e.* the *opposite* direction to that of the earth's rotation about its axis.

(4) This leads us into another very important elementary branch of our subject,

[1] When a Treatise comes to be written (in English) on this science, great care will have to be taken in *exactly* defining the senses in which such words as inversion, reversion, perversion, &c. are to be employed. There is much danger of confusion unless authoritative definitions be given once for all, and *not too late*.

[2] These relations, and many which follow, were illustrated by *models*, not by diagrams; and the reader who wishes fully to comprehend them will find no reason to grudge the little trouble involved in constructing such models for himself.

one in which Listing (it is to be feared) introduced complication rather than simplification, by his endeavours to extricate the botanists from the frightful chaos in which they had involved themselves by their irreconcilable descriptions of vegetable spirals. [He devotes a good many pages to showing how great was this confusion.]

When we compare the tendrils of a hop with those of a vine, we see that while they both grow upwards, as in coiling themselves round a vertical pole, the end of the hop tendril goes round *with the sun* (*secundum solem*), that of the vine tendril *against the sun* (*contra solem*).

Thus the vine tendril forms an ordinary or (as we call it) right-handed screw, the hop tendril a left-handed screw.

Now, if a point move in a circle in the plane of yz in the positive direction, and if the circle itself move in the direction of x positive, the resultant path of the point will be a vine-, or right-handed screw. But if the circle's motion as a whole, *or* the motion of the point in the circle, be reversed, we have a left-handed screw; while if *both* be reversed, it remains right-handed. Every one knows the combination of the rotatory and translatory motions involved in the use of an ordinary corkscrew; but there are comparatively few who know that a screw is *the same at either end*—that it has, in fact, what is called *dipolar symmetry*.

With a view to assist the botanists, Listing introduced a fancied resemblance between the threads of the two kinds of (double-threaded) screws and the Greek letters λ and δ, for the latter of which he also proposed the long ∫ used as a sign of integration; thus λλλλ and δδδδ, or ∫∫∫∫.

The first, which is our vine- or right-handed screw, he calls from his point of view (which is taken *in* the axis of the screw) *laeotrop*, the other *dexiotrop*. He also proposes to describe them as *lambda-* or *delta-Windungen*. But it is clear that all this "makes confusion worse confounded." Every one knows an ordinary screw. *It* is right-handed or positive. Hence he can name, at a glance, any vegetable or other helix.

(5) A symmetrical solid of revolution, an ellipsoid for instance (whether prolate or oblate), has, *if at rest*, dipolar symmetry. But if it rotate about its axis, we can at once distinguish one end of the axis from the other, and there is *dipolar asymmetry*.

This distinction is dynamical as well as kinematical, as every one knows who is conversant with gyroscopes or gyrostats.

A flat spiral spring, such as a watch- or clock-spring, or the gong of an American clock, if the inner coils be pulled out to one side, becomes a right-handed screw; if to the other, a left-handed screw. In either case it retains the dipolar symmetry which it had at first, while plane.

But when we pass an electric current round a circle of wire, we at once give it dipolar asymmetry. The current appears, from the one side, to be going round in the positive direction; from the other, in the negative. This is, in fact, the *point* of Ampère's explanation of magnetism.

A straight wire heated at one end has dipolar asymmetry, not only because of the different temperatures of its ends, but because of the differences of their electric potential (due to the "Thomson effect").

The same is generally true of every vector (or directed) quantity, such as a velocity, a force, a flux, an axis of rotation, &c.

(6) An excellent example of our science is furnished by the *Quincunx*, which is the basis of the subject of *Phyllotaxis* in botany, as well as of the arrangement of scales on a fish.

A quincunx (from the scientific point of view) is merely the system of points of intersection of two series of equidistant parallel lines in the same plane. By a simple shear parallel to one of the two series of lines, combined (if necessary) with mere uniform extensions or contractions along either or both series, any one quincunx can be changed into any other. Hence the problems connected with the elements of the subject are very simple; for it follows from the above statements that any quincunx can be reduced to square order. The botanist who studies the arrangement of buds or leaf-stalks on a stem, or of the scales on a fir-cone, seeks the *fundamental spiral*, as he calls it, that on which all the buds or scales lie. And he then fully characterizes each particular arrangement by specifying whether this spiral is a right- or left-handed screw, and what is its *divergence*. The divergence is the angle (taken as never greater than π) of rotation about the axis of the fundamental spiral from one bud or scale to the next.

(7) It is clear that if the stem or cone (supposed cylindrical) were inked and rolled on a sheet of paper, a quincunx (Plate III. fig. 1) would be traced, consisting of continuously repeated (but, of course, *perverted*) impressions of the whole surface. Hence if A, A_1, be successive prints of the *same* scale, B a scale which can be reached from A by a right-handed spiral, AB, of m steps, or by a left-handed spiral, A_1B, of n steps, these two spirals being so chosen that *all* the scales lie on n spirals parallel to AB and also on m spirals parallel to A_1B, we shall find a scale of the fundamental spiral by seeking the scale *nearest* to AA_1 within the space ABA_1.

Here continued fractions perforce come in. Let μ/ν be the last convergent to m/n. Then, if it be greater than m/n, count μ leaves or scales from A along AB, and thence ν leaves or scales parallel to BA_1, and we arrive at the required leaf or scale. If the last convergent be less than m/n, count ν leaves along A_1B, and thence μ parallel to BA. If the leaf, a, so found in either case, be nearer to A than to A_1, the fundamental spiral (as printed, *i.e. perverted*) is right-handed; and *vice versâ*. Thus the first criterion is settled.

To find the divergence, take the case of μ/ν greater than m/n; and a, so found, nearer to A than to A_1. Draw ac perpendicular to AA_1, and let the spirals through a, parallel to BA and BA_1 respectively, cut AA_1 in d and e. Then the divergence is $2\pi Ac/AA_1$. This is obviously greater than $2\pi Ad/AA_1$ (*i.e.* $2\pi\nu/n$), and less than $2\pi Ae/AA_1$ (*i.e.* $2\pi\mu/m$); and can be altered by shearing the diagram parallel to AA_1, or (what comes to the same thing) *twisting* the stem or cone. To find its exact value, draw through B a line perpendicular to AA_1 (*i.e.* parallel to the axis of the stem or cone), and let C, the first leaf or scale it meets, be reached from B by r steps along BA, followed by s steps parallel to BA_1. Then the divergence is easily seen to be $2\pi(\mu s+\nu r)/(ms+nr)$; and we have the complete description of the object, so far as our science goes.

In the figure, which is taken from an ordinary cone of *Pinus pinaster*, we have $m=5$, $n=8$; whence $\mu=2$, $\nu=3$. Also $r=3$, $s=2$; and the fundamental spiral (*perverted*) is therefore right-handed, with divergence $2\pi 13/34$.

Should m and n have a common divisor p, it is easily seen that the leaves are arranged in *whorls*; and, instead of one fundamental spiral, there is a group of p such spirals, forming a multiple-threaded screw. Each is to be treated by a process similar to that above.

(8) The last statement hints at a subject treated by Listing, which he calls *paradromic winding*. Some of his results are very curious and instructive.

Take a long narrow tape or strip of paper. Give it any number, m, of half-twists, then bend it round and paste its ends together.

If m be zero, or any other even number, the two-sided surface thus formed has two edges, which are paradromic. If the strip be now slit up midway between the edges, it will be split into two. These have each $m/2$ full twists, like the original, and (except when there is no twist, when of course the two can be separated) are $m/2$ times *linked* together.

But if m be odd, there is *but one surface* and *one edge*; so that we may draw a line on the paper from any point of the original front of the strip to any point of the back, *without crossing the edge*. Hence, when the strip is slit up midway, it remains one, but with m *full* twists, and (if $m>1$) it is *knotted*. It becomes, in fact, as its single edge was before slitting, a paradromic knot, a double clear coil with m crossings.

[This simple result of Listing's was the sole basis of an elaborate pamphlet which a few years ago had an extensive sale in Vienna; its object being to show how to perform (without the usual conjuror's or spiritualist's deception) the celebrated trick of tying a knot on an endless cord.]

The study of the one-sided autotomic surface which is generated by increasing indefinitely the breadth of the paper band, in cases where m is odd, is highly interesting and instructive. But we must get on.

(9) I may merely mention, in passing, as instances of our subject, the whole question of the *Integral Curvature* of a closed plane curve; with allied questions such as "In an assigned walk through the streets of Edinburgh, how often has one rotated *relatively to* some prominent object, such as St Giles' (supposed within the path) or Arthur's Seat (supposed external to it)?" We may vary the question by supposing that he walks so as always to turn his face to a particular object, and then inquire how often he has turned about his own axis. But here we tread on Jellinger Symonds' ground, the *non*-rotation of the moon about her axis!

But the subject of the *area* of an autotomic plane curve is interesting. It is one of Listing's examples. De Morgan, W. Thomson, and others in this country have also developed it as a supposed new subject. But its main principles (as Muir has shown in *Phil. Mag.* June, 1873) were given by Meister 113 years ago. It is now so well known that I need not dilate upon it.

(10) A curious problem, which my colleague Chrystal recently mentioned to me, appears to be capable of adaptation as a good example of our subject. It was to this effect:—

Draw the circle of least area which includes four given points in one plane.

In this form it is a question of ordinary geometry. But we may modify it as follows:—

Given three points in a plane; divide the whole surface into regions such that wherever in any one of those regions a fourth point be chosen, the rule for constructing the least circle surrounding the four shall be the same.

There are two distinct cases (with a transition case which may be referred to either), according as the given points A, B, C (suppose) form an acute- or an obtuse-angled triangle.

(α) When ABC is acute-angled (fig. 2). Draw from the ends of each side perpendiculars towards the quarter where the triangle lies, and produce each of them indefinitely from the point in which it again intersects the circumscribing circle.

The circle ABC is itself the required one, so long as D (the fourth point) lies within it.

If D lie *between* perpendiculars drawn (as above) from the ends of a side, as AB, then ABD is the required circle.

If it lie in any other region, the required circle has D for one extremity of a diameter, and the most distant of A, B, C for the other.

(β) When there is an obtuse angle, at C say (fig. 3). Make the same construction as before, but, in addition, describe the circle whose diameter is AB. All is as before, except that AB is the circle required, if D lie within it; and that if D lie within the middle portion of the larger of the two lunes formed the required circle is ABD.

[In figs. 2, 3, 4, which refer to these two cases in order, and to the intermediate case in which the triangle is right-angled at C, each region is denoted by three or by two letters. When there are three, the meaning is that the required circle passes through the corresponding points; when there are but two, these are the ends of a diameter. The separate regions are, throughout, bounded by full lines; the dotted lines merely indicate constructions.]

(11) A very celebrated question, directly connected with our subject, is to make a Knight (at chess) move to each square on the board once only till it returns to its original position. From the time of Euler onwards numerous solutions have been given. To these I need not refer further.

A much simpler question is the motion of a Rook, and to this the lately popular American "15-puzzle" is easily reduced. For *any* closed path of a rook contains an even number of squares, *since it must pass from white to black alternately.* [This furnishes a good instance of the extreme simplicity which often characterizes the solutions of questions in our subject which, at first sight, appear formidable.] And in the American puzzle every piece necessarily moves like a rook. Hence if an *even* number of interchanges of pieces will give the required result, the puzzle can be solved; if not, the arrangement is irreducible.

(12) A few weeks ago, in a railway-train, I saw the following problem proposed:—Place four sovereigns and four shillings in close *alternate* order in a line. Required, in four moves, each of *two* contiguous pieces (without altering the relative position of the two), to form a *continuous* line of four sovereigns followed by four shillings. Let sovereigns be represented by the letter B, shillings by A.

One solution is as follows:—

Before starting:—	. . A B A B A B A B
1st move	B A A B A B A . . B
2nd „	B A A B . . A A B B
3rd „	B . . B A A A A B B
4th „	B B B B A A A A . .

If we suppose the pieces to be originally arranged in circular order, with two contiguous blank spaces, the law of this process is obvious. Operate always with the *penultimate* and *antepenultimate*, the gap being looked on as the end for the time being. With this hint it is easy to generalize, so as to get the nature of the solution of the corresponding problem in any particular case, whatever be the number of coins. It is also interesting to vary the problem by making it a condition that the two coins to be moved at any instant shall first be made to change places.

(13) Another illustration, commented on by Listing, but since developed from a different point of view in a quite unexpected direction, was originated by a very simple question propounded by Clausen in the *Astronomische Nachrichten* (No. 494). In its general form it is merely the question, "What is the smallest number of pen-strokes with which a given figure, consisting of lines only, can be traced?" No line is to be gone over twice, and every time the pen has to be lifted counts one.

The obvious solution is:—Count the number of points in the figure at each of which an *odd* number of lines meet. There must always be an even number of such (zero included). Half of this number is the number of necessary separate strokes (except in the zero case, when the number of course *must* be unity). Thus the boundaries of the squares of a chess-board can be traced at 14 separate pen-strokes; the usual figure for Euclid I. 47 at 4 pen-strokes; and fig. 5 at one.

(14) But, if $2n$ points in a plane be joined by $3n$ lines, no two of which intersect, (*i.e.* so that *every* point is a terminal of 3 different lines), the figure requires n separate pen-strokes. It has been shown that in this case (unless the points be divided into two groups, between which there is but *one* connecting line, fig. 7) the $3n$ lines may be divided into 3 groups of n each, such that one of each group ends at each of the $2n$ points. See fig. 6, in which the lines are distinguished as α, β, or γ. Also note that $\alpha\beta\alpha\beta$ &c., and $\alpha\gamma\alpha\gamma$ &c. form entire cycles passing through all the trivia, while $\beta\gamma\beta\gamma$ &c. breaks up into detached subcycles.

Thus, if a Labyrinth or Maze be made, such that every intersection of roads is a *Trivium*, it may always be arranged so that the several roads meeting at each intersection may be one a grass-path, one gravel, and the other pavement. To make sure of getting out of such a Labyrinth (if it be possible), we must select two kinds of road to be taken alternately at each successive trivium. Thus we may elect to take grass, gravel, grass, gravel, &c., in which case we *must* either come to the exit point or (without reaching it) return to our starting-point, to try a new combination. For it is obvious that, if we follow our rule, we cannot possibly pass through the same trivium twice before returning to our starting-point.

(15) This leads to a very simple solution of the problem of *Map-colouring with four colours*, originally proposed by Guthrie, and since treated by Cayley, Kempe, and others.

The boundaries of the counties in a map generally meet in threes. But if four, or more, meet at certain points, let a small county be inserted surrounding each such point; and there will then be trivia of boundaries only. These various boundaries may, by our last result, be divided (usually in many different ways) into three categories, α, β, γ suppose, such that each trivium is formed by the meeting of one from each category. Now take four colours, A, B, C, D, and apply them, according to rule, as follows; so that

α	separates	A and B	or	C and D,
β	„	A and C	„	B and D,
γ	„	A and D	„	B and C,

and the thing is done. For the small counties, which were introduced for the sake of the construction, may now be made to contract without limit till the boundaries become as they were at first.

The connection between these two theorems gives an excellent illustration of the principle involved in the reduction of a biquadratic equation to a cubic.

Kempe has pointed out that four colours do not in general suffice for a map drawn upon a multiply-connected surface, such as that of a *tore* or anchor-ring. This you can easily prove for yourselves by establishing *one* simple instance. (This is an example of a case of Listing's *Census*.)

(16) From the very nature of our science, the systems of trivia, as we described them in § 14, may be regarded as mere distorted *plane projections of polyhedra which have trihedral summits only*. There are two obvious classes of exceptions, which will be at once understood from the simple figures 7 and 8. Their characteristic is that parts of the figure containing closed circuits (*i.e. faces* of the polyhedron) are connected to the rest by *one* or by *two* lines (*edges*) only. The lines are always $3n$ in number, and, excluding only the first class of exceptions, can be marked in 3 groups α, β, γ, one of each group ending at each point (*trihedral angle*).

Now in *every one* of the great variety of cases which I have tried (where the figure was, like fig. 6, a projection of a *true* polyhedron) I have found that a *complete* circuit of edges, alternately of two of these groups (such as $\alpha\beta\alpha\beta$ &c.) can be found, usually in many ways, so as to exhaust both groups and pass once through each of the angles. That is, in another form, every such polyhedron may be projected in a figure of the type shown in fig. 9, where the *dotted* lines are supposed to lie below the full lines. But, in the words of the extraordinary mathematician Kirkman, whom I consulted on the subject, "the theorem......has this provoking interest, that it mocks alike at doubt and proof[1]." Probably the proof of this curious proposition has (§ 11) hitherto escaped detection from its sheer simplicity. Habitual stargazers are apt to miss the beauties of the more humble terrestrial objects.

(17) Kirkman himself was the first to show, so long ago as 1858, that a "clear circle of edges" of a unique type passes through all the summits of a pentagonal dodecahedron. Then Hamilton pounced on the result and made it the foundation of his *Icosian Game*, and also of a new calculus of a very singular kind. See figures 9, 10, 11, which are all equivalent projections of a pentagonal dodecahedron.

At every trivium you must go either to right or to left. Denote these operations by r and l respectively. In the pentagonal dodecahedron, start where you will, either r^5 or l^5 brings you back to whence you started. Thus, in this case, r and l are to be regarded as operational symbols—each (in a sense) a *fifth* root of $+1$. In this notation Kirkman's Theorem is formulated by the expression

$$rlrlrrrlllrlrlrrrlll = 1;$$

or, as we may write it more compactly,

$$[(rl)^2r^3l^3]^2 = 1, \text{ or } [(lr)^3r^2l^2]^2 = 1.$$

It may be put in a great many apparently different, but really equivalent, forms; for, so long as the *order* of the operations is unchanged, we may begin the cycle where we please. Also we may, of course, interchange r and l throughout, in consequence of the symmetry of the figure.

It is curious to study, in such a case as this, where it can easily be done, the essential nature of the various kinds of necessarily abortive attempts to get out of such a labyrinth. Thus if we go according to such routes as $(rl)^2lrl^3$, or r^3lr^3 (sequences which do not occur in the general cycle), the next step, whatever it be, brings us to a point already passed through. We thus obtain other relations between the symbols r and l. We can make special partial circuits of this kind, including any number of operations from 7 up to 19.

All of these remarks will be obvious from any one of the three (equivalent) diagrams 9, 10, or 11.

(18) As I have already said, the subject of knots affords one of the most typical applications of our science. I had been working at it for some time, in consequence of Thomson's admirable idea of Vortex-atoms, before Clerk-Maxwell referred me to Listing's Essay; and I had made out for myself, though by methods entirely different from those of Listing, all but one of his published results. Listing's remarks on this fascinating branch of the subject are, unfortunately, very brief; and it is here especially, I hope, that we shall learn much from his posthumous papers. In the *Vorstudien* he

[1] 'Reprint of Math. Papers from the Ed. Times,' 1881, p. 113.

looks upon knots simply from the point of view of screwing or winding; and he designates the angles at a crossing of two laps of the cord by the use of his λ and δ notation (§ 4). Fig. 12 will show the nature of such crossings. Figs. 13, 14, and 15 show what he calls *reducible* and *reduced* knots. In a reducible knot the angles in some compartments at least are not all λ or all δ (the *converse* is not necessarily true). In a *reduced* knot, each compartment is all λ or all δ.

(19) My first object was to *classify* the simpler forms of knots, so as to find to what degree of complexity of knotting we should have to go to obtain a special form of knotted vortex for each of the known elements. Hence it was necessary to devise a mode of notation, by means of which any knot could be so fully described that it might, from the description alone, be distinguished from all others, and (if requisite) constructed in cord or wire.

This I obtained, in a manner equally simple and sufficient, from the theorem which follows, one which (to judge from sculptured stones, engraved arabesques, &c.) must have been at least *practically* known for very many centuries.

Any closed plane curve, which has double points only, may be looked upon as the projection of a knot *in which each portion of the cord passes alternately under and over the successive laps it meets.* [The same is easily seen to hold for any number of self-intersecting, and mutually intersecting, closed plane curves, in which cases we have in general both *linking* and *locking* in addition to knotting.]

The proof is excessively simple (§ 11). If both ends of one continuous line lie *on the same side* of a second line, there must be an even number of crossings.

(20) To apply it, go continuously round the projection of a knot (fig. 16), putting A, B, C, &c. at the *first, third, fifth,* &c. crossing you pass, until you have put letters to all. Then go round again, writing down the name of *each* crossing in the order in which you reach it. The list will consist of each letter employed, taken twice over. A, B, C, &c. will occupy, in order, the first, third, fifth, &c. places; but *the way in which these letters occur in the even places fully characterizes the drawing of the projected knot.* It may therefore be described by the order of the letters in the even places alone; and it does not seem possible that *any* briefer description could be given.

To prove that this description is complete, so far as the projection is concerned, all that is required is to show that from it we can at once construct the diagram. Thus let it be, as in fig. 16, $EFBACD$. Then the full statement is

$$AEBFCBDAECFD/A \text{ \&c.}$$

(21) To draw from such a statement, choose in it two apparitions of the same letter, *between which no other letter appears twice.* Thus $AECFD/A$ (at the end of the statement) forms such a group. It must form a loop of the curve. Draw such a loop, putting A at the point where the ends cross, and the other letters in order (either way) round the loop. Proceed to fill in the rest of the cycle in the same way. The figures thus obtained may present very different appearances; but they are all projections of the *same* definite knot. The only further information we require for its full construction is *which branch passes over the other* at each particular crossing.

This can be at once supplied by a + or − sign attached to each letter where it occurs in the statement of the order in the even places.

(22) Furnished with this process, we find that it becomes a mere question of skilled labour to draw all the possible knots having any assigned number of crossings. The requisite labour increases with extreme rapidity as the number of crossings is increased. For we must take every possible arrangement of the letters in the even places, and try whether it is compatible with the properties of a self-intersecting plane curve. Simple rules for rejecting useless or impracticable combinations are easily formed. But then we have again to go through the list of survivors, and reject all but one of each of the numerous groups of different distortions of one and the same species of knot.

I have not been able to find time to carry out this process further than the knots with *seven* crossings. But it is very remarkable that, so far as I have gone, the number of knots of each class belongs to the series of powers of 2. Thus:

Number of crossings 3, 4, 5, 6, 7,
Number of distinct forms... 1, 1, 2, 4, 8.

It is greatly to be desired that some one, with the requisite leisure, should try to extend this list, if possible up to 11, as the next prime number. The labour, great as it would be, would not bear comparison with that of the calculation of π to 600 places, and it would certainly be much more useful. [But see Nos. XL, XLI, which are of later date than this Address. 1899.]

Besides, it is probable that modern methods of analysis may enable us (by a single "happy thought" as it were) to avoid the larger part of the labour. It is in matters like this that we have the true "raison d'être" of mathematicians.

(23) There is one very curious point about knots which, so far as I know, has as yet no analogue elsewhere. In general the perversion of a knot (*i.e.* its image in a plane mirror) is non-congruent with the knot itself. Thus, as in fact Listing points out, it is impossible to change even the simple form (fig. 14) into its image (fig. 15). But I have shown that there is at least one form, for every *even* number of crossings, which is congruent with its own perversion. The unique form with four crossings gave me the first hint of this curious fact. Take one of the larger laps of fig. 17, and turn it *over* the rest of the knot, fig. 18 (which is the perversion) will be produced.

We see its nature better from the following process (one of an infinite number) for forming *Amphicheiral* knots. Knot a cord as in fig. 19, the number of complete figures of "eight" being at pleasure. Turn the figure upside down, and it is seen to be merely its own image. Hence, when the ends are joined, it forms a knot which is congruent with its own perversion.

(24) The general treatment of links is, unless the separate cords be also knotted, much simpler than that of knots—*i.e.* the measurement of *belinkedness* is far easier than that of *beknottedness.*

I believe the explanation of this curious result to lie mainly in the fact that it is possible to interweave three or more continuous cords, *so that they cannot be separated, and yet no one shall be knotted, nor any two linked together.*

This is obvious at once from the simplest possible case, shown in fig. 20. Here the three rings are not linked but *locked* together.

Now mere linkings and mere lockings are very easy to study. But the various loops of a knot may be linked *or* locked with one another. Thus the full study of a knot requires in general the consideration of linking and locking also.

(25) But it is time to close, in spite of the special interest of this part of the subject. And I have left myself barely time to mention the very interesting portion of the *Topologie* which Listing worked out in detail. You will find a brief synopsis of a part of it prefixed to Clerk-Maxwell's *Electricity and Magnetism*, and Cayley has contributed an elementary statement of its contents to the *Messenger of Mathematics* for 1873; but there can be no doubt that so important a paper as the *Census räumlicher Complexe* ought to be translated into English.

To give an exceedingly simple notion of its contents I may merely say that Listing explains and generalizes the so-called *Theorem of Euler about Polyhedra* (which all of us, whose reading dates some twenty years back or more, remember in Snowball's or Hymers' *Trigonometry*), viz. that "if S be the number of solid angles of a polyhedron, F the number of its faces, and E the number of its edges, then

$$S + F = E + 2."$$

The mysterious 2 in this formula is shown by Listing to be the number of *spaces* involved; *i.e.* the content of the polyhedron, and the *Amplexum*, the rest of infinite space.

And he establishes a perfectly general relation of the form

$$V - S + L - P = 0,$$

where V is the number of spaces, S of surfaces, L of lines, and P of points in any complex; these numbers having previously been *purged* in accordance with the amount of *Cyclosis* in the arrangement studied. But to make even the elements of this intelligible I should require to devote at least one whole lecture to them.

Meanwhile I hope I have succeeded in showing to you how very important is our subject, loose and intangible as it may have at first appeared to you; and in proving, if only by special examples, that there are profound difficulties (of a kind different altogether from those usually attacked) which are to be met with even on the very threshold of the Science of Situation.

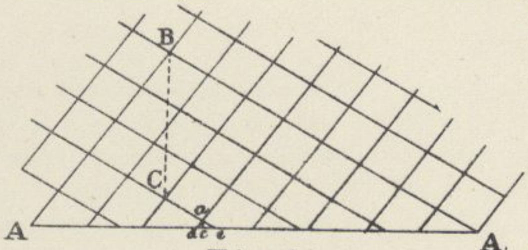


Fig. 1.

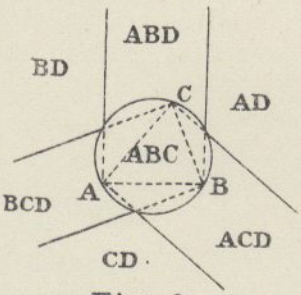


Fig. 2.

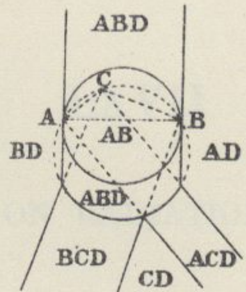


Fig. 3.

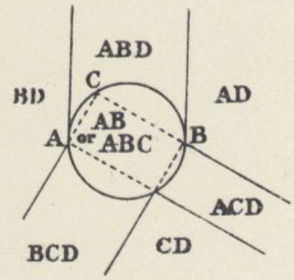


Fig. 4.

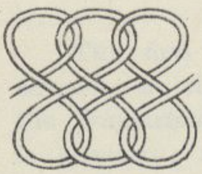


Fig. 19.

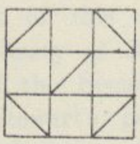


Fig. 5.

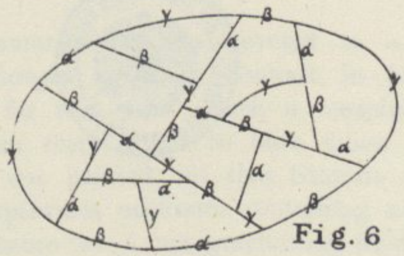


Fig. 6

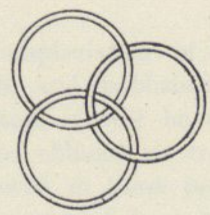


Fig. 20.

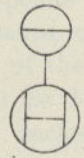


Fig. 7.

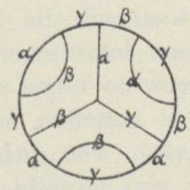


Fig. 8.

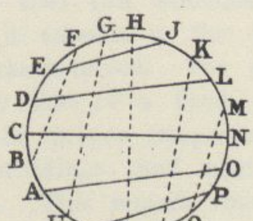


Fig. 9

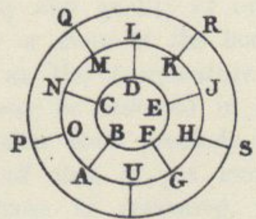


Fig. 11.

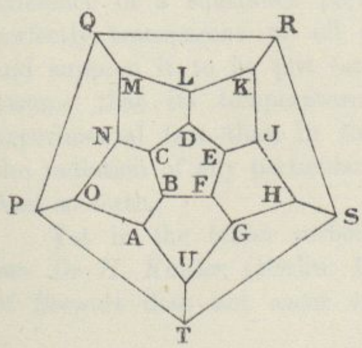


Fig. 10.

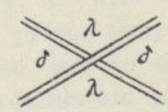


Fig. 12.


Fig. 13.

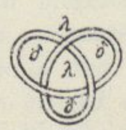


Fig. 14.


Fig. 15.

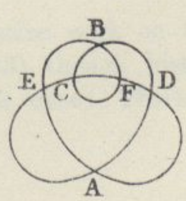


Fig. 16.


Fig. 17.


Fig. 18.

LXVII.

ON RADIATION.

[*Proceedings of the Royal Society of Edinburgh, February* 18, 1884.]

The first part of this communication was devoted to a recapitulation of the advances in the *Theory of Exchanges* made by Stewart in 1858, and published in the *Transactions* of the Society for that year. Such a recapitulation it will be seen is *necessary*; as Stewart's papers seem either to have fallen into oblivion or to be deemed unworthy of notice. It was pointed out that Stewart showed in these papers that the radiation within an impervious enclosure containing no source of heat must ultimately become, like the pressure of a non-gravitating fluid at rest, the same at all points and in all directions; but that this sameness is not, like that of fluid pressure, one of mere total amount; it extends to the quantity and quality of every one of the infinite series of wave-lengths involved. For, as one or more of the bodies may be *black*, the radiation is simply that of a black body at the temperature of the enclosure. Any new body, at the proper temperature, may be inserted in the enclosure without altering this state of things; and must *therefore* emit precisely the amount and quality which it absorbs. This remark contains *all* that is yet known on the subject. For we have only to assume for the purpose of reasoning, the existence of a substance partially, or wholly, opaque to one definite wave-length, and perfectly transparent to all others; or with any other limited properties we choose; and suppose it to be put (at the proper temperature) into the enclosure. If we next assume that its temperature when put in differs from that of the enclosure, the experimental fact that, in time, equilibrium of temperature is arrived at, shows that the radiation of any particular wave-length by a body increases with rise of temperature. And so forth.

Yet in the latest authoritative work on the subject, *Lehrbuch der Spektralanalyse, von Dr H. Kayser* (Berlin, 1883), though historical details are freely given, the name of Stewart does not occur even once! There are in the same work other instances of historical error nearly as grave. Thus the physical analogy, by which Stokes in 1852 first explained the basis of spectrum analysis, is given in Dr Kayser's work; but it is introduced by the very peculiar phrase "......wollen wir versuchen, eine *mechanische Erklärung* der Erscheinungen zu geben, welche auf unsere Anschauungen über das Leuchten begründet ist......"; and the name of Stokes is not even mentioned in connection with it!

The second part of the paper deals with the question of the limits of accuracy of the reasoning which led Stewart, and those who have followed him, to results of such vast importance. Dr Kayser, indeed, announces his intention "in aller Strenge mathematisch zu beweisen" the equality of emissive and absorptive powers. But the mere fact that phosphorescent bodies, such as luminous paint, give out visible radiations while at ordinary temperatures, shows at once that there are grave exceptions even to the fundamental statement that the utmost radiation, both as to quantity and as to quality, at any one temperature, is that of a black body:—and very simple considerations show that all the reasoning which has been applied to the subject is ultimately based on the *Second Law* of Thermodynamics (or Carnot's principle), and is therefore true only in the sense in which that law is true, *i.e.* in the statistical sense. The assumed ultimate uniformity of temperature in an enclosure, which is practically the basis of every demonstration of the extended law of exchanges, is merely an expression for the average of irregularities which are in the majority of cases too regularly spread, and on a scale too minute, to be detected by our senses, even when these are aided by the most delicate instruments. The kinetic theory of gases here furnishes us with something much closer than a mere analogy. For the very essence of what appears to us uniform temperature in a gas is the regularity of distribution of the irregularities of speed of the various particles. And, just as in every mass of gas there are a few particles moving with speed far greater than that of mean square, so it is at least probable that a black body at ordinary temperatures emits (though, of course, excessively feebly) radiations of wave-lengths corresponding to those of visible light. Effects apparently or at least conceivably due to this cause have been obtained by various experimenters.

If we could realise a dynamical system, analogous to that of a gas on the kinetic theory, but such that none of the particles could have any but one of a certain limited number of definite speeds, and if there were *still* a tendency to the nearest statistical average, we should have something capable of explaining phosphorescence at ordinary temperatures.

LXVIII.

ON AN EQUATION IN QUATERNION DIFFERENCES.

[*Proceedings of the Royal Society of Edinburgh, February* 18, 1884.]

WHEN the sides of a closed polygon are bisected, and the points of bisection joined in order, a new polygon is formed. It has the same number of sides, and the same *mean point* of its corners, as the original polygon. In what cases is it similar to the original polygon? In what cases will two, three, or more successive operations of this kind produce (for the first time) a polygon similar to the original one?

Take the mean point as origin, and let $q_1\alpha$, $q_2\alpha$, ... $q_n\alpha$ be the n corners. Here α is any vector, which, if the polygon be plane, may be taken in that plane; and q_1, ... q_n are quaternions, which in the special case just mentioned are powers of one quaternion in the same plane. We obviously have, if $Dq_r = q_{r+1}$, for the plane polygon two conditions:—the first,

$$(1 + D + D^2 + \ldots + D^{n-1})\, q_r\alpha = 0,$$

depending on our choice of origin; and the second

$$\frac{1}{2^m}(1 + D)^m q_r\alpha = QD^s q_r\alpha,$$

depending on the similarity of the mth derived polygon to the original. In this last equation, Q is a scalar multiple of an unknown power of the quaternion of which the q's are powers, expressing how the original polygon must be turned in its own plane, and how its linear dimensions must be altered, so that it may be superposed on the mth derived polygon. Also s is an unknown integer, but it has (like Q) a definite value or values when the problem admits of solution. r has *any* value from 1 to n inclusive, as may be seen at once by operating by any integral power of D, and remembering that we have necessarily

$$D^n q_r = q_r.$$

The solution of this case is easily effected, and gives the well-known results:—the general solution involving all equilateral and equiangular polygons, where m may have any integral value. Besides this, there are special solutions for the triangle, and for the quadrilateral reduced at one operation to a parallelogram. In the former of these m may have any value; in the latter (unless the figure be a square) m must be even.

But, when the polygon is gauche, the second of the above conditions becomes

$$\frac{1}{2^m}(1+D)^m q_r\alpha = QD^s q_r \alpha Q^{-1},$$

and the solution is somewhat more difficult. Its interest consists in its leading to a new and curious question in quaternions.

APPENDIX.

Theorem relating to the Sum of Selected Binomial-Theorem Coefficients.

[*Messenger of Mathematics, February,* 1884.]

Let equal masses be placed, two and two together, at the corners of an m-sided polygon. Slide one from each end of a side till they meet at its middle point. They now form a new, and smaller, m-sided polygon, but their centre of inertia has not been disturbed. Repeat the process indefinitely, and the masses will ultimately be collected in the centre of inertia.

Now if the distances of the corners of the original polygon from a fixed plane be

$$u_1, \quad u_2, \quad \ldots, \quad u_m,$$

those of the first derived polygon will be

$$\tfrac{1}{2}(u_1+u_2), \quad \tfrac{1}{2}(u_2+u_3), \quad \ldots, \quad \tfrac{1}{2}(u_m+u_1).$$

These are all included in the expression

$$\tfrac{1}{2}(1+D)\,u_r,$$

with the proviso that $$D^m u_r = u_r.$$

Similarly, the first corner of the nth derived polygon is

$$2^{-n}(1+D)^n u_1.$$

Now let N_r^m, where r is not greater than m, be the sum of the rth, $(r+m)$th, $(r+2m)$th, &c. coefficients of the binomial $(1+x)^n$; the above expression becomes

$$2^{-n}(N_1^m u_1 + N_2^m u_2 + \ldots + N_r^m u_r + \ldots + N_m^m u_m).$$

But, when n is infinite, its ultimate value is (as above)

$$\frac{1}{m}(u_1+u_2+\ldots+u_m).$$

Hence $$L_{n=\infty}(2^{-n}N_r^m) = \frac{1}{m};$$

and it seems remarkable that the limit is independent of r.

LXIX.

ON VORTEX MOTION.

[*Proceedings of the Royal Society of Edinburgh, February* 18, 1884.]

THIS paper contained a discussion of the consequences of the *assumption* of continuity of motion throughout a perfect fluid; one of the bases of von Helmholtz's grand investigation, on which W. Thomson founded his theory of vortex-atoms. It is entirely on the assumed absence of finite slip that von Helmholtz deduces the action of a rotating element on any other element of the fluid, and that Thomson calculates the action of one vortex-atom or part of such an atom on another atom, or on the remainder of itself. The creation of a single vortex-atom, in the sense in which it is defined by Thomson, involves action applied simultaneously to all parts of the fluid mass, not to the rotating portion alone.

LXX.

NOTE ON REFERENCE FRAMES.

[*Proceedings of the Royal Society of Edinburgh, July* 7, 1884.]

As I understand Prof. J. Thomson's problem (*Proc. R. S. E.* XII. p. 568) it is equivalent to the following:—

A set of points move, Galilei-wise, with reference to a system of co-ordinate axes; which may, itself, have any motion whatever. From observations of the *relative* positions of the points, merely, to find such co-ordinate axes.

It is obvious that there is an infinitely infinite number of possible solutions; because, if one origin moves Galilei-wise with respect to another, and the axes drawn from the two origins have no *relative* rotation, any point moving Galilei-wise with respect to either set of axes will necessarily move Galilei-wise with respect to the other. Hence any one solution suffices, for all the others can be deduced from it by the above consideration.

Referred to any one set of axes which satisfy the conditions, the positions of the points are, at time t, given by the vectors

$$\alpha_1 + \beta_1 t \text{ for } A, \quad \alpha_2 + \beta_2 t \text{ for } B, \quad \&c., \quad \&c.$$

But it is clear, from what is stated above, that we may look on the pair of vectors for any *one* of the points, say α_1 and β_1 for A, as being absolutely arbitrary:—though, of course, *constant*. We will, therefore, make each of them vanish. This amounts to taking A as the origin of the co-ordinate system. The other expressions, above, will then represent the relative positions of B, C, &c., with regard to A.

The observer on A is supposed to be able to measure, at any moment, the lengths AB, AC, AD, &c.; the angles BAC, BAD, CAD, &c.; and also to be able to recognise whether a triangle, such as BCD, is gone round positively or negatively when its corners are passed through in the order named. What this leaves undetermined, at any particular instant, is merely the absolute direction of *any one line* (as AB), and the aspect of *any one plane* (as ABC) passing through that line. These being assumed at random, the simultaneous positions of all the points can be constructed from the permissible observations. But it is interesting to inquire how many observations are necessary; and how the β's depend on the α's.

Thus, at time t, whatever be the mode of measurement of time, we have equations such as follow:—

$$\begin{aligned} -a &= \alpha_2^2 + 2S\alpha_2\beta_2 \,.\, t + \beta_2^2 t^2, \\ -b &= S\alpha_2\alpha_3 + S(\alpha_2\beta_3 + \beta_2\alpha_3) \,.\, t + S\beta_2\beta_3 \,.\, t^2, \\ -c &= \alpha_3^2 + 2S\alpha_3\beta_3 \,.\, t + \beta_3^2 t^2, \\ \ldots &= \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

For any one value of t we have n equations of each of the 1st and 3rd of these types, and $n(n-1)/2$ of the 2nd, $n+1$ being the whole number of points. In all, $n(n+1)/2$ equations.

The scalar unknowns involved in these equations are (1) the values of t; (2) α_2^2, α_3^2, &c.; (3) β_2^2, β_3^2, &c.; (4) $S\alpha_2\alpha_3$, &c.; (5) $S\beta_2\beta_3$, &c.; (6) $S\alpha_2\beta_2$, $S\alpha_3\beta_3$, &c.; and (7) $S(\alpha_2\beta_3 + \beta_2\alpha_3)$, &c. Their numbers are, for (2), (3), (6), n each; for (4), (5), (7), $n(n-1)/2$ each; in all $3n(n+1)2$. Suppose that observations are made on m successive occasions. Since our origin, and our unit, of time are alike arbitrary, we may put $t=0$ for the first observation, and merge the value of t at the second observation in the tensors of β_2, β_3, &c. This amounts to taking the interval between the first two sets of observations as unit of time. Thus the unknowns of the form (1) are $m-2$ in number. There are therefore

$$mn(n+1)/2 \text{ equations and } 3n(n+1)/2 + m - 2 \text{ unknowns.}$$

Thus $m=3$ gives an insufficient amount of information, but $m=4$ gives a superfluity.

In particular, if there be three points only, which is in general sufficient, 3 complete observations give

9 equations with 10 unknowns;

while 4 complete observations give

12 equations with 11 unknowns.

Thus we need take only two of the three possible measurements, at the fourth instant of observation.

The solution of the equations, supposed to be effected, gives us among other things, α_2^2, α_3^2, and $S\alpha_2\alpha_3$. *Any* direction may be assumed for α_2, and *any* plane as that of α_2 and α_3. From these assumptions, and the three numerical quantities just named, the co-ordinate system can be at once deduced.

This solution fails if $(S\alpha_2\alpha_3)^2 = \alpha_2^2\alpha_3^2$, or $TV\alpha_2\alpha_3 = 0$; for then the three points A, B, C, are in one line at starting. But this, and similar cases of failure (when they *are* really cases of failure) are due to an improper selection of three of the points. We need not further discuss them.

But it is interesting to consider how the vectors β can be found when one position of the reference frame has been obtained. Keeping, for simplicity, to the system of three points, we have by the solution of the equations above the following data:—

$$S\alpha_2\beta_2 = e, \quad S\alpha_3\beta_3 = e', \quad S(\alpha_2\beta_3 + \beta_2\alpha_3) = f, \quad T\beta_2 = g, \quad T\beta_3 = g', \quad S\beta_2\beta_3 = h;$$

where e, e', f, g, g', h are known numbers; which, as the equations from which they were derived were not linear, have in general more than one system of values. The second, third, and sixth of these equations give

$$\beta_3 S . \alpha_2\alpha_3\beta_2 = hV\alpha_2\alpha_3 + (f - S\beta_2\alpha_3)\, V\alpha_3\beta_2 + e'V\beta_2\alpha_2.$$

Provided β_2 is not coplanar with α_2, α_3, this equation gives, by the help of the fifth above, a surface of the 4th order of which β_2 is a vector. But β_2 is also a vector of the plane $S\alpha_2\beta_2 = e$, and of the sphere $T\beta_2 = g$. Hence it is determined by the intersections of those three surfaces.

But if $S . \alpha_2\alpha_3\beta_2$ vanishes, the equation above gives (by operating with $S . V\alpha_2\alpha_3$)

$$0 = h\,(V\alpha_2\alpha_3)^2 - (f - S\beta_2\alpha_3)\, S . \beta_2 V . \alpha_3 V\alpha_2\alpha_3 + e'S . \beta_2 V . \alpha_2 V\alpha_2\alpha_3,$$

which gives a surface of the second order (a hyperbolic cylinder) in place of the surface of the fourth order above mentioned. This may, however, be dispensed with:—for β_2 is in this case determined by the planes $S\alpha_2\beta_2 = e$ and $S . \alpha_2\alpha_3\beta_2 = 0$, together with the sphere $T\beta_2 = g$.

LXXI.

ON VARIOUS SUGGESTIONS AS TO THE SOURCE OF ATMOSPHERIC ELECTRICITY[1].

[*Nature, March* 27, 1884.]

WE have seen that, taking for granted the electrification of clouds, all the ordinary phenomena of a thunderstorm (except *globe* lightning) admit of easy and direct explanation by the known laws of statical electricity. Thus far we are on comparatively sure ground.

But the case is very different when we attempt to look a little farther into the matter, and to seek the source of atmospheric electricity. One cause of the difficulty is easily seen. It is the scale on which meteorological phenomena usually occur; so enormously greater than that of any possible laboratory arrangement that effects, which may pass wholly unnoticed by the most acute experimenter, may in nature rise to paramount importance. I shall content myself with one simple but striking instance.

Few people think of the immense transformations of energy which accompany an ordinary shower. But a very easy calculation leads us to startling results. To raise a single pound of water, in the form of vapour, from the sea or from moist ground, requires an amount of work equal to that of a horse for about half an hour! This is given out again, in the form of heat, by the vapour when it condenses; and the pound of water, falling as rain, would cover a square foot of ground to the depth of rather less than one-fifth of an inch. Thus a fifth of an inch of rain represents a horse-power for half an hour on every square foot, or, on a square mile, about a million horse-power for fourteen hours! A million horses would barely have standing room on a square mile. Considerations like this show that we can account for the most violent hurricanes by the energy set free by the mere condensation of vapour required for the concomitant rain.

[1] Read at the meeting of the Scottish Meteorological Society on March 17, and communicated by the Society.

Now the modern kinetic theory of gases shows that the particles of water-vapour are so small that there are somewhere about three hundred millions of millions of millions of them in a single cubic inch of saturated steam at ordinary atmospheric pressure. This corresponds to $\frac{1}{1600}$ or so of a cubic inch of water, *i.e.* to about an average raindrop. But if each of the vapour particles had been by any cause electrified to one and the same potential, and all could be made to unite, the potential of the raindrop formed from them would be fifty million million times greater.

Thus it appears that if there be any cause which would give each particle of vapour an electric potential, even if that potential were far smaller than any that can be indicated by our most delicate electrometers, the aggregation of these particles into raindrops would easily explain the charge of the most formidable thundercloud. Many years ago it occurred to me that the mere *contact* of the particles of vapour with those of air, as they interdiffuse according to the kinetic theory of gases, would suffice to produce the excessively small potential requisite. Thus the source of atmospheric electricity would be the same as that of Volta's electrification of dry metals by contact. My experiments were all made on a small scale, with ordinary laboratory apparatus. Their general object was, by various processes, to precipitate vapour from damp air, and to study either (1) the electrification produced in the body on which the vapour was precipitated; or (2) to find on which of two parallel, polished plates, oppositely electrified and artificially cooled, the more rapid deposition of moisture would take place. After many trials, some resultless, others of a more promising character, I saw that experiments on a comparatively large scale would be absolutely necessary in order that a definite answer might be obtained. I communicated my views to the Royal Society of Edinburgh in 1875, in order that some one with the requisite facilities might be induced to take up the inquiry, but I am not aware that this has been done.

I may briefly mention some of the more prominent attempts which have been made to solve this curious and important problem. Some of them are ludicrous enough, but their diversity well illustrates the nature and amount of the difficulty.

The oldest notion seems to have been that the source of atmospheric electricity is aërial friction. Unfortunately for this theory, it is *not* usually in windy weather that the greatest development of electricity takes place.

In the earlier years of this century Pouillet claimed to have established by experiment that in all cases of combustion or oxidation, in the growth of plants, and in evaporation of *salt* water, electricity was invariably developed. But more recent experiments have thrown doubt on the first two conclusions, and have shown that the third is true only when the salt water is boiling, and that the electricity then produced is due to friction, not to evaporation. Thus Faraday traced the action of Armstrong's hydro-electric machine to friction of the steam against the orifice by which it escaped.

Saussure and others attributed the production of atmospheric electricity to the condensation of vapour, the reverse of one of Pouillet's hypotheses. This, however, is a much less plausible guess than that of Pouillet; for we could understand a particle of vapour carrying positive electricity with it, and leaving an equal charge of negative electricity in the water from which it escaped. But to account for the separation of the two electricities when two particles of vapour unite is a much less promising task.

Peltier (followed by Lamont) assumed that the earth itself has a permanent charge of negative electricity whose distribution varies from time to time, and from place to place. Air, according to this hypothesis, can neither hold nor conduct electricity, but a cloud can do both; and the cloud is electrified by conduction if it touch the earth, by induction if it do not. But here the difficulty is only thrown back one step. How are we to account for the earth's permanent charge?

Sir W. Thomson starts from the experimental fact that the layer of air near the ground is often found to be strongly electrified, and accounts for atmospheric electricity by the carrying up of this layer by convection currents. But this process also only shifts the difficulty.

A wild theory has in recent times been proposed by Becquerel. Corpuscles of some kind, electrified by the outbursts of glowing hydrogen, travel from the sun to the upper strata of the earth's atmosphere.

Mühry traces the source of electricity to a direct effect of solar radiation falling on the earth's surface.

Lüddens has recently attributed it to the friction of aqueous vapour against dry air. Some still more recent assumptions attribute it to capillary surface-tension of water, to the production of hail, &c.

Blake, Kalischer, &c., have lately endeavoured to show by experiment that it is not due to evaporation, or to condensation of water. Their experiments, however, have all been made on too small a scale to insure certain results. What I have just said about the extraordinary number of vapour particles in a single raindrop, shows that the whole charge in a few cubic feet of moist air may altogether escape detection.

And so the matter will probably stand, until means are found of making these delicate experiments in the only way in which success is likely to be obtained, viz. on a scale far larger than is at the command of any ordinary private purse. It is a question of real importance, not only for pure science but for the people, and ought to be thoroughly sifted by means which only a wealthy nation can provide.

LXXII.

NOTE ON A SINGULAR PASSAGE IN THE *PRINCIPIA*.

[*Proceedings of the Royal Society of Edinburgh, January* 19, 1885.]

In the remarkable *Scholium*, appended to his chapter on the Laws of Motion, where Newton is showing what Wren, Wallis, and Huygens had done in connection with the impact of bodies, he uses the following very peculiar language:—

"Sed et veritas comprobata est a *D. Wrenno* coram *Regiâ Societate* per experimentum Pendulorum, quod etiam *Clarissimus Mariottus* Libro integro exponere mox dignatus est."

The last clause of this sentence, which I had occasion to consult a few days ago, appeared to me to be so sarcastic, and so unlike in tone to all the context, that I was anxious to discover its full intention.

Not one of the Commentators, to whose works I had access, makes any remark on the passage. The Translators differ widely.

Thus Motte softens the clause down into the trivial remark "which Mr Mariotte soon after thought fit to explain in a treatise entirely on that subject."

The Marquise du Chastellet (1756) renders it thus:—

"......mais ce fut *Wrenn* qui les confirma par des Expériences faites avec des Pendules devant la Société Royale: lesquelles le célébre *Mariotte* a rapportées depuis dans un Traité qu'il a composé exprès sur cette matière."

Thorp's translation (1777) runs:—

"which the very eminent Mr Mariotte soon after thought fit to explain in a treatise entirely upon that subject."

Finally, Wolfers (1872) renders it thus:—

"der zweite zeigte der Societät die Richtigkeit seiner Erfindung an einem Pendelversuche, den der berühmte Mariotte in seinem eigenen Werke aus einander zu setzen, für würdig erachtete."

Not one of these seems to have remarked anything singular in the language employed. But when we consult the "entire book" in which Mariotte is said by Newton to have "expounded" the result of Wren, and which is entitled *Traité de la*

Percussion ou Choc des Corps, we find that the name of Wren is not once mentioned in its pages! From the beginning to the end there is nothing calculated even to hint to the reader that the treatise is not wholly original.

This gives a clue to the reason for Newton's sarcastic language; whose intensity is heightened by the contrast between the *Clarissimus* which is carefully prefixed to the name of Mariotte, and the simple *D.* prefixed, not only to the names of Englishmen like Wren and Wallis, but even to that of a specially distinguished foreigner like Huygens.

Newton must, of course, like all the scientific men of the time (Mariotte included), have been fully cognizant of Boyle's celebrated controversy with Linus, which led to the publication, in 1662, of the *Defence of the Doctrine touching the Spring and Weight of the Air*. In that tract, Part II. Chap. 5, the result called in Britain *Boyle's Law* is established (by a very remarkable series of experiments) for pressures less than, as well as for pressures greater than, an atmosphere; and it is established by means of the very form of apparatus still employed for the purpose in lecture demonstrations. Boyle, at least, claimed originality, for he says in connection with the difficulties met with in the breaking of his glass tube:—

"......an accurate Experiment of this nature would be of great importance to the Doctrine of the Spring of the Air, and has not yet been made (that I know) by any man......."

In Mariotte's *Discours de la Nature de l'Air*, published FOURTEEN years later than this work of Boyle, we find no mention whatever of Boyle, though the identical form of apparatus used by Boyle is described. The whole work proceeds, as does that on *Percussion*, with a calm ignoration of the labours of the majority of contemporary philosophers.

This also must, of course, have been perfectly well known to Newton:—and we can now see full reason for the markedly peculiar language which he permits himself to employ with reference to Mariotte.

What was thought of this matter by a very distinguished foreign contemporary, appears from the treatise of James Bernoulli, *De Gravitate Ætheris*, Amsterdam, 1683, p. 92.

"Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis ab Illustr. Dn. Boylio hanc in rem factis, quæ videsis in *Tractatu ejus contrà Linum, Cap. V.*, cui duas Auctor subjunxit Tabulas pro diversis Condensationis et Rarefactionis gradibus."

In order to satisfy myself that Newton's language, taken in its obvious meaning, really has the intention which I could not avoid attaching to it, I requested my colleague Prof. Butcher to state the impression which it produced on him. I copied for him the passage above quoted, putting *A* for the word *Wrenno*, and *B* for *Mariottus*; and I expressly avoided stating who was the writer. Here is his reply:—

"I imagine the point of the passage to be something of this kind (speaking without farther context or acquaintance with the Latinity of the learned author):—

"*A* established the truth by means of a (simple) experiment, before the Royal Society; later, *B* thought it worth his while to write a whole book to prove the same point.

"I should take the tone to be highly sarcastic at *B*'s expense. It *seems* to suggest that *B* was not only clumsy but dishonest. The latter inference is not certain, but at any rate we have a *hint* that *B* took no notice of *A*'s discovery, and spent a deal of useless labour."

This conclusion, it will be seen, agrees exactly with the complete ignoration of Wren by Mariotte.

When I afterwards referred Prof. Butcher to the whole context, in my copy of the first edition of the *Principia*, and asked him whether the use of *Clarissimus* was sarcastic or not, he wrote—

"I certainly think so. Indeed, even apart from the context, I thought the *Clarissimus* was ironical, but there can be no doubt of it when it corresponds to *D.* Wren."

In explanation of this I must mention that, when I first sent the passage to Prof. Butcher, I had copied it from Horsley's sumptuous edition; in which the *D*'s are omitted, while the *Clarissimus* is retained.

Alike in France and in Germany, to this day, the Law in question goes by the name of Mariotte. The following extracts, from two of the most recent high-class text-books, have now a peculiar interest. I have put a word or two of each in Italics. These should be compared with the dates given.

"Diese Frage ist schon frühzeitig untersucht und zwar *fast gleichzeitig* von dem französischen Physiker Mariotte (1679) und dem englischen Physiker Boyle (1662)." Wüllner, *Lehrbuch der Experimentalphysik*, 1882, § 98.

"La loi qui régit la compressibilité des gaz à température constante a été trouvée *presque simultanément* par Boyle (1662) en Angleterre et par Mariotte (1676) en France; toutefois, si Boyle a publié le premier ses expériences, il ne sut pas en tirer l'énoncé clair que donna le physicien français. C'est donc avec quelque raison que le nom de loi de Mariotte a passé dans l'usage." Violle, *Cours de Physique*, 1884, § 283.

On this I need make no remark further than quoting one sentence from Boyle, where he compares the actual pressure, employed in producing a certain compression in air, with "what the pressure should be according to the *Hypothesis*, that supposes the pressures and expansions to be in reciprocal proportion." M. Violle has probably been misled by the archaic use of "expansion" for volume.

It must be said, in justice to Mariotte, that he does not appear to have *claimed* the discovery of any new facts in connection either with collision or with the effect of pressure on air. He rather appears to write with the conscious infallibility of a man for whom nature has no secrets. And he transcribes, or adapts, into his writings (without any attempt at acknowledgment) whatever suits him in those of other people. He seems to have been a splendidly successful and very early example of the highest class of what we now call the *Paper-Scientists.* Witness the following extracts from Boyle, with a parallel citation from Mariotte of *fourteen* years' later date *at least.* The comparison of the sponges had struck me so much, in Mariotte's work, that I was induced to search for it in Boyle, where I felt convinced that I should find it.

"This Notion may perhaps be somewhat further explain'd, by conceiveing the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which, may indeed, like a little Spring, be easily bent or rouled up; but will also, like a Spring, be still endeavouring to stretch itself out again. For though both these Haires, and the Æreal Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by virtue of its structure) is endow'd with a Power or Principle of Selfe-Dilatation; by virtue whereof, though the hairs may by a Mans hand be bent and crouded closer together, and into a narrower room then suits best with the Nature of the Body, yet, whils't the compression lasts, there is in the fleece they composeth an endeavour outwards, whereby it continually thrusts against the hand that opposeth its Expansion. And upon the removall of the external pressure, by opening the hand more or less, the compressed Wooll doth, as it were, spontaneously expand or display it self towards the recovery of its former more loose and free condition till the Fleece hath either regain'd its former Dimensions, or at least, approached them as neare as the compressing hand, (perchance not quite open'd) will permit. The power of Selfe-Dilatation is somewhat more conspicuous in a dry Spunge compress'd, then in a Fleece of Wooll. But yet we rather chose to employ the latter, on this occasion, because it is not like a Spunge, an intire Body; but a number of slender and flexible Bodies, loosely complicated, as the Air itself seems to be."

And, a few pages later, he adds:—

"......a Column of Air, of many miles in height, leaning upon some springy Corpuscles of Air here below, may have weight enough to bend their little springs, and keep them bent: As, (to resume our former comparison,) if there were fleeces of Wooll pil'd up to a mountainous height, upon one another, the hairs that compose the lowermost Locks which support the rest, would, by the weight of all the Wool above them, be as well strongly compress'd as if a Man should squeeze them together in his hands, or imploy any such other moderate force to compress them. So that we need not wonder, that upon the taking off the incumbent Air from any parcel of the Atmosphere here below, the Corpuscles, whereof that undermost Air consists, should display themselves, and take up more room than before."

Mariotte (p. 151). "On peut comprendre à peu près cette différence de condensation de l'Air, par l'exemple de plusieurs éponges qu'on auroit entassées les unes sur les autres. Car il est évident, que celles qui seroient tout au haut, auroient leur entenduë naturelle: que celles qui seroient immédiatement au dessous, seroient un peu moins dilatées; et que celles qui seroient au dessous de toutes les autres, seroient très-serrées et condensées. Il est encore manifeste, que si on ôtoit toutes celles du dessus, celles du dessous reprendroient leur étenduë naturelle par la vertu de ressort qu'elles ont, et que si on en ôtoit seulement une partie, elles ne reprendroient qu'une partie de leur dilatation."

Those curious in such antiquarian details will probably find a rich reward by making a careful comparison of these two works; and in tracing the connection between the *Liber integer*, and its fons et origo, the paper of Sir Christopher Wren.

Condorcet, in his *Éloge de Mariotte*, says:—"Les lois du choc des corps avaient été trouvées par une métaphysique et par une application d'analyse, nouvelles l'une et l'autre, et si subtiles, que les démonstrations de ces lois ne pouvaient satisfaire que les grands mathématiciens. Mariotte chercha à les rendre, pour ainsi dire, populaires, en les appuyant sur des expériences, &c." *i.e.*, *precisely* what Wren had thoroughly done before him.

"Le discours de Mariotte sur la nature de l'air renferme encore une suite d'expériences intéressantes, et qui étaient absolument neuves." This, as we have seen, is entirely incorrect.

But Condorcet shows an easy way out of all questions of this kind, however delicate, in the words:—"On ne doit aux morts que ce qui peut être utile aux vivants, la vérité et la justice. Cependant, lorsqu'il reste encore des amis et des enfants que la vérité peut affliger, les égards deviennent un devoir; mais au bout d'un siècle, la vanité peut seule être blessée de la justice rendue aux morts."

Thus it is seen that even the turn of one of Newton's phrases serves, when rightly viewed, to dissipate a widespread delusion:—and that while Boyle, though perhaps he can scarcely be said to have been "born great," certainly "achieved greatness"; the assumed parent of *La Loi de Mariotte* (otherwise *Mariotte'sches Gesetz*) has as certainly had "greatness thrust upon" him.

LXXIII.

NOTE ON A PLANE STRAIN.

[*Proceedings of the Edinburgh Mathematical Society, February* 13, 1885. Vol. III.]

THE object of this note is to point out, by a few remarks on a single case, how well worth the attention of younger mathematicians is the *full* study of certain problems, suggested by physics, but limited (so far as that science is concerned) by properties of matter.

In de St Venant's beautiful investigations of the flexure of prisms, there occurs a plane strain involving the displacements

$$\xi = \frac{xy}{D}, \quad \eta = \frac{y^2 - x^2}{2D}.$$

Physically, this is applicable to de St Venant's problem only when x and y are each small compared with D. But it is interesting to consider the results of extending it to all values of the coordinates. This I shall do, but very briefly.

1. The altered coordinates of any point are given, in terms of the original coordinates, by

$$x' = x\left(1 + \frac{y}{D}\right), \quad y' = y + \frac{y^2 - x^2}{2D}.$$

Hence
$$\delta x' = \delta x\left(1 + \frac{y}{D}\right) + \delta y\,\frac{x}{D},$$

$$\delta y' = -\,\delta x\,\frac{x}{D} + \delta y\left(1 + \frac{y}{D}\right).$$

From these we see at once that, so far as an indefinitely small area is concerned, the strain is a mere extension in all directions in the ratio

$$\sqrt{\left(1+\frac{y}{D}\right)^2+\frac{x^2}{D^2}} : 1,$$

combined with a rotation through an angle whose tangent is

$$-\frac{x}{D+y}.$$

2. Hence elementary squares remain squares; and any two series of lines, dividing the plane into little squares, will continue to do so after the strain.

One simple case is furnished by sets of lines parallel to the axes. Thus $y = b$ becomes the parabola

$$x'^2 = -\frac{2(D+b)^2}{D}\left(y' - b - \frac{b^2}{2D}\right) \quad\text{.................................(1)},$$

and $x = a$ becomes a parabola

$$x'^2 = \frac{2a^2}{D}\left(y' + \frac{a^2+D^2}{2D}\right) \quad\text{.................................(2)}.$$

These groups of parabolas, (1) and (2), must evidently be orthogonal, and if the simultaneous small increments of a and b be *equal*, must divide the plane into little squares. But, as it is clear from (2) that the sign of a is immaterial, the two lines

$$x = a, \quad x = -a$$

are *both* deformed into the same parabola. Hence it appears that every part of the area becomes *duplex*. This will be examined by another and more suitable method later.

Having thus obtained another set of lines which divide the plane into squares, we may begin again with it and obtain a third set, &c.

3. A line, $y = mx$, passing through the origin, becomes the parabola

$$\left(\frac{m^2-1}{2}x' - my'\right)^2 = D\frac{m^2+1}{2}(mx' - y').$$

The orthogonal trajectories of all such parabolas are the curves into which the circles

$$x^2 + y^2 = c^2$$

are deformed. Their equation may be put in the form

$$\sqrt{x'^2 + y''^2} - \frac{c}{2} = \pm\sqrt{\frac{c^2 y''}{D} + \frac{c^2}{4}},$$

where y'' is written instead of $y' + \dfrac{c^2}{2D}$.

These curves have the property that, at every point, *the sum* (*or difference*) *of the distance from a given point, and of a multiple of the square root of the distance from a given line, is constant.*

4. But, if we express the new rectangular coordinates of a point in terms of its original polar coordinates, we have

$$x' = r\cos\theta + \frac{r^2}{2D}\cos\left(2\theta - \frac{\pi}{2}\right),$$

$$y' = r\sin\theta + \frac{r^2}{2D}\sin\left(2\theta - \frac{\pi}{2}\right).$$

Thus the deformed circles, above spoken of, are seen to be *epicycloids of the cardioid series.* Their orthogonal trajectories are the parabolas just mentioned.

5. Another curious set of questions is, as it were, the reverse of these:—*i.e.*, what were the curves, in the unstrained plate, which became the system

$$x = a, \quad y = b,$$

or the other (also orthogonal) system

$$y = mx, \quad x^2 + y^2 = c^2\,?$$

6. But a different transformation is still more explicit in the information it gives. Shift the origin to $(0, -D)$, and we have

$$x' = \frac{xy}{D}, \quad y' = \frac{y^2 - x^2 + D^2}{2D}.$$

If we put $x = \rho\sin\phi$, $y = \rho\cos\phi$, these give

$$x' = \frac{\rho^2}{2D}\sin 2\phi, \quad y' - \frac{D}{2} = \frac{\rho^2}{2D}\cos 2\phi.$$

Hence a circle, of radius ρ, surrounding the new origin, becomes a circle of radius $\frac{\rho^2}{2D}$ surrounding the point $\left(0, -\frac{D}{2}\right)$ half-way between the new and old origins. The ϕ of any point in the circle becomes 2ϕ.

Hence the whole surface is opened up like a fan round the new origin, *every* radius through this origin having its inclination to the axis of y *doubled.* Thus the parts of a diameter, on opposite sides of the centre, are brought to coincide; and an infinitely extended line, through the centre, becomes limited at the centre. Thus what was a single sheet becomes duplex, as was said above.

7. It suffices to have indicated, by a partial examination of some of the curious features of a single case, the stores of novelties which are thus easily reached. See especially, for additional materials of the same kind, the investigation in §§ 706–7 of Thomson and Tait's *Natural Philosophy.*

LXXIV.

SUMMATION OF CERTAIN SERIES.

[*Proceedings of the Edinburgh Mathematical Society, June* 12, 1885. Vol. III.]

[*Abstract*[1].]

THE attempt to enumerate the possible distinct forms of knots of any order, though unsuccessful as yet, has led me to a number of curious results, some of which may perhaps be new. The general character of the methods employed will be obvious from an inspection of a few simple cases, and any one who has some practice in algebra may extend the results indefinitely.

Take, for instance, the series

$$r^m - n(r+s)^m + \frac{n.\overline{n-1}}{1.2}(r+2s)^m - \&c.$$

where the coefficients are the terms of $(1-1)^n$, and the other factors are the mth powers of the terms of an arithmetical series:—m being a positive integer. The well-known properties of exponential series give us an easy method of summing all expressions of this form. For we have

$$(\epsilon^{px} - \epsilon^{qx})^n = \epsilon^{npx} - n\epsilon^{(\overline{n-1}p+q)x} + \frac{n.\overline{n-1}}{1.2}\epsilon^{(\overline{n-2}p+2q)x} - \&c.$$

which may be written in the form

$$\left((p-q)x + \frac{p^2-q^2}{2!}x^2 + \frac{p^3-q^3}{3!}x^3 + \&c.\right)^n$$

$$= \Sigma\frac{1}{m!}\left(\overline{np}^m - n(np+q-p)^m + \frac{n.\overline{n-1}}{1.2}(np+2\overline{q-p})^m - \&c.\right)x^m.$$

[1] This abstract is part of the paper read in June, entitled "On the detection of amphicheiral knots, with special reference to the mathematical processes involved." I have unfortunately mislaid the MS.—P. G. T.

Make $np = r$, $q - p = s$; and p and q are known.

The required sum is then the coefficient of x^m in the expansion of

$$m!\left((p-q)x + \frac{p^2-q^2}{2!}x^2 + \dots\right)^n.$$

It vanishes therefore, so long as $m < n$; and for $m = n$ its value is

$$m!\,(p-q)^m = (-)^m\, m!\, s^m.$$

When the coefficients in the given series are the *alternate* terms of $(1-1)^n$, we have only to treat, as above, the expression

$$(\epsilon^{px} + \epsilon^{qx})^n \pm (\epsilon^{px} - \epsilon^{qx})^n.$$

Such results may be varied *ad libitum*, by introducing two or more quantities in place of x, and comparing coefficients of like terms:—*e.g.*, as in finding, by the two methods of expansion, the term in $x^r y^s$ of the quantity

$$(\epsilon^{px} - \epsilon^{qy})^n.$$

But it suffices to have called attention to processes which can give endless varieties of results, some of which may have useful applications.

LXXV.

ON CERTAIN INTEGRALS.

[*Proceedings of the Edinburgh Mathematical Society, December* 11, 1885. Vol. IV.]

THIS paper was based mainly on the results of an investigation which will appear in full in the *Transactions* of the Royal Society of Edinburgh. Incidentally, however, it led to a discussion of the question:—*Find the law of density of a planet's atmosphere, supposing Boyle's law to be true for all pressures, and the temperature to be uniform throughout.*

Boyle's law gives $p = k\rho$, where ρ is the density at distance r from the planet's centre.

The Hydrostatic condition is $\frac{dp}{dr} = -\rho R$, where R is the attraction on unit of mass.

Hence $k\frac{d\rho}{dr} = -\rho\frac{M + \int_{r_0}^{r} 4\pi r^2 \rho\, dr}{r^2}$, where r_0 is the radius, and M the mass of the planet.

Write this as

$$\frac{kr^2}{\rho}\frac{d\rho}{dr} = -M - \int_{r_0}^{r} 4\pi r^2 \rho\, dr$$

and differentiate; and we obtain the curious equation

$$\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{d\rho}{dr}\right) = -\frac{4\pi}{k} r^2 \rho \qquad \text{.........(1).}$$

A *special* value of ρ (compatible with the absence of a solid nucleus) is

$$\rho = + \frac{k}{2\pi r^2},$$

but this cannot be generalised.

The finding of the integral of (1) in a form convergent for all values of r greater than r_0 presents novel and grave difficulties; but it is clear from the physical question on which the whole is based that such a solution exists.

If we change the independent variable to s, where $rs = 1$, (1) becomes

$$\frac{d^2 \log \rho}{ds^2} = -\frac{4\pi}{k}\frac{\rho}{s^4};$$

or, if $\log \rho = u$, $\frac{4\pi}{k} = e$,

$$\frac{d^2 u}{ds^2} = -\frac{e}{s^4}\epsilon^u.$$

This seems to be the simplest form into which the equation can be transformed.

[See a paper by Sir W. Thomson, "*On the Equilibrium of a Gas under its own Gravity only.*" *Proc. R. S. E.* Feb. 21, 1887; or *Phil. Mag.* 1887, I., 287. 1899.]

LXXVI.

HOOKE'S ANTICIPATION OF THE KINETIC THEORY, AND OF SYNCHRONISM.

[*Proceedings of the Royal Society of Edinburgh, March* 16, 1885.]

WHILE collecting materials for a Text-book of the *Properties of Matter*, the author had occasion to consult the very curious pamphlet by Robert Hooke, entitled *Lectures de Potentia Restitutiva, or of Spring* (London, 1678).

In this work there is a clear statement of the principle of Synchronism, which was applied by Stokes to the explanation of the basis of Spectrum Analysis. There is also a very remarkable statement of the elementary principles of the modern Kinetic Theory of Gases, the first mention of which is usually fixed sixty years later, and ascribed to D. Bernoulli in his *Hydrodynamica* (Argentorati, 1738).

[Here is the chief passage referred to:—

"In the next place for fluid bodies, amongst which the greatest instance we have is air, though the same be in some proportion in all other fluid bodies.

"The Air then is a body consisting of particles so small as to be almost equal to the particles of the Heterogeneous fluid medium incompassing the earth. It is bounded but on one side, namely, towards the earth, and is indefinitely extended upward, being only hindred from flying away that way by its own gravity, (the cause of which I shall some other time explain.) It consists of the same particles single and separated, of which water and other fluids do, conjoyned and compounded, and being made of particles exceeding small, its motion (to make its ballance with the rest of the earthy bodies) is exceeding swift, and its Vibrative Spaces exceeding large, comparative to the Vibrative Spaces of other terrestrial bodies. I suppose that of the Air next the Earth in its natural state may be 8000 times greater than that of Steel, and above a thousand times greater than that of common water, and proportionably I suppose that its motion must be eight thousand times swifter than the former, and above a thousand times swifter than the latter. If therefore a quantity of this body be inclosed by a solid body, and that be so contrived as to compress it into less room, the motion thereof (supposing the heat the same) will continue the same, and consequently the Vibrations and Occursions will be increased in reciprocal proportion, that is, if it be Condensed into half the space the Vibrations and Occursions will be double in number: If into a quarter the Vibrations and Occursions will be quadruple, *&c.*

"Again, If the conteining Vessel be so contrived as to leave it more space, the length of the Vibrations will be proportionably inlarged, and the number of Vibrations and Occursions will be reciprocally diminished, that is, if it be suffered to extend to twice its former dimensions, its Vibrations will be twice as long, and the number of its Vibrations and Occursions will be fewer by half, and consequently its indeavours outward will be also weaker by half.

"These Explanations will serve *mutatis mutandis* for explaining the Spring of any other Body whatsoever." 1898.]

LXXVII.

ON THE FOUNDATIONS OF THE KINETIC THEORY OF GASES.

[*Transactions of the Royal Society of Edinburgh, May* 14, 1886, Vol. XXXIII.]

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THE attempt to account for the behaviour of gases by attributing their apparently continuous pressure to exceedingly numerous, but nearly infinitesimal, impacts on the containing vessel is probably very old. It certainly occurs, with some little development, in Hooke's tract of 1678, *Lectures de potentiâ restitutivâ, or of Spring;* and, somewhat more fully developed, in the *Hydrodynamica* of D. Bernoulli, 1738. Traces of it are to be found in the writings of Le Sage and Prévost some 80 or 90 years ago. It was recalled to notice in 1847 by Herapath in his *Mathematical Physics,* and applied, in 1848, by Joule to the calculation of the average speed of the particles in a mass of hydrogen at various temperatures. Joule expressly states* that his results are independent of the *number* of the particles, and of their directions of motion, as also of their mutual collisions.

* The paper is reprinted *Phil. Mag.* 1857, II. See especially p. 215.

In and after 1857 Clausius greatly improved the treatment of the problem by taking account not only of the mutual impacts of the particles but also of the rotations and internal vibrations which they communicate to one another, with the bearing of this on the values of the specific heats; at the same time introducing (though only to a limited extent) the statistical method. In this series of papers we find the first hint of the length of the mean free path of a particle, and the explanation of the comparative slowness of the process of diffusion of one gas into another. But throughout it is assumed, so far as the calculations are concerned, that the particles of a gas are all moving with equal speeds. Of the Virial, which Clausius introduced in 1870, we shall have to speak later.

In the *Philosophical Magazine* for 1860 Clerk-Maxwell published his papers on the "Collisions of Elastic Spheres," which had been read to the British Association in the previous year. In this very remarkable investigation we have the first attempts at a numerical determination of the length of the mean free path. These are founded on the observed rate of diffusion of gases into one another; and on the viscosity of gases, which here first received a physical explanation. The statistical method is allowed free play, and consequently the law of distribution of speed among the impinging particles is investigated, whether these be all of one kind or a mixture of two or more kinds. One of his propositions (that relating to the ultimate partition of energy among two groups of colliding spheres), which is certainly fundamental, is proved in a manner open to very grave objections:—not only on account of the singular and unexpected ease with which the proof is arrived at, but also on account of the extraordinary rapidity with which (it seems to show) any forced deviation from its conclusions will be repaired by the natural operation of the collisions, especially if the mass of a particle be nearly the same in each system. As this proposition, in the extended form given to it by Boltzmann and others, seemed to render the kinetic theory incapable of explaining certain well-known experimental facts, I was induced to devote some time to a careful examination of Maxwell's proof (mainly because it appears to me to be the only one which does not seem to evade rather than boldly encounter the real difficulties of the question*), with the view of improving it, or of disproving the theorem, as the case might be. Hence the present investigation, which has incidentally branched off into a study of other but closely connected questions. The variety of the traps and pit-falls which are met with even in the elements of this subject, into some of which I have occasionally fallen, and into which I think others also have fallen, is so great that I have purposely gone into very minute detail in order that no step taken, however slight, might have the chance of escaping criticism, or might have the appearance of an attempt to gloss over a real difficulty.

* Compare another investigation, also by Clerk-Maxwell but based on Boltzmann's processes, which is given in *Nature*, VIII. 537 (Oct. 23, 1873). Some remarks on this will be made at the end of the paper. Meanwhile it is sufficient to point out that this, like the (less elaborate) investigations of Meyer and Watson, merely attempts to show that a certain state, once attained, is permanent. It gives no indication of the rate at which it would be restored if disturbed. As will be seen later, I think that this "rate" is an element of very great importance on account of the reasons for confidence (in the general results of the investigation) which it so strikingly furnishes.

The greater part of the following investigation is concerned only with the most elementary parts of the kinetic theory of gases, where the particles are regarded as hard smooth spheres whose coefficient of restitution is unity. The influence of external forces, such as gravity, is neglected; and so is that of internal (molecular) forces. The number of spheres is regarded as extremely great (say of the order 10^{20} per cubic inch): but the sum of their volumes is regarded as very small in comparison with the space through which they are free to move; as, for instance, of the order 10^{-3} or 10^{-4}. It will be seen that several of the fundamental assumptions, on which the whole investigation rests, are justified only by reference to numbers of such enormous magnitude, or such extreme minuteness, as the case may be. The walls of the containing vessel are supposed simply to *reverse* the normal velocity of every sphere impinging on them.

I. *One set of Equal Spheres.*

1. Very slight consideration is required to convince us that, unless we suppose the spheres to collide with one another, it would be impossible to apply any species of finite reasoning to the ascertaining of their distribution at each instant, or the distribution of velocity among those of them which are for the time in any particular region of the containing vessel. But, when the idea of mutual collisions is introduced, we have at once, in place of the hopelessly complex question of the behaviour of innumerable absolutely isolated individuals, the comparatively simple statistical question of the average behaviour of the various groups of a community. This distinction is forcibly impressed even on the non-mathematical, by the extraordinary steadiness with which the numbers of such totally unpredictable, though not uncommon, phenomena as suicides, twin or triple births, dead letters, &c., in any populous country, are maintained year after year.

On those who are acquainted with the higher developments of the mathematical *Theory of Probabilities* the impression is still more forcible. Every one, therefore, who considers the subject from either of these points of view, must come to the conclusion that continuous collisions among our set of elastic spheres will, *provided they are all equal*, produce a state of things in which the percentage of the whole which have, at each moment, any distinctive property must (after *many* collisions) tend towards a definite numerical value; from which it will never afterwards markedly depart.

This principle is of the utmost value, when legitimately applied; but the present investigation was undertaken in the belief that, occasionally at least, its powers have been to some extent abused. This appears to me to have arisen from the difficulty of deciding, in any one case, what amount of completeness or generality is secured when the process of averaging is applied in successive steps from the commencement to the end of an investigation, instead of being reserved (as it ought to be) for a single comprehensive step at the very end.

Some of the immediate consequences of this principle are obvious without calculation: such as (*a*) Even distribution, at any moment, of all the particles throughout the space in which they move.

(*b*) Even distribution of direction of motion among all particles having any one speed, and therefore among all the particles.

(*c*) Definite percentage of the whole for speed lying between definite limits.

These apply, not only to the whole group of particles but, to those in any portion of space sufficiently large to contain a very great number of particles.

(*d*) When there are two or more sets of mutually colliding spheres, *no one of which is overwhelmingly more numerous than another, nor in a hopeless minority as regards the sum of the others,* similar assertions may be made as to each set separately.

2. But calculation is required in order to determine the law of grouping as to speeds, in (*c*) above. It is quite clear that the spheres, even if they once had equal speed, could not possibly maintain such a state. (I except, of course, such merely artificial distributions as those in which the spheres are supposed to move in groups in various non-intersecting sets of parallel lines, and to have none but direct impacts. For such distributions are thoroughly unstable; the very slightest transverse impact, *on any one sphere,* would at once upset the arrangement.) For, when equal smooth spheres impinge, they *exchange* their velocities along the line of centres at impact, the other components being unchanged; so that, *only* when that line is equally inclined to their original directions of motion, do their speeds, if originally equal, remain equal after the completion of the impact. And, as an extreme case, when two spheres impinge so that the velocity of one is wholly in the line of centres at impact, and that of the other wholly perpendicular to it, the first is brought to rest and the second takes the whole kinetic energy of the pair. Still, whatever be the final distribution of speeds, it is obvious that it must be independent of any special system of axes which we may use for its computation. This consideration, taken along with (*b*) above, suffices to enable us to find this final distribution.

3. For we may imagine a space-diagram to be constructed, in which lines are laid off from an origin so as to represent the simultaneous velocities of all the spheres in a portion of space large enough to contain a very great number of them. Then (*b*) shows that these lines are to be drawn evenly in all directions in space, and (*c*) that their ends are evenly distributed throughout the space between any two nearly equal concentric spheres, whose centres are at the common origin. The density of distribution of the ends (*i.e.*, the number in unit volume of the space-diagram) is therefore a function of r, that is, of $\sqrt{x^2+y^2+z^2}$. But the argument above shows, further, that this density must be expressible in the form

$$f(x)f(y)f(z)$$

whatever rectangular axes be chosen, passing through the origin. These joint conditions give only two admissible results: viz., either

$$f(x) = A, \text{ or } f(x) = B\epsilon^{Cx^2}.$$

The first is incompatible with the physical problem, as it would make the percentage of the whole particles, which have one definite speed, increase *indefinitely* with that speed. The same consideration shows *à fortiori* that, in the second form of solution, *which is the only one left*, C must be negative. Hence the density of the distribution of "ends" already spoken of is

$$B^3\epsilon^{-hr^2}.$$

If n be the whole number of particles, *i.e.*, of "ends," we must obviously have

$$4\pi B^3\int_0^\infty \epsilon^{-hr^2}r^2dr = n.$$

The value of the integral is $$\frac{1}{4}\sqrt{\frac{\pi}{h^3}};$$

so that the number of spheres whose speed is between r and $r+dr$ is

$$4\sqrt{\frac{h^3}{\pi}}\, n\epsilon^{-hr^2}r^2dr \quad\text{.......................................}(1).$$

This distribution will hereafter be spoken of as the "*special*" state.

The mean speed is therefore

$$4\sqrt{\frac{h^3}{\pi}}\int_0^\infty \epsilon^{-hr^2}r^3dr = \frac{2}{\sqrt{\pi h}};$$

while the mean-square speed is

$$4\sqrt{\frac{h^3}{\pi}}\int_0^\infty \epsilon^{-hr^2}r^4dr = \frac{3}{2h}.$$

This shows the meaning of the constant h. (Several of the results we have just arrived at find full confirmation in the investigations (regarding mixed systems) which follow, if we only put in these P for Q *passim*:—*i.e.*, pass back from the case of a mixture of spheres of two different groups to that of a single group.)

4. Meanwhile, we can trace the general nature of the process by which the "special" arrangement of speed expressed by (1) is brought about from any initial distribution of speed, however irregular. For impacts on the containing vessel do not alter r, but merely shift the particular "end" in question to a different position on its spherical locus. Similarly, impact of equal particles does not alter the *distribution* of velocity along the line of centres, nor along any line perpendicular to it. But it does, in general, produce alterations in the distribution parallel to any line other than these.

Hence impacts, in all of which the line of centres is parallel to one common line, produce no change in the arrangement of velocity-components along that line, nor along any line at right angles to it. But there will be, in general, changes along every other line. It is these which lead gradually (though very rapidly) to the final result, in which the distribution of velocity-components is the same for all directions.

When this is arrived at, collisions will not, in the long run, tend to alter it. For then the uniformity of distribution of the spheres in space, and the symmetry of distribution of velocity among them, enable us (by the principle of averages) to dispense with the only limitation above imposed; viz., the parallelism of the lines of centres in the collisions considered.

5. In what precedes nothing whatever has been said as to the ratio of the diameter of one sphere to the average distance between two proximate spheres, except what is implied in the preliminary assumption that the sum of the volumes of the spheres is only a very small fraction of the space in which they are free to move. It is probable, though not (so far as I know) thoroughly proved, that if this fraction be exceedingly small the same results will ultimately obtain, but only after the lapse of a proportionately long time; while, if it be infinitely small, there will be no law, as there will be practically no collisions. On the other hand, if the fraction be a large one (*i.e.*, as in the case of a highly compressed gas), it seems possible that these results may be true, at first, only as a very brief *time-average* of the condition of the spheres in any region large enough to contain a great number:—that, in fact, the distribution of particles and speeds in such a region will be for some time subject to considerable but extremely rapid fluctuations. Reasons for these opinions will be seen in the next section of the paper. But it must also be noticed that when the particles fill the greater part of the space in which they move, *simultaneous* impacts of three or more will no longer be of rare occurrence; and thus a novel and difficult feature forces itself into the question.

Of course with infinitely hard spheres the probability of such multiple collisions would be infinitely small. It must be remembered, however, that the investigation is meant to apply to physical particles, and not to mere mathematical fictions; so that we must, in the case of a highly compressed gas, take account of the possibility of complex impacts, because the duration of an impact, though excessively short, is essentially finite.

II. *Mean Free Path among Equal Spheres.*

6. Consider a layer, of thickness δx, in which quiescent spheres of diameter s are evenly distributed, at the rate of n_1 per unit volume. If the spheres were opaque, such a layer would allow to pass only the fraction

$$1 - n_1 \pi s^2 \delta x / 4$$

of light falling perpendicularly on it. But if, instead of light, we have a group of spheres, also of diameter s, falling perpendicularly on the layer, the fraction of these which (whatever their common speed) pass without collision will obviously be only

$$1 - n_1 \pi s^2 \delta x \, ;$$

for two spheres must collide if the least distance between their centres is not greater than the sum of their radii. It is, of course, tacitly understood when we make such a statement that *the spheres in the very thin layer are so scattered that no one* *prevents another from doing its full duty in arresting those which attempt to pass.* Thus the fraction above written must be considered as differing very little from unity. In fact, if it differ much from unity, this consideration shows that the estimate of the number arrested will necessarily be exaggerated. Another consideration, which should also be taken into account is that, in consequence of the finite (though very small) diameter of the spheres, those whose centres are not in the layer, but within one diameter of it, act as if they were, in part, in the layer. But the corrections due to these considerations can be introduced at a later stage of the investigation.

7. If the spheres impinge obliquely on the layer, we must substitute for δx the thickness of the layer in the direction of their motion.

If the particles in the layer be all moving with a common velocity parallel to the layer, we must substitute for δx the thickness of the layer in the direction of the *relative* velocity.

If the particles in the layer be moving with a common velocity inclined to the plane of the layer, and the others impinge perpendicularly to the layer, the result will be the same as if the thickness of the layer were reduced in the ratio of the relative to the actual speed of the impinging particles, and it were turned so as to be perpendicular to the direction of the relative velocity.

8. Now suppose the particles in the layer to be moving with common speed v_1, but in directions uniformly distributed in space. Those whose directions of motion are inclined at angles between β and $\beta + d\beta$ to that of the impinging particles are, in number,

$$n_1 \sin \beta d\beta / 2;$$

and, by what has just been said, if v be the common speed of the impinging particles, the *virtual* thickness of the layer (so far as these particles are concerned) is

$$v_0 \delta x / v,$$

where $$v_0 = \sqrt{v^2 + v_1^2 - 2vv_1 \cos \beta}$$

is the *relative* speed, a quantity to be treated as essentially positive.

Thus the fraction of the impinging particles which traverses this set without collision is

$$1 - n_1 \pi s^2 \delta x v_0 \sin \beta d\beta / 2v.$$

To find the fraction of the impinging particles which pass without collision through the layer, we must multiply together all such expressions (each, of course, infinitely nearly equal to unity) between the limits 0 and π of β. The logarithm of the product is

$$- \frac{n_1 \pi s^2 \delta x}{2v} \int_0^\pi \sqrt{v^2 + v_1^2 - 2vv_1 \cos \beta} \, . \sin \beta d\beta.$$

Making v_0 the variable instead of β, this becomes

$$-\frac{n_1\pi s^2\delta x}{2v^2v_1}\int v_0^2 dv_0.$$

If v be greater than v_1, the limits of integration are $v-v_1$, and $v+v_1$, and the expression becomes

$$-n_1\pi s^2\delta x\left(1+\frac{v_1^2}{3v^2}\right);$$

but, if v be less than v_1, the limits are v_1-v and v_1+v, and the value is

$$-n_1\pi s^2\delta x\left(\frac{v}{3v_1}+\frac{v_1}{v}\right).$$

These give, as they should, the common value

$$-4n_1\pi s^2\delta x/3$$

when $v=v_1$.

9. Finally, suppose the particles in the layer to be in the "special" state. If there be n in unit volume, we have for the number whose speed is between the limits v_1 and v_1+dv_1

$$n_1 = 4nv_1^2 dv_1\sqrt{\frac{h^3}{\pi}}\,\epsilon^{-hv_1^2}.$$

Hence the logarithm of the fraction of the whole number of impinging particles, whose speed is v and which traverse the layer without collision, is

$$-4\pi ns^2\sqrt{\frac{h^3}{\pi}}\,\delta x\left\{\int_0^v \epsilon^{-hv_1^2}\left(v_1^2+\frac{v_1^4}{3v^2}\right)dv_1+\int_v^\infty \epsilon^{-hv_1^2}\left(\frac{vv_1}{3}+\frac{v_1^3}{v}\right)dv_1\right\}.$$

The value of the factor in brackets is easily seen to be

$$-\frac{dV}{dh}+\frac{1}{3v^2}\frac{d^2V}{dh^2}+\left(\frac{2v}{3h}+\frac{1}{2h^2v}\right)\epsilon^{-hv^2},$$

or

$$\frac{1}{4h^2v}\epsilon^{-hv^2}+\left(\frac{1}{4h^2v^2}+\frac{1}{2h}\right)V,$$

where

$$V=\int_0^v \epsilon^{-hv^2}dv,$$

and thus it may readily be tabulated by the help of tables of the error-function.

When v is very large, the ultimate value of the expression is

$$\frac{1}{4}\sqrt{\frac{\pi}{h^3}};$$

which shows that, in this case, the "special" state of the particles in the layer does not affect its permeability.

10. Write, for a moment, $$-e\delta x$$
as the logarithm of the fraction of the particles with speed v which traverse the layer unchecked. Then it is clear that

$$\epsilon^{-ex}$$

represents the fraction of the whole which penetrate unchecked to a distance x into a group in the "special" state. Hence the mean distance to which particles with speed v can penetrate without collision is

$$\frac{\int_0^\infty \epsilon^{-ex} x\,dx}{\int_0^\infty \epsilon^{-ex}\,dx} = \frac{1}{e}.$$

This is, of course, a function of v; and the remarks above show that it increases continuously with v to the maximum value (when v is infinite)

$$\frac{1}{n\pi s^2};$$

i.e., the mean path for a particle moving with infinite speed is the same as if the particles of the medium traversed had been at rest.

11. Hence, to find the *Mean Free Path* among a set of spheres all of which are in the special state, the natural course would appear to be to multiply the average path for each speed by the probability of that speed, and take the sum of the products. Since the probability of speed v to $v + dv$ is

$$4\sqrt{\frac{h^3}{\pi}}\,\epsilon^{-hv^2} v^2 dv,$$

the above definition gives for the length of the mean free path,

$$4\sqrt{\frac{h^3}{\pi}}\int_0^\infty \epsilon^{-hv^2} v^2 dv/e,$$

or, by the expression for e above,

$$\frac{1}{n\pi s^2}\int_0^\infty \frac{\epsilon^{-hv^2} v^2 dv}{\int_0^v \epsilon^{-hv_1^2}\left(v_1^2 + \frac{v_1^4}{3v^2}\right) dv_1 + \int_v^\infty \epsilon^{-hv_1^2}\left(\frac{vv_1}{3} + \frac{v_1^3}{v}\right) dv_1}.$$

This may without trouble (see § 9) be transformed into the simpler expression

$$\frac{1}{n\pi s^2}\int_0^\infty \frac{4x^4\epsilon^{-x^2}dx}{x\epsilon^{-x^2} + (2x^2+1)\int_0^x \epsilon^{-x^2}dx},$$

which admits of easy numerical approximation. The numerical work would be simplified by dividing above and below by ϵ^{-x^2}, but we prefer to keep the present form on account of its direct applicability to the case of mixed systems. And it is curious to note that $4\epsilon^{-x^2}$ is the third differential coefficient of the denominator.

The value of the definite integral (as will be shown by direct computation in an *Appendix* to the paper) is about

$$0{\cdot}677\,;$$

and this is the ratio in which the mean path is diminished in consequence of the motion of the particles of the medium. For it is obvious, from what precedes, that the mean path (at any speed) if the particles were quiescent would be

$$\frac{1}{n\pi s^2}.$$

[The factor by which the mean path is reduced in consequence of the "special" state is usually given, after Clerk-Maxwell, as $1/\sqrt{2}$ or 0·707.

But this appears to be based on an erroneous definition. For if n_v be the fraction of the whole particles which have speed v, p_v their free path; we have taken the mean free path as

$$\Sigma\,(n_v p_v),$$

according to the usual definition of a "mean."

Clerk-Maxwell, however, takes it as

$$\frac{\Sigma\,(n_v v)}{\Sigma\,(n_v v/p_v)},$$

i.e., the quotient of the average speed by the average number of collisions per particle per second. But those who adopt this divergence from the ordinary usage must, I think, face the question "Why not deviate in a different direction, and define the mean path as the product of the average speed into the average time of describing a free path?" This would give the expression

$$\Sigma\,(n_v v)\,.\,\Sigma\,(n_v p_v/v).$$

The latter factor involves a definite integral which differs from that above solely by the factor $\sqrt{h}/x$ in the numerator, so that its numerical determination is easy from the calculations already made. It appears thus that the reducing factor would be about

$$\frac{2}{\sqrt{\pi}} \times 0{\cdot}650, \quad = 0{\cdot}734 \text{ nearly};$$

i.e., considerably more in excess of the above value than is that of Clerk-Maxwell. Until this comparatively grave point is settled, it would be idle to discuss the small effect, on the length of the mean free path, of the diameters of the impinging spheres.]

III. *Number of Collisions per Particle per Second.*

12. Here again we may have a diversity of definitions, leading of course to different numerical results. Thus, with the notation of § 11, we may give the mean number of collisions per particle per second as

$$\Sigma\,(n_v v/p_v).$$

This is the definition given by Clerk-Maxwell and adopted by Meyer; and *here* the usual definition of a "mean" is employed. The numerical value, by what precedes, is

$$16ns^2h^3\int_0^\infty \epsilon^{-hv^2}v^2dv\left\{\int_0^v \epsilon^{-hv_1^2}\left(v_1^2+\frac{v_1^4}{3v^2}\right)dv_1+\int_v^\infty \epsilon^{-hv_1^2}\left(\frac{vv_1}{3}+\frac{v_1^3}{v}\right)dv_1\right\}.$$

Meyer evaluates this by expanding in an infinite series, integrating, and summing. But this circuitous process is unnecessary; for it is obvious that the two parts of the expression must, *from their meaning,* be equal; while the second part is integrable directly.

13. On account of its bearing (though somewhat indirectly) upon the treatment of other expressions which will presently occur, it may be well to note that a mere inversion of the order of integration, in either part of the above double integral, changes it into the other part.

Otherwise:—we may reduce the whole to an immediately integrable form by the use of polar co-ordinates; putting

$$v = r\cos\theta,\quad v_1 = r\sin\theta,$$

and noting that the limits of r are 0 to ∞ in both parts, while those of θ are 0 to $\pi/4$ in the first part, and $\pi/4$ to $\pi/2$ in the second. [*This* transformation, however, is not well adapted to the integrals which follow, with reference to two sets of spheres, because h has not the same value in each set.]

14. Whatever method we adopt, the value of the expression is found to be

$$\sqrt{\frac{8\pi}{h}}\,.\,ns^2 = 2\sqrt{\frac{2}{\pi h}}\,\pi ns^2;$$

and, as the mean speed is (§ 3) $\dfrac{2}{\sqrt{\pi h}}$,

we obtain Clerk-Maxwell's value of the mean path, above referred to, viz.,

$$\frac{1}{n\pi s^2\sqrt{2}}.$$

But (in illustration of the remarks at the end of § 11) we might have defined the mean number of collisions per particle per second as

$$\frac{\Sigma\,(n_v v)}{\Sigma\,(n_v p_v)}, \text{ or as } \frac{1}{\Sigma\,(n_v p_v/v)}; \text{ \&c., \&c.}$$

The first, which expresses the ratio of the mean speed to the mean free path, gives

$$\frac{2}{\sqrt{\pi h}} \cdot \frac{\pi n s^2}{0{\cdot}677};$$

and the second, which is the reciprocal of the mean value of the time of describing a free path, gives

$$\frac{1}{\sqrt{h}} \frac{\pi n s^2}{0{\cdot}650}.$$

The three values which we have adduced as examples bear to one another the reciprocals of the ratios of the above-mentioned determinations of the mean free path.

IV. *Clerk-Maxwell's Theorem.*

15. In the ardour of his research of 1859*, Maxwell here and there contented himself with very incomplete proofs (we can scarcely call them more than illustrations) of some of the most important of his results. This is specially the case with the investigation of the law of ultimate partition of energy in a mixture of smooth spherical particles of two different kinds. He obtained, in accordance with the so-called *Law of Avogadro*, the result that the average energy of translation is the same per particle in each system; and he extended this in a Corollary to a mixture of any number of different systems. This proposition, if true, is of fundamental importance. It was extended by Maxwell himself to the case of rigid particles of any form, where rotations perforce come in. And it appears that in such a case the whole energy is ultimately divided *equally* among the various degrees of freedom. It has since been extended by Boltzmann and others to cases in which the individual particles are no longer supposed to be rigid, but are regarded as complex systems having great numbers of degrees of freedom. And it is stated, as the result of a process which, from the number and variety of the assumptions made at almost every stage, is rather of the nature of playing with symbols than of reasoning by consecutive steps, that in such groups of systems the ultimate state will be a partition of the whole energy in equal shares among the classes of degrees of freedom which the individual particle-systems possess. This, if accepted as true, at once raises a formidable objection to the kinetic theory. For there can be no doubt that each individual particle of a gas has a very great number of degrees of freedom besides the six which it would have if it were rigid:—the examination of its spectrum while incandescent proves this at once. But if all these degrees of freedom are to share the whole energy (on the average) equally among them, the results of theory will no longer be consistent with our experimental knowledge of the two specific heats of a gas, and the relations between them.

* *Phil. Mag.*, 1860.

16. Hence it is desirable that Clerk-Maxwell's proof of his fundamental Theorem should be critically examined, and improved where it may be found defective. If it be shown in this process that certain preliminary conditions are absolutely necessary to the proof even of Clerk-Maxwell's Theorem, and if these *cannot* be granted in the more general case treated by Boltzmann, it is clear that Boltzmann's Theorem must be abandoned.

17. The chief feature in respect of which Maxwell's investigation is to be commended is its courageous recognition of the difficulties of the question. In this respect it far transcends all other attempts which I have seen. Those features, besides too great conciseness, in respect of which it seems objectionable, are:—

(*a*) He *assumes* that the transference of energy from one system to the other can be calculated from the results of a single impact between particles, one from each system, each having the average translational energy of its system.

Thus (so far as this step is concerned) the distribution of energy in each system may be any whatever.

(*b*) In this typical impact the velocities of the impinging spheres are taken as at right angles to one another, so that the relative speed may be that of mean square as between the particles of the two systems. The result obtained is fallacious, because in general the directions of motion after impact are found not to be at right angles to one another, as they would certainly be (on account of the perfect reversibility of the motions) were this really a typical impact.

(*c*) Clerk-Maxwell proceeds as if every particle of one system impinged upon one of the other system at each stage of the process—*i.e.*, he calculates the transference of energy as if each pair of particles, one from each system, had simultaneously a typical impact. This neglect of the immensely greater number of particles which either had no impact, or impinged on others of their own group, makes the calculated rate of equalisation far too rapid.

(*d*) Attention is not called to the fact that impacts between particles are numerous in proportion to their *relative* speed, nor is this consideration introduced in the calculations.

(*e*) Throughout the investigation each step of the process of averaging is performed (as a rule) before the expressions are ripe for it.

18. In seeking for a proof of Maxwell's Theorem it seems to be absolutely essential to the application of the statistical method to premise:—

(A) That the particles of the two systems are thoroughly mixed.

(B) That in any region containing a very large number of particles, the particles of each kind separately acquire and maintain the error-law distribution of speeds—*i.e.*, each set will ultimately be in the "special" state. The disturbances of this arrangement produced in either system by impacts on members of the other are regarded as being promptly repaired by means of the internal collisions in the system itself. This is the sole task assigned to these internal collisions. We assume that they accomplish it, so we need not further allude to them.

[The warrant for these assumptions is to be sought as in § 4; and in the fact that only a small fraction of the whole particles are at any instant in collision; *i.e.*, that each particle advances, on the average, through a considerable multiple of its diameter before it encounters another.]

(C) That there is perfectly free access for collision between each pair of particles, whether of the same or of different systems; and that, in the mixture, the number of particles of one kind is not overwhelmingly greater than that of the other kind.

[This is one of the essential points which seem to be wholly ignored by Boltzmann and his commentators. There is no proof given by them that one system, while regulating by its internal collisions the distribution of energy among its own members, can also by impacts regulate the distribution of energy among the members of another system, when these are not free to collide with one another. In fact, if (to take an extreme case) the particles of one system were so small, in comparison with the average distance between any two contiguous ones, that they practically had no *mutual* collisions, they would behave towards the particles of another system much as Le Sage supposed his ultra-mundane corpuscles to behave towards particles of gross matter. Thus they would merely alter the apparent amount of the molecular forces between the particles of a gas. And it is specially to be noted that this is a question of *effective diameters* merely, and not of masses:—so that those particles which are virtually free from the self-regulating power of mutual collisions, and therefore form a disturbing element, may be much more massive than the others.]

19. With these assumptions we may proceed as follows:—Let P and Q be the masses of particles from the two systems respectively; and when they impinge, let u, v be their velocity-components measured towards the same parts along the line of centres at impact. If these velocities become, after impact, u′, v′ respectively, we have at once

$$P(\mathrm{u}'-\mathrm{u}) = -\frac{2PQ}{P+Q}(\mathrm{u}-\mathrm{v}) = -Q(\mathrm{v}'-\mathrm{v});$$

an immediate consequence of which is

$$P(\mathrm{u}'^2-\mathrm{u}^2) = -\frac{4PQ}{(P+Q)^2}\{P\mathrm{u}^2 - Q\mathrm{v}^2 - (P-Q)\,\mathrm{uv}\} = -Q(\mathrm{v}'^2-\mathrm{v}^2).$$

Hence, denoting by a bar the average value of a quantity, we see that transference of energy between the systems must cease when

$$P\overline{\mathrm{u}^2} - Q\overline{\mathrm{v}^2} - (P-Q)\,\overline{\mathrm{uv}} = 0 \quad\text{.................................(1)},$$

and the question is reduced to finding these averages.

[I thought at first that $\overline{\mathrm{uv}}$ might be assumed to vanish, and that $\overline{\mathrm{u}^2}$ and $\overline{\mathrm{v}^2}$ might each be taken as one-third of the mean square speed in its system. This set of suppositions would lead to Maxwell's Theorem at once. But it is clear that, when two particles have each a *given* speed, they are more likely to collide when they are moving towards opposite parts than when towards the same parts. Hence $\overline{\mathrm{uv}}$ must be an essentially negative quantity, and therefore $P\overline{\mathrm{u}^2}$ necessarily less than $Q\overline{\mathrm{v}^2}$, if P be greater than Q. Thus it seemed as if the greater masses would have on the average less energy than the smaller. These are two of the pitfalls to which I have alluded. Another will be met with presently.]

20. But these first impressions are entirely dissipated when we proceed to calculate the average values. For it is found that if we write (1) in the form

$$P\overline{\mathrm{u}^2 - \mathrm{uv}} - Q\overline{\mathrm{v}^2 - \mathrm{uv}} = 0,$$

the terms on the left are equal multiples of the average energy of a P and of a Q respectively. Thus Maxwell's Theorem is rigorously true, though in a most unexpected manner. There must surely be some extremely simple and direct mode of showing that $\overline{\mathrm{u}^2 - \mathrm{uv}}$ is independent of the mean-square speed of the system of Q's. Meanwhile, in default of anything more simple, I give the investigation by which I arrived at the result just stated.

21. Suppose a particle to move, with constant speed v, among a system of other particles in the "special" state; the fraction of the whole of its encounters which takes place with particles, whose speed is from v_1 to $v_1 + dv_1$ and whose directions of motion are inclined to its own at angles from β to $\beta + d\beta$, is (§ 8) proportional to

$$\epsilon^{-kv_1^2} v_1^2 dv_1 v_0 \sin\beta \, d\beta,$$

or as we may write it for brevity

$$\nu_1 v_0 \sin\beta \, d\beta.$$

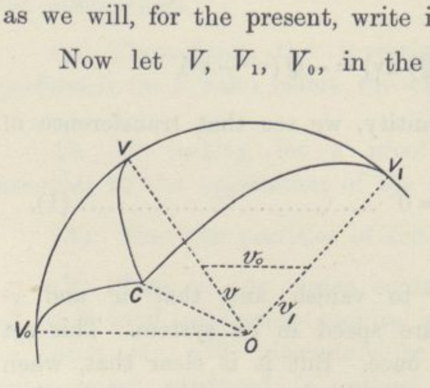
This is easily seen by remarking that, by § 8, while the particle advances through a space δx, it virtually passes through a layer of particles (such as those specified) of thickness $v_0 \delta x / v$. Here (§ 3) $3/2k$ is the mean-square speed of the particles of the system.

Let the impinging particle belong to another group, also in the special state. Then the number of particles of that group which have speeds between v and $v + dv$ is proportional to

$$\epsilon^{-hv^2} v^2 dv = \nu,$$

as we will, for the present, write it.

Now let V, V_1, V_0, in the figure, be the projections of v, v_1, v_0 on the unit sphere whose centre is O; C that of the line of centres at impact. Then $VOV_1 = \beta$. Let $V_0OV = \alpha$, $V_0OV_1 = \alpha_1$, $V_0OC = \gamma$, and $VV_0C = \phi$. The limits of γ are 0 and $\pi/2$; those of ϕ are 0 and 2π. Also the chance that C lies within the spherical surface-element $\sin\gamma \, d\gamma \, d\phi$, is proportional to the area of the projection of that element on a plane perpendicular to the direction of v_0, *i.e.*, it is proportional to



$$\cos\gamma \sin\gamma \, d\gamma \, d\phi.$$

But by definition we have

$$\mathrm{u} = v \cos VOC = v\,(\cos\alpha\cos\gamma + \sin\alpha\sin\gamma\cos\phi),$$

$$\mathrm{v} = v_1 \cos V_1OC = v_1\,(\cos\alpha_1\cos\gamma + \sin\alpha_1\sin\gamma\cos\phi);$$

and by the Kinematics of the question, as shown by the dotted triangle in the figure, we have

$$v\cos\alpha - v_1\cos\alpha_1 = v_0,$$

$$v\sin\alpha - v_1\sin\alpha_1 = 0.$$

Thus, as indeed is obvious from much simpler considerations,

$$\mathrm{u} - \mathrm{v} = v_0\cos\gamma,$$

so that $$\overline{\mathrm{u}^2 - \mathrm{uv}} = \frac{\int vv_1v_0\sin\beta d\beta\,\mathrm{u}\,(\mathrm{u}-\mathrm{v})\cos\gamma\sin\gamma d\gamma d\phi}{\int vv_1v_0\sin\beta d\beta\cos\gamma\sin\gamma d\gamma d\phi}$$

$$= \frac{\int vv_1v_0\sin\beta d\beta\,v\,(\cos\alpha\cos\gamma+\sin\alpha\sin\gamma\cos\phi)\,v_0\cos^2\gamma\sin\gamma d\gamma d\phi}{\int vv_1v_0\sin\beta d\beta\cos\gamma\sin\gamma d\gamma d\phi},$$

where each of the integrals is quintuple.

The term in $\cos\phi$ vanishes when we integrate with respect to ϕ:—and, when we further integrate with respect to γ, we have for the value of the expression

$$\frac{\frac{1}{2}\int vv_1v_0\sin\beta d\beta\,vv_0\cos\alpha}{\int vv_1v_0\sin\beta d\beta},$$

where the integrals are triple.

Now $$2vv_0\cos\alpha = v^2 + v_0^2 - v_1^2,$$

and $$vv_1\sin\beta d\beta = v_0dv_0,$$

so that the expression becomes

$$\frac{\frac{1}{4}\int vv_1\frac{v_0^2dv_0}{vv_1}(v^2+v_0^2-v_1^2)}{\int vv_1\frac{v_0^2dv_0}{vv_1}}.$$

It will be shown below (Part VI.), that we have, generally,

$$\int vv_1\frac{v_0^{2n}dv_0}{vv_1} = \frac{I_{2n+1}}{2n+1} = \frac{\sqrt{\pi}}{4}n!\frac{(h+k)^{\frac{2n-1}{2}}}{(hk)^{n+1}},$$

and that it is lawful to differentiate such expressions with regard to h or to k. Hence

$$\overline{\mathrm{u}^2 - \mathrm{uv}} = \frac{1}{4}\frac{I_5/5 - \left(\frac{d}{dh}-\frac{d}{dk}\right)I_3/3}{I_3/3} = \frac{1}{h}.$$

Thus Clerk-Maxwell's Theorem is proved.

22. The investigation of the separate values of the parts of this expression is a little more troublesome, as the numerators now involve second partial differential coefficients of I_1; but it is easy to see that we have

$$\overline{\mathrm{u}^2} = \frac{1}{16}\frac{\left(\frac{d}{dh}-\frac{d}{dk}\right)^2 I_1 - 2\left(3\frac{d}{dh}-\frac{d}{dk}\right)I_3/3 + I_5/5}{I_3/3} = \frac{h+2k}{2h(h+k)},$$

$$\overline{\mathrm{uv}} = \frac{1}{16}\frac{\left(\frac{d}{dh}-\frac{d}{dk}\right)^2 I_1 - 2\left(\frac{d}{dh}+\frac{d}{dk}\right)I_3/3 - 3I_5/5}{I_3/3} = -\frac{1}{2(h+k)},$$

and, from these, the above result again follows.

[It is clear, from the investigation just given, that the expression for the value of $\overline{\mathrm{u}^2 - \mathrm{uv}}$ would be the same (to a numerical factor *près*) *whatever* law we assumed for the probability of the line of centres having a definite position, and thus that Maxwell's Theorem would be true, provided only that the law were a function of γ alone, and not of ϕ (*i.e.*, that the possible positions of the line of centres were symmetrically distributed round the direction of relative motion of the impinging particles). In my first non-approximate investigation (read to the Society on Jan. 18, and of which an Abstract appeared in *Nature*, Jan. 21, 1886) I had inadvertently assumed that the possible positions of C were equally distributed over the surface of the hemisphere of which V_0 is the pole, instead of over the surface of its diametral plane. The forms, however, of $\overline{\mathrm{u}^2}$ and of $\overline{\mathrm{uv}}$ separately, suffer more profound modifications when such assumptions are made.]

V. *Rate of Equalisation of Average Energy per particle in two Mixed Systems.*

23. To obtain an idea of the rate at which a mixture of two systems approaches the Maxwell final condition, suppose the mixture to be complete, and the systems each in the special state, but the average energy per particle to be different in the two. As an exact solution is not sought, it will be sufficient to adopt, throughout, roughly approximate expressions for the various quantities involved. We shall choose such as lend themselves most readily to calculation.

It is easy to see, by making the requisite slight modifications in the formula of § 12, that, if m be the number of P's and n that of Q's in unit volume, the number of collisions per second between a P and a Q is

$$2mns^2\sqrt{\frac{\pi(h+k)}{hk}},$$

where s now stands for the sum of the radii of a P and of a Q. For if, in the formula referred to, we put $(hk)^{3/2}$ for h^3, and also put k for h in the exponentials where the integration is with respect to v_1, it becomes

$$8ns^2(hk)^{\frac{3}{2}}I_3/3,$$

according to the notation of § 21. This is the average number of impacts per second which a P has with Q's.

Hence, if ϖ be the *whole* energy of the P's, ρ that of the Q's, per unit volume, the equations of § 19 become

$$\dot{\varpi} = -\frac{16}{3}\frac{PQ}{(P+Q)^2}s^2\sqrt{\frac{\pi(h+k)}{hk}}(n\varpi - m\rho) = -\dot{\rho},$$

from which we obtain, on the supposition (approximate enough for our purpose) that we may treat $1/h + 1/k$ as constant,

$$n\varpi - m\rho = C\epsilon^{-t/T},$$

where
$$\frac{1}{T} = \frac{16}{3}\frac{PQ}{(P+Q)^2}s^2(m+n)\sqrt{\frac{\pi(h+k)}{hk}}.$$

The quantity
$$n\varpi - m\rho = mn(\varpi/m - \rho/n)$$

is mn times the difference of the average energies of a P and a Q, and (since $\epsilon^{4\cdot6} = 100$ nearly) we see that it is reduced to one per cent. of its amount in the time

$$t_1 = 4\cdot6T = \frac{13\cdot8}{16s^2(m+n)}\frac{(P+Q)^2}{PQ}\sqrt{\frac{hk}{\pi(h+k)}}\text{ seconds.}$$

24. For a mixture, in equal volumes, of two gases in which the masses of the particles are not very different, say oxygen and nitrogen, we may assume as near enough for the purposes of a rough approximation

$$m = n = \frac{3}{2}\times 10^{20},$$

whence $m + n$ (per cubic inch) is double of this,

$$\frac{3}{2h} = \frac{3}{2k} = (12\times 1600\text{ inch sec.})^2,$$

$$s = 3\times 10^{-8}\text{ inch},$$

so that
$$t_1 = \frac{13\cdot8\times 10^{16}\times 4}{16\times 9\times 3\times 10^{20}\times 12\times 1600}\sqrt{\frac{3}{4\pi}} = \frac{1}{3\times 10^9}\text{ seconds, nearly;}$$

and the difference has fallen to 1 per cent. of its original amount in this period, *i.e.*, after each P has had, on the average, about four collisions with Q's. This calculation has no pretensions to accuracy, but it is excessively useful as showing the nature of the warrant which we have for some of the necessary assumptions made above. For if the rapidity of equalisation of average energy in two systems is of this extreme order of magnitude, we are entitled to suppose that the restoration of the special state in any one system is a phenomenon taking place at a rate of at least the same if not a higher order of magnitude.

Clerk-Maxwell's result as regards the present question is that, at every typical impact between a P and a Q, the difference of their energies is reduced in the ratio

$$\left(\frac{P-Q}{P+Q}\right)^2;$$

so that, if the masses were equal, the equalisation would be instantaneous.

VI. *On some Definite Integrals.*

25. It is clear that expressions of the forms

$$\int_0^\infty \epsilon^{-hx^2} x^r dx \int_0^x \epsilon^{-ky^2} y^s dy \text{ and } \int_0^\infty \epsilon^{-hx^2} x^r dx \int_x^\infty \epsilon^{-ky^2} y^s dy,$$

where r and s are essentially positive integers, may lawfully be differentiated under the integral sign with regard to h or to k. In fact they, and their differential coefficients, which are of the same form, are all essentially finite.

As, in what immediately follows, we shall require to treat of the first of these forms only when r is odd and s even, and of the second only when r is even and s odd, it follows that their values can all be obtained by differentiation from one or other of the integrals

$$\int_0^\infty \epsilon^{-hx^2} x dx \int_0^x \epsilon^{-ky^2} dy = \frac{\sqrt{\pi}}{4h\sqrt{h+k}},$$

and

$$\int_0^\infty \epsilon^{-hx^2} dx \int_x^\infty \epsilon^{-ky^2} y dy = \frac{\sqrt{\pi}}{4k\sqrt{h+k}}.$$

These values may be obtained at once by noticing that the second form is integrable directly; while, by merely inverting the *order* of integration, it becomes the first with h and k interchanged.

26. In §§ 21, 22 we had to deal with a number of integrals, all of one form, of which we take as a simple example

$$I_3/3 = \int vv_1 \frac{v_0^2}{vv_1} dv_0$$

$$= \frac{1}{3}\int_0^\infty \epsilon^{-hx^2} x dx \left\{\int_0^x \epsilon^{-ky} y dy \left((x+y)^3 - (x-y)^3\right) + \int_x^\infty \epsilon^{-ky^2} y dy \left((y+x)^3 - (y-x)^3\right)\right\}.$$

From the remarks above it is clear that this can be expressed as

$$\frac{2}{3}\frac{\sqrt{\pi}}{4}\left\{\left(3\frac{d^2}{dhdk}+\frac{d^2}{dk^2}\right)\frac{1}{h\sqrt{h+k}}+\left(3\frac{d^2}{dkdh}+\frac{d^2}{dh^2}\right)\frac{1}{k\sqrt{h+k}}\right\}$$

$$=\frac{2}{3}\frac{\sqrt{\pi}}{4}\frac{3}{2}\left(\frac{k+3h}{h^2(h+k)^{\frac{5}{2}}}+\frac{3k+h}{k^2(h+k)^{\frac{5}{2}}}\right)$$

$$=\frac{\sqrt{\pi}}{4}\frac{(k^3+3k^2h)+(3kh^2+h^3)}{h^2k^2(h+k)^{\frac{5}{2}}}$$

$$=\frac{\sqrt{\pi}}{4}\frac{(h+k)^{\frac{1}{2}}}{(hk)^2}.$$

The peculiar feature here shown is the making up of the complete cube of $k+h$ in the numerator by the supply of the *first half* of its terms from the first part of the integral, and of the remainder from the second*. On trial I found that the same thing holds for I_5 and I_7, so that I was led to conjecture that, generally, as in § 21

$$\frac{I_{2n+1}}{2n+1}=\frac{\sqrt{\pi}}{4}n!\frac{(h+k)^{\overline{\frac{2n-1}{2}}}}{(hk)^{n+1}}.$$

After the preliminary work we have just given, it is easy to prove this as follows. We have always

$$\left((x+y)^{2n+1}-(x-y)^{2n+1}\right)\left((x+y)^2+(x-y)^2\right)$$
$$=(x+y)^{2n+3}-(x-y)^{2n+3}+\left(x^2-y^2\right)^2\left((x+y)^{2n-1}-(x-y)^{2n-1}\right).$$

Operate on this by $\displaystyle\int_0^\infty \epsilon^{-hx^2}xdx\int_0^x \epsilon^{-ky^2}ydy\left(\quad\right)$,

and on the same expression, with x and y interchanged (when, of course, it remains true), by

$$\int_0^\infty \epsilon^{-hx^2}xdx\int_x^\infty \epsilon^{-ky^2}ydy\left(\qquad\right),$$

and add the results. This gives at once

$$-2\left(\frac{d}{dh}+\frac{d}{dk}\right)I_{2n+1}=I_{2n+3}+\left(\frac{d}{dh}-\frac{d}{dk}\right)^2 I_{2n-1};$$

which is found on trial to be satisfied by the general value given above.

* Prof. Cayley has called my attention, in connection with this, to the following expression from a Trinity (Cambridge) Examination Paper:—

$$(a+b)^{2n}=(a+b)^n(a^n+b^n)$$
$$+(a+b)^{n-1}(na^nb+nab^n)$$
$$+(a+b)^{n-2}\left(\frac{n\,.\,n+1}{1\,.\,2}a^nb^2+\frac{n\,.\,n+1}{1\,.\,2}a^2b^n\right)$$
$$\cdots\cdots\cdots\cdots$$
$$+(a+b)\frac{n\,.\,n+1\ldots\ldots(2n-2)}{1\,.\,2\ldots\ldots(n-1)}(a^nb^{n-1}+a^{n-1}b^n).$$

27. Partly as a matter of curiosity, but also because we shall require a case of it, it may be well to mention here that similar processes (in which it is no longer necessary to break the y integration into two parts) lead to the companion formula

$$\frac{I_{2n}}{2n} = \int_0^\infty \epsilon^{-hx^2} x\,dx \int_0^\infty \epsilon^{-ky^2} y\,dy\,((x+y)^{2n} - (x-y)^{2n})/2n$$

$$= \frac{\pi}{4} \frac{1 \,.\, 3 \,.\, 5 \ldots (2n-1)}{2^n} \frac{(h+k)^{n-1}}{(hk)^{\frac{2n+1}{2}}}.$$

And we see, by Wallis' Theorem, that (when n is increased without limit) I_{2n} is ultimately the geometric mean between I_{2n-1} and I_{2n+1}.

VII. *Mean Path in a Mixture of two Systems.*

28. If we refer to § 10, we see that, instead of what was there written as $-e\delta x$, we must now write $-(e+e_1)\,\delta x$; where e_1, which is due to stoppage of a particle of the first system by particles of the second, differs from e in three respects only. Instead of the factor $4s^2$, which appears in e, we must now write $(s+s_1)^2$; where s_1 is the diameter of a particle of the second system. Instead of h and n we must write h_1 and n_1 respectively.

Hence the mean free path of a particle of the first system is

$$4\sqrt{\frac{h^3}{\pi}} \int_0^\infty \frac{v^2 dv}{e+e_1} \epsilon^{-hv^2};$$

which, when the values of e and e_1 are introduced, and a simplification analogous to those in §§ 9, 11, is applied, becomes

$$\frac{1}{n\pi s^2} \int_0^\infty \frac{4\epsilon^{-x^2} x^4 dx}{x\epsilon^{-x^2} + (1+2x^2)\int_0^x \epsilon^{-x^2} dx + \frac{n_1 h}{n h_1}\left(\frac{s+s_1}{2s}\right)^2 \left(x_1 \epsilon^{-x_1^2} + (1+2x_1^2)\int_0^{x_1} \epsilon^{-x^2} dx\right)},$$

in which $$x_1 = x\sqrt{\frac{h_1}{h}}.$$

Thus the values tabulated at the end of the paper for the case of a single system enable us to calculate the value of this expression also.

VIII. *Pressure in a System of Colliding Particles.*

29. There are many ways in which we may obtain, by very elementary processes, the pressure in a system of colliding particles.

(a) It is the rate at which momentum passes across a plane unit area; or the whole momentum which so passes per second. [It is to be noted that a loss of negative momentum by the matter at either side of the plane is to be treated as a gain of positive.]

In this, and the other investigations which follow, we deal with planes supposed perpendicular to the axis of x; or with a thin layer bounded by two such planes.

The average number of particles at every instant per square unit of a layer, whose thickness is δx, is $n\delta x$. Of these the fraction

$$\nu = 4\sqrt{\frac{h^3}{\pi}}\,\epsilon^{-hv^2}v^2 dv$$

have speeds from v to $v+dv$. And of these the fraction

$$\sin\beta d\beta/2$$

are moving in directions inclined from β to $\beta+d\beta$ to the axis of x. Each of them, therefore, remains in the layer for a time

$$\delta x/v\cos\beta,$$

and carries with it momentum $\qquad Pv\cos\beta$

parallel to x. Now from $\beta=0$ to $\beta=\frac{\pi}{2}$ we have positive momentum passing towards x positive. From $\beta=\frac{\pi}{2}$ to $\beta=\pi$ we have an *equal* amount of negative momentum leaving x positive. Hence the whole momentum which passes per second through a plane unit perpendicular to x is

$$2\times\frac{1}{2}Pn\int_0^\infty \nu v^2\int_0^{\frac{\pi}{2}}\cos^2\beta\sin\beta d\beta=\frac{1}{3}Pn\overline{v^2},$$

where the bar indicates mean value. That is

$$\text{Pressure}=p=\frac{2}{3}\text{ (Kinetic Energy in Unit Volume).}$$

(*b*) Or we might proceed as follows, taking account of the position of each particle when it was last in collision.

Consider the particles whose speeds are from v to $v+dv$, and which are contained in a layer of thickness δx, at a distance x from the plane of yz. Each has (§ 10) on the average ev collisions per second. Thus, by the perfect reversibility of the motions, from each unit area of the layer there start, per second,

$$n\nu ev\delta x$$

such particles, which have just had a collision. These move in directions uniformly distributed in space; so that

$$\sin\beta d\beta/2$$

of them are moving in directions inclined β to $\beta+d\beta$ to the axis of x. Of these the fraction

$$\epsilon^{-ex\sec\beta}$$

(where x is to be regarded as signless) reach the plane of yz, and each brings momentum

$$Pv\cos\beta$$

perpendicular to that plane. Hence the whole momentum which reaches unit area of the plane is

$$2\times\frac{1}{2}nP\int_0^\infty vv^2\int_0^{\frac{\pi}{2}}\cos\beta\sin\beta d\beta\int_0^\infty edx\epsilon^{-ex\sec\beta}$$

$$=nP\int_0^\infty vv^2\int_0^{\frac{\pi}{2}}\cos^2\beta\sin\beta d\beta,$$

the same expression as before.

(c) Clausius' method of the virial, as usually applied, also gives the same result.

30. But this result is approximate only, for a reason pointed out in § 6 above. To obtain a more exact result, let us take the virial expression itself. It is, in this case, if N be the number of particles in volume V,

$$\frac{1}{2}PN\overline{v^2}=\frac{3}{2}pV+\frac{1}{2}\Sigma(Rr),$$

where R is the mutual action between two particles whose centres are r apart, and is positive when the action is a stress tending to bring them nearer to one another. Hence, omitting the last term, we have approximately

$$p=\frac{1}{3}\frac{N}{V}P\overline{v^2},$$

which we may employ for the purpose of interpreting the value of the term omitted.

[It is commonly stated (see, for instance, Clerk-Maxwell's Lecture to the Chemical Society*) that, when the term $\frac{1}{2}\Sigma(Rr)$ is negative, the action between the particles is in the main repulsive:—"a repulsion so great that no attainable force can reduce the distance of the particles to zero." There are grave objections to the assumption of molecular repulsion; and therefore it is well to inquire whether the mere impacts, which must exist if the kinetic theory be true, are not of themselves sufficient to explain the experimental results which have been attributed to such repulsion. The experiments of Regnault on hydrogen first showed a deviation from Boyle's Law in the direction of less compression than that Law indicates. But Andrews showed that the same thing holds for all gases at temperatures and pressures over those corresponding to their critical points. And Amagat has experimentally proved that in gaseous hydrogen, which has not as yet been found to exhibit any traces of molecular attraction between its particles, the graphic representation of pV in terms of p (at least for pressures above an atmosphere, and for common temperatures) consists of a series of parallel straight lines. If this can be accounted for, without the assumption of molecular repulsion but simply

* *Chem. Soc. Jour.*, XIII. (1875), p. 493.

by the impacts of the particles, a real difficulty will be overcome. And it is certain that, at least in dealing with hard colliding spheres if not in all cases, we have no right to extract from the virial, as the pressure term, that part only which depends upon impacts on the containing vessel; while leaving unextracted the part depending on the mutual impacts of the particles. The investigation which follows shows (so far as its assumptions remain valid when the particles are not widely scattered) that no pressure, however great, can bring a group of colliding spheres to a volume less than four times the sum of their volumes. If they were motionless they could be packed into a space exceeding the sum of their volumes in the ratio $6 : \pi\sqrt{2}$, or about 1·35 : 1, only.]

In the case of hard spheres we have obviously $r = s$; and, with the notation of § 19, remembering that $Q = P$, $k = h$, we have

$$R = -P(\mathrm{u} - \mathrm{v}).$$

Hence we must find, by the method of that section, the mean value of the latter expression. It is easily seen to be

$$-P\frac{\int vv_1v_0^2 \sin\beta\, d\beta \cos^2\gamma \sin\gamma d\gamma d\phi}{\int vv_1v_0 \sin\beta\, d\beta \cos\gamma \sin\gamma d\gamma d\phi} = -\frac{2P}{3}\frac{\int vv_1v_0^3 dv_0/vv_1}{\int vv_1v_0^2 dv_0/vv_1}$$

$$= -\frac{2P}{3}\frac{I_4/4}{I_3/3} = -P\sqrt{\frac{\pi}{2h}}.$$

But, § 14, the average number of collisions, per particle per second, is

$$2\sqrt{\frac{2}{\pi h}}\frac{N}{V}\pi s^2.$$

Hence, for any one particle, the sum of the values of R (distributed, on the average, uniformly over its surface) is, in one second,

$$\Sigma(R) = -\frac{2NP}{hV}\pi s^2 = -\frac{4}{3}\frac{N}{V}P\bar{v^2}\pi s^2 = -p\,.\,4\pi s^2.$$

Thus it would appear that we may regard each particle as being subjected to the general pressure of the system; but as having its own diameter *doubled*. It is treated, in fact, just as it would then be if all the others were reduced to massive points.

The value of the term in the virial is

$$\frac{1}{4}ns\Sigma(R)$$

because, though every particle suffers the above average number of collisions, it takes two particles to produce a collision. This is equal to

$$-np\pi s^3 = -6p \text{ (sum of volumes of spheres)};$$

so that the virial equation becomes

$$nP\bar{v^2}/2 = \frac{3}{2}p\{V - 4 \text{ (sum of volumes of spheres)}\},$$

which, in *form* at least, agrees exactly with Amagat's* experimental results for hydrogen.

* *Annales de Chimie*, XXII. 1881.

These results are closely represented at 18° C. by

$$p(V-2{\cdot}6) = 2731;$$

and at 100° C. by

$$p(V-2{\cdot}7) = 3518.$$

The quantity subtracted from the volume is sensibly the same at both temperatures. The right-hand members are nearly in proportion to the absolute temperatures. The pressure is measured in mètres of mercury. Hence the volume of the gas, at 18° C. and one atmosphere, is (to the unit employed)

$$2{\cdot}6 + 2731/0{\cdot}76 = 3596 \text{ nearly.}$$

Thus, by the above interpretation of Amagat's results, we have at 18° C.

$$n\pi s^3 = 3{\cdot}9/3596.$$

Clerk-Maxwell, in his *Bradford Lecture**, ranks the various numerical data as to gases according to "the completeness of our knowledge of them." The mean free path appears in the second rank only, the numbers in which are regarded as rough approximations. In the third rank we have two quantities involved in the expression for the mean free path, viz., the absolute diameter of a particle, and the number of particles per unit volume (s and n of the preceding pages).

To determine the values of s and n separately, a second condition is required. It has usually been assumed, for this purpose, that the volume of a gas, "when reduced to the liquid form, is not much greater than the combined volume of the molecules." Maxwell justifies this assumption by reference to the small compressibility of liquids.

But, if the above argument be, even in part, admitted, we are not led to any such conclusion, and we can obtain ns^3 (as above) as a quantity of the second rank. We have already seen that ns^2 is inversely proportional to the mean free path, and is thus also of the second rank. From these data we may considerably improve our approximations to the values of n and of s.

Taking Maxwell's estimate of the mean free path in hydrogen, we have (to an inch as unit of length)

$$\frac{0{\cdot}677}{\pi n s^2} = 380 \,.\, 10^{-8}.$$

From these values of ns^2 and ns^3 we have, approximately, for 0° C. and 1 atmosphere,

$$n = 16 \,.\, 10^{20}, \quad s = 6 \,.\, 10^{-9}.$$

The values usually given are

$$n = 3 \,.\, 10^{20}, \quad s = 2{\cdot}3 \,.\, 10^{-8}.$$

It must be recollected that the above estimate rests on two assumptions, neither

* *Phil. Mag.*, 1873, II. 453. See also *Nature*, VIII. 298.

of which is more than an approximation, (a) that the particles of hydrogen behave like hard spheres, (b) that they exert no mutual molecular forces. If there were molecular attraction the value of ns^3 would be greater than that assumed above, while ns^2 would be unaltered. Thus the particles would be larger and less numerous than the estimate shows.

[Of course, after what has been said, it is easy to see that V should be diminished further by a quantity proportional to the surface of the containing vessel and to the radius of a sphere. But though this correction will become of constantly greater importance as the bulk occupied by a given quantity of gas is made smaller, it is probably too minute to be detected by experiment.]

IX. *Effect of External Potential.* (*Added June* 15, 1886.)

31. Another of Maxwell's most remarkable contributions to the Kinetic Theory consists in the Theorem that a vertical column of gas, when it is in equilibrium under gravity, has the same temperature throughout. He states, however, that an erroneous argument on the subject, when it occurred to him in 1866, "nearly upset [his] belief in calculation."* He has given various investigations of the action of external forces on the distribution of colliding spheres, but all of them are complex. The process of Boltzmann, alluded to in a foot-note to the introduction (*anté*, p. 125), and which Clerk-Maxwell ultimately preferred to his own methods, involves a step of the following nature.

An expression, analogous to the f of § 3, but in which B and C are undetermined functions of the coordinates x, y, z of a point, is formed for the number of particles per unit volume, at that point, whose component speeds, parallel to the axes, lie between given narrow limits. I do not at present undertake to discuss the validity or the sufficient generality of the process by which this expression is obtained, though the same process is (substantially) adopted by Watson and others who have written on the subject. However obtained, the expression is correct. It can be established at once by reasoning such as that in §§ 2, 3, 4. To determine the forms of the aforesaid functions, however, a most peculiar method is adopted by Boltzmann and Maxwell. The number of the particles per unit volume at x, y, z whose corresponding "ends" occupy unit volume at u, v, w in the velocity space-diagram (§ 3), is expressed in terms of these functions, and of $u^2+v^2+w^2$. The variation of the logarithm of this number of particles is then taken, on the assumption that

$$\delta x = u\delta t, \text{ \&c.}, \quad \delta u = -\frac{dU}{dx}\delta t, \text{ \&c.},$$

where U is the external potential; and it is equated to zero, *because the number of*

* *Nature*, VIII., May 29, 1873. Maxwell's name does not occur in the Index to this volume, though he has made at least five contributions to it, most of which bear on the present subject:—viz. at pp. 85, 298, 361, 527, 537.

particles is unchangeable. As this equation must hold good for all values of u, v, w, it furnishes sufficient conditions for the determination of B and C. The reasons for this remarkable procedure are not explained, but they seem to be as below. The particles are, as it were, followed in thought into the new positions which they would have reached, and the new speeds they would have acquired, in the interval δt, *had no two of them collided or had there been no others to collide with them.* But this is not stated, much less justified, and I cannot regard the argument (in the form in which it is given) as other than an exceedingly dangerous one; almost certain to mislead a student.

What seems to underlie the whole, though it is not enunciated, is a postulate of some such form as this:—

When a system of colliding particles has reached its final state, we may assume that (on the average) for every particle which enters, and undergoes collision in, a thin layer, another goes out from the other side of the layer precisely as the first would have done had it escaped collision.

32. If we make this assumption, which will probably be allowed, it is not difficult to obtain the results sought, without having recourse to a questionable process of variation. For this purpose we must calculate the changes which take place in the momentum, and in the number of particles, in a layer; or, rather, we must inquire into the nature of the processes which, by balancing one another's effects, leave these quantities unchanged.

Recur to § 29, and suppose the particles to be subject to a potential, U, which depends on x only. Then the whole *momentum* passing per unit of time perpendicularly across unit surface of any plane parallel to yz is

$$\frac{1}{3}Pn\int_0^\infty vv^2 = \frac{Pn}{2h},$$

where n (the number of particles per cubic unit), and h (which involves the mean-square speed), are functions of x.

At a parallel plane, distant α from the first in the direction of x positive, the corresponding value is

$$\frac{1}{2}P\left(1+\alpha\frac{d}{dx}\right)\frac{n}{h}.$$

But the difference must be sufficient to neutralise, in the layer between these planes, the momentum which is due to the external potential, *i.e.*,

$$-Pn\alpha\frac{dU}{dx}.$$

Hence

$$\frac{1}{2}P\alpha\frac{d}{dx}\frac{n}{h} = -Pn\alpha\frac{dU}{dx},$$

or

$$-2h\frac{dU}{dx} = \frac{1}{n}\frac{dn}{dx} - \frac{1}{h}\frac{dh}{dx} \quad\text{.............................(1).}$$

Again, the *number* of particles which, in unit of time, leave the plane unit towards the side x positive is

$$\frac{1}{2}n\int_0^\infty vv\int_0^{\frac{\pi}{2}}\cos\beta\sin\beta d\beta=\frac{1}{4}n\int_0^\infty vv.$$

Hence those which leave the corresponding area at distance α are, in number,

$$\frac{1}{4}\left(1+\alpha\frac{d}{dx}\right)\left(n\int_0^\infty vv\right).$$

But, by our postulate of last section, they can also be numbered as

$$\frac{1}{4}n\int_\zeta^\infty vv(1-\zeta^2/v^2),$$

where
$$\zeta^2=2\alpha\frac{dU}{dx}.$$

This expression is obtained by noting that none of those leaving the first plane can pass the second plane unless they have

$$v^2\cos^2\beta>2\alpha\frac{dU}{dx}.$$

All of the integrals contained in these expressions are *exact*, and can therefore give no trouble. The two reckonings of the number of particles, when compared, give

$$-2h\frac{dU}{dx}=\frac{1}{n}\frac{dn}{dx}-\frac{1}{2h}\frac{dh}{dx}\text{ ....(2).}$$

From (1) and (2) together we find, first

$$\frac{dh}{dx}=0,$$

which is the condition of uniform temperature; and again

$$n=n_0\epsilon^{-2h(U-U_0)},$$

which is the usual relation between density and potential.

[In obtaining (2) above it was assumed that, with sufficient accuracy,

$$\epsilon^{-h\zeta^2}=1-h\zeta^2.$$

To justify this:—note that in oxygen, at ordinary temperatures and under gravity,

$$\frac{3}{2h}=1550^2\text{ in foot-second units,}$$

$$\frac{dU}{dx}=32\quad\text{,,}\quad\text{,,}\quad\text{,,}$$

so that, even if $\alpha=1$ inch, we have approximately

$$h\zeta^2=2h\frac{\alpha dU}{dx}=\frac{1}{300,000}.]$$

It is easy to see that exactly similar reasoning may be applied when U is a function of x, y, z; so that we have, generally,

$$n=n_0\epsilon^{-2h(U-U_0)},$$

where h is an absolute constant. And it is obvious that similar results may be obtained for each separate set of spheres in a mixture, with the additional proviso from Maxwell's Theorem (§§ 20, 21) that P/h has the same value in each of the sets.

APPENDIX.

The following little table has been calculated for the purposes of §§ 11, 28, by Mr J. B. Clark, Neil-Arnott Scholar in the University of Edinburgh, who used six-place logarithms:—

x	X_1	X_2	X_1/X_2	X_3	X_3/X_2
·1	·000099	·200665	·00049 +	·000990	·00493 +
·2	·001537	·405312	·00379 +	·007686	·01896 +
·3	·007420	·617838	·01198 +	·024676	·03994 −
·4	·021814	·841997	·02591 −	·054537	·06477 +
·5	·048675	1·081321	·04501 +	·097350	·09003 −
·6	·090418	1·339068	·06752 +	·150698	·11254 −
·7	·147091	1·618194	·09089	·210130	·12985 +
·8	·215978	1·921318	·11241 −	·269973	·14051 +
·9	·291870	2·250723	·12968 +	·324301	·14409 −
1·0	·367879	2·608351	·14104 −	·367879	·14104 −
1·1	·436590	2·995825	·14572 +	·396900	·13249 −
1·2	·491380	3·414479	·14388 +	·409409	·11990 +
1·3	·527004	3·865384	·13633 +	·405388	·10488 −
1·4	·541119	4·349386	·12441 +	·386514	·08887 −
1·5	·533581	4·867132	·10962 +	·355721	·07309 −
1·6	·506619	5·419114	·09348 −	·316637	·05843 −
1·7	·464174	6·005696	·07729 −	·273044	·04546 +
1·8	·409127	6·627149	·06203 +	·228404	·03447 −
1·9	·352543	7·283658	·04840 −	·185549	·02547 +
2·0	·293040	7·975359	·03674 +	·146520	·01837 +
2·1	·236390	8·702340	·02715 +	·112567	·01294 −
2·2	·185224	9·464667	·01956 −	·084193	·00889
2·3	·141065	10·262360	·01373 +	·061333	·00598 −
2·4	·104541	11·095474	·00941 +	·043559	·00393 −
2·5	·075390	11·964016	·00630 +	·030156	·00252 +
2·6	·052962	12·867980	·00411 −	·020370	·00158 +
2·7	·036242	13·807388	·00262 +	·013423	·00097 +
2·8	·024155	14·782249	·00162 +	·008627	·00058 +
2·9	·015700	15·792549	·00099 +	·005414	·00034 +
3·0	·009963	16·838302	·00057 +	·003321	·00019 +

Here $X_1 = x^4\epsilon^{-x^2}$ and $X_3 = x^3\epsilon^{-x^2}$, while $X_2 = x\epsilon^{-x^2} + (2x^2+1)\int_0^x \epsilon^{-x^2}dx$.

The sum of the numbers in the fourth column is 1·69268, so that the approximate value of the integral in § 11, which is 0·4 of this, is 0·67707.

The sum of the numbers in the sixth column is 1·62601, so that the value of the integral in [the addition to] § 11 is about 0·6504.

LXXVIII.

ON THE FOUNDATIONS OF THE KINETIC THEORY OF GASES. II.

[*Transactions of the Royal Society of Edinburgh*, 1887, Vol. XXXIII.]

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IN the present communication I have applied the results of my first paper to the question of the transference of momentum, of energy, and of matter, in a gas or gaseous mixture; still, however, on the hypothesis of hard spherical particles, exerting no mutual forces except those of impact. The conclusions of §§ 23, 24 of that paper form the indispensable preliminary to the majority of the following investigations. For, except in extreme cases, in which the causes tending to disturb the "special" state are at least nearly as rapid and persistent in their action as is the process of recovery, we are entitled to assume, from the result of § 24, that in every part of a gas or gaseous mixture a local special state is maintained. And it is to be observed that this may be accompanied by a common translatory motion of the particles (or of each separate class of particles) in that region; a motion which, at each instant, may vary continuously in rate and direction from region to region; and which, in any one region, may vary continuously with time. This is a sort of generalisation of the special state, and all that follows is based on the assumption that such is the most general kind of motion which the parts of the system can have, at least in any of the questions here treated. Of course this translational speed is not the same for all particles in any small part of the system. It is merely an average, which is maintained in the same roughly approximate manner as is the "special state," and can like it be assumed to hold with sufficient accuracy to be made the basis of calculation. The mere fact that a "steady" state, say of diffusion, can be realized experimentally is a sufficient warrant for this assumption; and there seems to be no reason for supposing that the irregularities of distribution of the translatory velocity among the particles of a group should be more serious for the higher than for the lower speeds, or *vice versâ*. For each particle is sometimes a quick, sometimes a slow, moving one:—and exchanges these states many thousand times per second. All that is really required by considerations of this kind is allowed for by our way of looking at the mean free paths for different speeds.

I may take this opportunity of answering an objection which has been raised in correspondence by Professor Newcomb, and by Messrs Watson and Burbury, to a passage in § 3 of the First Part of this paper*. The words objected to are put in Italics:—

"But *the argument above shows*, further, that this density must be expressible in the form

$$f(x)f(y)f(z),$$

whatever rectangular axes be chosen, passing through the origin."

The statement itself is not objected to, but it is alleged that it does not follow from the premises assumed.

This part of my paper was introduced when I revised it for press, some months after it was read; the date of revision, not of reading, being put at the head. It was written mainly for the purpose of stringing together what had been a set of detached fragments, and was in consequence not so fully detailed as they were. I made some general statements as to the complete verification of these preliminary propositions which was to be obtained from the more complex investigations to which they led; thus showing that I attached comparatively little weight to such introductory matters. If necessary, a detailed proof can be given on the lines of § 21 of the paper. The "argument" in question, however, may be given as below. It is really involved in the italicised words of the following passage of § 1:—"in place of the hopeless question of the behaviour of innumerable absolutely isolated individuals, the comparatively simple statistical question of *the average behaviour of the various groups of a community.*"

Suppose two ideal planes, parallel to $x=0$, to move with common speed, x, through the gas. The portion of gas between them will consist of two quite distinct

* In the *Phil. Mag.*, for April 1887, the same objection is raised by Prof. Boltzmann; who has appended it to the English translation of his paper presently to be referred to. But he goes farther than the other objectors, and accuses me of reasoning in a circle.

classes of particles:—the greatly more numerous class being mere fleeting occupants, the minority being (relatively) as it were permanent lodgers. These are those whose speed perpendicular to the planes is very nearly that of the planes themselves. The *individuals* of each class are perpetually changing, those of the majority with extraordinary rapidity compared with those of the minority; but each *class*, as such, forms a definite "*group* of the community." The method of averages obviously applies to each of these classes separately; and it shows that the minority will behave, so far as y and z motions are concerned, as if they *alone* had been enclosed between two *material* planes, and as if their lines of centres at impact were always parallel to these. The instant that this ceases to be true of any one of them, that one ceases to belong to the group;—and another takes its place. Their behaviour under these circumstances (though not their number) must obviously be independent of the speed of the planes. Hence the law of distribution of components in the velocity space-diagram must be of the form

$$f(x) \cdot F(y, z);$$

and symmetry at once gives the result above.

[(*Inserted March 5th,* 1887.) Another objection, but of a diametrically opposite character, raised by Mr Burbury* and supported by Professor Boltzmann†, is to the effect that in my first paper I have unduly multiplied the number of preliminary assumptions necessary for the proof of Maxwell's Theorem concerning the distribution of energy in a *mixture* of two gases. In *form,* perhaps, I may inadvertently have done so, but certainly not in *substance.*

The assumptions which (in addition to that made at the commencement of the paper (§ 5) for provision against simultaneous impacts of three or more particles, which was introduced expressly for the purpose of making the results applicable to real gases, not merely to imaginary hard spheres,) I found it necessary to make, are (§ 18) as follows; briefly stated.

(A) That the particles of the two systems are thoroughly mixed.

(B) That the particles of each kind, separately, acquire and maintain the "special state."

(C) That there is free access for collision between each pair of particles, whether

* The Foundations of the Kinetic Theory of Gases. *Phil. Mag.* 1886, I, p. 481.

† Über die zum theoretischen Beweise des Avogadro'-schen Gesetzes erforderlichen Voraussetzungen. *Sitzb. der kais. Akad.*, XCIV., 1886, Oct. 7. In this article Prof. Boltzmann states that I have nowhere expressly pointed out that my results are applicable only to the case of hard spheres. I might plead that the article he refers to is a brief *Abstract* only of my paper; but it contains the following statements, which are surely explicit enough as to the object I had in view:—

"This is specially the case with his [Maxwell's] investigation of the law of ultimate partition of energy in a mixture of smooth spherical particles of two different kinds."

"It has since been extended by Boltzmann and others to cases in which the particles are no longer supposed to be hard smooth spheres."

"Hence it is desirable that Maxwell's proof of his fundamental Theorem should be critically examined." Then I proceed to examine it, *not* Professor Boltzmann's extension of it. In my paper itself this limitation is most expressly insisted on.

of the same kind or of different systems; and that the number of particles of one kind is not overwhelmingly greater than that of the other.

Of these, (A) and (B), though enunciated separately, are regarded as *consequences* of (C), which is thus my sole assumption for the proof of Clerk-Maxwell's Theorem. Professor Boltzmann states that the only necessary assumptions are:—that the particles of each kind be uniformly distributed in space, that they behave on the average alike in respect of all directions, and that (for any one particle?) the duration of an impact is short compared with the interval between two impacts. His words are as follows:—"Die einzigen Voraussetzungen sind, dass sowohl die Moleküle erster als auch die zweiter Gattung gleichförmig im ganzen Raume vertheilt sind, sich durchschnittlich nach allen Richtungen gleich verhalten und dass die Dauer eines Zusammenstosses kurz ist gegen die Zeit, welche zwischen zwei Zusammenstossen vergeht."

He farther states that neither system need have internal impacts; and that Mr Burbury is correct in maintaining that a system of particles, which are so small that they practically do not collide with one another, will ultimately be thrown into the "special" state by the presence of a *single* particle with which they can collide.

Assuming the usual data as to the number of particles in a cubic inch of air, and the number of collisions per particle per second, it is easy to show (by the help of Laplace's remarkable expression* for the value of $\Delta^n 0^m/n^m$ when m and n are very large numbers) that somewhere about 40,000 *years* must elapse before it would be so much as *even betting* that Mr Burbury's single particle (taken to have twice the diameter of a particle of air) had encountered, once at least, each of the $3 \,.\, 10^{20}$ very minute particles in a single cubic inch. He has not stated what is the average number of collisions necessary for each of the minute particles, before it can be knocked into its destined phase of the special state; but it must be at least considerable. Hence, even were the proposition true, æons would be required to bring about the result. As a result, it would be very interesting; but it would certainly be of no importance to the kinetic theory of gases in its practical applications.

I think it will be allowed that Professor Boltzmann's assumptions, which (it is easy to see) practically beg the whole question, are themselves inadmissible *except as consequences of the mutual impacts of the particles in each of the two systems separately.* Professor Boltzmann himself, indirectly and without any justification (such as I have at least attempted to give), *assumes* almost all that he objects to as redundant in my assumptions, with a good deal more besides. But he says nothing as to the *relative* numbers of the two kinds of particles. Thus I need not, as yet, take up the question of the validity of Professor Boltzmann's method of investigation (though, as hinted in § 31 of my first paper, I intend eventually to do so); and this for the simple reason that, in the present case, I cannot admit his premises.

* *Théorie Analytique des Probabilités*, Livre II. chap. II. 4. [In using this formula, we must make sure that the ratio m/n is sufficiently large to justify the approximation on which it is founded. It is found to be so in the present case. At my request Professor Cayley has kindly investigated the correct formula for the case in which m and n are of the *same* order of large quantities. His paper will be found in *Proc. R. S. E.*, April 4, 1887.]

Mr Burbury assumes the non-colliding particles to be in the "special state," and proceeds to prove that the single additional particle will not disturb it. But, supposing for a moment this to be true, it does *not* prove that the solitary particle would (even after the lapse of ages) reduce any non-colliding system, with positions at any instant, speeds, and lines of motion, distributed absolutely at random (for here there cannot be so much as plausible grounds for the introduction of Professor Boltzmann's assumptions) to the "special state." If it could do so, the perfect reversibility of the motions, practically limited in this case to the reversal of the motion *of the single particle alone,* shows that the single particle would (for untold ages) continue to throw a system of non-colliding particles further and further *out* of the "special" state; thus expressly contradicting the previous proposition. In this consequence of reversal we see the reason for postulating a very great number of particles of *each* kind. If Mr Burbury's sole particle possessed the extraordinary powers attributed to it, it would (except under circumstances of the most exact adjustment) not only be capable of producing, but *would* produce, absolute confusion among non-colliding particles already in the special state. Considering what is said above, I do not yet see any reason to doubt that the assumption of collisions among the particles of each kind, separately, is quite as essential to a valid proof of Maxwell's Theorem as is that of collisions between any two particles, one from each system. I have not yet seen any attempt to *prove* that two sets of particles, which have no internal collisions, will by their mutual collisions tend to the state assumed by Professor Boltzmann. Nor can I see any ground for dispensing with my farther assumption that the number of particles of one kind must not be overwhelmingly greater than that of the other. A small minority of one kind must (on any admissible assumption) have an average energy which will fluctuate, sometimes quickly sometimes very slowly, within very wide and constantly varying limits.

De Morgan* made an extremely important remark, which is thoroughly applicable to many investigations connected with the present question. It is to the effect that "no *primary* considerations connected with the subject of *Probability* can be, or ought to be, received if they depend upon the results of a complicated mathematical analysis." To this may be added the obvious remark, that the purely mathematical part of an investigation, however elegant and powerful it may be, is of no value whatever in physics unless it be based upon admissible assumptions. In many of the investigations, connected with the present subject, alike by British and by foreign authors, the above remark of De Morgan has certainly met with scant attention.]

In my first paper I spoke of the errors in the treatment of this subject which have been introduced by the taking of means before the expressions were ripe for such a process. In the present paper I have endeavoured throughout to keep this danger in view; and I hope that the results now to be given will be found, even where they are most imperfect, at least more approximately accurate than those which have been obtained with the neglect of such precautions.

* *Encyc. Metropolitana. Art.* Theory of Probabilities.

The nature of Clerk-Maxwell's earlier investigations on the Kinetic theory, in which this precaution is often neglected, still gives them a peculiar value; as it is at once obvious, from the forms of some of his results, that he must have *thought them out* before endeavouring to obtain them, or even to express them, by analysis. One most notable example of this is to be seen in his *Lemma* (*Phil. Mag.* 1860, II. p. 23) to the effect that

$$\int_{-r}^{r} \pm U x^m dx = \frac{2}{m+2} \frac{d}{dx}(U r^{m+2});$$

where U and r are functions of x, not vanishing with x, and varying but slightly between the limits $-r$ and r of x;—and where the signs in the integrand depend upon the character of m as an even or odd integer. This forms the starting point of his investigations in Diffusion and Conductivity. It is clear from the context why this curious proposition was introduced, but its accuracy, and even its exact meaning, seem doubtful.

In all the more important questions now to be treated, the mean free path of a particle plays a prominent part, and integrals involving the quantities e, or $e+e_1$ (as defined in §§ 9, 10, 28) occur throughout. We commence, therefore, with such a brief discussion of them as will serve to remove this merely numerical complication from the properly physical part of the reasoning.

X. *On the Definite Integrals,*

$$\int_0^\infty \frac{vv^r}{e} \text{ and } \int_0^\infty \frac{vv^r}{e_1 + ze_2}.$$

33. In the following investigations I employ, throughout, the definition of the mean free path for each speed as given in § 11. Thus all my results necessarily differ, at least slightly, from those obtained by any other investigator.

By § 11 we see at once that

$$\int_0^\infty \frac{vv^r}{e} = \frac{1}{n\pi s^2} \int_0^\infty \frac{\epsilon^{-hv^2} v^{r+2} dv}{\int_0^v \epsilon^{-hv_1^2}(v_1^2 + v_1^4/3v^2)\, dv_1 + \int_v^\infty \epsilon^{-hv_1^2}(vv_1/3 + v_1^3/v)\, dv_1}$$

$$= \frac{1}{n\pi s^2 \sqrt{h^r}} \int_0^\infty \frac{4x^{r+4} \epsilon^{-x^2} dx}{x\epsilon^{-x^2} + (2x^2+1)\int_0^x \epsilon^{-x^2} dx}$$

$$= \frac{C_r}{n\pi s^2 \sqrt{h^r}}, \text{ suppose.}$$

The finding of C_r is of course a matter of quadratures, as in the *Appendix* to the First Part of this paper, where the values calculated are, in this notation, C_{-1} and C_0; and Mr Clark has again kindly assisted me by computing the values of

C_1, C_3, C_5, which are those required when we are dealing with Viscosity and with Heat-Conduction in a single gas. The value of C_2 has also been found, with a view to the study of the general expression for C_r. These will be given in an *Appendix* to the present paper.

34. When we come to deal with Diffusion, except in the special case of equality of density in the gases, this numerical part of the work becomes extremely serious, even when the assumption of a "steady" state is permissible. As will be seen in § 28 of my first paper, we should have in general to deal with tables of double entry, for the expressions to be tabulated are of the form—

$$\int_0^\infty \frac{vv^r}{e_1 + ze_2} = \frac{1}{n\pi s^2\sqrt{h^r}} \int_0^\infty \frac{4x^{r+4}\epsilon^{-x^2}\,dx}{x\epsilon^{-x^2} + (2x^2+1)\int_0^x \epsilon^{-x^2}\,dx + z\left(x_1\epsilon^{-x_1^2} + (2x_1^2+1)\int_0^{x_1}\epsilon^{-x^2}\,dx\right)}$$

$$= {}_1\mathfrak{C}_r = \frac{{}_1C_r}{n\pi s^2\sqrt{h^r}}, \text{ suppose.}$$

For the second gas the corresponding quantity will be written as ${}_2\mathfrak{C}_r$. Here

$$x_1 = x\sqrt{h_1/h},$$

and
$$z = \frac{n_1 h}{nh_1}\left(\frac{s+s_1}{2s}\right)^2;$$

so that they are numerical quantities, of which the first depends on the relative masses of particles of the two gases, while the second involves, in addition, not only their relative size but also their relative number. It is this last condition which introduces the real difficulty of the question, for we have to express the value of the integral as a function of z before we can proceed with the further details of the solution, and then the equation for Diffusion ceases to resemble that of Fourier for Heat-Conduction.

The difficulty, however, disappears entirely when we confine ourselves to the study of the "steady state" (and is likewise much diminished in the study of a variable state) in the special case when the mass of a particle is the same in each of the two gaseous systems, whether the diameters be equal or no. For, in that case, we have $h_1 = h$ and $x_1 = x$, so that the factor $1/(1+z)$ can be taken outside the integral sign. Thus, instead of ${}_1\mathfrak{C}_r$, we have only to calculate C_r of the previous section.

XI. *Pressure in a Mixture of Two Sets of Spheres.*

35. Suppose there be n_1 spheres of diameter s_1 and mass P_1, and n_2 with s_2, P_2, per cubic unit. Let $s = (s_1 + s_2)/2$.

Then the average number of collisions of each P_1 with P_1s is, per second,

$$2\sqrt{\frac{2\pi}{h_1}}\, n_1 s_1^2.$$

The impulse is, on the average (as in § 30),

$$-P_1\sqrt{\frac{\pi}{2h}}.$$

Similarly, each P_1 encounters, in each second (§ 23), the average number

$$2n_2\sqrt{\frac{\pi(h_1+h_2)}{h_1h_2}}\,.\,s^2$$

of P_2s, and the average impact is

$$-\frac{P_1P_2}{P_1+P_2}\sqrt{\frac{\pi(h_1+h_2)}{h_1h_2}}.$$

Thus the average sum of impacts on a P_1 is, per second,

$$-2P_1\frac{\pi}{h_1}n_1s_1^2, \text{ due to } P_1\text{s};$$

and
$$-2\frac{P_1P_2}{P_1+P_2}\frac{h_1+h_2}{h_1h_2}\pi n_2s^2, \text{ due to } P_2\text{s}.$$

In the Virial expression $\frac{1}{2}\Sigma(Rr)$, {§ 30}, r must be taken as s_1 for the first of these portions, and as s for the second. Hence we have

$$\frac{1}{4}\Sigma(Rr) = -\frac{\pi}{2}\left\{\frac{P_1}{h_1}n_1^2s_1^3 + 2\frac{P_1P_2(h_1+h_2)}{(P_1+P_2)h_1h_2}n_1n_2s^3 + \frac{P_2}{h_2}n_2^2s_2^3\right\}$$

$$= -\frac{\pi}{n}p\{n_1^2s_1^3 + 2n_1n_2s^3 + n_2^2s_2^3\};$$

for
$$\frac{P_1}{h_1} = \frac{P_2}{h_2} = \frac{P_1+P_2}{h_1+h_2} = \frac{1}{n}\left(\frac{n_1P_1}{h_1} + \frac{n_2P_2}{h_2}\right) = \frac{2p}{n},$$

where
$$n = n_1 + n_2.$$

In the special case $s_1 = s_2 = s$, this becomes, as in § 30,

$$\frac{1}{4}\Sigma(Rr) = -\pi pns^3.$$

To obtain an idea as to how the "ultimate volume," spoken of in that section, is affected by the difference of size of the particles, suppose $n_1 = n_2$. The values of the above quantities are

$$-\frac{\pi n}{4}p\{s_1^3 + 2s^3 + s_2^3\} \text{ and } -\pi nps^3;$$

so that (as we might have expected) disparity of size, with the same mean of diameters, *increases* the quantity in question.

Thus, if
$$s_1 : s : s_2 :: 1 : 2 : 3,$$

the ratio of the expressions above is 11 : 8. The utmost value it can have (when s_1/s_2 is infinite, or is evanescent) is 5 : 2.

XII. *Viscosity.*

36. Suppose the motion of the gas, *as a whole*, to be of the nature of a simple shear; such that, relatively to the particles in the plane of yz, those in the plane x have a common speed

$$V = Bx$$

parallel to y. V, even when x is (say) a few inches, is supposed small compared with the speed of mean square. We have to determine the amount of momentum parallel to y which passes, per second, across unit area of the plane of yz.

In the stratum between x and $x+\delta x$ there are, per second per unit surface, $nve v\delta x$ collisions discharging particles with speed v to $v+dv$ (distributed uniformly in all directions) combined, of course, with the speed of translation of the stratum. The number of these particles which cross the plane of yz at angles θ to $\theta+d\theta$ with the axis of x is

$$\epsilon^{-ex\sec\theta}\sin\theta d\theta/2.$$

[Strictly speaking, the exponent should have had an additional term of the order eBx^2/v; but this is insensible compared with that retained until x is a very large multiple of the mean free path. See the remarks in § 39 below.] Each takes with it (besides its normal contribution, which need not be considered) the abnormal momentum

$$PBx,$$

relatively to yz and parallel to y.

Hence the whole momentum so transferred from x positive is

$$\frac{PBn}{2}\int_0^\infty vv\int_0^{\frac{\pi}{2}}\sin\theta d\theta\int_0^\infty \epsilon^{-ex\sec\theta}exdx,$$

or

$$\frac{PBn}{2}\int_0^\infty \frac{vv}{e}\int_0^{\frac{\pi}{2}}\cos^2\theta\sin\theta d\theta = \frac{PBn}{6}\int_0^\infty \frac{vv}{e}.$$

Doubling this, to get the full differential effect across the plane of yz, it becomes (§ 33)

$$\frac{PBnC_1}{3\pi ns^2\sqrt{h}} = \frac{PBn\,0{\cdot}838}{3\pi ns^2\sqrt{h}}.$$

The multiplier of B, *i.e.* of dV/dx, is the coefficient of Viscosity. Its numerical value, in terms of density and mean path, is

$$\frac{\rho\lambda}{\sqrt{h}}0{\cdot}412.$$

Clerk-Maxwell, in 1860, gave the value

$$\frac{\rho l}{\sqrt{h}}0{\cdot}376,$$

which (because $l = 707\lambda/677$, as in § 11) differs from this in the ratio 20 : 21. In this case the short cuts employed have obviously entailed little numerical error. Since $\rho\lambda$ is constant for any one gas, the Viscosity (as Maxwell pointed out) is independent of the density.

37. Both expressions are proportional to the square-root of the absolute temperature. We may see at once that, on the hypothesis we have adopted, such must be the case. For, if we suppose the speed of every sphere to be suddenly increased m fold, the operations will go on precisely as before, only m times faster. But the absolute temperature will be increased as $m^2 : 1$. Similar anticipations may be made in the cases of Diffusion and of Thermal Conductivity.

Maxwell was led by his experimental measures of Viscosity, which seemed to show* that it increases nearly in proportion to the first power of the absolute temperature, to discard the notion of hard spheres, and to introduce the hypothesis of particles repelling one another with force inversely as the fifth power of the distance. I have already stated that there are very grave objections to the introduction of *repulsion* into this subject, except of course in the form of elastic restitution. That the particles of a gas have *this* property is plain from their capability of vibrating, so that they must lose energy of translation by impact; and I intend, in the next instalment of this investigation, so far to modify the fundamental assumption hitherto made as to deduce the effects corresponding to a coefficient of restitution less than unity; and also to take account of molecular *attraction*, specially limited in its range to distances not much greater than the diameter of a sphere.

XIII. *Thermal Conductivity.*

38. We must content ourselves with the comparatively simple case of the steady flow of heat in one direction; say parallel to the axis of x. This will be assumed to be vertical, the temperature in the gas increasing upwards, so as to prevent convection currents. No attention need, otherwise, be paid to the effects of gravity.

Hence the following conditions must be satisfied:—

(*a*) Each horizontal layer of the gas is in the special state, compounded with a definite translation *vertically*.

(*b*) The pressure is constant throughout the gas.

(*c*) There is, on the whole, no passage of gas across any horizontal plane.

(*d*) Equal amounts of energy are, on the whole, transferred (in the same direction) across unit area of all such planes.

39. Let n be the number of particles per unit volume in the layer between x and $x+dx$; ν the fraction of them whose speed, relatively to the neighbours as a whole, lies between v and $v+dv$; α the speed of translation of the layer.

* Cf., however, Stokes, *Phil. Trans.*, 1886, vol. CLXXVII. p. 786.

The *number* of particles which pass, per unit area per second, from x positive through the plane $x=0$, is the sum of those escaping, after collision, from all the layers for positive x, and not arrested on their way:—viz.,

$$\frac{1}{2}\int_0^\infty \int_0^{\frac{\pi}{2}} \int_0^a nvve\epsilon^{-\sec\theta\int_0^x edx} \sin\theta d\theta \frac{v\cos\theta - \alpha}{v\cos\theta}\, dx.$$

Here a, though in any ordinary case it need not be more than a very small fraction of an inch, is a quantity large compared with the mean free path of a particle. Its value will be more exactly indicated when the reason for its introduction is pointed out.

The last factor of the integrand depends on the fact that the particles are emitted from *moving* layers:—involving the so-called Döppler, properly the Römer, principle.

We neglect, however, as insensible the difference between the absorption due to *slowly* moving layers and that due to the same when stationary.

Because a, the range of x, is small we may write with sufficient approximation

$$n = n_0 + n_0'x, \text{ \&c., \&c.}$$

Introducing this notation, the expression above becomes

$$\frac{1}{2}\int_0^\infty \int_0^{\frac{\pi}{2}} \int_0^a n_0v_0ve_0\left\{1+\left(\frac{n_0'}{n_0}+\frac{v_0'}{v_0}+\frac{e_0'}{e_0}\right)x+\ldots\right\}\epsilon^{-\sec\theta\int_0^x edx} \sin\theta d\theta \frac{v\cos\theta - \alpha}{v\cos\theta}\, dx.$$

Now, to the degree of approximation adopted,

$$\int_0^x edx = e_0x + e_0'x^2/2.$$

The second term of this must always be very small in comparison with the first, even for an exceptionally long free path. But, if we were to make

$$x = 2e_0/e_0',$$

the second term would become *equal* to the first. Hence a, the upper limit of the x integration, must be made much smaller than this quantity. Thus we may write

$$\epsilon^{-\sec\theta\int_0^x edx} = \epsilon^{-e_0x\sec\theta}(1 - e_0'x^2\sec\theta/2 + \ldots).$$

We said, above, that $$e_0a = a\bigg/\frac{1}{e_0}$$

is a large number, say of the order 10^2. It appears then at once that terms in

$$\epsilon^{-e_0a} = \epsilon^{-100} = 10^{-43} \text{ nearly}$$

may be neglected. Such terms occur at the upper limit in the integration with regard to x above, and what we have said shows, *first* why a had to be introduced, *second* why it disappears from the result.

Writing now only those factors of the above expression which are concerned in the integration with respect to x, we have

$$\int_0^a \left\{1 + \left(\frac{n_0'}{n_0} + \frac{\nu_0'}{\nu_0} + \frac{e_0'}{e_0}\right)x + \ldots\right\}(1 - e_0' x^2 \sec\theta/2 + \ldots)\,\epsilon^{-e_0 x \sec\theta} dx,$$

or

$$\frac{1}{e_0}\left\{\cos\theta + \frac{1}{e_0}\left(\frac{n_0'}{n_0} + \frac{\nu_0'}{\nu_0}\right)\cos^2\theta\right\}.$$

The terms in e_0' are found to have cancelled one another, a result which greatly simplifies the investigation.

Had we complicated matters by introducing $\alpha_0 + \alpha_0' x$ in place of α, the term in α_0' (which, if it exist at all, is at least very small) would have been divided on integration *twice* by e_0, a quantity whose value is, on the average, of the order 5.10^5 (to an inch as unit of length).

The expression now becomes

$$\frac{1}{2}\int_0^\infty\int_0^{\frac{\pi}{2}} n\nu\left\{1 + \left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)\frac{\cos\theta}{e}\right\}(v\cos\theta - \alpha)\sin\theta d\theta.$$

We have omitted the zero suffixes, as no longer required; and, as the plane $x = 0$ is arbitrary, the expression is quite general.

Omitting the product of the two small terms, and integrating with respect to θ, we have

$$\frac{1}{2}\int_0^\infty n\nu\left\{v/2 - \alpha + \left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)v/3e\right\}.$$

The corresponding expression for the number of particles which pass through the plane from the negative side is, of course, to be obtained by simply changing the signs of the two last terms. Thus, by (c) of § 38, we have

$$\int_0^\infty n\nu\left\{\alpha - \left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)v/3e\right\} = 0,$$

or

$$\alpha = \int_0^\infty \nu\left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)v/3e \quad \text{...(1).}$$

40. The *pressure* at the plane $x = 0$, taken as the whole momentum (parallel to x) which crosses it per unit area per second, is to be found by introducing into our first integrand the additional factor

$$P(v\cos\theta - \alpha),$$

where P is the mass of a particle. There results

$$\frac{P}{2}\int_0^\infty n\nu\left\{v^2/3 - v\alpha + \left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)v^2/4e\right\}.$$

We must take the *sum* of this, and of the same with the signs of the two last terms changed; so that the pressure (which is constant throughout, by (*b*) of § 38) is

$$p = \frac{P}{3}\int_0^\infty n\nu v^2 = \frac{Pn}{2h} \quad \text{.....................................(2).}$$

Thus n/h is constant throughout the gas.

[If a very small, thin, disc were placed in the gas, with its plane parallel to yz, *and the steady state not thereby altered*, the difference of pressures on its sides would be

$$nP\int_0^\infty \nu \left\{2v\alpha - \left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)v^2/2e\right\},$$

or (see § 42 below) $$p\frac{\lambda}{0{\cdot}677}\frac{h'}{h}\left\{\frac{8}{3\sqrt{\pi}}\left(\frac{5}{2}C_1 - C_3\right) - \frac{5}{2}C_2 + C_4\right\}.$$

For ordinary pressures, and a temperature gradient 10° C. per inch, this is of the order 10^{-7} atmosphere only.]

41. For the *energy* which passes per second per unit of area across $x = 0$, we must introduce into the first integrand of § 39 the additional factor

$$\frac{P}{2}(v^2 - 2v\alpha\cos\theta);$$

and the result of operations similar to those for the number of particles is

$$E = -\frac{P}{6}\int_0^\infty n\nu v^3\left\{\left(\frac{n'}{n} + \frac{\nu'}{\nu}\right)\Big/ e - 5\alpha/v\right\} \quad \text{.............................(3).}$$

This expresses the excess of the energy passing from the negative to the positive side of $x = 0$, over that passing from positive to negative; and, by (*d*) of § 38, must be constant.

42. To put (1) and (3) in a more convenient and more easily intelligible form, note that because

$$\nu = 4\sqrt{\frac{h^3}{\pi}}\,\epsilon^{-hv^2}v^2dv,$$

we have $$\frac{\nu'}{\nu} = \frac{3}{2}\frac{h'}{h} - h'v^2.$$

But, by (2), $$\frac{n'}{n} = \frac{h'}{h}.$$

Thus, by (1), $$\alpha = \frac{h'}{h}\int_0^\infty \nu v\left(\frac{5}{2} - hv^2\right)\Big/ 3e,$$

$$= \frac{h'}{3\sqrt{h^3}}\frac{1}{n\pi s^2}\left(\frac{5}{2}C_1 - C_3\right)$$

$$= \frac{h'}{\sqrt{h^5}}\frac{P}{6p\pi s^2}\left(\frac{5}{2}C_1 - C_3\right) \quad \text{.....................................(1').}$$

Similarly (3) becomes $$E = \frac{h'}{\sqrt{h^5}}\frac{P}{6\pi s^2}\left(\frac{25}{4}C_1 - 5C_3 + C_5\right) \quad \text{..........................(3').}$$

43. The only variable factor $(h'/h^{\frac{5}{2}})$ in these expressions for α, and for E, is the same in both. Hence, as E does not vary with x, $h'/h^{\frac{5}{2}}$ is constant, and so also is α. Thus since, if τ be absolute temperature, we have

$$h\tau = \text{constant};$$

we find at once, $$\tau^{\frac{3}{2}} = A + Bx.$$

Thus the distribution of temperature, and therefore that of density, is determined when the terminal conditions are given. The formula just given agrees with the result first obtained by Clausius in an extremely elaborate investigation*, in which he showed that Maxwell's earliest theory of Heat-Conduction by gases is defective.

The general nature of the motion of the gas is now seen to be analogous to that of liquid mud when a scavenger tries to sweep it into a heap. The broom produces a translatory motion of the mud, which is counteracted by gravitation-sliding due to the surface gradient:—just as the displacement (by translation) of the whole gas, from hot to cold, is counteracted by the greater number of particles discharged (after collisions) from a colder and denser layer, than from an adjoining warmer and less dense layer.

44. The results of calculation of values of C_r given in the *Appendix* enable us to put the expressions (1′) and (3′) into the more convenient forms

$$\alpha = \frac{h'}{\sqrt{h^5}} \frac{\rho\lambda}{p} 0{\cdot}06 \qquad\qquad (1''),$$

$$E = \frac{h'}{\sqrt{h^5}} \rho\lambda 0{\cdot}45 \qquad\qquad (3''),$$

where it is to be remarked that the product $\rho\lambda$ is independent of the temperature of the gas.

The Conductivity, k, is defined by the equation

$$k\frac{d\tau}{dx} = -E,$$

and thus its value is $$k = \sqrt{\frac{\tau}{\tau_0^3}} \frac{\rho\lambda}{\sqrt{h_0^3}} 0{\cdot}45,$$

where τ_0, h_0 are simultaneous values of τ and h.

At 0° C. (*i.e.* $\tau = 274$) this is, for air, nearly $3 . 10^{-5}$ in thermal units on the pound-foot-minute-Centigrade system:—*i.e.* about 1/28,000 of the conductivity of iron, or 1/3600 of that of lead†. Of course, with our assumption of hard spherical particles, we have not reckoned the part of the conducted energy which, in real gases, is due to rotation or to vibration of individual particles.

* *Pogg. Ann.*, cxv. 1862; *Phil. Mag.*, 1862, I.

† *Trans. R. S. E.*, 1878, p. 717.

XIV. *Diffusion.*

45. The complete treatment of this subject presents difficulties of a very formidable kind, several of which will be apparent even in the comparatively simple case which is treated below. We take the case of a uniform vertical tube, of unit area in section, connecting two vessels originally filled with different gases, or (better) mixtures of the same two gases in different proportions, both, however, maintained at the same temperature; and we confine ourselves to the investigation of the motion when it can be treated as approximately steady. We neglect the effect of gravity (the denser gas or mixture being the lower), and we suppose the speeds of the group-motions to be very small in comparison with the speed of mean square in either gas. [In some of the investigations which follow, there are (small) parts of the diffusion-tube in which one of the gases is in a hopeless minority as regards the other. Though one of the initial postulates (*d* of § 1) is violated, I have not thought it necessary to suppress the calculations which are liable to this objection; for it is obvious that the conditions, under which alone it could arise, are unattainable in practice.]

Clerk-Maxwell's Theorem (§ 15), taken in connection with our preliminary assumption, shows that at every part of the tube the number of spheres per cubic unit, and their average energy, are the same. Hence, if n_1, n_2, be the numbers of the two kinds of spheres, per cubic unit, at a section x of the tube

$$n_1 + n_2 = n = \text{constant} \qquad \text{(1)}.$$

Also, if P_1, P_2, be the masses of the spheres in the two systems respectively, h_1 and h_2 the measures (§ 3) of their mean square speeds, we have

$$P_1/h_1 = P_2/h_2 = (n_1P_1/h_1 + n_2P_2/h_2)/n = 2p/n \qquad \text{(2)},$$

where p is the constant pressure.

Strictly speaking, the fact that there is a translational speed of each layer of particles must affect this expression, but only by terms of the first order of small quantities.

46. The number of particles of the P_1 kind which pass, on the whole, towards positive x through the section of the tube is (as in § 39)

$$n_1\alpha_1 - n_1' \int_0^\infty v_1 v/3e_1 ;$$

where α_1 is the (common) translational speed of the P_1's, and $1/e_1$ the mean free path of a P_1 whose speed is v. We obtain this by remarking that, in the present problem, h_1 is regarded as constant, so that there is no term in v_1'.

Hence, if G_1 be the mass of the first gas on the negative side of the section, divided by the area of the section, we have

$$\frac{dG_1}{dt} = -P_1\left(n_1\alpha_1 - n_1'{}_1\mathfrak{C}_1/3\right) \qquad \text{(3)}.$$

If G_2 be the corresponding mass of the second gas, we have (noting that, by (1), $n_1' + n_2' = 0$)

$$\frac{dG_2}{dt} = -P_2 (n_2\alpha_2 + n_1'{}_2\mathfrak{C}_1/3) \quad\text{.......................................(4).}$$

From the definitions of the quantities G_1, G_2, we have also

$$\left.\begin{array}{ll} \dfrac{dG_1}{dx} = P_1 n_1, & \dfrac{d^2G_1}{dx^2} = P_1 n_1' \\ \dfrac{dG_2}{dx} = P_2 n_2, & \dfrac{d^2G_2}{dx^2} = -P_2 n_1' \end{array}\right\} \quad\text{.......................................(5).}$$

47. We have now to form the equations of motion for the layers of the two gases contained in the section of the tube between x and $x + \delta x$. The increase of momentum of the P_1 layer is due to the difference of pressures, behind and before, caused by P_1's; minus the resistance due to that portion of the impacts of some of the P_1's against P_2's in the section itself, which depends upon the relative speeds of the two systems, each as a whole. This is a small quantity of the order the whole pressure on the surfaces of the particles multiplied by the ratio of the speed of translation to that of mean square. The remaining portion (relatively very great) of the impacts in the section is employed, as we have seen, in maintaining or restoring the "special state" in each gas, as well as the Maxwell condition of partition of energy between the two gases. If R be the resistance in question, the equations of motion are

$$\left.\begin{array}{l} \dfrac{\partial}{\partial t}(P_1 n_1 \alpha_1 \delta x) = -\dfrac{1}{2}\dfrac{d}{dx}\left(\dfrac{P_1 n_1}{h_1}\right)\delta x - R\delta x \\ \dfrac{\partial}{\partial t}(P_2 n_2 \alpha_2 \delta x) = -\dfrac{1}{2}\dfrac{d}{dx}\left(\dfrac{P_2 n_2}{h_2}\right)\delta x + R\delta x \end{array}\right\} \quad\text{.................................(6),}$$

where ∂ represents *total* differentiation.

48. To calculate the value of R, note that, in consequence of the assumed smallness of α_1, α_2, relatively to the speeds of mean square of the particles, the number of collisions of a P_1 with a P_2, and the circumstances of each, may be treated as practically the same as if α_1 and α_2 were each zero:—*except* in so far that there will be, in the expression for the relative speed in the direction of the line of centres at impact, an additional term

$$(\alpha_1 - \alpha_2)\cos\psi,$$

where ψ is the inclination of the line of centres to the axis of x. Thus to the impulse, whose expression is of the form

$$-\frac{2PQ}{P+Q}(\mathrm{u} - \mathrm{v}),$$

as in § 19 of the First Part of the paper, there must be added the term we seek, viz.,

$$-\frac{2P_1P_2}{P_1+P_2}(\alpha_1 - \alpha_2)\cos\psi.$$

This must be resolved again parallel to x, for which we must multiply by $\cos\psi$. Also, as the line of centres may have with equal probability all directions, we must multiply further by $\sin\psi d\psi/2$, and integrate from 0 to π. The result will be the average transmission, per collision, per P_1, of *translatory* momentum of the layer parallel to x. Taking account of the number of impacts of a P_1 on a P_2, as in § 23, we obtain finally

$$R = \frac{4}{3} n_1 n_2 s^2 \sqrt{\frac{\pi(h_1+h_2)}{h_1 h_2}} \frac{P_1 P_2}{P_1 + P_2} (\alpha_1 - \alpha_2) \quad \text{.......................} (7),$$

where s is the semi-sum of the diameters of a P_1 and a P_2.

49. To put this in a more convenient form, note that (2), in the notation of (5), gives us the relation

$$\frac{1}{h_1}\frac{dG_1}{dx} + \frac{1}{h_2}\frac{dG_2}{dx} = 2p,$$

whence $$G_1/h_1 + G_2/h_2 = 2px \quad \text{.................................} (8).$$

We have not added an arbitrary constant, for no origin has been specified for x. Nor have we added an arbitrary function of t, because (as will be seen at once from (3)) this could only be necessary in cases where the left-hand members of (6) are quantities comparable with the other terms in these equations. They are, however, of the order of

$$\frac{d^2G_1}{dt^2}, \quad \frac{d^2G_1}{dx\,dt}\alpha_1, \quad \&c.,$$

and cannot rise into importance except in the case of motions much more violent than those we are considering.

From (8) we obtain $$\frac{dG_1}{dt}\Big/h_1 + \frac{dG_2}{dt}\Big/h_2 = 0 \quad \text{...................................} (9),$$

which signifies that equal *volumes* of the two gases pass, in the same time, in opposite directions through each section of the tube. This gives a general description of the nature of the cases to which our investigations apply.

But, by (3) and (4), we have for the value of

$$P_1 P_2 n_1 n_2 (\alpha_1 - \alpha_2)$$

the expression $$-P_2 n_2 \left(\frac{dG_1}{dt} - \frac{1}{3} P_1 n_1{}'_1 \mathfrak{C}_1\right) + P_1 n_1 \left(\frac{dG_2}{dt} + \frac{1}{3} P_2 n_1{}'_2 \mathfrak{C}_1\right);$$

or, by (9), (2), and (5)

$$-2ph_2 \left(\frac{dG_1}{dt} - \frac{1}{3n}\frac{d^2G_1}{dx^2}(n_{2\,1}\mathfrak{C}_1 + n_{1\,2}\mathfrak{C}_1)\right).$$

Substituting this for the corresponding factors of R in the first of equations (6), and neglecting the left-hand side, we have finally

$$0=-\frac{1}{2h_1}\frac{d^2G_1}{dx^2}+\frac{8}{3}s^2\sqrt{\frac{\pi(h_1+h_2)}{h_1h_2}}\frac{ph_2}{P_1+P_2}\left\{\frac{dG_1}{dt}-\frac{1}{3n}\frac{d^2G_1}{dx^2}(n_{21}\mathfrak{C}_1+n_{12}\mathfrak{C}_1)\right\},$$

or
$$\frac{dG_1}{dt}=\left(\frac{3}{16s^2}\frac{P_1+P_2}{\sqrt{\pi(h_1+h_2)h_1h_2}}\cdot\frac{1}{p}+\frac{1}{3n}(n_{21}\mathfrak{C}_1+n_{12}\mathfrak{C}_1)\right)\frac{d^2G_1}{dx^2};$$

or, somewhat more elegantly,

$$\frac{dG_1}{dt}=\left(\frac{3}{8ns^2}\sqrt{\frac{h_1+h_2}{\pi h_1h_2}}+\frac{1}{3n}(n_{21}\mathfrak{C}_1+n_{12}\mathfrak{C}_1)\right)\frac{d^2G_1}{dx^2}\;\ldots\ldots\ldots\ldots\ldots\ldots\;(10).$$

50. This equation resembles that of Fourier for the linear motion of heat; but, as already stated in § 34, the quantities $\mathfrak{C}_1$ which occur in it render it in general intractable. The first part of what is usually called the *diffusion-coefficient* (the multiplier of d^2G_1/dx^2 above) is constant; but the second, as is obvious from (5) and (8), is, except in the special case to which we proceed, a function of dG_1/dx; *i.e.* of the percentage composition of the gaseous mixture.

51. In the special case of equality, both of mass and of diameter, between the particles of the two systems, the diffusion-coefficient becomes

$$D=\frac{3}{8ns^2}\sqrt{\frac{2}{\pi h}}+\frac{C_1}{3n\pi s^2\sqrt{h}},$$

or
$$D=\left(\frac{3}{4}\sqrt{\frac{\pi}{2}}+\frac{C_1}{3}\right)\frac{\lambda}{0{\cdot}677\sqrt{h}}=\frac{\lambda}{\sqrt{h}}1{\cdot}8,$$

where λ is the mean free path in the system. Hence the diffusion-coefficient among equal particles is directly as the mean free path, and as the square root of the absolute temperature. Fourier's solutions of (10) are of course applicable in this special case.

If we now suppose that our arrangement is a tube of length l and section S, connecting two infinite vessels filled with the two gases respectively; and, farther, assume that the diffusion has become steady, the equation (10) becomes

$$\frac{dG_1}{dt}=D\frac{d^2G_1}{dx^2},$$

where the left-hand member is constant. Also, it is clear that, since dG_1/dx must thus be a *linear* function of x, we have

$$\frac{dG_1}{dx}=Pn_1=Pn\left(1-\frac{x}{l}\right),$$

so that the mass of either gas which passes, per second, across any section of the tube is

$$SD\rho/l,$$

where ρ is the common density of the two gases.

For comparison with the corresponding formulæ in the other cases treated below, we may now write our result as

$$l\frac{dG_1}{dt} = -\frac{P}{\pi s^2\sqrt{h}}1\cdot22.$$

Also, to justify our assumption as to the order of the translatory speed, we find by (3)

$$\alpha_1 = \frac{1\cdot38\lambda}{(l-x)\sqrt{h}}.$$

Hence, except where $l-x$ is of the order of one-thousandth of an inch or less, this is very small compared with $h^{-\frac{1}{2}}$. And it may safely be taken as impossible that n_1 can (experimentally) be kept at 0 at the section $x=l$.

If the vessels be of finite size, and if we suppose the contents of each to be always thoroughly mixed, we can approximate to the law of mixture as follows. On looking back at the last result, we see that for ρ we must now substitute the *difference* of densities of the first gas at the ends of the connecting tube. Let g_1, g_2 be the quantities of the two gases which originally filled the vessels respectively; and neglect, in comparison with them, the quantity of either gas which would fill the tube. Then, obviously,

$$\frac{dG_1}{dt} = -\frac{SD\rho}{l}\left(\frac{G_1}{g_1} - \frac{g_1 - G_1}{g_2}\right),$$

whence $$G_1 = \frac{g_1 g_2}{g_1 + g_2}\left\{\frac{g_1}{g_2} + \epsilon^{-\frac{SD\rho}{l}\frac{g_1+g_2}{g_1 g_2}t}\right\}.$$

This shows the steps by which the initial state $(g_1,\ 0)$ tends asymptotically to the final state $\left(\frac{g_1}{g_1+g_2}g_1,\ \frac{g_2}{g_1+g_2}g_1\right)$, in which the gases are completely mixed. When the vessels are equal this takes the simple form

$$G_1 = \frac{g}{2}\left(1 + \epsilon^{-\frac{2SD\rho t}{gl}}\right).$$

52. In the case just treated there is no transmission of energy, so that the fundamental hypotheses are fully admissible. In general, however, it is not so. The result of § 41, properly modified to apply to the present question, shows that the energy which, on the whole, passes positively across the section x is, per unit area per second,

$$\frac{5}{4}\left(\frac{P_1 n_1 \alpha_1}{h_1} + \frac{P_2 n_2 \alpha_2}{h_2}\right) - \frac{1}{6}n_1'(P_{1\,1}\mathfrak{C}_3 - P_{2\,2}\mathfrak{C}_3).$$

This, of course, in general differs from section to section, and thus a disturbance of temperature takes place. In such a case we can no longer assume that h_1 and h_2 are absolute constants; and thus terms in $\mathfrak{C}_5$ would come in; just as a term in C_5 appeared in the expression for energy conducted (§ 42). Thus, in order that our investigation may be admissible, the process must be conducted at constant temperature. This, in general, presupposes conditions external to the apparatus.

53. Though it appears hopeless to attempt a general solution of equation (10), we can obtain from it (at least approximately) the conditions for a steady state of motion such as must, we presume, finally set in between two infinite vessels filled with different gases at the same temperature and pressure. For the left-hand member is then an (unknown) constant, a second constant is introduced by integrating once with respect to x; and these, which determine the complete solution, are to be found at once by the terminal conditions

$$\frac{1}{P_1}\frac{dG_1}{dx} = n_1 = \begin{cases} n \text{ for } x = 0 \\ 0 \quad ,, \quad = l \end{cases} \qquad \text{(11)}.$$

And, by a slight but obvious modification of the latter part of § 51 above, we can easily extend the process to the case in which the vessels are of finite size:— always, however, on the assumption that their contents may be regarded as promptly assuming a state of uniform mixture. The consideration of § 52, however, shows that the whole of the contents must be *kept* at constant temperature, in order that this result may be strictly applicable.

54. Recurring to the special case of § 51, let us now suppose that, while the masses of the particles remain equal, their diameters are different in the two gases. Thus, suppose $s_1 > s_2$. Then it is clear that

$$s_1^2 - s^2, \text{ and } s^2 - s_2^2,$$

are both positive. In this case, infinite terminal vessels being supposed, (10) gives for the steady state

$$A = \frac{P}{\pi n\sqrt{h}}\left\{\frac{3}{4s^2}\sqrt{\frac{\pi}{2}} + \frac{C_1}{3}\left(\frac{n_2}{n_1 s_1^2 + n_2 s^2} + \frac{n_1}{n_1 s^2 + n_2 s_2^2}\right)\right\}\frac{dn_1}{dx} \qquad \text{(12)};$$

whose integral, between limits as in (11) above, is

$$-Al = \frac{P}{\pi n\sqrt{h}}\left\{\frac{3n}{4s^2}\sqrt{\frac{\pi}{2}} + \frac{C_1 n}{3}\left(\frac{1}{s^2 - s_2^2} - \frac{1}{s_1^2 - s^2} + \frac{2s_1^2}{(s_1^2 - s^2)^2}\log\frac{s_1}{s} + \frac{2s_2^2}{(s^2 - s_2^2)^2}\log\frac{s_2}{s}\right)\right\}.$$

Here A is the rate of passage of the first gas, in *mass* per second per unit area of the section of the tube.

If now we put $\qquad s_1 = s + \sigma, \quad s_2 = s - \sigma,$

then, when σ is small compared with s, the multiplier of $C_1 n/3$ is

$$(1 + \sigma^2/3s^2)/s^2, \text{ nearly.}$$

When σ is nearly equal to s, *i.e.* one of the sets of particles exceedingly small compared with the other, it is nearly

$$1{\cdot}283/s^2.$$

Thus it appears that a difference in size, the mean of the diameters being unchanged, favours diffusion.

Suppose, for instance, $\quad s_1 : s : s_2 :: 3 : 2 : 1,$

and we have
$$A = -\frac{P}{\pi l s^2 \sqrt{h}} \left\{\frac{3}{4}\sqrt{\frac{\pi}{2}} + \frac{2C_1}{3}\left(\frac{4}{15} + \frac{36}{25}\log\frac{3}{2} + \frac{4}{9}\log\frac{1}{2}\right)\right\},$$
$$= -\frac{P}{\pi l s^2 \sqrt{h}} \left\{\frac{3}{4}\sqrt{\frac{\pi}{2}} + \frac{C_1}{3}\,1{\cdot}085\right\} = -\frac{P}{\pi l s^2 \sqrt{h}}\,1{\cdot}24,$$
$$= -\frac{\rho\lambda}{l\sqrt{h}}\,1{\cdot}83.$$

Compare this with the result for equal particles (§ 51), remembering that λ now stands for the mean free path of a particle of either gas in a space filled with the other:—and we see that (so long at least as the masses are equal) diffusion depends mainly upon the mean of the diameters, being but little affected by even a considerable disparity in size between the particles of the two gases. Thus it appears that the viscosity and (if the experimental part of the inquiry could be properly carried out) conductivity give us much more definite information as to the relative sizes of particles of different gases than we can obtain from the results of diffusion.

Equation (12) shows how the *gradient* of density of either gas varies, in the stationary state, with its percentage in the mixture. For the multiplier of $\frac{dn_1}{dx}$ is obviously a maximum when

$$\frac{1}{s^2 + y s_1^2} + \frac{1}{s^2 + s_2^2/y},$$

in which $y = n_1/n_2$, is so. This condition gives

$$n_1/n_2 = y = s_2/s_1.$$

Hence the gradient is *least* steep at the section in which the proportion of the two gases is inversely as the ratio of the diameters of their particles; and it increases either way from this section to the ends of the tube, at each of which it has the same (greatest) amount. This consideration will be of use to the full understanding of the more complex case (below) in which the masses, as well as the diameters, of the particles differ in the two gases.

55. Let us now suppose the mass per particle to be different in the two gases. The last terms of the right-hand side of (10), viz.,

$$\frac{1}{3n}\left(n_{2\,1}\mathfrak{C}_1 + n_{1\,2}\mathfrak{C}_1\right)\frac{d^2G_1}{dx^2},$$

may be written in the form

$$\frac{P_1}{3\pi n}\frac{dn_1}{dx}\left\{\frac{(n-n_1)h_2}{\sqrt{h_1}}\int_0^\infty \frac{f(y)\,dy}{n_1h_2s_1^2F(y)+(n-n_1)h_1s^2F\left(y\sqrt{\frac{h_2}{h_1}}\right)}\right.$$

$$\left.+\frac{n_1h_1}{\sqrt{h_2}}\int_0^\infty \frac{f(y)\,dy}{(n-n_1)h_1s_2^2F(y)+n_1h_2s^2F\left(y\sqrt{\frac{h_1}{h_2}}\right)}\right\}$$

where the meanings of f and F are as in § 34.

If we confine ourselves to the steady state, we may integrate (10) directly with respect to x, since dG_1/dt is constant. In thus operating on the part just written, the integration with regard to x (with the limiting conditions as in (11)) can be carried out under the sign of integration with respect to y:—and then the y integration can be effected by quadratures.

The *form* of the x integral is the same in each of the terms. For

$$\int_n^0 \frac{(n-n_1)\,dn_1}{An_1+B(n-n_1)}=\int_n^0 \frac{n_1dn_1}{A(n-n_1)+Bn_1}=\frac{n}{A-B}\left\{1+\frac{A}{A-B}\log\frac{B}{A}\right\}.$$

This expression is necessarily negative, as A and B are always positive. When A and B are nearly equal, so that $B=(1+e)A$, its value is

$$-\frac{n}{A}\left(\frac{1}{2}-\frac{e}{3}+\ldots\ldots\right),$$

so that, even when A and B are equal, there is no infinite term.

It is easy to see, from the forms of $F(y)$, and of its first two differential coefficients, that the equation

$$h_2s_1^2F(y)=h_1s_2^2F\left(y\sqrt{\frac{h_2}{h_1}}\right)$$

can hold for, at most, *one* finite positive value of y.

56. As a particular, and very instructive case, let us suppose

$$P_1 : P_2 :: h_1 : h_2 :: 16 : 1,$$

the case of oxygen and hydrogen.

(*a*) First, assume the diameters to be equal. Then the integral of (10), with limits as in (11), taken on the supposition that the flow is constant, is

$$l\frac{dG_1}{dt}=-\frac{P_1}{\pi s^2\sqrt{h_1}}\left\{\frac{3}{8}\sqrt{17\pi}-\frac{1}{3}\int_0^\infty dy\left(\frac{f(y)-16f\left(\frac{y}{4}\right)}{F(y)-16F\left(\frac{y}{4}\right)}\right.\right.$$

$$\left.\left.+\frac{F(y)f(y)-16^2F\left(\frac{y}{4}\right)f\left(\frac{y}{4}\right)}{\left\{F(y)-16F\left(\frac{y}{4}\right)\right\}^2}\log\frac{16F\left(\frac{y}{4}\right)}{F(y)}\right)\right\}.$$

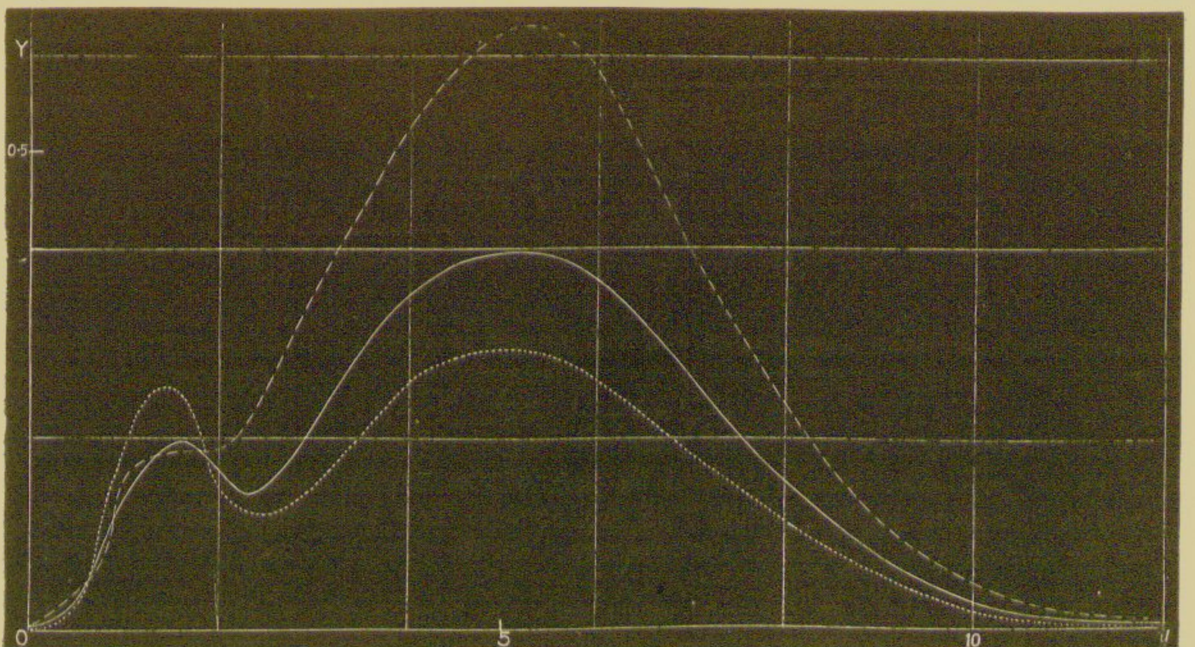
As remarked above, the definite integral is essentially negative. For so is every expression of the form

$$\frac{a-b}{A-B}+\frac{Aa-Bb}{(A-B)^2}\log\frac{B}{A}$$

provided A, B, a, and b be all positive. When A and B are equal its value is

$$-\frac{1}{2A}(a+b).$$

I have made a rough attempt at evaluation of the integral, partly by calculation, partly by a graphic method. My result is, at best, an approximation, for the various instalments of the quadrature appear as the relatively small *differences* of two considerable quantities. Thus the three decimal places, to which, from want of leisure, I was obliged to confine myself, are not sufficient to give a very exact value. The graphical representations of my numbers were, however, so fairly smooth that there seems to be little risk of large error. The *full* curve in the sketch below shows (on a ten-fold scale) the values of the integrand (with their signs



changed), as ordinates, to the values of y as abscissæ. The area is about $-2{\cdot}165$. Hence we have

$$t\frac{dG_1}{dt}=-\frac{P_1}{\pi s^2\sqrt{h_1}}\,3{\cdot}463.$$

(*b*) Suppose next that the diameter of a P_1 is three times that of a P_2, but the semi-sum of the diameters is s as before. The definite integral takes the form

$$\int_0^\infty dy \left\{ \frac{f(y)}{\frac{9}{4}F(y) - 16F\left(\frac{y}{4}\right)} - \frac{16f\left(\frac{y}{4}\right)}{F(y) - 4F\left(\frac{y}{4}\right)} \right.$$

$$\left. + \frac{\frac{9}{4}F(y)f(y)}{\left\{\frac{9}{4}F(y) - 16F\left(\frac{y}{4}\right)\right\}^2} \log \frac{64F\left(\frac{y}{4}\right)}{9F(y)} - \frac{64F\left(\frac{y}{4}\right)f\left(\frac{y}{4}\right)}{\left\{F(y) - 4F\left(\frac{y}{4}\right)\right\}^2} \log \frac{4F\left(\frac{y}{4}\right)}{F(y)} \right\}.$$

The corresponding curve is exhibited by the dashed line in the sketch, and its area is about $-3{\cdot}157$. Hence, in this case,

$$l\frac{dG_1}{dt} = -\frac{P_1}{\pi s^2 \sqrt{h_1}} 3{\cdot}793.$$

(*c*) Still keeping the sum of the semidiameters the same, let the diameter of a P_2 be three times that of a P_1. The integral is

$$\int_0^\infty dy \left\{ \frac{f(y)}{\frac{1}{4}F(y) - 16F\left(\frac{y}{4}\right)} - \frac{16f\left(\frac{y}{4}\right)}{F(y) - 36F\left(\frac{y}{4}\right)} \right.$$

$$\left. + \frac{\frac{1}{4}F(y)f(y)}{\left\{\frac{1}{4}F(y) - 16F\left(\frac{y}{4}\right)\right\}^2} \log \frac{64F\left(\frac{y}{4}\right)}{F(y)} - \frac{576F\left(\frac{y}{4}\right)f\left(\frac{y}{4}\right)}{\left\{F(y) - 36F\left(\frac{y}{4}\right)\right\}^2} \log \frac{36F\left(\frac{y}{4}\right)}{F(y)} \right\}.$$

The curve is the dotted line in the cut, and its area is about $-1{\cdot}713$. Hence we have

$$l\frac{dG_1}{dt} = -\frac{P_1}{\pi s^2 \sqrt{h_1}} 3{\cdot}312.$$

If we compare these values, obtained on such widely different assumptions as to the relative diameters of the particles, we see at once how exceedingly difficult would be the determination of diameters from observed results as to diffusion. (Compare § 54.)

But we see also how diffusion varies with the relative size of the particles, the sum of the diameters being constant. For the smaller, relatively, are the particles of smaller mass (those which have the greater mean-square speed) the more rapid is the diffusion.

And further, by comparison with the results of §§ 51, 54, we see how much more quickly a gas diffuses into another of different specific gravity than into another of the same specific gravity.

When the less massive particles are indefinitely small in comparison with the others, the diameter of these is s; and for their rate of diffusion we have

$$l\frac{dG}{dt} = -\frac{P_1}{\pi s^2 \sqrt{h_1}} 4{\cdot}26.$$

When it is the more massive particles which are evanescent in size, the numerical factor seems to be about 3·48. Hence it would appear that, even in the case of masses so different, there is a *minimum* value of the diffusion-coefficient, which is reached before the more massive particles are infinitesimal compared with the others.

[At one time I thought of expressing the results of this section in a form similar to that adopted in the expression for D in § 51. It is easy to see that the quantity corresponding to λ would now be what may be called the mean free path of a *single* particle of one gas in a space filled with another. Its value would be easily calculated by the introduction of h_1 for h in the factor ν of the integral

$$\int_0 \frac{\nu}{e}$$

while keeping e in terms of h. This involves multiplication of each number in the fourth column of the *Appendix* to Part I. by the new factor $\epsilon^{-(h_1-h)x^2} h_1^{\frac{3}{2}}/h^{\frac{3}{2}}$. But, on reflection, I do not see that much would be gained by this.]

APPENDIX.

The notation is the same as in the Appendix to Part I.

x	xX_1/X_2	x^2X_1/X_2	x^3X_1/X_2	x^5X_1/X_2
0·1	·000049	·000005	·000001	·000000
·2	·000758	·000152	·000030	·000001
·3	·003594	·001078	·000323	·000029
·4	·010364	·004146	·001658	·000265
·5	·022505	·011252	·005626	·001407
·6	·040512	·024307	·014584	·005250
·7	·063623	·044536	·031175	·015276
·8	·089928	·071942	·057554	·036834
·9	·116712	·105041	·094537	·076575
1·0	·141040	·141040	·141040	·141040
1·1	·160292	·176321	·193953	·234683
1·2	·172656	·207187	·248624	·358019
1·3	·177229	·230398	·299517	·506184
1·4	·174174	·243844	·341382	·669108
1·5	·164430	·246645	·369968	·832427
1·6	·149568	·239309	·382894	·980209
1·7	·131393	·223368	·379726	1·097407
1·8	·111654	·200977	·361758	1·172098
1·9	·091960	·174724	·331976	1·198432
2·0	·073480	·146960	·293920	1·175680
2·1	·057015	·119731	·251435	1·108829
2·2	·043032	·094670	·208274	1·008046
2·3	·031579	·072632	·167054	·883714
2·4	·022584	·054202	·130085	·749288
2·5	·015750	·039375	·098438	·615234
2·6	·010686	·027784	·072238	·488332
2·7	·007074	·019099	·051567	·375926
2·8	·004536	·012701	·035563	·278812
2·9	·002871	·008326	·024145	·203063
3·0	·001710	·005130	·015390	·138510
3·1	·001071	·003320	·010294	·098925
3·2	·000629	·002014	·006445	·065997
3·3	·000361	·001192	·003935	·042852
3·4	·000211	·000689	·002344	·027098
3·5	·000111	·000389	·001361	·016671
3·6	·000066	·000240	·000865	·010004
3·7	·000037	·000136	·000505	·005839
3·8			·000229	·003307
3·9			·000118	·001798
4·0			·000062	·000985
	2·095244	2·954862	4·630593	14·624154

Thus the values of C_1, C_2, C_3, and C_5 are respectively 0·838, 1·182, 1·852, and 5·849.

LXXIX.

ON THE FOUNDATIONS OF THE KINETIC THEORY OF GASES. III.

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I HAVE explained at some length, in my "Reply to Prof. Boltzmann *," the circumstances under which the present inquiry originated and has been pursued. Of these I need now only mention two:—*first*, the very limited time which I can spare for such work; *second*, the very meagre acquaintance I possessed of what had been already done with regard to the subject. My object has been to give an easily intelligible investigation of the *Foundations* of the Kinetic Theory; and I have, in consequence, abstained from reading the details of any investigation (be its author who he may) which seemed to me to be unnecessarily complex. Such a course has, inevitably, certain disadvantages, but its manifest advantages far outweigh them.

In August 1888, however, I was led in the course of another inquiry † to peruse rapidly the work of Van der Waals, *Die Continuität des gasförmigen und flüssigen*

* *Proc. R. S. E.*, January 1888; *Phil. Mag.*, March 1888.

† "Report on some of the Physical Properties of Water," *Phys. Chem. Chall. Exp.*, Part IV. [LXI. above, p. 56.]

Zustandes. This shows me that Lorenz had anticipated me in making nearly the same correction of the Virial equation as that given in the earlier part of § 30 of my first paper. His employment of the result is a totally different one from mine; he uses it to find a correction for the number of impacts. The desire to make, at some time, this investigation arose with me when I was writing my book on *Heat*, as will be seen in the last paragraphs of § 427 of that book. [First edition, 1884.] It was caused by my unwillingness to contemplate the existence of molecular *repulsion* in any form, and my conviction that the effects ascribed to it could be explained by the mere resilience involved in the conception of impacts.

The present paper consists of instalments read to the Society at intervals during the years 1887–8. The first of these, which is also the earliest in point of date, deals with a special case of molecular attraction, on which, of course, depends the critical temperature, and the distinction between gases and vapours. Here the particles which, at any time, are under molecular force have a greater average kinetic energy than the rest. Mathematical, or rather *numerical*, difficulties of a somewhat formidable nature interfered with the exact development of these inquiries. I found, for instance, that in spite of the extreme simplicity of the special assumption made as to the molecular force, the investigation of the average time between the encounter of two particles and their final disengagement from one another involves a quadrature of a very laborious kind. Thus the correction of the number of impacts could not easily be made except by some graphic process.

One reason for the postponement of publication of the present part was the hope that I might be enabled to append tables of the numerical values of the chief integrals which it involves, especially the peculiarly interesting one

$$y = \epsilon^{-x^2} \int_0^x \epsilon^{x^2} dx.$$

Want of time, however, forced me to substitute for complete tables mere graphical representations of the corresponding curves, drawn from a few carefully calculated values. These are not fitted for publication, though they were quite sufficient to give a general notion of the numerical values of the various results of the investigation; and enabled me to take the next step:—viz. the approximate determination of the form of the Virial equation when molecular attraction is taken account of. Part IV. of this investigation, containing this application, was read to the Society on Jan. 21, 1889, and an Abstract has appeared in the *Proceedings*. It appears that the difference of average kinetic energy between a free, and an entangled, particle is of special importance in the physical interpretation of the Virial Equation.

An Appendix is devoted to the consideration of the modification which the previous results undergo when the coefficient of restitution is supposed to be less than 1. This extension of the investigation was intended as an approximation to the case of radiation from the particles of a gas, and the consequent loss of energy. But, so far as I have developed it, no results of any consequence were obtained. I met with difficulties of a very formidable order, arising mainly from the fact that the particles after impact do not always separate from one another. The full treatment of the impact of a single particle with a double one is very tedious; and the conditions of impact of two double particles are so complex as to be totally unfit for an elementary investigation like the present.

The remainder of the Appendix is devoted to two points, raised by Professors Newcomb and Boltzmann, respectively:—the first being the problem of distribution of speed in the "special" state;—the other involving a second approximation to the estimates of Viscosity and Thermal Conductivity already given in Part II.

XV. *Special Assumption as to Molecular Force.*

57. To simplify the treatment of the molecular attraction between two particles, let us make the assumption that the kinetic energy of their relative motion changes by a constant (finite) amount at the instant when their centres are at a distance a apart. This will be called an *Encounter*. There will be a refraction of the direction of their relative path, exactly analogous to that of the path of a refracted particle on the corpuscular theory of light. To calculate the term of the virial (§ 30) which corresponds to this, we must find

(*a*) The probability that the relative speed before encounter lies between u and $u + du$.

(*b*) The probability that its direction is inclined from θ to $\theta + d\theta$ to the line of centres at encounter.

(*c*) The magnitude of the encounter under these conditions, and its average value.

Next, to find the (altered) circumstances of impact, we must calculate

(*d*) The probability that an encounter, defined as above, shall be followed by an impact.

(*e*) The circumstances of the impact.

(*f*) The magnitude of the impact, and its average value per encounter.

In addition to these, we should also calculate the number of encounters per second, and the average duration of the period from encounter to final disentanglement, in order to obtain (from the actual speeds before encounter) the correction for the length of the free path of each. This, however, is not easy. But it is to be observed that, in all probability, this correction is not so serious as in the case when no molecular force is assumed. For, in that case the free path is *always* shortened; whereas, in the present case it depends upon circumstances whether it be shortened or lengthened. Thus, if the diameters of the particles be *nearly* equal to the encounter distance, there will in general be shortening of the paths, and consequent diminution of the time between successive impacts:—if the diameters be *small* in comparison with the encounter distance, the whole of the paths will be lengthened and the interval between two encounters may be lengthened or shortened. Thus if we assume an intermediate relation of magnitude, there will be (on the average) but little change in the intervals between successive impacts. Hence also the time during which a particle is wholly free will be nearly that calculated as in § 14, with the substitution, of course, of a for s.

XVI. *Average Values of Encounter and of Impact.*

58. The number of encounters of a v, with a v_1, in directions making an angle β with one another, is by § 21 proportional to

$$vv_1v_0 \sin\beta\, d\beta,$$

where $$v_0^2 = v^2 + v_1^2 - 2vv_1 \cos\beta.$$

Hence the number of encounters for which the relative speed is from u to $u+du$ is proportional to

$$u^2du\int\frac{vv_1}{vv_1}\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(1)}.$$

The limits of v_1 are $v \pm u$, or $u \pm v$, according as $v \gtrless u$, and those of v are 0 to ∞, so that the integral is

$$\int_0^\infty \frac{v}{v}\int_{v-u}^{v+u}\frac{v_1}{v_1} = \int_0^\infty \frac{v}{2hv}\left(\epsilon^{-h(v-u)^2} - \epsilon^{-h(v+u)^2}\right)$$

$$= \frac{\epsilon^{-hu^2/2}}{2h}\int_0^\infty vdv\left(\epsilon^{-2h\left(v-\frac{u}{2}\right)^2} - \epsilon^{-2h\left(v+\frac{u}{2}\right)^2}\right).$$

The first term of this integral may be written as

$$\int_{-\frac{u}{2}}^\infty \left(x + \frac{u}{2}\right) dx\, \epsilon^{-2hx^2},$$

and the second as $$-\int_{\frac{u}{2}}^\infty \left(x - \frac{u}{2}\right) dx\, \epsilon^{-2hx^2}.$$

Together, these amount to $$\int_{-\frac{u}{2}}^{\frac{u}{2}} xdx\, \epsilon^{-2hx^2} + u\int_0^\infty dx\, \epsilon^{-2hx^2}.$$

The first term vanishes, and the second is

$$\frac{u}{2}\sqrt{\frac{\pi}{2h}}.$$

Thus the value of (1) is $$\frac{u^2du}{4}\,\epsilon^{-hu^2/2}\sqrt{\frac{\pi}{2h^3}}\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(2)}.$$

But, on the same scale, the whole number of encounters in the same time is

$$\int vv_1 v_0 \sin \beta d\beta = \int \frac{vv_1}{vv_1} v_0^2 dv_0 = \frac{I_3}{3} = \frac{1}{4}\sqrt{\frac{2\pi}{h^7}}.$$

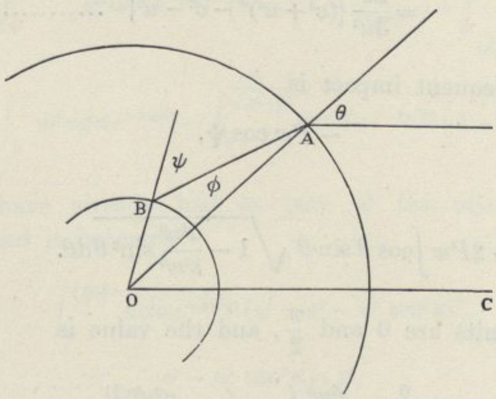
Thus the fraction of the whole encounters, which takes place with relative speed u to $u + du$, is

$$\frac{h^2}{2} u^3 du\, \epsilon^{-hu^2/2};$$

whose integral, from 0 to ∞, is 1 as it ought to be.

59. Now these relative motions are before encounter distributed equally in all directions. Let us deal therefore only with those which are parallel to a given line. The final result will be of the same character relative to all such lines; and therefore the encounters will not disturb the even distribution of directions of motion.

Refer the motion to the centre, O, of one of the encountering particles. Let A be the point midway between the particles at encounter, B that of impact, the



encountering particle coming parallel to CO. Let $OA = a/2$, OB (as before) $= s/2$. Let θ, ϕ be the angles of incidence and refraction at encounter, ψ that of incidence at impact, u and w the relative speeds before and after the encounter. Then

$$u \sin \theta = w \sin \phi;$$

and, if Pc^2 represent double the work done in the encounter by the molecular forces,

$$u^2 \cos^2 \theta + c^2 = w^2 \cos^2 \phi,$$

so that

$$u^2 + c^2 = w^2.$$

Also it is obvious from the diagram that

$$s \sin \psi = a \sin \phi = \frac{au}{w} \sin \theta.$$

Hence the encounter will not be followed by an impact if

$$\sin\theta > \frac{sw}{au}.$$

60. We must next find the average value of an encounter, and also of an impact; in the latter case taking account of all the encounters whether or not they involve an impact.

The numerical value of the encounter-impulse in the above figure is evidently

$$P(w\cos\phi - u\cos\theta)/2,$$

which must be doubled to include the repetition on separation; and the average value, when the relative speed is u, is

$$2P\int_0^{\frac{\pi}{2}} \sin\theta\cos\theta\,(w\cos\phi - u\cos\theta)\,d\theta$$

$$= \frac{2P}{3u^2}\{(c^2+u^2)^{\frac{3}{2}} - c^3 - u^3\} \quad \text{.................................} (3).$$

The value of the subsequent impact is

$$-Pw\cos\psi,$$

and the average value

$$-2Pw\int\cos\theta\sin\theta\sqrt{1-\frac{a^2u^2}{s^2w^2}\sin^2\theta}\,d\theta.$$

When $sw > au$, the limits are 0 and $\frac{\pi}{2}$, and the value is

$$-\frac{2}{3}Pw\frac{s^2w^2}{a^2u^2}\left\{1-\left(1-\frac{a^2u^2}{s^2w^2}\right)^{\frac{3}{2}}\right\} \text{.................................} (4).$$

But when $sw < au$, the limits are 0 and $\sin^{-1}\frac{sw}{au}$, and the value is

$$-\frac{2}{3}Pw\frac{s^2w^2}{a^2u^2} \text{.................................} (5).$$

By (2) and (3) we find as the average value of the encounter, taking account of all possible relative speeds,

$$+\frac{P}{3}h^2\int_0^\infty u\,du\,\epsilon^{-hu^2/2}\{(c^2+u^2)^{\frac{3}{2}} - c^3 - u^3\},$$

$$= \frac{P}{3}h^2\left\{\frac{\epsilon^{hc^2/2}}{\sqrt{h^5}}\left(3\sqrt{\frac{\pi}{2}} - 4\sqrt{2}\int_0^{c\sqrt{\frac{h}{2}}}\epsilon^{-y^2}y^4\,dy\right) - \frac{c^3}{h} - 3\sqrt{\frac{\pi}{2h^5}}\right\},$$

or, if we write for simplicity, $e^2 = hc^2/2$,

$$= \frac{P}{3\sqrt{h}}\left\{\epsilon^{e^2}\left(3\sqrt{\frac{\pi}{2}} - 4\sqrt{2}\int_0^e \epsilon^{-y^2}y^4dy\right) - 2\sqrt{2}e^3 - 3\sqrt{\frac{\pi}{2}}\right\}$$

$$= \frac{P}{\sqrt{h}}\left\{\sqrt{\frac{\pi}{2}}(\epsilon^{e^2}-1) + e\sqrt{2} - \sqrt{2}\epsilon^{e^2}\int_0^e \epsilon^{-y^2}dy\right\}$$

$$= \frac{P}{\sqrt{h}}\left\{\sqrt{2}\epsilon^{e^2}\int_e^\infty \epsilon^{-y^2}dy + e\sqrt{2} - \sqrt{\frac{\pi}{2}}\right\} \quad\ldots\ldots\ldots\ldots (6).$$

The expression obviously vanishes, as it ought to do, when $e = 0$. And it is always positive, for its differential coefficient with respect to e is

$$\frac{P}{\sqrt{h}} 2^{\frac{3}{2}} e\epsilon^{e^2}\int_e^\infty \epsilon^{-y^2}dy.$$

In a similar way (4) and (5) give, with (2), as the average impact *per encounter*,

$$R = -\frac{P}{3}\frac{s^2h^2}{a^2}\left\{\int_0^{\frac{sc}{\sqrt{a^2-s^2}}} w^3udu\,\epsilon^{-hu^2/2}\left(1 - \left(1 - \frac{a^2u^2}{s^2w^2}\right)^{\frac{3}{2}}\right) + \int_{\frac{sc}{\sqrt{a^2-s^2}}}^\infty w^3udu\,\epsilon^{-hu^2/2}\right\}$$

$$= -\frac{P}{3}\frac{s^2h^2}{a^2}\left\{\int_0^\infty w^3udu\,\epsilon^{-hu^2/2} - \int_0^{\frac{sc}{\sqrt{a^2-s^2}}} w^3udu\,\epsilon^{-hu^2/2}\left(1 - \frac{a^2u^2}{s^2w^2}\right)^{\frac{3}{2}}\right\}.$$

The first integral we have already had as part of the encounter. To simplify the second, let $s/a = \cos\alpha$, and it becomes

$$\int_0^{c\cot\alpha} udu\,\epsilon^{-hu^2/2}(u^2 + c^2 - u^2\sec^2\alpha)^{\frac{3}{2}},$$

which, with $$c^2 - u^2\tan^2\alpha = z^2,$$

gives $$\cot^2\alpha\int_0^c z^4dz\,\epsilon^{+\frac{h}{2}(z^2-c^2)\cot^2\alpha}$$

or $$\left(\frac{2}{h}\right)^{\frac{5}{2}}\tan^3\alpha\,\epsilon^{-hc^2\cot^2\alpha/2}\int_0^{c\sqrt{\frac{h}{2}}\cot\alpha} x^4dx\,\epsilon^{x^2}.$$

The whole is now

$$R = -\frac{P}{3}h^2\cos^2\alpha\left\{\frac{\epsilon^{hc^2/2}}{\sqrt{h^5}}\left(3\sqrt{\frac{\pi}{2}} - 4\sqrt{2}\int_0^{c\sqrt{\frac{h}{2}}} y^4dy\,\epsilon^{-y^2}\right) - \left(\frac{2}{h}\right)^{\frac{5}{2}}\tan^3\alpha\,\epsilon^{-\frac{hc^2}{2}\cot^2\alpha}\int_0^{c\sqrt{\frac{h}{2}}\cot\alpha} x^4dx\,\epsilon^{x^2}\right\}$$

$$= -\frac{P\cos^2\alpha}{\sqrt{h}}\left\{\epsilon^{e^2}\sqrt{\frac{\pi}{2}} + \sqrt{2}e - \sqrt{2}\epsilon^{e^2}\int_0^e \epsilon^{-y^2}dy + \sqrt{2}e\tan^2\alpha - \sqrt{2}\epsilon^{-e^2\cot^2\alpha}\tan^3\alpha\int_0^{e\cot\alpha}\epsilon^{x^2}dx\right\}$$

$$= -\frac{P}{\sqrt{h}}\cos^2\alpha\left\{\epsilon^{e^2}\sqrt{\frac{\pi}{2}} + \sqrt{2}e\sec^2\alpha - \sqrt{2}\epsilon^{e^2}\int_0^e \epsilon^{-y^2}dy - \sqrt{2}\epsilon^{-e^2\cot^2\alpha}\tan^3\alpha\int_0^{e\cot\alpha}\epsilon^{x^2}dx\right\},$$

which, when $e=0$ and $\cos\alpha=1$, becomes

$$-P\sqrt{\frac{\pi}{2h}},$$

as in § 30.

It would at first sight appear that the value of the impact is finite $\left(=-Pe\sqrt{\frac{2}{h}}\right)$ when there is *no nucleus* $\left(i.e.\ \alpha=\frac{\pi}{2}\right)$. But, in such a case, we must remember that the second part of the first expression for R above has no existence. In fact the value of the second of the two integrals is $\sqrt{2}\tan^3\alpha \, . \, e\cot\alpha$, when $e\cot\alpha$ is small; and this destroys the apparently non-vanishing term.

XVII. *Effect of Encounters on the Free Path.*

61. If two particles of equal diameters impinge on one another, the *relative* path must obviously be shortened on the average by

$$s\frac{\int_0^{\frac{\pi}{2}} 2\pi\sin\theta\cos^2\theta\, d\theta}{\int_0^{\frac{\pi}{2}} 2\pi\sin\theta\cos\theta\, d\theta}=\frac{2s}{3}.$$

But if v, v_1 be their speeds, and v_0 their relative speed, the paths are shortened respectively by the fractions v/v_0 and v_1/v_0 of this. The average values must be equal, so that we need calculate one only.

Now the average value of v/v_0 is obviously

$$\frac{\int \nu\nu_1 v\sin\beta\, d\beta}{\int \nu\nu_1 v_0\sin\beta\, d\beta},$$

where β is the angle between the directions of motion, so that

$$vv_1\sin\beta\, d\beta = v_0\, dv_0.$$

Hence the average above is

$$\frac{\int\frac{\nu\nu_1 v_0\, dv_0}{v_1}}{\int\frac{\nu\nu_1 v_0^2\, dv_0}{vv_1}}=\frac{2\int \nu\nu_1 v}{I_3/3}=\frac{\frac{\sqrt{\pi}}{4}h^{-\frac{7}{2}}}{\frac{\sqrt{2\pi}}{4}h^{-\frac{7}{2}}}=\frac{1}{\sqrt{2}}.$$

Hence the mean of the free paths during a given period becomes

$$\frac{1}{\sqrt{2}n\pi s^2} - \frac{\sqrt{2}s}{3};$$

that is, it is shortened in the ratio

$$1 - \frac{2}{3}n\pi s^3 : 1,$$

or $$1 - 4 \text{ (sum of vols. of spheres in unit vol.) } : 1 = 1 - \frac{\alpha}{V} : 1 \text{ say.}$$

Hence the number of collisions per second, already calculated, is too small in the same ratio.

Thus the value of $\Sigma(R)$ in § 30 must be increased in the ratio $1 : 1 - \frac{\alpha}{V}$, and the virial equation there given becomes

$$nP\bar{v}^2/2 = \frac{3}{2}p\left(V - \frac{\alpha}{1 - \frac{\alpha}{V}}\right).$$

If this were true in the limit, the ultimate volume would be double of that before calculated, *i.e.* 8 times the whole volume of the particles.

62. Another mode of obtaining the result of § 61 is to consider the particles as mere points, and to find the average interval which elapses between their being at a distance s from one another and their reaching the positions where their mutual distance is least. The space passed over by each during that time will have to be subtracted from the length of the *mean free* path calculated as in § 11 when the particles were regarded as mere circular discs.

The average interval just mentioned is obviously

$$\frac{1}{u}\frac{\int_0^{\frac{\pi}{2}} s\cos\theta \,.\, \sin\theta\cos\theta d\theta}{\int_0^{\frac{\pi}{2}} \sin\theta\cos\theta d\theta} = \frac{2s}{3u}.$$

Hence the average space passed over in that interval is

$$\frac{2s}{3u}\int \nu v \,.\, \frac{\nu_1 v_0^2}{v v_1} dv \Big/ \frac{I_3}{3} = \frac{\sqrt{2}s}{3}.$$

If we put a for s in this expression we have the amount to be subtracted from the average path between two encounters in consequence of the finite size of the region of encounter.

XVIII. *Average Duration of Entanglement, and consequent Average Kinetic Energy.*

63. We have next to find the average duration of entanglement of two particles:—*i.e.*, the interval during which their centres are at a distance less than a.

The whole relative path between the entering and leaving encounters is

$$2\,(a\cos\phi - s\cos\psi),$$

or

$$2a\cos\phi,$$

according as there is, or not, an impact.

Hence the whole time of entanglement is the quotient, when one or other is divided by w. And the average value, for relative speed u, is

$$\tau = \frac{4}{w^2}\int_0^{\frac{\pi}{2}} (a\sqrt{w^2 - u^2\sin^2\theta} - \sqrt{w^2s^2 - a^2u^2\sin^2\theta})\cos\theta\sin\theta\, d\theta$$

$$= \frac{4}{3w^2}\left\{\frac{a}{u^2}(w^3 - c^3) - \frac{1}{a^2u^2}\left(w^3s^3 - (w^2s^2 - a^2u^2)^{\frac{3}{2}}\right)\right\},$$

when

$$ws > au;$$

and

$$= \frac{4}{w^2}\left\{\int_0^{\frac{\pi}{2}} a\sqrt{w^2 - u^2\sin^2\theta}\cos\theta\sin\theta\, d\theta - \int_0^{\sin^{-1}\frac{ws}{au}} \sqrt{w^2s^2 - a^2u^2\sin^2\theta}\cos\theta\sin\theta\, d\theta\right\},$$

$$= \frac{4}{3w^2}\left(\frac{a}{u^2}(w^3 - c^3) - \frac{1}{a^2u^2}w^3s^3\right),$$

when

$$ws < au.$$

These must be multiplied by the chance of relative speed u, as in § 58, and the result is

$$\frac{2h^2}{3}\left\{\int_0^\infty \frac{u\,du}{w^2}\left(a(w^3 - c^3) - \frac{w^3s^3}{a^2}\right)\epsilon^{-hu^2/2} + \frac{1}{a^2}\int_0^{\frac{cs}{\sqrt{a^2-s^2}}} \frac{u\,du}{w^2}(w^2s^2 - a^2u^2)^{\frac{3}{2}}\epsilon^{-hu^2/2}\right\};$$

or, with the notation of § 60,

$$= \frac{2ah^2}{3}\epsilon^{hc^2/2}\left\{\int_c^\infty \frac{dw}{w}\left(w^3(1 - \cos^3\alpha) - c^3\right)\epsilon^{-hw^2/2} + \int_c^{c\operatorname{cosec}\alpha} \frac{dw}{w}(c^2 - w^2\sin^2\alpha)^{\frac{3}{2}}\epsilon^{-hw^2/2}\right\}$$

$$= \frac{2ah^2}{3}\epsilon^{hc^2/2}\int_c^\infty \frac{dw}{w}\left(w^3(1 - \cos^3\alpha) - c^3\right)\epsilon^{-hw^2/2} + \frac{2ah^2}{3}\epsilon^{-\frac{hc^2}{2}\cot^2\alpha}\int_0^{c\cos\alpha} \frac{z^4\,dz}{c^2 - z^2}\epsilon^{+hz^2\operatorname{cosec}^2\alpha/2}.$$

As the value of this expression depends in no way on the length of the free path, it is clear that the average energy of all the particles is greater than that of the free particles, by an amount which increases rapidly as the length of the free path is diminished.

APPENDIX.

A. *Coefficient of Restitution less than Unity.*

Let us form again the equations of § 19, assuming e to be the coefficient of restitution. We have

$$P(u'-u) = -\frac{PQ(1+e)}{P+Q}(u-v) = -Q(v'-v),$$

so that $$P(u'^2-u^2) = -\frac{PQ(1+e)}{(P+Q)^2}(u-v)\{(2P+Q\overline{1-e})u + Q(1+e)v\},$$

$$Q(v'^2-v^2) = \frac{PQ(1+e)}{(P+Q)^2}(u-v)[P(1+e)u + \{2Q+P(1-e)\}v].$$

The whole change of energy in the collision is half the sum of these quantities, viz.,

$$-\frac{1}{2}\frac{PQ(1-e)^2}{P+Q}(u-v)^2.$$

With the help of the expressions in § 22, we find for the average changes of energy of a P and of a Q, respectively,

$$\tfrac{1}{2}P(\overline{u'^2}-\overline{u^2}) = -\frac{PQ(1+e)}{2hk(P+Q)^2}\{2(Pk-Qh)+Q(1-e)(h+k)\},$$

$$\tfrac{1}{2}Q(\overline{v'^2}-\overline{v^2}) = \frac{PQ(1+e)}{2hk(P+Q)^2}\{2(Pk-Qh)-P(1-e)(h+k)\}.$$

The first term on the right is energy exchanged between the systems; and, as in the case of $e=1$, it vanishes when the average energy per particle is the same in the two systems. The second term (intrinsically negative for each system) is the energy lost, and is always greater for the particles of smaller mass. The average energy lost per collision is

$$\frac{PQ(1-e^2)}{2(P+Q)}\left(\frac{1}{h}+\frac{1}{k}\right).$$

It is easy to make for this case an investigation like that of § 23. But we must remember that there is loss of energy by the internal impacts of each system, which must be taken into account in the formation of the differential equations. This is easily found from the equations just written, by putting $Q=P$:—but the differential equations become more complex than before, and do not seem to give any result of value. [Shortly after Part I. was printed off, Prof. Burnside called my attention to the fact that the equations of interchange of energy in § 23 are easily integrable without approximation. But the approximate solution in the text suffices for the application made.]

B. *The Law of Distribution of Speed.*

In addition to what is said on this subject in the Introduction to Part II., it may be well to take the enclosed (from *Proc. R. S. E.*, Jan. 30, 1888).

"The behaviour parallel to y and z (though not the number) of particles whose velocity-components are from x to $x+dx$, must *obviously* be independent of x, so that the density of 'ends' in the velocity space diagram is of the form

$$f(x)\ F(y,\ z).$$

The word I have italicised may be very easily justified. No collisions count, except those in which the line of centres is practically perpendicular to x (for the others each dismiss a particle from the minority; and its place is instantly supplied by another, which behaves exactly as the first did), and therefore the component of the relative speed *involved in the collisions which we require to consider* depends wholly on y and z motions. Also, for the same reason, the frequency of collisions of various kinds (so far as x is concerned) does not come into question. Thus the y and z speeds, not only in one x layer but in all, are entirely independent of x; though the *number* of particles in the layer depends on x alone."

C. *Viscosity.*

In my "Reply to Prof. Boltzmann" I promised to give a further approximation to the value of the coefficient of Viscosity, by taking account of the alteration of permeability of a gas which is caused by (slow) shearing disturbance. I then stated that a rough calculation had shown me that the effect would be to change my first, *avowedly approximate*, result by 11 or 12 per cent. only. I now write again the equations of § 36, modifying them in conformity with the altered point of view.

The exponential expression in that section for the number of particles crossing the plane of yz, must obviously now be written

$$\epsilon^{-\sec\theta\int_0^x ev_0 d\xi/v} \sin\theta d\theta/2,$$

where v_0 is the velocity *relative* to the absorbing layer at ξ, and e also is no longer constant. But we have at once

$$v_0 = v + B\xi\sin\theta\cos\phi,$$

so that the exponent above is $-\dfrac{\sec\theta}{v}\displaystyle\int_0^x \{ev + (ev)'\, B\xi\sin\theta\cos\phi\}\, d\xi.$

Thus the differential of the whole y-momentum which comes to unit surface on $x=0$ from the layer x, $x+dx$, is

$$\frac{d\phi}{4\pi} Pnvev\epsilon^{-ex\sec\theta}\left(1 - \frac{(ev)'}{2v}Bx^2\frac{\sin\theta\cos\phi}{\cos\theta}\right)(v\sin\theta\cos\phi + Bx)\sin\theta d\theta.$$

Integrating with respect to ϕ from 0 to 2π, to x from 0 to ∞, and to θ from 0 to $\frac{\pi}{2}$, and doubling the result, we have

$$BPn\int_0^\infty vv\left(\frac{1}{3e} - \frac{(ev)'}{15e^2}\right).$$

The first term expresses my former result, viz.

$$\frac{BPC_1}{3\pi s^2\sqrt{h}}.$$

But the whole is $$\frac{BPn}{15}\int_0^\infty \nu v\left(\frac{4}{e}-\frac{ve'}{e^2}\right)=\frac{2BPn}{15}\int_0^\infty \frac{\nu h v^3}{e}=\frac{2BPC_3}{15\pi s^2\sqrt{h}}.$$

The ratio is $2C_3/5C_1 = 3{\cdot}704/4{\cdot}19 = 0{\cdot}882$.

It is worthy of remark that the term

$$\int_0^\infty \nu v\frac{(ev)'}{15e^2}$$

has the value $$\frac{5C_1-2C_3}{15n\pi s^2\sqrt{h}},$$

and that 4/5ths of the C_1 term are due to e'.

D. *Thermal Conductivity.*

Applying a process, such as that just given, to the expressions in § 39, we find that the exponential in the integral for the *number* of particles must be written

$$\epsilon^{-e_0x\sec\theta+\frac{\alpha_0'x^2e_0}{2v}-\frac{e_0'x^2}{2}\sec\theta}=\epsilon^{-e_0x\sec\theta}\left(1-e_0'x^2\sec\theta/2v+\frac{e_0\alpha'x^2}{2}\right)$$

to the required degree of approximation. [Properly, the superior limit of the θ integration should be $\cos^{-1}\frac{\alpha}{v}$; but this introduces quantities of the order α^2 only.] Thus equation (1) becomes

$$\alpha=\int_0^\infty \nu\left(\frac{n'}{n}+\frac{\nu'}{\nu}\right)v/3e-\frac{\alpha'}{4}\int\frac{\nu}{e}.$$

In the same way equation (3) of § 41 becomes

$$E=-\frac{P}{6}\int_0^\infty n\nu v^3\left\{\left(\frac{n'}{n}+\frac{\nu'}{\nu}\right)\Big/e-5\alpha/v-9\alpha'/4ev\right\}.$$

Thus equations (1′) and (3′) of § 42 become, respectively,

$$\alpha=\frac{h'}{\sqrt{h^5}}\frac{P}{6\pi ps^2}\left(\frac{5}{2}C_1-C_3\right)-\frac{PC_0\alpha'}{8\pi phs^2},$$

and $$E=\frac{h'}{\sqrt{h^5}}\frac{P}{6\pi s^2}\left(\frac{25}{4}C_1-5C_3+C_5\right)+\frac{3P}{8}\frac{C_2\alpha'}{\pi hs^2}-\frac{5P}{16}\frac{C_0\alpha'}{\pi hs^2}.$$

Thus we have finally to deal with the new forms of (1″) and (3″) of § 43, viz. :—

$$\alpha=\frac{h'}{\sqrt{h^5}}\frac{\rho\lambda}{p}0{\cdot}06-\frac{\rho\lambda\alpha'}{ph}0{\cdot}12,$$

$$E=\frac{h'}{\sqrt{h^5}}\rho\lambda 0{\cdot}45+\frac{\rho\lambda\alpha'}{h}0{\cdot}44.$$

When similar methods are applied to the diffusion equations, they become hopelessly complicated.

LXXX.

ON THE FOUNDATIONS OF THE KINETIC THEORY OF GASES. IV.

[*Transactions of the Royal Society of Edinburgh*, Vol. XXXVI. Read Jan. 21, 1889, and April 6, 1891.]

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[A FEW words are necessary to explain why the present paper has hitherto been printed in Abstract only, and to show what modifications it has undergone since it was read more than two years ago.

In the paper, as it was first presented to the Society, I contented myself with the usual practice of extracting from the virial a negative term $(-\beta p)$ to represent at least a portion of the part due to the molecular repulsion at impact. But, as will be seen by the Abstract printed at the time (*Proc. Roy. Soc. Edin.*, 21/1/89), I stated that though this procedure is correct when molecular attraction is not taken into account, it requires considerable modification when such attractions are introduced. I also stated that its main effect would be to alter one of the disposable quantities (A) in my equation. I have since seen that the definition, of what we are now to understand by "temperature," which I then introduced, leads naturally and directly to the writing of a part of A in the form

$$-e\{E+C/(v+\gamma)\},$$

where E is proportional to the absolute temperature and to the average energy of a free particle. This remark really substitutes the new undetermined quantity e for the β which occurred in my former expression. But the equation in its new form, though containing as many arbitrary constants as before, is considerably more simple to deal with, as p occurs only in the term pv, in which both factors are directly given by experiment. The term $p(v-\beta)$ was a source of great trouble in the attempt to determine the proper values of the constants. It was recognised by Van der Waals, even in his earliest paper, that the quantity β suffers large changes of value, with changes of volume of the gas, so that no formula in which it is treated as a constant could suffice to represent more than a moderate volume-range of the isothermals with any consistent degree of accuracy.

When I first read my paper, I had made no serious attempt to attack the formidable numerical problem of determining values of the constants which should adapt my main formula to Andrews' experimental data. I contented myself with obviously (and professedly) provisional assumptions, which showed that it was well fitted to represent the results; but I also gave the relations among the constants of the formula and the data as to the mass, and the critical values of the pressure, volume, and temperature of the substance.

Later, having carefully reduced Andrews' data to true pressures (by the help of Amagat's determinations of the isothermals of air at ordinary temperatures), I proceeded to try various assumptions as to the values of the quantities $\bar{v}$, $\bar{p}$, α in my formulæ, on which (as $\bar{t} = 30^\circ{\cdot}9$ C. was already given by Andrews with great precision) all the constants can be made to depend. I at first endeavoured to adjust these so as to make $\beta = 0{\cdot}0017$, in consequence of a statement by Amagat (*Ann. de Chimie*, 1881, XXII. p. 397) as to the ultimate volume of CO_2. But I failed to get results giving more than a general accordance with Andrews' experiments; so that I made further guesses without taking account of this datum. I had, however, become accustomed to the employment of it, as a quantity of the order 10^{-3} of the volume of the gas at 0° C. and 1 atm., so that I was much surprised to find that one of my chance assumptions, which gave $\beta = 0{\cdot}00005$, led to a formula far more closely agreeing with Andrews than any I had till then met with. The reason for this agreement is now obvious:—The term $-\beta p$ is *not* the proper expression for the part of the virial which it is intended to represent; and the true mode of introducing that part is, as pointed out in my *Abstract*, to alter the value of A from isothermal to isothermal, and from volume to volume.

In January last I happened to ask M. Amagat if he could give me the value of pv for CO_2 at 0° C. and 1 atm., which is wanting in his remarkable table (in the *Ann. de Chimie*, above referred to). In reply he kindly furnished me with a new and extremely complete set of determinations of pv, in terms of p, for CO_2; the range of pressures being 1 to 1000 atm., and of temperature 0° to 100° C., some special isothermals up to 258° being added. My first step on receiving these data was to try how far they agreed with Andrews' results, which I had carefully plotted (to true pressures) from $31^\circ{\cdot}1$ to 41° C., and for volumes from ·03 to ·002. My object was to discover, if possible, by comparison of the results of two such exceptionally trustworthy experimenters, whether any modification of the behaviour of CO_2 is (as some theoretical writers have asserted) produced by the molecular forces due to the walls of the very fine tubes in which Andrews' measurements were made. I could find nothing of the sort. The isothermals, plotted from Amagat's numbers (which in no case were for any of Andrews' temperatures), took their places in the diagram almost as if they had been an additional part of the work of one experimenter. The slight discrepancies at the smaller volumes were obviously due to the trace (1/500) of air which, as Andrews pointed out, was associated with the carbonic acid in his tubes.

But, although I have got from them only negative information as to the molecular effects said to be due to glass, Amagat's isothermals are so regularly spread over the diagram as to be far more readily available for calculation than are those of Andrews. I have not, however, the leisure requisite for anything like an exhaustive treatment of them; and all that I have attempted is to obtain values of the constants in my formula which make it a fair representation of the phenomena in the experimentally investigated range of the gas region of the diagram; and, more especially, that portion of it where the volume exceeds the critical volume. It appears to me that to try to push the approximation further at present would be waste of time; it cannot be attempted with any hope of much improvement until certain points, referred to below, have been properly investigated. These may lead to modifications of parts of the formula which, though unimportant in the regions now treated, may greatly improve its agreement with the facts, in the remaining portions of the diagram. Besides, there is in the data the uncertainty due to the presence of air, which was not wholly removed (though reduced to 1/2500) even in Amagat's experiments. This, as above remarked, begins to tell especially when the volume is small.

It is very much to be regretted that Clausius did not avail himself of Amagat's data in reducing Andrews' scale of pressures. He expressly says he rejected them because they were not consistent with those of Cailletet. Hence the formula which he obtained after great arithmetical labour, though it is in close, sometimes in almost startling, agreement with the data through the range of Andrews' work, is not properly a relation among p, v, and t. If we make it such, by putting in the correction (in terms of v) for the pressures as measured by the air-manometer, a new v-factor is introduced into the equation, and its simplicity (which is one of its most important characteristics) is lost. I tried to obtain hints for the values of the constants in my own formula by making this change in that of Clausius. But I found that the factor $1/t$ which Clausius introduced into the virial term (in order to approximate to the effect of the aggregation of particles into groups at the lower ranges of temperature), made his formula inapplicable to the wide regions of the diagram which Andrews did not attack, but which have been so efficiently explored by Amagat. There are, no doubt, traces of this systematic divergence even in the special Andrews region, but they become much more obvious in the outlying parts.

It is certainly remarkable that my simple formula, based entirely on the behaviour of smooth spheres, should be capable of so close an adjustment to the observed facts; and I think that the agreement affords at least very strong testimony in favour of the proposed mode of reckoning the temperature of a group of particles. When this is introduced, it appears at once that the term of Van der Waals' equation, which he took to represent Laplace's K, is not the statical pressure due to molecular forces, but (approximately) its excess over the repulsion due to the speed of the particles. And hence the (external) pressure is not, as Clausius put it, ultimately the difference between two very large quantities, but the excess of one very large quantity over the very large difference between two enormously great quantities; and thus the whole phenomena of a highly-compressed gas, or a liquid, are to be regarded as singular examples of kinetic stability. 28/5/91.]

Preliminary.

In the preceding part of this paper I considered the consequences of a special assumption as to the nature of the molecular force between two particles, the particles themselves being still regarded as hard, smooth, spheres. My object was to obtain, by means of rigorous calculation, yet in as simple a form as possible, a general notion of the effects due to the molecular forces. My present objects are (1) to apply this general notion to the formation and interpretation of the virial equation (in an approximate form), and (2) to apply the results to the splendid researches of Andrews and their recent extension by the truly magnificent measurements of Amagat.

Passing over some papers of Hirn, and others, in which the earliest attempts were made (usually on totally erroneous grounds) to form the equation of the isothermals of a gas in which molecular forces are prominent, we come to the Thesis of Van der Waals*, who was the first to succeed in representing, by a simple formula, the main characteristics of Andrews' results. His process is based upon the virial equation, and his special object seems to have been an attempt to determine the value of the molecular constant usually called "Laplace's K." Though the whole of this essay is extremely ingenious, and remarkably suggestive, it contains (even in its leading ideas) much that is very doubtful, and some things which are certainly incorrect. One of these was specially alluded to by Clerk-Maxwell†, who, in reviewing the essay, said:—"Where he has borrowed results from Clausius and others, he has applied them in a manner which appears to me to be erroneous." It will conduce to clearness if I commence with an examination of the equation which is the main feature of Van der Waals' Thesis, and the modifications which it underwent in the hands of Clausius.

XIX. *The Isothermal Equations of Van der Waals and Clausius.*

64. The virial equation (§ 30, above) is

$$\tfrac{1}{2}\Sigma\,(mu^2) = \tfrac{3}{2}pv + \tfrac{1}{2}\Sigma\,(Rr)\,;$$

where, to save confusion, we employ u to denote the speed of the particle whose mass is m. From this Van der Waals derives the following expression:—

$$\left(p + \frac{a}{v^2}\right)(v - \beta) = \tfrac{1}{3}\Sigma\,(mu^2)\,;$$

* *Over de continuiteit van den gas- en vloeistoftoestand.* Leiden, 1873.

† *Nature*, Oct. 15, 1874.

and he treats the right-hand member as a constant multiple of the absolute temperature. (This last point is of extreme importance, but I shall discuss it farther on; at present I confine myself to the formation of the equation.)

It is certain (§ 30) that, when there is no molecular force except elastic resilience, the term

$$\tfrac{1}{2}\Sigma (Rr)$$

in the virial equation takes, to a first approximation at least, the form of a numerical multiple of

$$-\frac{\Sigma (mu^2)}{v};$$

and thus that, *if this term be small in comparison with the other terms in the equation*, we may call it

$$-\tfrac{3}{2}\beta p.$$

Thus the virial equation becomes $p(v-\beta) = \frac{1}{3}\Sigma (mu^2)$.

[So far, all seems perfectly *legitimate;* though, as will be seen later, I think it has led to a good deal of confusion:—at all events, it has retarded progress, by introducing what was taken as a direct representation of the "ultimate volume" to which a substance can be reduced by infinite pressure. When this idea was once settled in men's minds, it seemed natural and reasonable, and consequently the left-hand member of the virial equation is now almost universally written $p(v-\beta)$; although, even in Van der Waals' Thesis, it was pointed out that comparison with experiment shows that β cannot be regarded as a constant. But its introduction is obviously indefensible, except in the special case of no molecular force.]

Van der Waals' next step is as follows:—Although p, in the virial equation, has been strictly defined as *external* pressure (that exerted by the walls of the containing vessel), he adds to it, in the last-written form of the equation (deduced on the express assumption of the absence of molecular force), a term a/v^2, which is to represent Laplace's K. Thus he obtains his fundamental equation

$$\left(p + \frac{a}{v^2}\right)(v-\beta) = \tfrac{1}{3}\Sigma (mu^2),$$

or, as it is more usually written (in consequence of the assumption about absolute temperature, already noticed),

$$p = \frac{kt}{v-\beta} - \frac{a}{v^2},$$

where k is an absolute constant, depending on the quantity of gas, and to be determined by the condition that the gas has unit volume at 0° C. and 1 atmosphere.

I do not profess to be able fully to comprehend the arguments by which Van der Waals attempts to justify the mode in which he obtains the above equation. Their nature is somewhat as follows. He repeats a good deal of Laplace's capillary work; in which the existence of a large, but unknown, internal molecular pressure is established, entirely from a statical point of view. He then gives reasons (which seem, on the whole, satisfactory from this point of view) for assuming that the magnitude of this force is as the square of the density of the aggregate of particles considered. But his justification of the introduction of the term a/v^2 into an account already closed, as it were, escapes me. He seems to treat the surface-skin of the group of particles as if it were an additional bounding-surface, exerting an additional, and enormous, pressure on the contents. Even were this justifiable, nothing could justify the multiplying of this term by $(v-\beta)$ instead of by v alone. But the whole procedure is erroneous. If one begins with the virial equation, one must keep strictly to the assumptions made in obtaining it, and consequently *everything* connected with molecular force, whether of attraction or of elastic resilience, must be extracted from the term $\Sigma(Rr)$.

It is very strange that Clausius*, to whom we owe the virial equation, should not have protested against this striking misuse of it, but should have contented himself with making modifications (derived from general considerations, such as aggregation of particles, &c.) which put Van der Waals' equation in the form

$$p = \frac{kt}{v-\beta} - \frac{a}{t(v+a)^2}.$$

65. Van der Waals' equation gives curves, whose general resemblance to those plotted by Andrews for CO_2 is certainly remarkable:—and it has the further advantage of reproducing, for temperatures below the critical point, the form of isothermals (with physically unstable, and therefore experimentally unrealisable, portions) which was suggested by James Thomson, as an extension of Andrews' work. For a reason which will presently appear (§ 67), Van der Waals' curves cannot be made to coincide with those of Andrews.

The modified equation of Clausius, however, seems to fit Andrews' work much better:—but the coincidence with the true isothermals is much more apparent than real, because Clausius' work is based on the measurement of pressures by the air-manometer, as they were originally given by Andrews, who had not the means of reducing them to absolute measure.

But a further remark of Clerk-Maxwell's (in the review above cited) is quite as applicable to the results of Clausius as to those of Van der Waals, viz.:—"Though this agreement would be strong evidence in favour of the accuracy of an empirical formula devised to represent the experimental results, the equation of M. Van der Waals, professing as it does to be deduced from the dynamical theory, must be subjected to a much more severe criticism."

66. Before I leave this part of the subject, I will, for the sake of future reference, put the equations of Van der Waals and Clausius in a form which I have found to be very convenient, viz.:—

$$p = \bar{p}\left(1 - \frac{(v-\bar{v})^3}{(v-\beta)v^2}\right) + \frac{k}{v-\beta}(t-\bar{t}) \quad \text{(A)}$$

* *Annalen der Physik*, IX. 1880.

and $$p = \bar{p}\left(1 - \frac{(v-\bar{v})^3}{(v-\beta)(v+\alpha)^2}\right) + \left(\frac{k\bar{t}}{v-\beta} + \frac{a}{t(v+\alpha)^2}\right)\frac{t-\bar{t}}{\bar{t}} \text{ ....(B).}$$

In these equations $\bar{p}$, $\bar{v}$, $\bar{t}$ belong to the critical point, determined by the conditions that at such a point p is a minimax in terms of v. The special advantage of this mode of representing the isothermals depends on the fact that the first part of the value of p belongs to the critical isothermal; so that by comparing, at any one volume, the pressures in different isothermals (as given experimentally) we have a comparatively simple numerical method of calculating the values of some of the constants in the equation.

67. But, even if we were to regard the formula of Van der Waals as a purely empirical one, there is a fatal objection to it in the fact that it contains only two disposable constants. Thus, if it were correct, the extraordinary consequence would follow that there is a necessary relation among the three quantities, pressure, volume, and temperature, at the critical point:—so that, no matter what the substance, when two of these are given the third can be calculated from them. I do not see any grounds on which we are justified in assuming that this can be the case. Certainly, if it were established as a physical truth, it would give us views of a much stronger kind than any we yet have as to the essential unity of all kinds of matter. Van der Waals seems to have taken his idea in this matter from one of Andrews' papers, in which there is a hazardous, and therefore unfortunate, speculation of a somewhat similar character. Anyhow, it would seem that, at least until experiment proves the contrary, we are bound to provide, in our theoretical work, for the mutual independence of at least the three following quantities:—

1. The diameters of the particles.
2. The range of sensible molecular force.
3. The maximum relative potential energy of two particles.

Besides these, there is the question of the *law* of molecular force, which we are certainly not entitled to assume as necessarily the same in all bodies. This has most important bearings on the formation of doublets, triplets, &c., at lower temperatures.

The modified formula of Clausius has one additional constant, and is therefore not so much exposed to the above objections as is that of Van der Waals. Still I think it has at least one too few.

XX. *The Virial Equation for attracting Spherical Particles.*

68. What is required is not an exact equation, for this is probably unattainable even when we limit ourselves to hard spherical particles. To be of practical value the equation must (while presenting a fair approximation to the truth) be characterised by simplicity. And, should the experimental data require it, we must be prepared to give the equation of any one isothermal in two or more forms, corresponding to various ranges of volume. It is exceedingly improbable (when we think of the mechanism

involved) that any really simple expression will give a fair agreement with an isothermal throughout the whole range of volumes which can be experimentally treated.

From the general results of Part III. of this paper we see that the term

$$\tfrac{1}{3}\Sigma\,(mu^2)$$

in the virial equation must, when molecular forces are taken into account, contain a term proportional to the number of particles which are at any (and therefore at every) time within molecular range of one another. Hence if, when the volume is practically infinite, we have for the mean-square speed of a particle

$$\tfrac{1}{3}\bar{u}^2 = \frac{E}{mn}$$

(where n is the whole number of particles), we shall have, when the volume is not too much reduced, no work having been done on the group from without,

$$\tfrac{1}{3}\Sigma\,(mu^2) = E + \frac{C}{v+\gamma};$$

where C and γ may be treated as constants, the first essentially positive if the molecular force be attractive, the second of uncertain sign. Even if the volume be very greatly reduced it is easy to see, from the following considerations, that a similar expression holds. The work done on a particle which joins a dense group is, on account of the short range of the forces, completed before it has entered much beyond the skin, and is proportional, *ceteris paribus*, to the skin-density. Hence the whole work done on the group by the molecular forces is (roughly) proportional to

$$v\rho \,.\, \rho_0,$$

the first factor expressing the number of the particles, the second the work done on each. But, as we are dealing with a definite group of particles, the first factor is constant, so that the whole work is directly as ρ_0, or inversely as (say) $v+\gamma$, because $\rho_0 < \rho$. But the work represents the gain in kinetic energy over that in the free state, so that this mode of reasoning leads us to the same result as the former for the average kinetic energy of all the particles.

In so far as R depends on the molecular *attraction*, the term

$$\tfrac{1}{3}\Sigma\,(Rr)$$

is evidently proportional, per unit volume of the group, to the square of the density:—for the particles, in consequence of their rapid motions, may be treated as occupying within an excessively short time every possible situation with regard to one another. Thus, as regards any one, the mass of all the rest may be treated as diffused uniformly through the space they occupy. In volume v, therefore, the amount is as $v\rho^2$. But, in the present case, the quantity $v\rho$ is constant, so that, again, the approximate value of the term is directly as ρ, or inversely as v. But, once more, we must allow for the bounding film (though not necessarily to the same exact amount as before), so we may write this part of the term as

$$\frac{A}{v+\alpha}.$$

But there is another part (negative) which depends on resilience. This is (§ 30) proportional to the average kinetic energy, and to the number of particles and the number of collisions per particle per second. The two last of these factors are practically the same as those employed for the molecular attraction. Hence the whole of the virial term may be written as

$$\frac{A - e\{E + C/(v+\gamma)\}}{v+\alpha}.$$

Thus if we write again A and C for

$$A + \frac{eC}{\alpha - \gamma} \text{ and } C + \frac{eC}{\alpha - \gamma},$$

respectively, the complete equation takes the form

$$pv = E + \frac{C}{v+\gamma} - \frac{A - eE}{v+\alpha},$$

which is certainly characterised by remarkable simplicity.

69. We must now consider how far it is probable that the quantities in the above expression (other than p and v) can be regarded as constant. E, of course, can be altered only by direct communication of energy; but the case of the others is different. Generally, it may be stated that there must be a particular volume (depending primarily upon the diameters of the particles) at and immediately below which the mean free path undergoes an almost sudden diminution, and therefore we should expect to find corresponding changes in the constants. In particular, it must be noted that some of them depend directly on the length of the free path, and that somewhat abrupt changes in their values must occur as soon as the particles are so close to one another that the mean free path becomes nearly equal to their average distance from their nearest neighbours. For then the number of impacts per second suffers a sudden and large increase. Thus, *in consequence of the finite size of the particles*, we may be perfectly prepared to find a species of discontinuity in any simple approximate form of the virial equation. From this point of view it would appear that there is not (strictly) a "critical volume" of an assemblage of hard spheres, but rather a sort of short *range* of volume throughout which this comparatively sudden change takes place. Thus the critical Isothermal may be regarded as having (like those of lower temperature) a finite portion which is practically straight and parallel to the axis of volume. That this conclusion is apparently borne out by experimental facts (so far at least as these are not modified by the residual trace of air) will be seen when we make the comparison.

In fact we might speak of a superior and an inferior critical volume, and the portions of the isothermals beyond these limits on both sides may perhaps have equations of the same form, but with finite changes in some at least of the constants.

Another source of a species of discontinuity in some, at least, of the constants is a reduction of E to such an extent that grouping of the spheres into doublets, triplets, &c., becomes possible. Thus we have a hint of the existence of a "critical temperature."

It must be confessed that, while we have only an approximate knowledge of the length of the mean free path (even among equal non-attracting spheres) when it amounts only to some two or three diameters, we practically know almost nothing about its exact value when the volume is so much reduced that no particle has a path longer than one diameter.

[It might be objected to the equation arrived at above, should it be found on comparison with experiment that α and γ are both positive, that it will not make p infinite unless v *vanish.* To this I need only reply that the equation has been framed on the supposition that the particles are in motion, and therefore free to move. What may happen when they become jammed together is not a matter of much physical interest, except perhaps from the point of view of dilatancy. If the equation represents, with tolerable accuracy, all the cases which can be submitted to experiment, it will fully satisfy all lawful curiosity.]

XXI.—*Relation between Kinetic Energy and Temperature.*

70. Before we can put the above virial equation into the usual form of a relation among p, v, and t, it is necessary that we should consider how the temperature of an assemblage of particles depends upon their average kinetic energy.

Van der Waals and Clausius, following the usual custom, take the average kinetic energy as being *proportional to* the absolute temperature. Clerk-Maxwell is more guarded, but he says:—"The assumption that the kinetic energy is determined by the absolute temperature is true for perfect gases, and we have no evidence that any other law holds for gases, even near their liquefying point."

On this question I differ completely from these great authorities, and may err absolutely. Yet I have many grave reasons on my side, one of which is immediately connected with the special question on hand. To take this reason first, although it is by no means the strongest, it appears to me that *only* if E above (with a *constant* added, when required, as will presently be shown) is regarded as proportional to the absolute temperature, can the above equation be in any sense accurately considered as that of an *Isothermal.* If the whole kinetic energy of the particles is treated as proportional to the absolute temperature, the various stages of the gas as its volume changes with E constant correspond to changes of temperature without direct loss or gain of heat, and belong rather to a species of *Adiabatic* than to an *Isothermal.* Neither Van der Waals nor Clausius, so far as I can see, calls attention to the fact that when there are molecular forces the mean-square speed of the particles necessarily increases with diminution of volume, even when the mean-square speed of a free particle is maintained unaltered; and this simply because the time during which each particle is free is a smaller fraction of the whole time. But when the whole kinetic energy is treated as a constant (as it must be in an *Isothermal*, when that energy is taken as measuring the absolute temperature), it is clear that isothermal compression must reduce the value of E. It further follows that the temperature of a gas might be enormously raised if its volume were sufficiently reduced by the process (capable of being carried out by Clerk-Maxwell's *Demons*) of advancing, at every instant, those infinitesimal portions of the containing walls on which no impact is impending. This is certainly not probable. If, on the other hand, we were to look at the matter from the point of view of intense inter-molecular *repulsion* (such as, for instance, Clerk-Maxwell's well-known hypothesis of repulsion inversely as the fifth power of the distance, which was so enthusiastically lauded by Boltzmann), we should be led to the very singular conclusion that such an assemblage of particles might possibly be cooled even by ordinary compression; certainly that the *Demons* could immensely cool it by diminishing its volume without doing work upon it.

If this mode of reasoning be deemed unsatisfactory, we may at once fall back on thermodynamic principles; for these show that a gas could not be in equilibrium if either external, or molecular, potential could establish a difference of temperature from one region of it to another. For it must be carefully remembered (though it is very often forgotten) that temperature-differences essentially involve the *transference* of heat, on the whole, in one direction or the other between bodies in contact:—so that if there be a cause which can produce these temperature-differences, it is to be regarded as a source of at least restoration of energy. Let the contents of equal volumes at different parts of a tall column of gas under constant gravity be compared. In each the pressure may be regarded, so far as it is due to the external potential, as being applied by bounding walls. But the temperature is the same in each, and the only other quantity which is the same in each is E. For, as the particles are free to travel from point to point throughout the whole extent of the group, the average value of E must be the same for all; and, therefore, in regions where the density is small, it must be that of free particles:—*i.e.*, absolute temperature.

71. For the isothermal formation of liquid, heat must in all cases be taken from the group. This must have the effect of diminishing the value of E. Hence, in a liquid, the temperature is no longer measured by E, but by $E+c$, where c is a quantity whose value increases steadily, as the temperature is lowered, from the value zero at the critical point. Thus, since of course we must take the physical fact of the existence of liquids as a new datum in our calculations, and with it the agglomeration into doublets, triplets, &c. (whose share of the average energy differs in general from that of their components when free), we see that the state of aggregation which we call liquid is such that, as it is made colder and colder, a particle which *can* escape from it requires to have more and more than its average share of the non-molecular part of the energy.

We might be tempted to generalise further, and to speculate on the limiting conditions between the liquid and the solid states. But these, and a host of other curious and important matters suggested by the present speculation, prominent among which is the question of the density of saturated vapour at different temperatures (with the *mechanism* of the equilibrium of temperature between the liquid and the vapour), must be deferred to the next part of this paper. It is sufficient to point out here how satisfactorily the present mode of regarding the subject fits itself to the grand facts regarding latent heat, and to its steady diminution as the pressure under which ebullition takes place is gradually raised to the critical value. What we are called upon to do now is to justify, by comparison with experiment, the hypothesis which we have adopted as to the proper physical definition of temperature, and the form of the virial equation to which it has led us. If we have any measure of success in this, we may regard the main difficulty of at least the elements of these further problems as having been to some extent removed.

What has been said above leads us, in the succeeding developments, to write (so long at least as we are dealing with vapour or gas)

$$E = Rt;$$

where t is the absolute temperature, and R (whose employment is now totally changed) is practically the rate of increase of pressure with temperature at unit volume, under ordinary conditions.

XXII.—*The Equation of Isothermals.*

72. Assuming the definition of temperature given in last section, the virial equation of § 68 becomes

$$pv = R\left(1 + \frac{e}{v+\alpha}\right)t + \frac{C}{v+\gamma} - \frac{A}{v+\alpha}.$$

For the minimax, which occurs at the critical point, we must have simultaneously

$$\frac{dp}{dv} = 0, \quad \frac{d^2p}{dv^2} = 0.$$

But

$$v\frac{dp}{dv} + p = \frac{A - Ret}{(v+\alpha)^2} - \frac{C}{(v+\gamma)^2},$$

$$v\frac{d^2p}{dv^2} + 2\frac{dp}{dv} = -2\frac{A - Ret}{(v+\alpha)^3} + \frac{2C}{(v+\gamma)^3}.$$

Denoting by a bar quantities referring to the critical point, these equations give

$$\bar{p} = \frac{A - Re\bar{t}}{(\bar{v}+\alpha)^2} - \frac{C}{(\bar{v}+\gamma)^2},$$

$$0 = \frac{A - Re\bar{t}}{(\bar{v}+\alpha)^3} - \frac{C}{(\bar{v}+\gamma)^3};$$

whence

$$A - Re\bar{t} = \frac{\bar{p}(\bar{v}+\alpha)^3}{\alpha - \gamma}, \quad C = \frac{\bar{p}(\bar{v}+\gamma)^3}{\alpha - \gamma}.$$

But the first equation of this section can be written as

$$pv = R\left(1 + \frac{e}{v+\alpha}\right)(t - \bar{t}) + R\bar{t} - \frac{A - Re\bar{t}}{v+\alpha} + \frac{C}{v+\gamma}.$$

By the help of the values of $A - Re\bar{t}$, and C, just found, and the further condition that $\bar{p}$, $\bar{v}$, $\bar{t}$ satisfy this general equation, we can easily put it in the form

$$p = \bar{p}\left(1 - \frac{(v-\bar{v})^3}{v(v+\alpha)(v+\gamma)}\right) + R\left(1 + \frac{e}{v+\alpha}\right)\frac{t-\bar{t}}{v} \quad\text{.....................(C).}$$

There are seven constants in this equation:—viz., $\bar{p}$, $\bar{v}$, $\bar{t}$, α, γ, e, and R; but there are two relations among them, one furnished by the usual condition that the gas treated has unit volume at 0° C., and 1 atm.; the other (from the conditions of the minimax) being

$$3\bar{v} + \alpha + \gamma = \frac{R\bar{t}}{\bar{p}}.$$

73. If we compare (C) with the corresponding forms of the equations of Van der Waals and Clausius ((A) and (B) of § 66 above) we see that all three agree in a remarkable manner as to the form of the equation of the critical isothermal. In fact, the only difference is that in (C) the divisor of $(v - \bar{v})^3$ contains three distinct factors, while in each of (A) and (B) two of the three factors are equal. It is quite otherwise with the term which expresses the difference of ordinates between the critical isothermal and any other of the series:—so that even if all three equations agreed in giving the correct form of the critical isothermal no two of them could agree for any other.

XXIII.—*Comparison with Experiment.*

74. We must now compare our formula with experiment. And here I have been exceptionally fortunate, as the kindness of M. Amagat has not only provided me with a complete set of values of pv in terms of p for CO_2 between the limits 1 to 1000 atm. and 0° to 100° C., but has further replied to my request for a set of values of p, at different temperatures, for certain special values of v. This important table I give in full, inserting columns of differences. It is very much better adapted than the former to numerical calculation, as the form of the virial equation requires that v should, for this purpose, be treated as the independent variable.

Pressure of CO_2 *in terms of Volume and Temperature* (AMAGAT).

At 0° C. and 1 atm. the volume is unity. After the experiments were completed the CO_2 was tested, and left 0·0004 of its volume when absorbed by potash.

The interpolated columns are differences (or average differences, if in brackets) of pressure for 10° at constant volume.

Vol.	·02385	·01636	·013	·01	·00768	·00578	·00428	·00316	·0025	·002	·00187
0°	31	34·4	...	...	...	..	...	34·4	...	...	307·5
	2	7·4	10	10	10	10	10	10			96·5
10	33	41·8	44·4	...	...	...	...	44·4	...	...	404
	2	3·3	6·7	11·9	12	12	12	12			116
20	35	45·1	51·1	56·3	56·4	...	...	56·4	64	300	520
	2	3·2	5·4	6·5	11·9	14·3	14·3	15·1	45	84	107·5
30	37	48·3	55·5	62·8	68·3	70·7	...	71·5	109	384	627·5
32	37·4	49	56·4	64·1	70	73·7	74·6	77			
35	38	49·9	57·6	65·8	72·6	77·2	79·5	84·7			
	2	3·1	4·2	5·8	8·3	12·4	17·1	26·5	46	86·5	122·5
40	39	51·4	59·7	68·6	76·6	83·1	87·8	98	155	470·5	750
	1·9	3·1	4·1	5·9	8·2	11·6	17·0	27·3	46	89·5	106·5
50	40·9	54·5	63·8	74·5	84·8	94·7	104·8	125·3	201	560	856·5
	1·9	3·1	4·0	5·7	8·0	11·5	17·1	28·5	49·5	91	97
60	42·8	57·6	67·8	80·2	92·8	106·2	121·9	153·8	250·5	651	953·5
	1·9	3·0	4·0	5·6	7·8	11·3	17·0	29·4	48	94	
70	44·7	60·6	71·8	85·8	100·6	117·5	138·9	183·2	298·5	745	
	1·9	2·9	3·9	5·5	7·6	11·3	17·4	28·3	47·5	88·5	
80	46·6	63·5	75·7	91·3	108·2	128·8	156·3	211·5	346	832·5	
	1·9	3·0	3·9	5·4	7·8	11·4	17·2	29	48·5	85·5	
90	48·5	66·5	79·6	96·7	116	140·2	173·5	240·5	394·5	918	
	2	3·0	4·0	5·6	7·8	11·1	17·6	30·5	49	80	
100	50·5	69·5	83·6	102·3	123·8	151·3	191·1	271	443·5	998	
	[1·73]	[2·8]	[3·7]	[5·1]	[7·2]	[10·6]	[16·4]	[28]	[46·8]		
137·5	57	80	97·5	121·5	151	191	252·5	376	619		
	[1·81]	[2·8]	[3·7]	[5·3]	[7·2]	[10·9]	[17·1]	[29·4]	[48]		
198	68	97	120	153·5	195	257	356	554	909		
	[1·75]	[2·5]	[3·3]	[4·6]	[6·6]	[9·8]	[15·6]				
258	78·5	112	140	181	234·5	316	449·5				

It is obvious, from a glance at the columns of differences, that the change of pressure at constant volume, while the CO_2 is not liquid, is almost exactly proportional to the change of temperature. M. Amagat expressly warned me that the three last temperatures in the table are only approximate, as they were not derived from air-thermometers, but simply from the boiling-points of convenient substances.

They appear to indicate a slow diminution of dp/dt (v constant) as the temperature is raised above 100° C., but this is beside our present purpose.

Leaving them out of account, we find that in the range 31° to 100° C. the fluctuations of the changes of pressure per 10° (at constant volume) are very small, and do not seem to follow any law. These fluctuations besides are, especially when the volume of the gas is small, well within the inevitable errors of observation in a matter of such difficulty. Hence we take a simple average in each column; and thus we have the following table:—

Average Change of Pressure per 10° *of Temperature at Constant Volume.*

v	·02385	·01636	·013	·01	·00768	·00578	·00428	·00316	·0025	·002	·00187
Δp	1·93	3·0	4·0	5·6	7·9	11·5	17·2	28·5	47·8	87·7	108 ?
$v\Delta p$	·046	·049	·052	·056	·061	·066	·074	·090	·120	·175	·20 ?
Calc.	·046	·049	·052	·056	·061	·068	·077	·087			
						·061	·073	·093	·122	·175	·20

The numbers in the fourth row are the values of

$$10\left(0{\cdot}00371+\frac{0{\cdot}000021}{v+0{\cdot}001}\right),$$

and those in the fifth row are from

$$10\left(0{\cdot}00371+\frac{0{\cdot}000011}{v-0{\cdot}0012}\right).$$

It is clear that these formulæ give fair approximations to the data, the first for volumes down to 0·005 or so, the second for smaller volumes.

Comparing with formula (C) of § 72, we see that the values of R, Re, and α are respectively

0·00371, 0·000021, and 0·001

for the larger volumes, and

0·00371, 0·000011, and − 0·0012

for the smaller. The values of γ and $\bar{v}$ can now be determined by the relation in § 72, and a few experimental data. After a number of trials I arrived at

$$\bar{v}=0{\cdot}0046,$$

as most consonant with the data for larger volumes; and I have provisionally assumed the value

$$\bar{v}=0{\cdot}004$$

for the lower range of volumes, in agreement with what was said in § 69 above as to the probable existence of a short, horizontal, portion of the critical isothermal. The value of γ for the first portion of the curve is found to be 0·0008; and I have assumed it to be − 0·0008 for the rest, thus ignoring the condition for the minimax at the commencement of this part of the curve. I consider this course to be fully justified by the arguments given in § 69 above. Thus, taking from the assumption below the value 73 atm. for the critical pressure, we arrive at the following equations for the parts of the critical isothermal which lie on opposite sides of the short, approximately straight, portion:—

$$p=73\left(1-\frac{(v-0{\cdot}0046)^3}{v\,(v+0{\cdot}001)(v+0{\cdot}0008)}\right),$$

and

$$p=73\left(1-\frac{(v-0{\cdot}004)^3}{v\,(v-0{\cdot}0012)(v-0{\cdot}0008)}\right).$$

In a careful plotting of the isothermals of CO_2 from the whole of Amagat's data (including, of course, those given above), I inserted, by means of differences calculated from the preceding formulæ for dp/dt, the probable isothermal of 31° C. This is only 0°·1 higher than the critical temperature as given by Andrews, which is certainly a little too low in consequence of the small admixture of air. The experimental data in the following table were taken directly from the curve so drawn. They are, of course, only approximate:—especially for the smaller volumes, for there the curves are so steep that it is exceedingly difficult to obtain exact values of the ordinates for any assigned volume. It is also in this region that the effects of the slight trace of air are most prominent.

Approximate Isothermal of 31° C.

The third line is calculated from the first of the above formulæ, the fourth line from the second.

v	1	·024	·02	·015	·0125	·01	·0075	·006	·005	·0045	·004	·0035	·003	·0025	·002
p (exp.)	1·12	37·1	42·4	51·6	57·2	63·4	69·6	72·4	72·9	73	73	73·2	76·8	114	392
p (calc.)	1·13	37·2	42·5	51·4	57·0	63·3	69·6	72·3	72·95	73	73·16	74·4	79·6	96·4	149
											73·0	73·2	79·1	117·6	377

For volumes down to 0·0035 the agreement is practically perfect. The remainder of the data, even with the second formula, are not very well represented. The value of p for volume 0·003 has given much trouble, and constitutes a real difficulty which I do not at present see how to meet. It is quite possible that, in addition to the defects mentioned above, I may have myself introduced a more serious one by assuming too high a value for the lower critical volume, or by taking too low a temperature for the critical isothermal. Had I selected the data for the isothermal of 31°·3 or so, it is certain that (with a slight change in $\bar{v}$) the agreement with the formula would have been as good as at present for the larger volumes, and it might have been much better for the smaller. But I have not leisure to undertake such tedious tentative work. As it is, the formulæ given above represent Amagat's results from 31° to 100° C. for volumes from 1 to 0·0035, with a maximum error of considerably less than 1 atmosphere even at the smallest of these volumes. And, even with the least of the experimental volumes, the approximations to the corresponding (very large) pressures are nowhere in error by more than some 4 or 5 per cent. This is at least as much as could be expected even from a purely empirical formula, but I hope that the relations given above (though still extremely imperfect) may be found to have higher claims to reception.

[Since the above was put in type it has occurred to me that this remarkable agreement, between the results of experiment on a compound gas, and those of a formula deduced from the behaviour of hard, spherical, particles, may be traced to the fact that the virial method is applicable, not only to the whole group of particles but (at every instant) to the *free* particles, doublets, triplets, &c., in so far as the *internal* relations of each are concerned. Hence the terms due to vibrations, rotations, and stresses, in free particles, doublets, &c., will on the average cancel one another in the complete virial equation. How far this statement can be extended to particles which are not quite free will be discussed in the next instalment. 5/6/91.]

[Some of the above remarks on Van der Waals' treatment of the virial equation were objected to by Lord Rayleigh and by Prof. Korteweg. The correspondence will be found in *Nature* (Vols. XLIV. and XLV., 1891—2). I quote here a few sentences of my own which, had I been rewriting instead of merely reprinting my paper, might have been in part at least incorporated in it.

"I had not examined with any particular care the opening chapters, to which your letter chiefly refers; probably having supposed them to contain nothing beyond a statement and proof of the Virial Theorem (with which I was already familiar) along with a reproduction of a good deal of Laplace's work.

Of course your account of this earlier part of the pamphlet (which I have now, for the first time, read with care) is correct. But I do not see that any part of my statements (with perhaps the single exception of the now italicized word in the phrase 'the *whole* procedure is erroneous') is invalidated by it. No doubt, the sudden appearance of a/v^2 in the formula above quoted is, to some extent at least, accounted for; but is the term correctly introduced?"

"I think that the mere fact of Van der Waals's saying (in a passage which is evidently applicable to his own processes, though it is applied only to that of Lorentz) 'die ganze Rechnung doch nur bis auf Grössen der ersten Ordnung (wie b/v) genau ist' throws very grave doubt on the whole investigation. For in the most interesting part of the critical isothermal of CO_2 the fraction b/v cannot be looked upon as a small quantity of the first order. In fact, without raising the question, either of Van der Waals's mode of interpreting the term $\frac{1}{2}\Sigma(mV^2)$ or of the paucity of constants in his equation, the above consideration would of itself render the results untrustworthy. Van der Waals has most opportunely and effectively called attention to an exceedingly promising mode of attacking a very difficult problem, and his methods are both ingenious and suggestive; but I do not think that his results can be regarded, even from the most favourable point of view, as more than '*Guesses at Truth.*'

For, if we take the experimental test, there can be no doubt that (as I have stated in § 65 of my paper) 'Van der Waals's curves cannot be made to coincide with those of Andrews.' And I think I have given reasons for believing that 'the term of Van der Waals's equation, which he took to represent Laplace's K, is not the statical pressure due to molecular forces but (approximately) its excess over the repulsion due to the speed of the particles.' Of course I mean by this that, when Van der Waals, comparing his equation with experiment, assigns a numerical value to his term a/v^2, he is not justified in regarding it as the value of Laplace's K; though that quantity was, he tells us, the main object of his inquiry."

"I do not agree with Prof. Korteweg's statement that Van der Waals's method, if it could be worked out with absolute rigour, would give the same result as the direct method. There is but one way of dealing with the virial equation:—if we adopt it at starting we must develop its terms one by one, and independently. In this connection I may refer to Lord Rayleigh's statement (*Nature*, 26/11/91): 'It thus appears that, contrary to the assertion of Maxwell, p *is* subject to correction.' I cannot admit that p is 'corrected'; it is not even changed either in meaning or in value. It is introduced as, and remains (at the end of any legitimate transformations of the equation) the value of the pressure on the containing vessel. This, of course, depends upon what is going on in the interior. Other terms in the virial equation, which happen to have the same factor, may be associated with p for convenience; they assist in finding its value, but they do not change its meaning, nor do they 'correct' it." 1899.]

LXXXI.

ON THE FOUNDATIONS OF THE KINETIC THEORY OF GASES. V.

(*Abstract.*)

[*Proceedings of the Royal Society of Edinburgh, February* 15, 1892.]

THE first instalment of this part of my paper deals mainly with the theory of the behaviour of *mixtures* of CO_2 and N, for which some remarkable experimental results were given by Andrews about 1874. His full paper, so far as he had drawn it up for press, was published posthumously in the *Phil. Trans.* for 1886, and is reprinted in his *Scientific Papers*, No. L. One special reason for the introduction of this question at the present stage of my work was the desire to attempt a correction of Amagat's numbers, for the (very small) admixture of air with his CO_2. The virial equation for a mixture is formed on the same general principle as that I employed for a single gas. There are, of course, more undetermined constants:—and, unfortunately, the data for their determination are barely adequate. The general results, however, agree in character with those described by Andrews:—the particular phenomenon which is most closely studied being the increase of volume, at constant pressure, when the gases (originally separated by the liquefaction of one) were allowed to diffuse into one another.

Since Part IV. of this paper was printed, M. Amagat has published (*Comptes Rendus*, October 12, 1891) additional data of a most valuable character bearing on the isothermals of CO_2:—especially the very important isothermal of 32° C.; and he has given the pressure of the saturated vapour at 0°, 10°, 20°, and 30° C. I have endeavoured to utilise these, as far as possible, not only for my present main object:—the examination of the relation between temperature and kinetic energy:—but also, incidentally, for the determination of the latent heat of the saturated vapour at various temperatures, and the relative densities of the liquid and vapour when in equilibrium. These data have also enabled me to obtain more exact approximations to the values of the constants in my formula, and thence to improve my determinations of the critical temperature, pressure, and volume.

In § 71 of Part IV. I arrived at the conclusion that "in a liquid the temperature is no longer measured by E [the part of the kinetic energy which is independent of the molecular forces], but by $E+c$, where c is a quantity whose value increases steadily, as the temperature is lowered, from the value zero at the critical point." For numerical data to test this conclusion, I study a cycle formed from the critical isothermal and any lower one, and two lines of equal volume, corresponding to those of the liquid and the saturated vapour when in equilibrium at that lower temperature. The change of energy in passing from one of these limits of volume to the other is found to be less for the critical isothermal than for any lower one. Thus the mean specific heat at constant volume, for the range of temperature employed, is less in the vapour than in the liquid. But from the equation, which is found to satisfy very closely the data for the isothermals of the gas for some 70 degrees above the critical point and of the vapour for 30 degrees below that point, it appears that the specific heat at constant volume is sensibly constant within these limits. [At 100° C. and upwards, it appears that $\frac{dp}{dt}$ falls off; so that $\frac{d^2p}{dt^2}$ is negative, and the specific heat at constant volume is therefore, even in the gas, greater for smaller volumes. But this does not seriously affect the above statement.] Hence, at any volume less than the critical volume, more heat is required to raise the temperature 1 degree when the substance is wholly liquid than when it is gaseous. This completely justifies the statement quoted above, provided that we assume the properties of the liquid and gas to merge continuously into one another at the critical temperature; but, unfortunately, the data are not sufficient to give more than very rough estimates of the value of the quantity c there spoken of.

I am at present engaged in endeavouring to obtain more exact values of the constants in my equation, in order to improve my estimates. Thus the numbers which follow may have to undergo some modifications, but there seems to be no reason for thinking that these are likely to be serious.

If v_1, v_2 be the respective volumes of the saturated vapour, and of the liquid, at absolute temperature t, we know that the latent heat is expressed by the formula—

$$\lambda = t\frac{dp}{dt}(v_1 - v_2).$$

From Amagat's data I find for the values of this quantity, and for the ratio of the densities of the liquid and vapour:—

Temperature C.	λ	v_1/v_2
0°	4·369	9·023
10	3·788	6·200
20	2·882	3·823
30	1·460	1·906

Taking the density of CO_2 at 0° C. and 1 atm. as 0·002, it is easy to see that the values of λ must be multiplied by

$$\frac{500}{62{\cdot}5} \cdot \frac{2117}{1390}, = 12{\cdot}2 \text{ nearly},$$

to reduce them to ordinary heat units. Thus the latent heat at 0° C. is about 53, while at 30° C. it is only 17·8.

In the following table P represents the gain of energy from the liquid state to that of saturated vapour, at the indicated temperature:—*i.e.*,

$$P = \left(t\frac{dp}{dt} - p\right)(v_1 - v_2);$$

while

$$Q = \int_{v_2}^{v_1} \left(t\frac{dp}{dt} - p\right) dv$$

is the corresponding gain of energy, in the critical isothermal, between the same limits of volume.

Temperature C.	P	Q
0°	3·747	3·577
10	3·244	3·113
20	2·459	2·409
30	1·233	1·203

The difference, $P - Q$, is (when multiplied, as above, by 12·2) nearly equal to the excess of the heat required to raise the temperature of the liquid (at constant volume) to the critical point, over that required to raise the temperature of the vapour, from saturation, through the same range, the volume remaining unaltered.

It appears that CO_2, when passing through the range of volume spoken of in § 69 of Part IV. of my paper, has about half the density of water.

[The paper, of which the above is an Abstract, was never fully written out for press. Further papers of M. Amagat soon led to (slight) modifications of the curves which had been employed in my calculations; so that the numbers given above require some change, and we have now the data necessary. I find, for instance, that I had long ago noted (as an improved version of the last column of the table opposite) the figures

v_1/v_2
9·52
6·45
4·08
1·79

which are very nearly the same as those given by M. Amagat in the *Comptes Rendus* for March 13th of this year. 1899.]

LXXXII.

NOTE ON THE EFFECTS OF EXPLOSIVES.

[*Proceedings of the Royal Society of Edinburgh, February* 21, 1887.]

MANY of the victims of the dynamite explosion, a year or two ago, in the London Underground Railway, are said to have lost the drum of one ear only, that nearest to the source. This seems to point to a projectile, not an undulatory, motion of the air and of the gases produced by the explosion. So long, in fact, as the disturbance travels faster than sound, it must necessarily be of this character, and would be capable of producing such effects.

Another curious fact apparently connected with the above is the (considerable) finite diameter of a flash of forked lightning. Such a flash is always photographed as a line of finite breadth, even when the focal length is short and the focal adjustment perfect. This cannot be ascribed to irradiation. The air seems, in fact, to be driven outwards from the track of the discharge with such speed as to render the immediately surrounding air instantaneously self-luminous by compression.

Such considerations show at once how to explain the difference between the effects of dynamite and those of gunpowder. The latter is prepared expressly for the purpose of developing its energy gradually. Thus while the flash of gunpowder fired in the open is due mainly to combustion of scattered particles,—that produced by dynamite is mainly due to impulsive compression of the surrounding air, energy being conveyed to it much faster than it can escape in the form of sound.

LXXXIII.

ON THE VALUE OF $\Delta^n 0^m/n^m$, WHEN m AND n ARE VERY LARGE.

[*Proceedings of the Edinburgh Mathematical Society*, Vol. v., 1887.]

I HAD occasion, lately, to consider the following question connected with the Kinetic Theory of Gases:—

Given that there are 3.10^{20} particles in a cubic inch of air, and that each has on the average 10^{10} collisions per second; after what period of time is it even betting that any specified particle shall have collided, once at least, with each of the others?

The question obviously reduces to this:—Find m so that the terms in

$$X^m = (x_1 + x_2 + x_3 + \ldots\ldots + x_n)^m$$

which contain each of the n quantities, once at least, as a factor, shall be numerically equal to half the whole value of the expression when $x_1 = x_2 = \ldots\ldots = x_n = 1$. Thus we have

$$X^m - \Sigma (X - x_r)^m + \Sigma (X - x_r - x_s)^m - \ldots\ldots = \tfrac{1}{2} X^m$$

or

$$\Delta^n 0^m / n^m = \tfrac{1}{2}.$$

It is strange that neither Herschel, De Morgan, nor Boole, while treating differences of zero, has thought fit to state that Laplace had, long ago, given all that is necessary for the solution of such questions. The numbers $\Delta^n 0^m$ are of such importance that one would naturally expect to find in any treatise which refers to them at least a statement that in the *Théorie Analytique des Probabilités* (Livre II., chap. II., § 4) a closely approximate formula is given for their easy calculation. No doubt the process by which this formula is obtained is somewhat difficult as well as troublesome, but the existence of the formula itself should be generally known.

When it is applied to the above problem, it gives the answer in the somewhat startling form of "about 40,000 years." [*Ante*, No. LXXVIII., p. 156. 1899.]

P.S.—April 4, 1887.—Finding that Laplace's formula ceases to give approximate results, for very large values of m and n when these numbers are of the same order of magnitude, I applied to Prof. Cayley on the subject. He has supplied the requisite modification of the formula, and his paper has been to-night communicated to the *Royal Society of Edinburgh*. [*Cayley's Mathematical Papers*, Vol. XII., No. 853. 1899.]

LXXXIV.

NOTE ON MILNER'S LAMP.

[*Proceedings of the Edinburgh Mathematical Society*, Vol. v., 1887.]

THIS curious device is figured at p. 149 of De Morgan's *Budget of Paradoxes*, where it is described as a "hollow semi-cylinder, *but not with a circular curve*," revolving on pivots. The form of the cylinder is such that, whatever quantity of oil it may contain, it turns itself till the oil is flush with the wick, which is placed at the edge.

Refer the "curve" to polar coordinates, r and θ; the pole being on the edge, and the initial line, of length a, being drawn to the axis. Then if θ_0 correspond to the horizontal radius vector, β to any definite radius vector, it is clear that the couple due to the weight of the corresponding portion of the oil is proportional to

$$\int_{\theta_0}^{\beta} r^2 d\theta \left\{a \cos\theta_0 - \frac{2}{3} r \cos(\theta - \theta_0)\right\}.$$

This must be balanced by the couple due to the weight of the lamp, and of the oil beyond β; and this, in turn, may be taken as proportional to

$$\cos(\alpha + \theta_0).$$

Thus the equation is

$$a \cos\theta_0 \int_{\theta_0}^{\beta} r^2 d\theta - \frac{2}{3}\left(\cos\theta_0 \int_{\theta_0}^{\beta} r^3 \cos\theta d\theta + \sin\theta_0 \int_{\theta_0}^{\beta} r^3 \sin\theta d\theta\right) = b^3 \cos(\alpha + \theta_0).$$

Differentiating twice with respect to θ_0, and adding the result to the equation, we have (with θ now put for θ_0)

$$2ar^2 \sin\theta - 2ar\frac{dr}{d\theta}\cos\theta + 2r^2\frac{dr}{d\theta} = 0.$$

Rejecting the factor r, and integrating, we have

$$r^2 = 2ar\cos\theta + C.$$

This denotes a *circular* cylinder, in direct contradiction to De Morgan's statement!

As it was clear that this result, involving only one arbitrary constant, could not be made to satisfy the given differential equation for all values of b, α, and β, I fancied that it could not be the complete integral. I therefore applied to Prof. Cayley, who favoured me with the following highly interesting paper. It commences with the question I asked, and finishes with an unexpectedly simple solution of Milner's problem. [*Cayley's Mathematical Papers*, Vol. XIII., No. 889. 1899.]

It appears clear that De Morgan did not know the solution, for the curve he has sketched is obviously one of continued curvature—and he makes the guarded statement that a friend "vouched for Milner's Lamp."

LXXXV.

AN EXERCISE ON LOGARITHMIC TABLES.

[*Proceedings of the Edinburgh Mathematical Society*, Vol. v., 1887.]

IN reducing some experiments, I noticed that the logarithm of 237 is about 2·37.... Hence it occurred to me to find in what cases the figures of a number and of its common logarithm are identical:—*i.e.*, to solve the equation

$$\log_{10} x = x/10^m,$$

where m is any positive integer.

It is easy to see that, in all cases, there are two solutions; one greater than, the other less than, ϵ. This follows at once from the position of the maximum ordinate of the curve

$$y = (\log x)/x.$$

The smaller root is, for $m = 1,\quad x = 1{\cdot}371288\ldots\ldots$

$$m = 2,\quad x = 1{\cdot}023855\ldots\ldots$$

For higher values of m, it differs but little from 1, and the excess may be calculated approximately from

$$y - y^2/2 + \ldots = (1 + y)\log_e 10/10^m.$$

Ultimately, therefore, the value of the smaller root is

$$1{\cdot}00\ldots\ldots0230258\ldots\ldots$$

where the number of cyphers following the decimal point is $m - 1$.

The greater root must have $m+p$ places of figures before the decimal point; p being unit till $m=9$, then and thenceforth 2 till $m=98$, 3 till $m=997$, &c.* Thus, for example, if $m>8<98$ we may assume

$$x=(m+1)\,10^m+y,$$

so that $$\log_{10}\frac{m+1}{10}+\log_{10}\left\{1+\frac{y}{(m+1)\,10^m}\right\}=\frac{y}{10^m},$$

which is easily solved by successive approximations.

But it is simpler, and forms a capital exercise, to find, say to six places, the greater root, by mere inspection of a good Table of Logarithms.

Thus we find, for instance,

m	x
17	$182{,}615\,.\,10^{13}$
18	$192{,}852\,.\,10^{14}$
96	$979{,}911\,.\,10^{92}$
97	$989{,}956\,.\,10^{93}$

* [It is easy to see that the indices, of the integral powers of 10 which satisfy the original equation, are themselves of the form 10^q, where q is such that

$$m=10^q-q.$$

Thus, with $q=0$, we have 10 itself as the greater root when $m=1$. 1899.]

LXXXVI.

ON GLORIES.

[*Proceedings of the Royal Society of Edinburgh, July,* 1887.]

WHEN Mr Omond was appointed to the Ben-Nevis Observatory I requested him to take every opportunity of observing what are called Glories—especially noting, when possible, their angular diameters and the order of their colours, so that it might be possible to decide upon the exact mode in which they are produced.

Young, while attributing to their true cause the spurious (or supernumerary) rainbows, proceeds to say:—"The circles, sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection."—[*Lectures,* II. p. 645.]

Now from Mr Omond's observations it appears that the mists to which the glories are due produce coronæ of, say, 2° or 3° radius;—from which it follows that the diameter of the particles is somewhere of the order $\frac{1}{1000}$ inch. It is thence shown that, were Young's explanation correct, the radii of the rings would vary with great rapidity in passing from one kind of homogeneous light to another. This is altogether irreconcilable with Mr Omond's observations.

That the glories are not of the nature of spurious rainbows is shown very simply by the fact that they are more intense as their radii are smaller.

Hence, the only possible explanation is diffraction depending on the *form* of the vertex of the reflected wave. The form of an originally plane wave, once reflected inside a drop of water is, roughly, when the central ray has just emerged, a portion of an hyperboloid of revolution, doubled back cusp-wise round its border. An approximate calculation is given, based on this assumption.

A simple first approximation to the theory of glories is given by the behaviour of a plane wave incident normally on a screen pierced with a great number of very small circular apertures of nearly equal size. They are thus, to a certain extent, analogous to coronæ.

APPENDIX.

On Mr Omond's Observations of Fog-Bows.

[*Proceedings of the Royal Society of Edinburgh, January,* 1888.]

The author remarked that one of the constituents of the *double* fog-bow described in some of Mr Omond's recent observations*, is obviously the ordinary primary rainbow, diminished in consequence of the very small size of the water drops. But the other, having nearly the same radius but *with its colours in the opposite order*, appears to be due to ice-crystals in the fog. This is quite consistent with the record of temperatures. Just as small drops of water may remain unfrozen in air below 0° C., small ice-crystals may remain unmelted at temperatures above that point.

* *Proceedings R.S.E.* XIV. p. 314.

LXXXVII.

PRELIMINARY NOTE ON THE DURATION OF IMPACT.

[*Proceedings of the Royal Society of Edinburgh, Feb.* 20, 1888.]

THE results already obtained were got by means of a roughly made apparatus designed for the purpose of testing the method used, so that only a single instance, to show their general character, need now be given. When a wooden block of 10 lbs. mass fell through a height of $18\frac{1}{4}$ inches on a rounded lump of gutta-percha, the time of impact was found to be somewhere about 0·001 sec., and the coefficient of restitution was 0·26.

As the principle of the method has been found satisfactory in practice, new apparatus is in course of construction, which will enable me to use a fall amounting to 10 feet at least. It is proposed to make a series of experiments on different substances, with great varieties of mass and of speed in the impinging body.

LXXXVIII.

ON IMPACT.

[*Transactions of the Royal Society of Edinburgh*, Vol. XXXVI. Revised Nov. 8, 1890.]

THE present inquiry is closely connected with some of the phenomena presented in golf:—especially the fact that a ball can be "jerked" nearly as far as it can be "driven." For this, in itself, furnishes a complete proof that the duration of the impact is exceedingly short. But it does not appear that any accurate determination of the duration can be made in this way. Measurements, even of a rude kind, are impracticable under the circumstances.

In 1887 I made a number of preliminary experiments with the view of devising a form of apparatus which should trace a permanent record of the circumstances of impact. I found that it was necessary that one of the two impinging bodies should be fixed:—at least if the apparatus were to be at once simple and manageable. This arrangement gives, of course, a result not directly comparable with the behaviour of a golf-ball. For pressure is applied to one side only, both of ball and of club; but when one of two impinging bodies is fixed it is virtually struck simultaneously on both sides. Even with the altered conditions, however, the inquiry seemed to be worth pursuing. I determined to operate, at least at first, on *cylinders* of the elastic material; so fixed that considerable speed might be employed, while the details of several successive rebounds could be recorded. It is not at all likely that this will be found to be the best form for the distorted body; but it was adopted as, in many respects, convenient for preliminary work. For the main object of the experiments was to gain *some* information about a subject which seems to have been left almost entirely unexplored; and it is only by trial that we can hope to discover the best arrangement. Messrs Herbertson and Turnbull, who were at the time Neil-Arnott Scholars, and working in my Laboratory, rendered me great assistance in these preliminary trials, whose result was the construction of a first rude apparatus on the following plan.

A brick-shaped block of hard wood was dropped endwise from a measured height upon a short cylinder of cork, vulcanized india-rubber, gutta-percha, &c., which was imbedded to half its length in a mass of lead, firmly cemented to an asphalt floor. The block slid freely between guide-rails, precisely like the axe of a guillotine. In front of the block was a massive fly-wheel, fitted on one end of its axle, and carrying a large board (planed true) on which was stretched, by means of drawing-pins, a sheet of cartridge-paper. The sheet was thus made to revolve in its own plane. A pencil, projecting from the block, was caused by a spring to press lightly upon the paper; and it was adjusted so that its plane of motion passed as exactly as possible through the axis of the paper disc. To prevent breakage of the pencil on the edge of the disc, it was pushed into its bearings, and released by a trigger only after it had, in its fall, passed the edge. The block, having fallen, rebounded several times to rapidly diminishing heights and, after a second or two, came to rest on the cork cylinder. The pencil then traced a circle and, as soon as this was complete, the fly-wheel (previously detached from the gas-engine) was at once stopped by the application of a very powerful brake. The circle thus described was the datum line for all the subsequent measures; since the tracings which passed beyond it were obviously made *during* the impact, while those within it referred at least mainly to the comparatively free motion between two successive impacts. The duration of the impact was at once approximately given by the arc of the circle intercepted between the tracings of the pencil as it passed out and in, combined of course with the measured angular velocity of the fly-wheel. It is not yet known at what stage during the recovery of form the impinging bodies go out of contact with one another. In the present paper we are content to assume that contact commences and terminates at the instants of passage across the datum circle. This is certainly not rigorously true as regards the commencement, but the assumption cannot introduce any serious error; while of the termination we have no knowledge. It may be remarked, in passing, that the error at commencement will necessarily be greater the larger the mass of the falling body. It will also be greater for soft than for hard bodies, and especially for those of the former class which most depart from Hooke's Law.

In the winter 1887–8, and in the subsequent summer, some very curious results were obtained by Messrs Herbertson and Turnbull with this rough apparatus. Several of these were communicated to the Society at the time when they were obtained. Thus, for instance, it was found that although the mass of the block was over 5 lbs., the time of impact on a cork cylinder was of the order of $0^s{\cdot}01$ only, while with vulcanite it was of the order $0^s{\cdot}001$. Also, for one and the same body, the duration was *less*, the more violent the impact. [The golf result mentioned above was now at once explained; for, as the mass of a golf-ball is less than $\frac{1}{50}$ of that of the block, under equal forces its motions will be fifty times more rapid. Thus, even if it were of cork, the time of impact would be of the order of about one five-thousandth of a second only; and the shorter the more violent the blow.] Taking the coefficient of restitution as 0·5 on the average, the time-average of the force during impact after a fall of 4 feet was, for these classes of bodies respectively, of the orders 400 lbs. weight and 4000 lbs. weight. This result is of very high interest from many points of view.

The values of the coefficient of restitution for impacts of different intensity were obtained by drawing tangents to the fall-curve at its intersections with the datum circle corresponding to the assumed commencement and end of each impact, and finding their inclination, each to the corresponding radius of the circle. The coefficient of restitution is, of course, the ratio of the tangents of these angles. The results of these graphical methods could easily be checked by forming the polar equations of the various branches of the fall-curve (ascending and descending) and obtaining the above-mentioned tangents of angles by direct differentiation. If we assume the friction (whether of rails or pencil) to be approximately constant, it is easy to see that the equation of the part of the tracing made during a fall, or during a rise, can be put in the very simple form

$$r = A + B\theta^2.$$

Here the centre of the disc is the pole, and the initial line is the particular radius which was vertical when the block was at one of its successive highest positions. This radius separates the rise, from the fall, part of each branch of the curve. A is of course the same for both parts, but B (being directly as the acceleration of the block, and inversely as the square of the angular velocity of the disc) is larger for the rise than for the fall; because friction aids gravity in the ascent and acts against it in the descent. A number of sets of corresponding values of the polar coordinates were measured on each part of the curve, the angles being taken from an approximately assumed initial line. Three of these sets determined A, B, and the true position of the initial radius; and the others were found to satisfy (almost exactly) the equation thus formed. This shows that the assumption, of friction nearly constant throughout the whole trace, is sufficiently accurate. B is always positive in the equation, but A is negative or positive according as the block does, or does not, rebound to a height greater than the radius of the datum circle.

It is not necessary to tabulate here any of the very numerous results of these earlier experiments. While the work was in progress many valuable improvements of the apparatus suggested themselves, and I resolved to repeat the experiments after these had been introduced. The whole of these subsequent results are tabulated below. The following were found to be the chief defects of the earlier arrangement, so far at least as they were not absolutely inherent in the whole plan. These have been since remedied; and results obtained with the improved apparatus have been, from time to time, communicated to the Society.

1. The use of a pencil is objectionable from many points of view. Serious worry and much loss of time are incurred in consequence of the frequent breaking of the lead, even when every possible precaution seems to have been taken. Then the rapid wearing-down of the point by the cartridge-paper causes the later-traced portions of each diagram (including especially the datum circle, which is of vital importance) to be drawn in broad lines, whose exact point of intersection can be but roughly

guessed at. The friction, also, was (mainly on account of the roughness of the paper) so large that the values of B, for the ascending and descending parts of any one branch of the curve, differed from the mean by a large fraction of it, sometimes as much as 20 per cent. This is approximately the ratio which the acceleration due to friction bears to that due to gravity; so that the friction was, at least occasionally, as much as one pound weight. This, of course, seriously interfered with the accurate measure of the coefficient of restitution. Instead of the board and cartridge-paper I introduced a specially prepared disc of plate-glass, which ran perfectly true. It was covered uniformly with a thin layer of very fine printers' ink, which was employed *wet*. For the pencil was substituted a needle-point, so that this part of the apparatus was rendered exceedingly light, strong, and compact. The lines traced could easily be made as fine as those of an etching, but it was found that a slightly blunted point (giving a line of about 0·005 inch in breadth) produced probably less friction, at all events less irregularity, than did a very sharp one. The difference of either value of B from the mean rarely amounted to more than 1·5 or 2 per cent. of the mean. When the ink was dry, which happened after about a day, photographic prints were taken by using the disc as a negative. [In the later experiments it was found that, when proper precautions were taken, no delay on this account was necessary.] To test whether the paper of the positives had been distorted, in drying after fixing, a number of circles were described on the glass disc at various places before the ink was dry. They were found to remain almost exactly circular on the dried photograph. All the subsequent measurements were made on these photographs. In a subsequent paper I hope to give the results of careful micrometric measures, made on the glass plate itself, of the form of the trace during impact. This may lead to information which could not be derived from the photographs themselves with any degree of accuracy. My first object was to obtain a number of separate experiments, so as to get the general laws of the phenomena, and for this purpose the glass plate had to be cleaned and prepared for a new series of experiments as rapidly as possible. The micrometric measures cannot be effected in a short time.

2. In the earlier experiments the fly-wheel continued in connection with the gas-engine until the fall was completed. Hence the rate of rotation was irregular, and the mode adopted for its measurement gave an average value only. In the later experiments an electrically-controlled tuning-fork, furnished with a short bristle, made its record on the disc, simultaneously with the fall of the block; and the gas-engine belt was thrown on an idle pulley immediately before the experiment commenced. The angular velocity of the disc was sensibly different in different experiments, according as the engine was thrown off just before, or just after, an explosion. But the fact that its fly-wheel is a gigantic one made these differences of small importance. They were, however, always taken account of in the reductions. The disc, when left to itself, suffered no measurable diminution of angular velocity during a single turn. In the earlier experiments one rotation of the disc occupied about $0^s{\cdot}3$; but I was afraid to employ so great a speed with the glass plate, so its period was made not very different from one second. I found it easy to obtain on the glass disc the records of four successive falls, each with its series of gradually diminishing rebounds,

and along with these the corresponding serrated lines for the tuning-fork. These records were kept apart from one another by altering the position of the fork, as well as that of the needle-point on the block, immediately after each fall. The latter adjustment alters, of course, nothing but the radius of the datum-circle, and the corresponding values of the quantity A. As soon as the four falls had been recorded, the glass disc was dismounted, and all the necessary details of the experiment—*e.g.*, date, heights of fall, substance impinged on, mass of block, &c.—were written (backwards) on the printers' ink, with a sharp point, and of course appeared on the photograph. The changes of mass, just alluded to, were occasionally introduced by firmly screwing on the top of the block a thick plate of lead of mass equal to its own.

3. A very troublesome difficulty was now and then met with, but chiefly when the elastic substance employed was a hard one, such as vulcanite or wood. For the block was occasionally set in oscillation during the impact, and especially at the instant when it was beginning to rebound. The trace then had a wriggling o- outline, altogether unlike the usual smooth record. Sometimes the wriggle perpendicularly to the disc, and the trace was then alternately broadened an evanescent. After some trouble I found that the main cause was the slig nt (produced by repeated falls on hard bodies) in the striking part of the block, which had originally been plane. The wriggling always appeared when this dent did not fit *exactly* upon the (slightly convex) upper end of the hard cylinder. To give free play at the moment of impact, the lower part of the guide-rails had been, by filing, set a very little further apart than the rest, and thus small transverse oscillations of the block were possible. I hope to avoid this difficulty in future, by fixing a hard steel plate on the striking part of the block, and making all the remaining experiments with this. Of course a few of the former experiments must be repeated in order to discover whether the circumstances are seriously, or only slightly, modified by the altered nature of the striking surface. There can be no doubt that the distortion, as tabulated, belongs in part to each of the impinging bodies; but it is not easy to assign their respective shares.

The general nature of the whole trace of one experiment will be obvious from the upper figure in the Plate, which is reduced to about 0·3 of the actual size. The lower figures (drawn full size) show the nature of the trace during impact:—the first series, some of which exhibit the "wriggles" above described, belonging to the pencil records of the old apparatus; the second series containing some of those obtained with the improved form just described.

In the earlier work, with the cartridge-paper, falls of 8 and even of 12 feet were often recorded. The results of the later work have been, as yet, confined to falls of 4 feet at most. But I intend to pursue the experiments much further, after fitting an automatic catch on the apparatus; such as will prevent the block from descending a second time if it should happen to rebound so far that the needle-point leaves the glass disc.

What precedes is of course designed to furnish only a general notion of the nature of the apparatus, the principle on which it works, and the results already obtained with it. Some further remarks, on the physical principles involved, will be made after details of dimensions, and of numerical data have been given. But it must be stated here that with the later form of the apparatus it was found necessary to have a party of at least three engaged in each experiment; one to attend to the driving-gear, a second to the falling block, and a third to the tuning-fork. My assistant, Mr Lindsay, took the first post; I usually took the second myself; and the fork was managed by Mr Shand, to whom I am besides indebted for the greater part of the subsequent measurements and reductions. These, of course, involved an amount of work which, though not perhaps more difficult than the rest, was incomparably longer and more tiresome.

Description of the Apparatus.

Two beams nearly 12 feet long, and 6 inches by $2\frac{1}{2}$ inches cross section, are rigidly fixed, vertically, and at a distance of $8\frac{1}{2}$ inches from each other, to a massive stone pillar. To them the rails, which act as guides for the falling body, are screwed, the distance between them being $6\frac{5}{8}$ inches. At the base, between the rails, is a cylinder of lead, 6 inches by 6 inches, firmly imbedded in a mass of concrete, and having on its upper end a hole, $\frac{5}{8}$ inch deep and $1\frac{1}{4}$ inch diameter, for holding the lower end of the substance experimented on. This consists of cork, india-rubber, vulcanite, &c., as the case may be, cut into a cylinder, $1\frac{1}{4}$ inch diameter, and $1\frac{1}{4}$ inch long, with the lower end flat and the upper slightly rounded. It thus projects about $\frac{7}{8}$ inch after being thrust home into the hole in the leaden cylinder, in which it rests on a thin disc of gutta-percha. This was found effectually to prevent the cylinder's being displaced in the lead-block. Before it was introduced, the cylinder was occasionally left not in contact with the bottom of the hole, so that the record of the next impact was vitiated. Sometimes, indeed, the cylinder had jumped entirely out of the hole before the block redescended.

In a plane, parallel to that which contains the guides and nearly $2\frac{1}{2}$ inches from it, a massive fly-wheel, $28\frac{3}{4}$ inches diameter, whose moment of inertia is 102·6 in lbs. sq. ft., is placed. The iron frame supporting it is fixed to the concrete floor by means of bolts, so that the whole can be rigidly fixed in position or lifted back at pleasure A thick wooden board is firmly attached to the front of this wheel, aud on it is laid a sheet of felt. On the top of the felt, an octagonal plate of glass, about $\frac{3}{8}$ inch thick, the edges of which are bevelled, is placed, and then firmly pressed to the board by means of bevelled metal plates, covered with felt, and screwed down on four alternate edges.

The mass of the glass is 28 lbs., its moment of inertia 25·21. For the wood these are 21·5 and 24·19 respectively. The total mass (including the fly-wheel) being 122·5 lbs., k^2 is found to be about 1·24 sq. ft.

A rope passing up the outside of one of the beams, over two small pulleys, and down between the rails, serves to raise and lower the block, next to be described, or to keep it suspended by a hook at any desired height. A cord running parallel to the rope is attached to the catch of the hook at the end of the rope, so that by pulling this cord the hook is tilted and allows the block to fall.

The block is rectangular, and formed of hard wood (plane-tree along the grain), $11\frac{1}{2}$ by $7\frac{1}{2}$ by $2\frac{1}{2}$ inches, weighing $5\frac{1}{4}$ lbs. Down the centre of each of the edges runs a deep groove, at the ends of which pieces of iron with a polished groove of U section are screwed on. It is on these that the guides bear while the block is falling. The guides and Us being well oiled, the friction is reduced to a minimum.

A brass plate, $5\frac{1}{4}$ inches by $2\frac{1}{2}$ inches, is sunk into the face of the block about $\frac{1}{8}$ inch, and through the plate and wood a longitudinal slot, 3 inches by $\frac{5}{8}$ inch, is cut, the centre of the slot coinciding with the centre of the block. Another plate of brass, $3\frac{1}{8}$ inches by $2\frac{1}{4}$ inches, with two parallel slots $2\frac{1}{2}$ inches long and $\frac{1}{4}$ inch broad, half an inch distant from, and on either side of the centre, lies on the fixed plate, and can be clamped to it by means of flat-headed screws passing through the slots. This movable plate has, therefore, a longitudinal (vertical) play of about 2 inches when the screws are loose. It carries the tracing-point and its adjusting mechanism.

The tracing-point is at the extremity of a steel rod, one inch of whose length is of $\frac{1}{4}$ inch diameter, the remaining $\frac{3}{4}$ inch being of rather less than $\frac{1}{8}$ inch diameter. The thicker part works freely, but not loosely, in a cylindrical barrel, the thinner part passing through a collar at the front end. The cylinder is fixed, at right angles, to the movable brass plate, and passes through the slot in the block. The rod is lightly pressed forwards at the thicker end by a piece of watch-spring, so as to keep it, when required, steadily in contact with the revolving disc. In the wall of the cylindrical barrel is a long slot which runs backwards for $\frac{1}{2}$ inch parallel to the axis, and then, turning at right angles to its former direction, runs through a small fraction of the circumference of the barrel. In this slot works a stout wire screwed perpendicularly into the rod which carries the tracing-point. Of course when this wire is in the transverse part of the slot the needle-point is retracted; but as soon as it is turned into the axial part the spring makes the needle-point project through the collar. Before the block falls, the wire is in the transverse part of the slot, and the needle-point is retracted. But when, in its fall, the point has passed the edge of the glass disc, a pin fixed at the proper height catches the end of the wire and turns it into the axial slot. As soon as the tracing is complete, the wire is forced back (by means of a system of jointed levers) into the transverse slot, and thus the tracing-point is permanently withdrawn from the disc, so that the block can be pulled up, and adjusted for another fall.

The last part of the apparatus to be described is that for recording the time.

It consists of an electrically controlled tuning-fork, making 128 vibrations per second. A circular bar of iron, 8 inches long, is fixed perpendicularly to one of the beams, and in the plane of the beams. From this the tuning-fork is suspended by means of circular bearings. It therefore has a swinging motion perpendicularly to the disc, as well as a translatory motion parallel to it. By means of a screw it can be fixed in any position, and to any degree of stiffness. The bar is at such a height that the end of the tuning-fork carrying the tracing-point is in the same horizontal plane with the centre of the revolving glass plate. By this means it can be adjusted to trace its record anywhere between the edge of the plate and a circle whose radius is 5 or 6 inches, measured from the centre of the glass.

Theory of the Experiments.

So far as concerns the motion of the block between two successive impacts, the investigation is extremely simple. For we assume (in fair accordance with the results, as shown above) that the friction is practically constant. Thus the motion of the block is represented by

$$M\ddot{r} = Mg \pm F,$$

the positive sign referring to *upward* motion.

We have also, taking the angular velocity, ω, of the disc as uniform throughout the short period of the experiment,

$$d\theta = \omega dt.$$

Thus
$$\frac{d^2r}{d\theta^2} = \left(g \pm \frac{F}{M}\right) \Big/ \omega^2 = 2B, \text{ say};$$

so that
$$r = A + B\theta^2,$$

if we agree that θ is to be measured in each case from the particular radius which is vertical at the moment when the block is at one of its highest positions.

If our assumptions were rigorously correct, the equations of those branches of the curve which are traced during each successive rise of the block should differ from one another solely in the values of the constant A. Similarly with those traced during successive descents. The ascending and descending branches of the same free path should differ solely by the change of value of B, according as the friction aids, or opposes, the action of gravity. Also the two values of B should differ from their mean by a smaller percentage the greater is the mass of the block. This, however, will be necessarily true only if the friction be independent of the weight of the block.

As a test of the closeness of our approximation, to be applied to the experimental results below, it is clear that, if we call B_0 the mean of the values of B for the parts of the curve due to any one rebound, we have

$$2B_0 = \frac{g}{\omega^2}.$$

But, in the notation of the Tables as explained in the next section, we have

$$\omega = 2\pi/(6N/128).$$

Taking the value of g as 32·2 when a foot is unit of length, it is 9814 to millimètres; and the two equations above give the following simple relation between B_0 and N

$$B_0 = \frac{3}{11} N^2,$$

which is sufficiently approximate to be used as a test, the fraction being in defect by about 0·14 per cent. only, say 1/700th.

Thus, in the first experiment of those given below for date 23/7/90, we have

$$N = 21{\cdot}25,$$

which gives as the calculated value

$$B_0 = 123{\cdot}16\text{; or, with 1/700th added, } = 123{\cdot}33.$$

The actual value, as given by the equations for the two parts (β_1, β_2) of the first rebound, is

$$\tfrac{1}{2}(125{\cdot}73 + 120{\cdot}81) = 123{\cdot}27,$$

the difference being less than 0·05 per cent. In this case the acceleration due to friction bears to that of gravity the ratio

$$2{\cdot}46 : 123{\cdot}27\,;$$

almost exactly 2 per cent.

From the data (γ_1, γ_2) for the second rebound we find the actual value of B_0 to be

$$\tfrac{1}{2}(131{\cdot}31 + 121{\cdot}46) = 126{\cdot}33\,;$$

and the percentage of acceleration due to friction rather less than 4. As the whole rise in this second rebound was considerably less than an inch, these results are highly satisfactory.

It is a fairer mode of proceeding, however, to calculate the value of N from that of B_0, by means of the above relation The values, thus calculated, are inserted in the tables below, in the same column as the measured value of N, with the prefixed letters β, γ, &c., to show from which rebound, the first, second, &c., they have been calculated. These agree in a very satisfactory manner with the value of N given by the record of the tuning-fork.

From the facts, that the time of impact is nearly the same for all small distortions, and that it diminishes rapidly as the distortion is greater, it follows that the equation of motion must be of the form

$$M\ddot{x} = -Cx - X$$

during the first stage of the impact; and of approximately the same form, but with the square of the coefficient of restitution as a factor of the right, during the second stage. In this equation x (which is confined to positive values) is measured from the datum line, so that no term in g comes in explicitly. X is a function of x, which is small for small values of x, but increases faster than does the first power of x for larger values. Hence, for small relative speeds, the time of compression is

$$\frac{\pi}{2}\sqrt{\frac{M}{C}}$$

and that of rebound $1/e$ times as much. The utmost distortion is

$$V\sqrt{\frac{M}{C}} + \frac{Mg}{C}$$

where V is the speed at the datum line. The first term is due to the fall; the second, which is due to the weight of the block, does not appear in our Tables, as the measures are made from the datum line. Its value, however, is usually only a small fraction of that of the first term.

To compare the distortion with the duration of impact in experiments made with the same mass, falling from different heights, the following equation was tried:—

$$\ddot{x} = -n^2 x - \frac{3x^2}{2a},$$

where the numerical factors are introduced for convenience. This assumes X, above, to vary as the square of the distortion measured from the datum circle, and it gives, for the time of compression, in terms of the greatest distortion, α, the expression

$$T = \int_0^1 \frac{dz}{\sqrt{n^2(1-z^2) + \alpha(1-z^3)/a}},$$

$$= \frac{p}{\sqrt{n^2 + \alpha/a}}$$

to a sufficient approximation. Here p is a numerical quantity which is about 1·6 when α/a is small in comparison with n^2, and continuously approaches the value 1·4 as α gradually increases. It is easy to give similar expressions for other assumed laws of relation of stress to distortion; but, as will be seen later, this part of the inquiry has not yet led to any result of value.

In testing the results obtained with the earlier apparatus I assumed the force (for the more violent impacts) to be as the square of the distortion simply. This gives, in the notation of the Tables below,

$$D \propto T^{-2} \propto H^{\frac{1}{3}}.$$

Of course any investigations, based on such simple assumptions as those made above, can be only very rough approximations, since they ignore altogether the true nature of the distortion of either of the impinging bodies, as well as the internal wave disturbance which is constantly passing to and fro in the interior of each; part of it, no doubt, becoming heat, but another part ultimately contributing to the resilience. In such circumstances the impact may perhaps sometimes consist of a number of successive collisions; certainly the pressure between the two bodies will have a fluctuating value.

Measurements of the Tracings, and their Reduction.

From the tracing for each separate experiment the following quantities were carefully determined. Their values are given in the subsequent Tables, under the corresponding letters below.

1. Number of vibrations of the fork corresponding to one-sixth of a complete revolution of the disc ....N.

Three diameters of the disc were drawn, making angles of 60° with one another, and the number of undulations of the fork-tracing intercepted between each pair of radii was counted. This process was preferred to the simpler one, of counting the undulations in the entire circumference, for two reasons:—it tests the uniformity of the rotation, or a possible shrinking of the photographic paper; and it makes one common process of measurement applicable to complete traces, and to others which from some imperfection of adjustment presented only *parts* which were sufficiently distinct. When only one measurement is given under this head, it means either that only one was possible or that all six gave the same result. When two are given, they are chosen as the least and greatest of the six. They usually differ by a small quantity only, and may indicate distortion of the paper or irregularity of the fork (due to the bristle's being clogged with printers' ink, or to its pressing too strongly on the plate?). In these cases the arithmetical mean is to be taken for any subsequent calculation.

2. The radius of the datum circle ....R.

This, and the other measurements of length, are in millimètres.

3. The height of fall, or of rebound ....H.

For the first fall, this was of course measured on the rails:—for the subsequent rebounds it was measured on the tracing.

4. Chord of the arc of datum circle intercepted by the trace during impact ... C.
As this arc was, on the average, considerably less than one-tenth of radius, the chord is practically equal to it (differing at most by 1/1200th only), and it is thus a measure of the duration of the impact. The duration is, in fact,

$$\frac{C}{2\pi R}\cdot\frac{6N}{128}=\frac{3}{400}\cdot\frac{CN}{R}\text{ nearly;}$$

this approximation being much within the inevitable errors of experiment. It is tabulated under ....T.

5. Greatest distortion—*i.e.*, greatest distance of the trace beyond the datum circle (of course not including the (small) distortion due to the weight of the block). This datum is always, to a small but uncertain amount, increased by the distortion of the lower part of the falling block. This is probably nearly proportional to that of the elastic cylinder, so that the numbers given are all a little too large, but they are increased nearly in a common ratio ....D.

It was found impracticable to estimate with certainty the relative distances of this greatest ordinate from the ends of the intercepted arc; as the radial motion generally remains exceedingly small during a sensible fraction of the whole time of impact. This is true of all the substances examined, even when they have properties so different as those of vulcanite and vulcanised india-rubber. It seems as if the elastic substance were for a moment stunned (if such an expression can be permitted) when the sudden distortion is complete.

We can easily assign limits within which the time of compression must lie. For, since the elastic force resists the motion, and increases with the distortion, its time-average during the compression is greater than its space-average:—*i.e.*

$$\frac{mV}{t} > \frac{mV^2}{2D},$$

where m is the mass of the block, V its speed at the datum line, and t the time of compression. Hence

$$\frac{D}{V} < t < \frac{2D}{V}.$$

If we make the assumption that the force at each stage during restitution is e times its value during compression, this gives

$$\frac{D}{V} < \frac{T}{1+1/e} < \frac{2D}{V};$$

and the values tabulated satisfy these conditions. Thus the somewhat precarious assumption as to the circumstances of restitution is, so far, justified.

6. The tangents of the inclination of the trace to the radius of the datum circle drawn to the intersection of these curves before and after impact ....A_1, A_2.

These values were determined directly by drawing tangents to the trace; and indirectly by calculation from the equation of each part of the trace. The agreement of the observed (o) and calculated (c) values is satisfactory.

Attempts to form the equation of the part of the trace made before the first impact were not very successful, as the available range of polar angle was small, and the radius vector increases rapidly for small changes of that angle. Hence the *calculated* value of A_1 was obtained simply as the ratio of the tangential and radial speeds of the tracing-point at the moment of its first crossing the datum circle. This was taken as

$$\frac{R\omega}{\sqrt{2gH}} = \frac{R}{36{\cdot}5N} \text{ nearly.}$$

In this numerical reduction H is taken as 4 feet, *i.e.*, 1219 mm.; and the full value of g is employed, as we do not know the amount by which friction diminishes it, the contact of the tracing-point with the disc coming about only during an uncertain portion of the lower range of the fall; while it is not possible to estimate with any accuracy the effect of the impact on the trigger. The calculated value of the tangent will therefore always be too small, but (since the square-root of the acceleration is involved) rarely by more than 1 per cent. On the other hand, the graphic method employed for the direct measurement of this tangent usually exaggerates its value.

7. The ratios of these pairs of tangents—*i.e.*, the values of the coefficient of restitution ....e.

The equation of each distinct part of the trace (alluded to in 6, above) was found thus:—The minimum (or maximum) radius-vector was drawn approximately for each separate free path, and other radii were drawn, two on either side of it, making with it convenient angles:—usually 40°, 80°, $-40°$, $-80°$, or such like. The notation employed below for the measured lengths of these radii-vectores is simply square brackets enclosing the value of the angle-vector, thus:—

$$[80],\ [40],\ [0],\ [-40],\ [-80].$$

If x be the angular error introduced in the estimated position of the minimum radius, we determine it, as well as the A and B of the equation of the corresponding half of the branch of the curve in question, from three equations of the very simple form

$$[0] = A + Bx^2,$$
$$[40] = A + B(40 + x)^2,$$
$$[80] = A + B(80 + x)^2,$$

(which may be made even more simple for calculation by putting y for $40 + x$). The assumed initial radius was in most cases so near to the minimum that very little difference was found between [0] and A; x being usually very small.

We now write the equation of this part of the branch in the form

$$r = A + B(\theta + x)^2;$$

the numerical values of A, B, x being inserted, after x has been reduced to radians, and B modified accordingly. The equations, in this final form, are printed below—each with the data from which it was obtained. (A fine protractor, by Cary, London, reading to one minute over an entire circumference, belongs to the Natural Philosophy Class collection of Apparatus; so that it was found convenient to deal with degrees in all measurements of angle, and in the bulk of the subsequent calculations:—the results being finally reduced to circular measure.)

In the Tables below, after the data (enumerated above) from each experiment, come the equations of the successive parts of each trace in order. In these, β_1, β_2 refer respectively to the rise and fall due to the first rebound; γ_1, γ_2 to the second rebound, &c.

To test the formulæ thus obtained, other radii were measured, as far as possible from those already employed, say for instance [20], [60], [− 20], [− 60], &c. These measured values, and the corresponding values calculated from the equation (before reducing to circular measure), are also given below. The agreement is, in most cases, surprisingly close; and shows that the assumption of nearly constant friction cannot be far from correct.

The whole of the above statement presupposes that the adjustments have been so exact that the line of fall of the needle-point passes accurately through the centre of the disc. On a few occasions, only, it was not so:—but the necessary correction was easily calculated and applied, by means of the trace preceding the first impact; even if the trace of the first rebound did not reach to the level of the centre of the disc. In fact, if we wish to find the curve which would have been traced on the disc had the adjustment been perfect, it is easy to see that we must draw from

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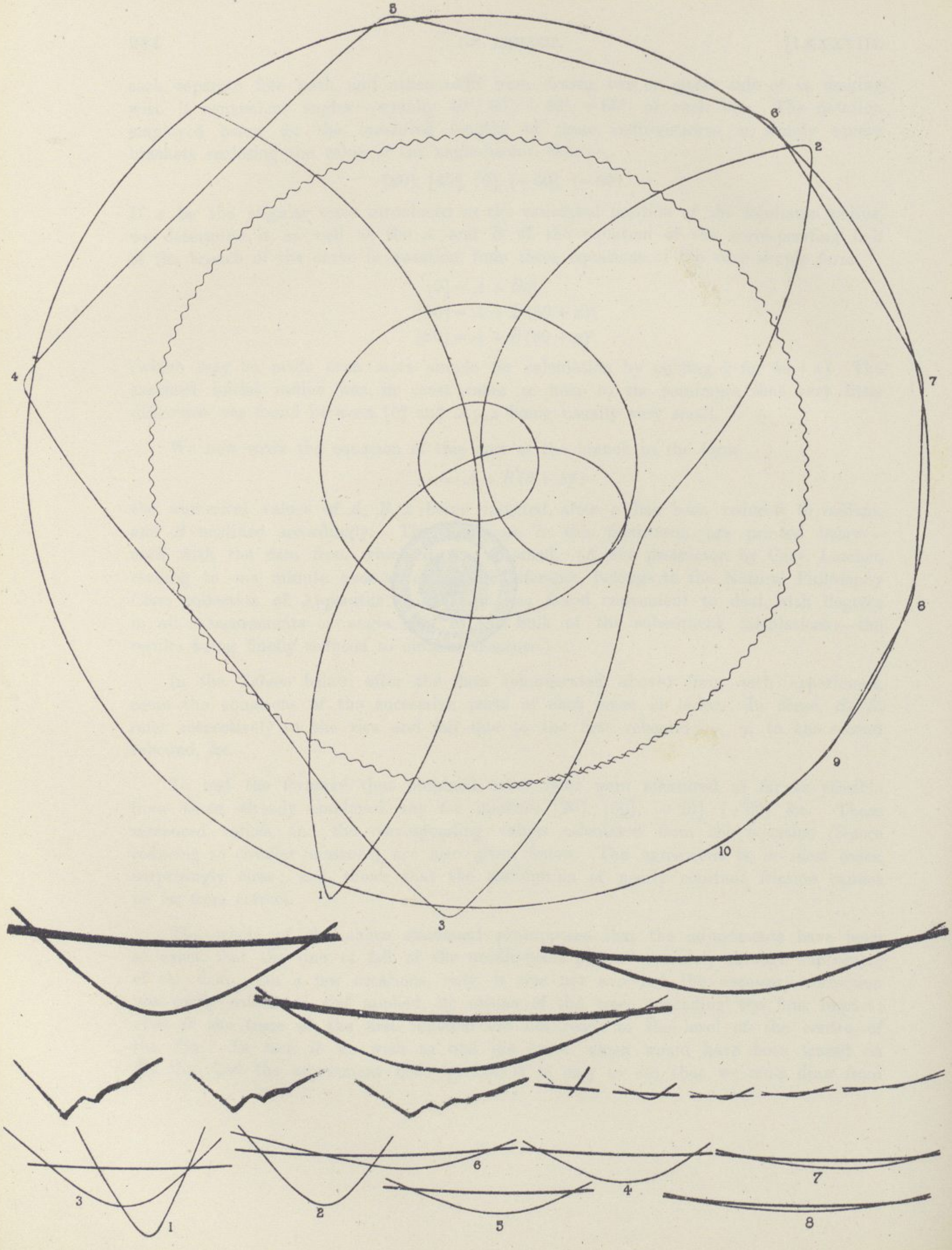

each point of the trace a tangent to the circle described about the centre of the disc so as to touch the true line of fall. The position of the centre of the disc, relatively to the point of contact of this tangent, is the same as that of the true point, relatively to the actual point, of the trace. This applies, of course, to *all* parts of the trace, including the datum circle.

In the special trace which has been selected for photolithography as an illustration (see Plate IV) this adjustment is markedly imperfect; much more so than in the worst of the others. The path of the tracing-point passed, in fact, about 3 mm. from the centre of the disc; while, in the worst of the other cases, the distance was not more than half as great. But this very imperfection serves to enable the reader to follow without any difficulty the various convolutions of the trace. The measurements and reductions, obtained from this specially imperfect figure, agree wonderfully with those obtained from the best traces. It would only have confused the reader had we selected one of the latter for reproduction, since each of them contains the record of four experiments—*i.e.*, it contains four times as much detail as does the trace reproduced.

Conclusions from the Experiments.

It will be observed from the following Tables that the assumed initial radius-vector was never very far from the true position of the minimum; the correction (in circular measure) being usually of the order 0·01, *i.e.*, about $0^{\circ}\cdot 6$, and very often much less. When the minimum was small, the correction was usually larger; but in few cases did it amount to 0·05, *i.e.*, 3°. This correction ought, of course, to have equal values for the two parts of each free path.

The substances experimented on were fresh specimens, not those which had been frequently battered by 8 and 12 foot falls in the earlier experiments. They were limited to four, Plane-tree, Cork, Vulcanised India-rubber, and Vulcanite. The first material was chosen the same as that of the falling block, in order that (if possible) a correction for the compression of the block might be determined, and applied to the results of the experiments on other materials. I do not as yet see any simple mode of obtaining approximately such a correction:—and the data from different experiments with the same materials are scarcely sufficiently consistent with one another to warrant the application of rigorous analysis, a task which would involve immense labour as well as difficulties of a most formidable order. Hence there is not much to be said, for the present at least, about the behaviour of a hard body such as vulcanite, whose distortion is only of the same order as that of the block. The time of the impact between it and the wood-block is somewhere about 1/500th of a second when the speed of the block is about 16 feet per second. For lower speeds it is longer; while for *very* low speeds this substance seems to show a peculiarity which is specially marked in cork, and will be considered below.

With vulcanised india-rubber, when the speed is 16 feet per second, the time of impact is about 1/130th of a second; it becomes longer as the relative speed is less; until, with very low speeds, it becomes practically constant.

With cork the period of impact for a speed of 16 feet per second is about 1/70th of a second; it increases as the speed is reduced to about 8 feet per second; and again steadily diminishes as the speed is still further reduced. This seems to indicate that (at least in circumstances of rapid distortion) the elastic force in cork increases in a slower ratio than does the distortion, while both are small, but at a higher ratio when they are larger.

In all the cases tested the coefficient of restitution seems steadily to diminish as the speed of impact is increased.

In some of the experiments the mass of the block was doubled; and occasionally the doubled mass was allowed to fall from half the previous height, so that its energy remained unaltered. But the number of cases is as yet too small to enable us to judge with certainty the consequences of these changes. I hope to discuss this point in a subsequent paper.

23/7/90. PLANE TREE, I.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·25	292·5	1219·2	3·8	0·00206	2·0	0·421	0·377	1·474	1·608	·286*
		67·0	4·7	255	0·8	1·600	1·626	2·651	2·720	·604
β 21·24		22·1	4·8	260	0·5	2·798	2·844	4·198		·667
γ 21·5		9·1	5·0	271	0·3					
		4·2	5·8	314						
		2·1								

					o	c
β_1,	[0]	225·0		[15]	232·8	232·9
	[20]	239·3	$r = 225 + 125{\cdot}73\,(\theta - {\cdot}0115)^2$	[35]	270·3	270·2
	[40]	284·3		[42]	291·8	290·5
β_2,	[0]	225·0				
	[−20]	240·8	$r = 225 + 120{\cdot}81\,(\theta - {\cdot}0128)^2$	[−30]	259·7	259·9
	[−40]	286·0		[−42]	293·0	291·7
γ_1,	[0]	270·8				
	[10]	274·4	$r = 270{\cdot}8 + 131{\cdot}31\,(\theta - {\cdot}0087)^2$	[24]	293·0	292·9
	[20]	286·0				
γ_2,	[0]	270·8				
	[−10]	274·7	$r = 270{\cdot}8 + 121{\cdot}46\,(\theta - {\cdot}0047)^2$	[−24]	292·7	292·6
	[−20]	286·0				

II.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·9	301·5	1219·2	3·0	0·00170	1·6	0·388	0·36	0·924	1·028	·42
		155·2	3·8	215	1·2	1·037	1·039	1·867	1·919	·555
β, 22·5		42·7	4·0	225	·6	1·982	1·942	3·271		·606
γ, 22·7		16·2	4·2	237	·4					
		7·5								
		3·7								

* *Note.*—It is clear from this value of e, and from the amount of the first rebound, that the cylinder was not home in the lead-block. This fall is therefore not trustworthy in some of its details.

				o	c
β_1, [0]	146·6				
[20]	162·3	$r = 146{\cdot}6 + 140{\cdot}50\,(\theta - {\cdot}0143)^2$	[30]	183·0	183·0
[40]	212·2		[60·5]	300·9	299·1
β_2, [0]	146·6		[−10]	151·4	151·4
[−20]	164·5	$r = 146{\cdot}6 + 135{\cdot}91\,(\theta - {\cdot}0141)^2$	[−50]	253·7	253·5
[−40]	215·5		[−60·5]	302·2	302·2
γ_1, [0]	258·8				
[10]	262·8	$r = 258{\cdot}8 + 142{\cdot}80\,(\theta - {\cdot}0072)^2$	[32]	302·2	302·2
[20]	275·5				
γ_2, [0]	258·8				
[−10]	262·9	$r = 258{\cdot}8 + 139{\cdot}52\,(\theta - {\cdot}003)^2$	[−32]	301·1	301·8
[−20]	275·5				

III. Double Mass.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·33	322·4	609·6	4·1	0·00212	1·9	0·575	0·56	1·281	1·356	·449
		104·4	5·6	289	1·0	1·385	1·368	2·718	2·680	·509
β, 22·23		27·5	5·2	269	·5	2·592	2·634	4·705		·551
γ, 22·06		9·7	7·8	403	·5					
		4·0	8·2	424	·3					
		2·0								

				o	c
β_1, [0]	218·3				
[20]	234·9	$r = 218{\cdot}3 + 136{\cdot}24\,(\theta)^2$	[30]	256·1	255·6
[40]	284·7		[50·08]	322·9	322·4
β_2, [0]	218·3				
[−20]	235·1	$r = 218{\cdot}3 + 133{\cdot}61\,(\theta - {\cdot}0056)^2$	[−30]	255·6	255·7
[−40]	284·5		[−50·08]	321·6	321·7
γ_1, [0]	294·5				
[10]	298·4	$r = 294{\cdot}5 + 132{\cdot}95\,(\theta - {\cdot}0031)^2$	[26·04]	321·6	321·6
[20]	310·4				
γ_2, [0]	294·5				
[−10]	298·8	$r = 294{\cdot}5 + 132{\cdot}95\,(\theta - {\cdot}0054)^2$	[−26·04]	322·1	322·6
[−20]	311·2				

IV.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·8	331·8	1219·2	4·5	0·00231	2·4	0·408	0·4	1·072	1·150	·381
		155·0	5·3	272	1·3	1·098	1·133	2·179	2·238	·504
β, 22·5		36·7	5·6	287	·8	2·371	2·397	4·127		·575
γ, 22·8		11·6	8·0	410	·6					
		4·7	8·1	415	·5					
		2·3								

				o	c
β_1, [0]	176·6				
[20]	193·3	$r = 176{\cdot}6 + 137{\cdot}88\,(\theta - {\cdot}0017)^2$	[30]	214·3	214·2
[40]	243·6		[60·05]	331·7	327·6
β_2, [0]	176·6				
[−20]	194·5	$r = 176{\cdot}6 + 138{\cdot}70\,(\theta - {\cdot}0103)^2$	[−30]	216·0	216·0
[−40]	246·2		[−60·05]	332·8	332·0

					o	c
γ_1, [0]	295·9					
[10]	300·2	$r = 295{\cdot}9 + 147{\cdot}73\,(\theta - {\cdot}0039)^2$		[29·04]	332·8	333·0
[20]	313·5					
γ_2, [0]	295·9					
[−10]	300·3	$r = 295{\cdot}9 + 136{\cdot}24\,(\theta - {\cdot}0010)^2$		[−29·04]	331·7	331·0
[−20]	313·0					

14/6/90. Cork, I.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·7	296·4	1219·2	30·5	0·0167	19·0	0·390	0·373	1·10	1·124	·355
21·8		122·8	44·5	243	8·2	1·250	1·230	2·71		·461
β, 21·79		22·0	39·8	218	3·3					
		4·4	37·0	202	1·5					

				o	c
β_1, [0]	173·5		[17·45]	182·5	182·9
[20]	186·5	$r = 173{\cdot}24 + 141{\cdot}16\,(\theta - {\cdot}0428)^2$	[32·45]	211·3	211·9
[40]	233·8		[56·06]	296·9	296·8
β_2, [0]	173·5		[−12·57]	181·0	181·4
[−20]	191·2	$r = 173{\cdot}29 + 118{\cdot}18\,(\theta - {\cdot}0424)^2$	[−28·57]	207·6	207·9
[−40]	238·0		[−56·06]	295·9	296·45

II.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·4	306·1	1219·2	29·6	0·0162	19·0	0·394	0·373	1·065	1·106	·37
22·5		131·5	45·2	247	8·9	1·204	1·196	2·578		·467
		23·7	40·3	220	3·6					
β, 22·2		4·9	38·5	210	1·6					

				o	c
β_1, [0]	174·8				
[20]	189·0	$r = 174{\cdot}7 + 144{\cdot}44\,(\theta - {\cdot}0244)^2$	[56·35]	306·5	306·5
[40]	238·3				
β_2, [0]	174·8				
[−20]	193·8	$r = 174{\cdot}6 + 124{\cdot}75\,(\theta - {\cdot}0408)^2$	[−56·35]	305·7	305·5
[−40]	243·6				

III.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·75	321·0	1219·2	30·2	0·0160	18·8	0·414	0·385	1·107	1·167	·374
22·8		128·2	47·0	249	8·7	1·226	1·249	2·633		·465
		23·8	42·1	223	3·6					
β, 22·47		4·9	39·2	208	1·6					

				o	c
β_1, [0]	192·0				
[20]	206·5	$r = 191{\cdot}8 + 147{\cdot}73\,(\theta - {\cdot}0340)^2$	[55·3]	321	319·9
[40]	257·0				
β_2, [0]	192·0				
[−20]	211·0	$r = 191{\cdot}8 + 128{\cdot}03\,(\theta - {\cdot}0380)^2$	[−55·3]	320·9	321·6
[−40]	261·0				

IV.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·25	329·1	1219·2	31·2	0·0157	18·5	0·427	0·405	1·126	1·195	·379
		137·5	50·3	254	9·1	1·257	1·263	2·611		·481
β, 21·94		26·0	45·0	227	3·7					
		5·4	41·6	210	1·65					

					o	c
β_1,	[0]	191·6				
	[20]	204·8	$r = 191·38 + 139·85\,(\theta - ·0393)^2$	[32·25]	229·9	229·7
	[40]	252·1		[58·5]	328·4	326·2
β_2,	[0]	191·6				
	[−20]	210·0	$r = 191·4 + 123·11\,(\theta - ·0394)^2$	[−27·74]	225·5	225·2
	[−40]	258·5		[−58·5]	329·8	329·8

28/7/90. VULCANITE, I.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·6	295·9	1219·2	3·5	0·00190	2·5	0·394	0·388	0·649	0·745	·607
		300·8	4·4	240	2·1	0·768	0·780	1·297	1·404	·592
β, 21·42		85·2	4·9	267	0·95	1·426	1·435	2·264		·630
γ, 21·6		32·0	5·0	272	0·5					
		15·0	5·1	278	0·3					
		7·3	5·8	316	0·2					
		4·0	6·5	354	0·15					

					o	c
β_1,	[0]	−5·2		[20]		7·76
	[40]	52·0	$r = -5·02 + 131·31\,(\theta - ·0370)^2$	[60]	128·0	128·98
	[80]	237·5		[88·72]	295·8	294·96
β_2,	[0]	−5·2		[−20]	14·0	12·8
	[−40]	60·0	$r = -5·41 + 119·49\,(\theta - ·0417)^2$	[−60]	136·0	136·3
	[−80]	241·8		[−88·72]	296·5	296·7
γ_1,	[0]	210·7				
	[20]	225·9	$r = 210·7 + 130\,(\theta - ·0007)^2$	[46·57]	296·5	296·4
	[40]	272·8				
γ_2,	[0]	210·7				
	[−20]	227·0	$r = 210·7 + 124·75\,(\theta - ·0126)^2$	[−46·57]	295·5	295·7
	[−40]	273·6				

II.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·0	313·2	1219·2	3·0	0·00157	1·5	0·396	0·39	0·714	0·779	·555
		292·8	4·0	209	1·3	0·833	0·821	1·338	1·436	·623
β, 21·91		88·0	4·4	231	·95	1·458	1·491	2·264		·644
γ, 21·83		35·0	4·1	215	·4					
		16·0								

					o	c
β_1,	[0]	20·6				
	[40]	80·0	$r = 20·35 + 138·53\,(\theta - ·0420)^2$	[60]	160·0	160·3
	[80]	274·5		[85·53]	313·2	311·9
β_2,	[0]	20·6				
	[−40]	89·2	$r = 20·32 + 123·76\,(\theta - ·0479)^2$	[−60]	168·6	168·6
	[−80]	278·5		[−85·53]	313·2	314·1

				o	c
γ_1, [0]	225·1				
[20]	240·5	$r = 225{\cdot}1 + 134{\cdot}92\,(\theta - {\cdot}0110)^2$	[46·92]	313·2	313·2
[40]	288·8				
γ_2, [0]	225·1				
[− 20]	242·0	$r = 225{\cdot}06 + 125{\cdot}40\,(\theta - {\cdot}0185)^2$	[− 46·92]	313·2	313·0
[− 40]	289·5				

III. DOUBLE MASS.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
21·8	326·7	609·6	4·3	0·00214	2·1	0·583	0·58	0·971	1·087	·600
		172·2	4·7	233	1·2	1·086	1·113	1·857		·585
β, 21·75		52·0	5·9	293	0·9					
		19·9	5·0	248	0·45					
		9·2								

				o	c
β_1, [0]	155·1				
[20]	170·6	$r = 155{\cdot}1 + 131{\cdot}97\,(\theta - {\cdot}0063)^2$	[65·62]	326·9	326·3
[40]	218·3				
β_2, [0]	155·1				
[− 20]	171·9	$r = 155{\cdot}07 + 126{\cdot}39\,(\theta - {\cdot}0159)^2$	[− 65·62]	326·7	325·5
[− 40]	219·5				

IV.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
22·1	343·0	1219·2	5·0	0·00240	3·2	0·425	0·425	0·892	0·989	·476
		228·5	4·9 ?	235 ?	1·4 ?	0·985 ?	0·992	1·706 ?		·577
β, 22·02		46·9	6·0	288	1·0					
		22·4	7·3	350	·65					
		9·3	10·7	513	·8					

				o	c
β_1, [0]	114·9				
[20]	131·0	$r = 114{\cdot}9 + 133{\cdot}28\,(\theta - {\cdot}0015]^2$	[60]	261	260·6
[40]	179·6		[74·62]	343	340·4
β_2, [0]	114·9				
[− 20]	131·9	$r = 114{\cdot}9 + 131{\cdot}64\,(\theta - {\cdot}0105)^2$	[− 60]	262·4	262·1
[− 40]	181·0		[− 74·62]	343·0	341·8

24/6/90. VULCANISED INDIA-RUBBER. I.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
21·95	300·8	1219·2	14·6	0·0079	11·7	0·400	0·374	0·607	0·656	·659
22·0		386·8	20·4	111	8·8	0·737	0·702	1·032	0·994	·714
β, 21·6		158·0	23·8	129	6·4	1·117	1·068	1·508	1·484	·741
γ, 22·13		72·3	25·3	139	4·5	1·570	1·570	2·260		·695
δ, 22·02		34·6	25·0	136	3·2					
		16·5	25·3	139	2·1					
		7·6	25·4	139	1·4					
		3·5	25·4	139	0·9					
		1·5	25·3	139	0·5					
		·6								

					o	c
β_1,	[0]	−86·7				
	[80]	199	$r = -87 + 135{\cdot}92\,(\theta + {\cdot}0541)^2$	[93·5]	300·5	299·2
	[90]	272				
β_2,	[0]	−86·7				
	[−80]	145	$r = -86{\cdot}7 + 118{\cdot}52\,(\theta)^2$	[−103·2]	300·5	297·8
	[−90]	206·5				
γ_1,	[0]	142·5		[10]	144·9	144·8
	[20]	155·8	$r = 142{\cdot}25 + 143{\cdot}13\,(\theta - {\cdot}0414)^2$	[30]	175·3	175·5
	[40]	204·0		[62·97]	300·8	302·3
γ_2,	[0]	142·5		[−30]	181·0	181·0
	[−20]	160·6	$r = 142{\cdot}36 + 124{\cdot}42\,(\theta - {\cdot}0338)^2$	[−50]	244·5	244·6
	[−40]	209·0		[−62·97]	301·2	302·0
δ_1,	[0]	228·5		[10]	231·8	231·7
	[20]	243·7	$r = 228{\cdot}44 + 141{\cdot}49\,(\theta - {\cdot}0206)^2$	[30]	264·4	264·2
	[40]	293·4		[42·27]	301·2	301·2
δ_2,	[0]	228·5				
	[−20]	245·5	$r = 228{\cdot}44 + 123{\cdot}43\,(\theta - {\cdot}0227)^2$	[−10]	233·0	233·2
	[−40]	292·6		[−42·27]	300·0	300·0

II.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
21·7	310·8	1219·2	15·5	0·0080	11·6	0·412	0·38	0·680	0·683	·606
21·6		389·3	21·3	111	8·8	0·742	0·722	1·054	1·058	·704
β, 21·5		159·8	25·3	131	6·3	1·132	1·132	1·600	1·589	·707
γ, 21·49		73·0	26·6	138	4·5	1·689	1·682	2·238		·755
δ, 21·39		35·1	26·5	138	3·2					
		17·0	26·9	140	2·2					
		7·9	27·7	144	1·5					
		3·6	27·5	143	0·9					
		1·5	27·5		0·5					

					o	c
β_1,	[0]	−78·5				
	[60]	80	$r = -78{\cdot}8 + 132{\cdot}63\,(\theta + {\cdot}0471)^2$	[20]	−61·5	−65·9
	[80]	197·5				
β_2,	[0]	−78·5				
	[−80]	136	$r = -78{\cdot}8 + 119{\cdot}17\,(\theta + {\cdot}0536)^2$	[−60]	37	38·7
	[−90]	195·5				
γ_1,	[0]	151·2		[30]	182·3	182·7
	[20]	164·0	$r = 151{\cdot}0 + 134{\cdot}92\,(\theta - {\cdot}0386)^2$	[50]	244·6	244·8
	[40]	209·7		[64·68]	311·2	311·3
γ_2,	[0]	151·2		[−10]	156·5	156·36
	[−20]	168·7	$r = 151{\cdot}02 + 117{\cdot}36\,(\theta - {\cdot}0391)^2$	[−50]	248·3	248·6
	[−40]	214·8		[−64·68]	310·3	310·9
δ_1,	[0]	238·4				
	[20]	252·4	$r = 238{\cdot}32 + 132{\cdot}53\,(\theta - {\cdot}0231)^2$	[10]	241·4	241·4
	[40]	298·7		[43·55]	310·3	310·3
δ_2,	[0]	238·4				
	[−20]	255·0	$r = 238{\cdot}3 + 117{\cdot}36\,(\theta - {\cdot}0281)^2$	[−10]	243·1	243·1
	[−40]	300·2		[−43·55]	311·1	311·1

III.

N	R	H	C	T	D	A_1 *o*	A_1 *c*	A_2 *o*	A_2 *c*	*e*
21·75	323·5	1219·2	15·6	0·0078	11·5	0·419	0·407	0·670	0·679	·625
β, 22·0		392·0	22·1	110	8·7	0·765	0·741	1·087	1·091	·703
γ, 21·73		162·3	25·8	129	6·4	1·160	1·141	1·616	1·581	·718
δ, 21·66		75·0	27·5	138	4·5	1·698	1·726	2·484		·683
		36·0	27·5	138	3·2					
		17·3	27·8	139	2·2					
		8·1	28·3	142	1·6					
		3·8	30·0	150	1·0					
		1·6	31·0	155	·6					

					o	*c*
β_1,	[0]	− 69·4				
	[80]	202·4	$r = -69·5 + 144·45\,(\theta - ·0244)^2$	[60]	87	81·6
	[90]	276·0				
β_2,	[0]	− 69·4				
	[− 80]	158·2	$r = -69·5 + 119·83\,(\theta - ·0314)^2$	[− 60]	56	54·1
	[− 100]	287·0				
γ_1,	[0]	160·9				
	[20]	173·9	$r = 160·7 + 135·81\,(\theta - ·0374)^2$	[30]	193·0	192·8
	[40]	220·0		[64·67]	323·4	322·5
γ_2,	[0]	160·9				
	[− 20]	178·5	$r = 160·8 + 122·28\,(\theta - ·0316)^2$	[− 10]	166·0	166·0
	[− 40]	225·9		[− 64·67]	323·8	325·2
δ_1,	[0]	248·3				
	[20]	263·0	$r = 248·2 + 137·88\,(\theta - ·0218)^2$	[10]	251·5	251·4
	[40]	311·3		[43·8]	323·8	324·2
δ_2,	[0]	248·3				
	[− 20]	264·7	$r = 248·2 + 118·51\,(\theta - ·0237)^2$	[− 30]	283·7	283·7
	[− 40]	310·0		[− 43·8]	322·5	321·8

IV.

N	R	H	C	T	D	A_1 *o*	A_1 *c*	A_2 *o*	A_2 *c*	*e*
21·75	333·5	1219·2	16·0	0·0078	11·5	0·432	0·42	0·722	0·723	·598
β, 21·6		392·6	22·6	110	8·9	0·771	0·767	1·097	1·088	·703
γ, 21·92		164·0	26·5	129	6·3	1·170	1·185	1·659	1·652	·705
δ, 21·81		74·2	28·1	137	4·4	1·739	1·759	2·402		·724
		35·4	28·6	139	3·1					
		17·3	29·2	142	2·3					
		8·2	29·4	143	1·6					
		3·9	31·5	153	1·0					
		1·7	31·1	151	·6					
		·6								

					o	*c*
β_1,	[0]	− 60·5				
	[80]	219	$r = -60·7 + 135·26\,(\theta - ·0419)^2$	[60]	100	99·7
	[90]	291				
β_2,	[0]	− 60·5				
	[− 80]	165·0	$r = -60·6 + 120·16\,(\theta - ·0262)^2$	[− 20]	− 44	− 48·1
	[− 100]	294·5				
γ_1,	[0]	169·9		[10]	172·3	172·3
	[20]	183·3	$r = 169·7 + 141·16\,(\theta - ·0384)^2$	[50]	267·5	267·9
	[40]	231·0		[64·5]	334·0	336·6

					o	c
γ_2,	[0]	169·9		[−10]	174·9	174·9
	[−20]	187·4	$r = 169{\cdot}8 + 121{\cdot}46\,(\theta - {\cdot}0318)^2$	[−30]	207·1	207·2
	[−40]	234·5		[−50]	269·7	269·1
				[−64·5]	333·3	332·5
δ_1,	[0]	259·6		[30]	294·6	294·6
	[20]	274·5	$r = 259{\cdot}6 + 138{\cdot}21\,(\theta - {\cdot}0201)^2$	[42·97]	333·3	333·2
	[40]	323·1				
δ_2,	[0]	259·6		[−30]	296·7	296·5
	[−20]	276·8	$r = 259{\cdot}5 + 121{\cdot}79\,(\theta - {\cdot}0278)^2$	[−42·77]	333·2	333·2
	[−40]	323·7				

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N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·75	293·4	1219·2	13·7	0·0076	11·6	0·370	0·37	0·601	0·629	·616
β, 22·0		418·1	18·2	100	9·2	0·637	0·621	0·875	0·922	·728
γ, 22·28		182·6	22·5	124	6·7	0·954	0·942	1·358	1·326	·702
δ, 22·31		89·5	24·0	133	5·1	1·368	1·342	1·836		·745
		45·7	24·1	134	3·7					
		23·9	24·5	135	2·7					
		12·5	25·2	139	1·9					
		6·3	25·0	138	1·4					
		3·0	25·3	139	0·9					

					o	c
β_1,	[0]	−124·5				
	[80]	158	$r = -124{\cdot}8 + 130{\cdot}00\,(\theta - {\cdot}0524)^2$	[90]	217	217·8
	[100]	293·5				
β_2,	[0]	−124·5				
	[−80]	123	$r = -124{\cdot}7 + 133{\cdot}29\,(\theta - {\cdot}0332)^2$	[−30]	−86·3	−90·0
	[−90]	190·5				
γ_1,	[0]	110·6		[50]	212·3	212·0
	[20]	125·7	$r = 110{\cdot}55 + 139{\cdot}12\,(\theta - {\cdot}0191)^2$	[66·68]	293·7	292·9
	[40]	174·7				
γ_2,	[0]	110·6		[−30]	148·9	148·5
	[−20]	128·0	$r = 110{\cdot}6 + 132{\cdot}13\,(\theta - {\cdot}0141)^2$	[−50]	214·0	214·3
	[−40]	177·6		[−66·68]	293·1	293·7
δ_1,	[0]	204·6		[30]	240·8	240·6
	[20]	220·2	$r = 204{\cdot}6 + 138{\cdot}2\,(\theta - {\cdot}0128)^2$	[46·55]	293·1	292·9
	[40]	269·5				
δ_2,	[0]	204·6		[−30]	242·2	242·0
	[−20]	221·4	$r = 204{\cdot}6 + 133{\cdot}77\,(\theta - {\cdot}0054)^2$	[−46·55]	293·7	294·0
	[−40]	270·8				

II.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·75	302·0	1219·2	14·4	0·0077	11·9	0·384	0·38	0·618	0·644	·622
β, 21·6		423·0	19·5	105	9·3	0·652	0·65	0·914	0·952	·713
γ, 21·96		183·9	23·1	124	6·9	0·983	0·989	1·428	1·383	·689
δ, 21·87		90·0	24·4	131	5·1	1·402	1·411	1·954		·718
		46·1	25·9 ?	139	3·7					
		24·4	25·4	136	2·7					
		13·0	25·1	135	1·9					
		6·8	26·2	141	1·4					
		3·4	26·5	142	1·0					

					o	c
β_1,	[0]	−121				
	[80]	146	$r = -121{\cdot}2 + 130{\cdot}00\,(\theta - {\cdot}0377)^2$	[+20]	−105	−101·8
	[90]	215				
β_2,	[0]	−121·0				
	[−80]	114·7	$r = -121{\cdot}1 + 125{\cdot}41\,(\theta - {\cdot}0251)^2$	[−106·2]	302·5	298·5
	[−90]	178·6				
γ_1,	[0]	118·0		[30]	153·0	153·0
	[20]	133·0	$r = 118 + 136{\cdot}24\,(\theta - {\cdot}0168)^2$	[50]	217·5	217·8
	[40]	181·2		[67·72]	302·3	302·9
γ_2,	[0]	118·0				
	[−20]	135·0	$r = 118{\cdot}0 + 127{\cdot}21\,(\theta - {\cdot}0169)^2$	[−50]	218·8	218·6
	[−40]	183·0		[−67·72]	301·6	300·8
δ_1,	[0]	212·6				
	[20]	228·2	$r = 212{\cdot}6 + 132{\cdot}95\,(\theta - {\cdot}0065)^2$	[10]	216·4	216·3
	[40]	276·2		[47·35]	301·6	302·0
δ_2,	[0]	212·6				
	[−20]	228·9	$r = 212{\cdot}6 + 128{\cdot}36\,(\theta - {\cdot}0074)^2$	[−10]	216·7	216·8
	[−40]	276·5		[−47·35]	302·1	301·8

III. Double Mass.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
22·45	325·6	1219·2	16·7	0·0086	13?	0·401	0·4	0·712	0·744	·563
β, 22·5		350·7	24·5	126	9·9	0·749	0·736	1·124	1·149	·667
γ, 22·57		147·2	31·0	159	8·0	1·154	1·131	1·648	1·641	·700
δ, 22·5		70·5	35·1	180	6·0	1·723	1·663	2·356		·731
		35·6	36·6	188	4·4					
		18·4	37·2	191	3·2					
		9·7	39·2	201	2·3					
		4·9	39·8	204	1·7					
		2·4	40·6	209	1·0					
		1·2								

					o	c
β_1,	[0]	−25·3				
	[60]	147·0	$r = -26{\cdot}1 + 136{\cdot}57\,(\theta - {\cdot}0787)^2$	[50]	100	97·5
	[80]	271·0		[87·2]	325·8	323·8
β_2,	[0]	−25				
	[−60]	102	$r = -25{\cdot}7 + 139{\cdot}53\,(\theta - {\cdot}0879)^2$	[−96·25]	325·8	327·8
	[−80]	214				
γ_1,	[0]	178·2				
	[20]	194·8	$r = 178{\cdot}2 + 137{\cdot}88\,(\theta - {\cdot}0021)^2$	[50]	283?	282·7
	[40]	245·0		[59]	325·7	323·8
γ_2,	[0]	178·2		[−30]	216·1	216·2
	[−20]	195·0	$r = 178{\cdot}2 + 140{\cdot}34\,(\theta - {\cdot}0031)^2$	[−50]	284·5	284·2
	[−40]	246·0		[−59]	325·9	325·9
δ_1,	[0]	255·3				
	[20]	272·3	$r = 255{\cdot}3 + 139{\cdot}52\,(\theta)^2$	[40·77]	325·9	325·9
	[40]	323·3				
δ_2,	[0]	255·3				
	[−20]	272·2	$r = 255{\cdot}3 + 137{\cdot}06\,(\theta - {\cdot}0021)^2$	[−10]	259·5	259·6
	[−40]	322·5		[−40·77]	325·4	325·0

IV.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·6	337·5	1219·2	17·8	0·0085	13 ?	0·434	0·428	0·774	0·787	·561
β, 22·0		360·9	25·6	122	10·2	0·772	0·759	1·163	1·162	·664
γ, 22·34		155·0	32·0	153	8·2	1·180	1·160	1·668		·707
		74·3	36·8	176	6·2	1·741		2·376		·733
		38·0	38·5	184	4·6					
		19·7	39·6	189	3·5					
		10·5	40·3	192	2·5					
		5·4	40·3	192	1·7					
		2·7	40·3	192	1·2					
		1·3	40·3	192	0·7					

					o	c
β_1,	[0]	− 22·5				
	[60]	146·5	$r = -24 + 127{\cdot}38\,(\theta - {\cdot}1094)^2$	[40]	63·5	
	[80]	265·0		[90·2]	337·8	337
β_2,	[0]	− 22·5				
	[− 60]	101	$r = -21{\cdot}71 + 136{\cdot}9\,(\theta - {\cdot}0928)^2$	[− 40]	30	28·4
	[− 80]	209		[− 98·6]	337·8	340·6
γ_1,	[0]	182·7		[30]	219·7	219·6
	[20]	199·0	$r = 182{\cdot}7 + 136{\cdot}24\,(\theta - {\cdot}0031)^2$	[50]	285·5	285·7
	[40]	248·5		[61·27]	337·5	337·5
γ_2,	[0]	182·7				
	[− 20]	199·1	$r = 182{\cdot}7 + 136{\cdot}56\,(\theta - {\cdot}0026)^2$	[− 50]	286·3	286·1
	[− 40]	248·8		[− 61·27]	337·9	338·1

24/7/90. VULCANISED INDIA-RUBBER. I.

N	R	H	C	T	D	A_1 o	A_1 c	A_2 o	A_2 c	e
21·6	302·9	1219·2	14·3	0·0076	12·0	0·387	0·384	0·617	0·628	·628
β, 21·7		451·4	19·6	104	9·6	0·639	0·632	0·917	0·924	·697
γ, 22·06		199·8	23·2	123	7·4	0·960	0·940	1·297	1·310	·740
δ, 22·42		97·0	25·5	135	5·7	1·309	1·320	1·778		·736
		49·0	26·2	139	4·0					
		25·5	27·0	143	3·1					
		13·3	28·0	149	2·3					
		6·9	29·5	157	1·6					
		3·5	29·5	157	1·0					
		1·6								

					o	c
β_1,	[0]	− 148				
	[80]	124·5	$r = -148{\cdot}4 + 129{\cdot}02\,(\theta - {\cdot}0581)^2$	[103·6]	303	300·7
	[100]	271·3				
β_2,	[0]	− 148				
	[− 80]	91	$r = -148 + 127{\cdot}38\,(\theta - {\cdot}0262)^2$	[− 40]	− 86	− 90·5
	[− 100]	228·3		[− 109·4]	303	304·5
γ_1,	[0]	103·7		[30]	139·0	139·1
	[20]	119·0	$r = 103{\cdot}7 + 135{\cdot}81\,(\theta - {\cdot}0132)^2$	[50]	204·1	204·1
	[40]	167·4		[60]	249·6	249·0
				[69·97]	303·1	302·0
γ_2,	[0]	103·7		[− 30]	141·8	141·6
	[− 20]	121·0	$r = 103{\cdot}7 + 130{\cdot}0\,(\theta - {\cdot}0161)$	[− 50]	206·7	206·4
	[− 40]	170·0		[− 60]	250·8	250·7
				[− 69·97]	302·3	302·7

					o	c
δ_1,	[0]	206·3				
	[20]	222·5	$r = 206·3 + 139·19\,(\theta - ·0077)^2$	[47·93]	302·3	301·7
	[40]	272·6				
δ_2,	[0]	206·3				
	[− 20]	224·0	$r = 206·3 + 135·42\,(\theta - ·0127)^2$	[− 47·93]	303·6	303·9
	[− 40]	274·7				

II. Double Mass.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
22·1	311·8	609·6	19·5	0·0103	11·5	0·563	0·545	0·814	0·863	·692
22·2		241·0	26·6	141	9·2	0·885	0·875	1·242	1·253	·713
		113·0	32·1	170	7·1	1·294	1·285	1·831	1·798	·707
β, 22·11		56·9	35·9	190	5·9	1·782	1·791	2·482		·718
γ, 22·03		29·3	34·8	184	4·0					
δ, 22·06		15·6	34·2	181	2·8					
		8·2	33·6	178	2·0					
		4·3	33·0	175	1·3					
		2·3	27·5	146	0·6					
		1·4								

					o	c
β_1,	[0]	70·4		[10]	74·3	74·1
	[20]	86·0	$r = 70·4 + 135·81\,(\theta - ·0100)^2$	[60]	216·0	216·5
	[40]	134·7		[76·87]	312·0	311·2
β_2,	[0]	70·4		[− 30]	108·1	108·4
	[− 20]	87·7	$r = 70·4 + 131·31\,(\theta - ·0142)^2$	[− 60]	218·8	218·3
	[− 40]	137·0		[− 76·87]	312·2	311·8
γ_1,	[0]	198·0				
	[20]	213·5	$r = 198·0 + 135·42\,(\theta - ·0106)^2$	[53·18]	312·2	312·0
	[40]	262·0				
γ_2,	[0]	198·0				
	[− 20]	214·5	$r = 198 + 129·67\,(\theta - ·0077)^2$	[− 53·18]	311·5	311·6
	[− 40]	262·6				
δ_1,	[0]	255·1				
	[10]	259·0	$r = 255·1 + 132·95\,(\theta - ·0032)^2$	[37·52]	311·5	311·5
	[20]	271·0				
δ_2,	[0]	255·1				
	[− 10]	259·2	$r = 255·1 + 132·95\,(\theta - ·0001)^2$	[− 37·52]	312·0	312·0
	[− 20]	271·4				

III.

N	R	H	C	T	D	A_1		A_2		e
						o	c	o	c	
22·25	328·8	1219·2	16·2	0·0081	12·2	0·417	0·4	0·645	0·704	·647
22·1		394·8	24·2	122	10·4	0·711	0·710	1·052	1·089	·676
β, 22·4		169·6	30·6	154	8·4	1·099	1·089	1·553	1·510	·707
γ, 22·26		82·7	34·5	173	6·6	1·530	1·522	2·087	2·061	·733
δ, 22·87		42·2	36·3	182	4·9		2·102		2·893	
ϵ, 23·12		22·0	37·3	187	3·7		2·917			
ζ, 22·93		11·6	37·0	186	2·5					
		6·1	38·9	195	1·8					
		3·1	38·9	195	1·3					
		1·5								

					o	c
β_1, [0]	−66·9					
[60]	86·5	$r = -66{\cdot}9 + 138{\cdot}21\,(\theta - {\cdot}0063)^2$		[96·7]	329	323·8
[80]	205					
β_2, [0]	−66·9					
[−60]	74·6	$r = -67 + 135{\cdot}91\,(\theta - {\cdot}0265)^2$		[−99·4]	329·0	329·6
[−80]	188					
γ_1, [0]	159·0					
[20]	175·1	$r = 159 + 136{\cdot}24\,(\theta - {\cdot}0053)^2$		[50]	261·3	261·5
[40]	224·0			[63·87]	329·1	326·7
γ_2, [0]	159·0					
[−20]	175·9	$r = 159 + 134{\cdot}59\,(\theta - {\cdot}0053)^2$		[−50]	262·9	262·7
[−40]	225·6			[−63·87]	328·2	327·8
δ_1, [0]	246·4					
[20]	263·1	$r = 246{\cdot}4 + 144{\cdot}05\,(\theta - {\cdot}0085)^2$		[43·72]	328·2	328·4
[40]	314·9					
δ_2, [0]	246·4					
[−20]	263·8	$r = 246{\cdot}4 + 141{\cdot}49\,(\theta - {\cdot}0016)^2$		[−43·72]	329·2	329·1
[−40]	315·7					
ϵ_1, [0]	286·2					
[10]	290·6	$r = 286{\cdot}2 + 147{\cdot}73\,(\theta - {\cdot}0019)^2$		[31·08]	329·2	329·36
[20]	304·0					
ϵ_2, [0]	286·2					
[−10]	290·5	$r = 286{\cdot}2 + 144{\cdot}44\,(\theta - {\cdot}0020)^2$		[−31·08]	328·2	328·4
[−20]	303·6					
ζ_1, [0]	306·2					
[10]	310·4	$r = 306{\cdot}2 + 146{\cdot}08\,(\theta - {\cdot}0049)^2$		[22·53]	328·2	328·2
[20]	323·5					
ζ_2, [0]	306·2					
[−10]	310·8	$r = 306{\cdot}2 + 141{\cdot}16\,(\theta - {\cdot}0061)^2$		[−22·53]	328·9	328·7
[−20]	324·0					

21/8/90. VULCANISED INDIA-RUBBER.

(This is the trace reproduced in the plate, and the details are given here to show that fair results can be obtained even when the adjustment is very imperfect.)

N	R	H	C	D	T	A_1		A_2		e
						o	c	o	c	
22·75	338	1219·2	15·3	11·9	0·0077	·383		·636	·655	·602
β, 22·5		456	21·5	9·6	·0108	·660	·693	·966	1·066	·683
γ, 22·9		197·5	25·0	7·6	·0125	·933	1·000	1·354	1·42	·689
δ, 23·4		95·0	26·6	5·5	·0133	1·418	1·42	1·842		·770
		48·0	28·0	4·2	·0140					
		24·5	30·0	3·0	·0150					
		12·5	31·5	2·4	·0158					
		6·0	33·0	1·9	·0165					

				o	c
β_1, [0]	−118		[55]	16·6	18
[80]	168·2	$r = -118 + 146{\cdot}09\,(\theta)^2$	[70]	100	101
[100]	331·7		[90]	242·4	244
β_2, [0]	−118		[−70]	85·5	88·6
[−80]	150·6	$r = -117{\cdot}8 + 130{\cdot}33\,(\theta - {\cdot}0368)^2$	[−75]	116	118·2
[−100]	300·1		[−90]	217·5	219·0

				o	c
γ_1, [0]	131·5				
[30]	171·0	$r = 131{\cdot}5 + 143{\cdot}14\,(\theta + {\cdot}0017)^2$	[15]	141·5	141·5
[60]	289·0		[45]	219·8	220·2
γ_2, [0]	131·5		[− 10]	135·9	135·9
[− 30]	171·0	$r = 131{\cdot}5 + 144{\cdot}45\,(\theta - {\cdot}0007)^2$	[− 50]	241·8	241·3
[− 60]	290·0		[− 65]	317·0	317·2
δ_1, [0]	232·6		[10]	237	237·2
[20]	251·0	$r = 232{\cdot}6 + 149{\cdot}37\,(\theta + {\cdot}0019)^2$	[30]	274	273·8
[40]	305·8		[40·5]	327·6	327·3
δ_2, [0]	232·6				
[− 20]	250·8	$r = 232{\cdot}6 + 149{\cdot}37(\theta)^2$	[− 30]	273·5	273·5
[− 40]	305·4		[− 40·75]	327·6	327·8

DESCRIPTION OF THE PLATE.

The chief figure is, as above stated, photo-lithographed on the scale of 0·3 from the record of a 4-foot fall on Vulcanised India-rubber. Even in this reduced scale it shows fairly enough the relative details of at least eight of the successive rebounds. These are numbered in order. The original showed several more. As its lines were not only very fine, but in blue, they had to be carefully gone over with a photographically inactive colour, so that much of the more delicate detail is unavoidably lost. The tuning-fork was kept in contact with the disc for a little more than a complete revolution. The consequent overlapping of the trace enables us to see that the angular velocity had not sensibly changed during one revolution of the disc.

The three figures immediately below are (pencil) records of successive impacts on Native India-rubber (9/1/89). Time of rotation of disc $0^s{\cdot}3$.

Then follow records of impacts on Pine Tree (7/11/88) from heights of 8, 4, and 2 feet. These show the "wriggles" spoken of in the text. Time $0^s{\cdot}3$.

The group of five which follows belongs to the experiment III. of 23/7/90 with Plane Tree, whose details are given in the Table. Some of these show traces of wriggles.

The final group contains details of the first eight successive impacts of IV. of 7/6/90 on Vulcanised India-rubber. To save space, the first and third, as also the second and sixth, which took place at the same portions of the datum circle, have been drawn together.

In each of the two later groups the time of rotation of the disc was a little more than one second.

The disc always had positive rotation; so that the older figures (those in pencil) must be read the opposite way to the others, which were reversed in printing from the disc:—*i.e.*, the compression part of the impact is to the left on the pencilled figures, to the right on the others.

LXXXIX.

ON IMPACT. II.

[*Transactions of the Royal Society of Edinburgh*, Vol. XXXVII. Read January 18, 1892.]

[SINCE this second instalment of my paper was read to the Society my attention has been called to a remarkable investigation by Hertz*; in which the circumstances of collision of two elastic spheres are fully worked out, under the special limitations that both are smooth, and that their deformations are exceedingly small. This forms a mere episode in the paper, which is devoted mainly to the statical form of the problem of deformation; as, for instance, the case of the ordinary apparatus for the production of Newton's rings. But it contains a definite numerical result; giving for the duration of impact between two iron spheres of 50 mm. diameter, which encounter one another directly with a relative speed of 10 mm. per second, the value $0^s{\cdot}00038$. This seems to be the earliest reckoning of the time of collision. The experimental verification of Hertz' formulæ was undertaken with success by Schneebeli†, who obtained results in close accordance with them. His mode of measuring the duration of impact was defective, though ingenious. But the speeds employed by him, though for the most part considerably greater than those contemplated in Hertz' work, were far inferior to the lowest of which I have availed myself:—and thus no comparison can be instituted between my results and the theoretical formulæ; first, because I have *necessarily* dealt with deformations so large as to be directly measurable; secondly, because the formulæ, being originally obtained for the statical problem, have left aside thermodynamical considerations, and thus assume equal duration for compression and for restitution, which is certainly incorrect; finally, because one of my colliding

* *Journal für die reine und angewandte Mathematik*, xcii., 1882. Über die Berührung fester elastischer Körper.

† *Archives des Sciences physiques*, &c., *Genève*, xv., 1885. Recherches expérimentales sur le Choc des Corps élastiques.

bodies was fixed, and thus virtually struck on both sides, besides being notably deformed throughout the greater part of its substance; while, except in the case of very hard bodies, the surface of contact was nearly equal to the whole section of the cylinder. I regret, however, that I had not seen Hertz' paper before I made my apparatus, as a study of it might have led to improvements in my arrangements; especially in the choice of the form of the elastic substance to be operated on. But my results have the advantage of being applicable to many practical questions (besides those of Golf, to which they owe their birth), such as the driving of a nail by a hammer, or of a pile by a ram, &c. One of Hertz' results is specially interesting, viz. that the duration of impact between two balls is infinite if the relative speed be indefinitely small. This may easily be seen to depend upon the fact that (in consequence of their form) the total force between them, at any instant, varies as a power of the deformation higher than the first.]

The experiments, whose results are tabulated at the end of the paper, were (with the exception of the first, presently to be noticed) made with a new set of specimens of various elastic substances, considerably larger in all their dimensions than those previously employed. They were, as before, cylinders very slightly rounded at their upper ends; but their lengths, as well as their diameters, were 56 mm. instead of 32 mm. as formerly. As I could not procure a piece of good cork of the requisite dimensions, the cylinder of that substance employed was built up of two semi-cylinders, gently kept together by two india-rubber bands. The glass cylinder turned out to be somewhat difficult of manufacture, and the experiments with it are altogether defective. But, after the third, and most considerable, impact to which it was subjected it presented a very interesting appearance. There was formed inside it a fissure somewhat in the shape of a portion of a bell; meeting the upper surface in a nearly circular boundary 12 mm. in diameter. This fissure showed the colours of thin plates in a magnificent manner. It gave the impression that the portion of the glass contained within it had, by the shock, been forced downwards relatively to the rest. Its lower, and wider, extremity did not come within 4 mm. of the sides of the cylinder, and this was at a depth of about 6 mm. below the upper surface.

One result of the new experiments is obvious at the first glance. The duration of impact is notably longer than before; in consequence of the increased dimensions of the elastic bodies operated on. But the coefficient of restitution is only slightly affected.

As the old block had been split during some experiments in which it was allowed to fall on vulcanite from heights of 3 m. and upwards, a new one (also of plane tree) was obtained. The mass of this new block was 3·75 lbs., and (except where it is otherwise specially noted in the tables of experimental results) had its lower end shod with a flat plate of hard steel 6 mm. in thickness, and 1 lb. mass. The main object of this was to prevent the "wriggles" formerly noticed. Another plate of the same material, with a blunted wedge-shaped ridge projecting from its lower surface, was occasionally substituted for this (as noted) in some of the experiments on vulcanised india-rubber. It was tried on cork also, but the result was disastrous.

The object of this ridge was to test the effect, on the coefficient of restitution and on the duration of impact, produced by applying a given momentum of the falling body in a more concentrated form, by restricting the surface-region of its application to the elastic solid. The results obtained by this process, though unfortunately limited to one elastic substance, are very interesting. The duration of impact is notably increased, in spite of the increased distortion; but the coefficient of restitution is practically unaltered.

The first set of experiments given below (7/4/91) was made with the old cylinder of Vulcanised India-Rubber. They were designed to form a link between the present experiments (with the steel plate) and the former set (in which the impinging surface was hard wood).

Mr Shand has again made the measurements of the traces, and reduced the observations, precisely in the same manner as before:—and it will be seen, from the numbers in the columns headed N, that the new series of results is at least as trustworthy as the old one. But I was not satisfied with the numbers in the columns A_1, A_2; nor, of course, with those in e, which are their respective ratios. These data are derived from the very difficult and uncertain process of drawing tangents at the *extremities* of portions of curves. I therefore calculated (to two places only) the values of the square-root of the quotient of each pair of successive numbers in the column H. If there were no friction, the results thus obtained should be the successive values of the coefficient of restitution. And, even taking friction into account, if we suppose the acceleration it produces to be m-fold that of gravity (m being, as shown in the first part of the paper, nearly constant and somewhere about 0·03) the values in the table so formed should be those of

$$e\sqrt{\frac{1-m}{1+m}} = e(1-m) \text{ nearly.}$$

This (though at a first glance it might not be suspected) is the result to which we should be led by calculating from the equations of the various parts of the trace the tangents of the inclination of the curve to the radius-vector at the points where it meets the datum circle. For

$$\tan\phi = \left(r\frac{d\theta}{dr}\right)_R = \frac{R}{2\sqrt{B(R-A)}},$$

so that

$$e = \frac{\tan\phi_1}{\tan\phi_2} = \sqrt{\frac{B_2(R-A_2)}{B_1(R-A_1)}} = \sqrt{\frac{B_2H_2}{B_1H_1}}.$$

Unfortunately, it is in general difficult to get a trustworthy value of B for the (first) incomplete branch of the curve. But, by various modes of calculation and measurement, I have made sure that the friction is practically the same whatever be the mass of the block, so that its effects are the less sensible the greater is that mass. The numbers thus obtained fluctuated through very narrow limits, at least for such bodies as native and vulcanised india-rubber, and therefore give for very extensive ranges of speed of impact a thorough verification of Newton's experimental law; viz. the constancy of the coefficient of restitution for any given impinging bodies. This had, however, been long ago carefully tested by the elaborate experiments of Hodgkinson*. There was, it is true, a slight falling off for the very high speeds, and likewise for the very low: as will be seen from the table of *Approximate Coefficients of Restitution* which follows the experimental results. The first may be due in part to a defect in the apparatus, the second will be accounted for below.

The approximate constancy of e, for all relative speeds, proves merely that the force of restitution is, at every stage, proportional to that required for compression. We must therefore look to the values of the total distortion, or to those of the duration of impact, for information as to the relation between the distortion and the force producing it. The equation of motion during the compression is, say,

$$M\ddot{x} = Mg - F - f'(x) \quad \text{(1)}.$$

Hence, as F may be considered to be nil while the datum circle is being traced, we have for the correction, ∂ suppose, to be applied to the tabulated values of D, that positive root of

$$Mg - f'(x) = 0 \quad \text{(2)}$$

which vanishes with M.

Integrating the equation of motion, we have

$$M\dot{x}^2/2 = MV^2/2 + (Mg - F)\,x - f(x) \quad \text{(3)},$$

where V is the speed at impact, and $f(x)$ vanishes with x. Thus, at the turning point,

$$0 = MV^2/2 + (Mg - F)(D + \partial) - f(D + \partial).$$

Now, by (2), we see that $(Mg - F)$ is of the order $f'(\partial)$ only, so that, when V (and therefore D) is considerable, we may write this in the approximate form

$$0 = MV^2/2 - f(D).$$

This equation enables us to get an approximate estimate of the form of the function f. A graphical representation of D in terms of MH, based on the various data of the experiments of 22/6/91, below, on vulcanised india-rubber, gave three nearly parallel, but closely coinciding curves, whose common equation (when the different values of ∂ for the different masses were approximately taken account of) was of the form

$$MH \propto D^{\frac{5}{2}};$$

for the subtangents were 2·5-fold the abscissæ. Hence we are entitled to write (3) in the tentative form

$$M\dot{x}^2/2 = MV^2/2 + (Mg - F)\,x - Ax^{\frac{5}{2}} \quad \text{(4)}.$$

Equation (2) now becomes $\qquad Mg = \frac{5}{2}A\partial^{\frac{3}{2}};$

whence ∂ may be found, A being determined from one of the larger values of D (and the corresponding kinetic energy) by the relation

$$MgH = AD^{\frac{5}{2}} \quad \text{(5)}.$$

* *British Association Report*, 1834.

These give the approximate value

$$\frac{\partial}{D} = \left(\frac{2D}{5H}\right)^{\frac{2}{3}}.$$

Thus I found that the values of D, for the experiment of 22/6/91 on vulcanised india-rubber, must be augmented by 0·75 mm., 1·2 mm., and 1·9 mm. respectively:—according as the mass was single, double, or quadruple. These agree remarkably well with the relative positions of the parallel curves already spoken of: and also with direct measurements of ∂ which have been recently made for me, by a statical process, by Mr Shand. In what follows, I shall assume that the values of D have had this (positive) correction applied.

By the help of (4) we now have, for the time of compression, the expression

$$\sqrt{\frac{M}{2A}}\int_0^D \frac{dx}{\sqrt{D^{\frac{5}{2}} - x^{\frac{5}{2}} - (Mg - F)(D - x)/A}}.$$

Except for the very small values of D, we may neglect the last term under the radical, and the expression, slightly diminished in value, becomes

$$\sqrt{\frac{M}{2A\sqrt{D}}}\int_0^1 \frac{dz}{\sqrt{1 - z^{\frac{5}{2}}}}.$$

The numerical value of the integral is approximately 1·5. For any one substance the time of compression is therefore inversely as the fourth root of D; and, of course, directly as the square root of M. But we may also write the expression, by means of equation (5) above, in a form which applies to all substances for which the elastic force is in the sesquiplicate ratio of the distortion, viz.

$$\frac{1{\cdot}5D}{\sqrt{2gh}}.$$

This result lies just half-way between the limits, D/V and $2D/V$, assigned (from general considerations) in the first part of this paper.

With the data for the first fall of the quadruple mass in the experiment last referred to, this expression becomes almost exactly $0^s{\cdot}01$. The value of e is about 0·77, so that the whole time of impact should be $\left(1 + \frac{1}{0{\cdot}77}\right) 0^s{\cdot}01$, or $0^s{\cdot}023$; while the experimental value of T is $0^s{\cdot}0211$. But, in consequence of the quantity ∂, above spoken of, *all* the measurements of arcs from which T is calculated are necessarily too small. Add to C, as measured, the product of ∂ by the sum of the two tangents, as given in the table; and diminish R by the amount ∂; the observed time becomes $0^s{\cdot}0224$; so that the formula gives a tolerably close approximation.

If we bear in mind that the values of D ought to be increased by the quantity ∂, we see at once the reason, already referred to, for the apparent falling off of the values of e at low speeds, when they are calculated from the values of H given in the tables.

Among the practical applications of the results above, we see that when a nail is driven, say by a $\frac{1}{4}$-lb. hammer moving at the rate of 10 feet per second, the time of impact being taken as $0^s{\cdot}0004$, the time-average force is some 300 lbs. weight. If the head be one-tenth inch square, this corresponds to a pressure of more than 2000 atmospheres.

Finally, to finish as I began, with an application to golf, although from the nature of the case, the experimental data are not very directly applicable:—we see that, as the coefficient of restitution from wood is about 0·66, and the mass of the ball about 0·1 lb., the club must be moving at some 300 feet per second to produce an initial speed of 500 feet per second:—and the time-average of the force during collision must be reckoned in tons' weight. The experiments on hammered, and on unhammered balls, all made at the same time and of the same material, show clearly how very small is the gain in coefficient of restitution, and therefore in initial speed, which is due to the hammering:—and thus force us to look in another direction for an explanation of the unquestionable superiority of hammered over unhammered balls.

[It is very curious that the law of force in terms of the distortion (as given above) is the same as that which results from Hertz' investigations. For, what is called D above is the diminution, in length, of the *whole* cylinder operated on; while, in Hertz' work, the quantity which he calls α, and to whose 3/2th power the force is proportional, is the advance towards one another (since the first contact) made by points chosen in the two bodies, whose distance from the (infinitesimal) surface of contact is finite, yet very small in comparison with the dimensions of the bodies themselves. In my experiments the vertical shortening extends throughout the whole of at least the protruding part of the cylinder, and in extreme cases the distortion is so great that the diameter at the middle becomes more than double that at the ends; in Hertz' investigations it is assumed to be mainly confined to the immediate neighbourhood of the surface of contact.

It is even more curious to find that the same law holds, at least in a closely approximate manner, for the very large and unsymmetrical distortions produced by the ridged base, as shown by the data of 7/11/91.

Some additional details connected with this investigation, including a sketch of the apparatus and of the trace of 13/7/92, will be found in an article *Sur la Durée du Choc*, which appeared in the *Revue des Sciences pures et appliquées*, 30/11/92.]

[In the following experiments, unless some contrary statement is made, the falling block terminated in a horizontal plate of steel.]

7/4/91. Vulcanised India-Rubber. (Old Small Cylinder.)

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·7	337·0	1219·2	15·5	11·0	·0075	·4396	·6200	·709
β, 20·8		510·6	18·1	8·5	·0087	·6903	·8693	·794
γ, 21·6		283·5	19·3	6·7	·0093	·9163	1·1165	·821
δ, 21·6		163·6	19·7	5·3	·0095	1·1875	1·4496	·819
		96·7	21·5	4·4	·0103			
		58·6	22·2	3·6	·0107			
		35·5	22·9	2·8	·0110			
		21·8	23·5	2·3	·0113			
		13·6	24·1	1·8	·0116			
		8·4	25·0	1·6	·0120			
		5·0	27·1	1·3	·0130			
		3·1	27·1	1·0	·0130			
		1·8	27·1	0·9	·0130			

						o	c
β_1,	[0]	−174·0					
	[20]	−158·4	$r = -174 + 120{\cdot}16\,(\theta + {\cdot}01133)^2$				
	[100]	212·5					
β_2,	[0]	−174					
	[− 20]	−157·6	$r = -174 + 116{\cdot}22\,(\theta + {\cdot}02754)^2$				
	[− 100]	207·6					
γ_1,	[0]	53·4					
	[30]	94·5	$r = 53{\cdot}4 + 137{\cdot}89\,(\theta + {\cdot}02266)^2$		[80·75]	337	336·1
	[60]	211·3					
γ_2,	[0]	53·4					
	[− 30]	85·3	$r = 53{\cdot}4 + 117{\cdot}20\,(\theta - {\cdot}00174)^2$		[− 89·15]	337	336·5
	[− 60]	181·7					
δ_1,	[0]	173·3					
	[30]	209·7	$r = 173{\cdot}3 + 133{\cdot}29\,(\theta - {\cdot}00105)^2$		[63·17]	337·0	335·0
	[60]	319·2					
δ_2,	[0]	173·3					
	[− 30]	208·3	$r = 173{\cdot}3 + 122{\cdot}13\,(\theta + {\cdot}01186)^2$		[− 65·7]	337·0	337·2
	[− 60]	310·2					

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·7	326·7	1219·2	15·0	10·8	·0074	·4330	·5639	·768
β, 20·9		548·0	17·6	8·7	·0087	·6403	·7766	·824
γ, 21·4		303·0	19·0	7·0	·0094	·8606	1·0289	·837
δ, 21·5		177·6	19·1	5·6	·0095	1·1054	1·3581	·814
		106·6	19·9	4·5	·0099			
		64·5	21·0	3·7	·0104			
		40·4	22·3	3·0	·0110			
		25·1	22·5	2·5	·0116			
		15·7	23·0	2·0	·0114			
		10·1	23·3	1·5	·0115			

					o	c
β_1,	[0]	−221·8				
	[30]	−184·1	$r = -221{\cdot}8 + 128{\cdot}14\,(\theta + {\cdot}01918)^2$	[117]	326·7	322·6
	[100]	177·3				
β_2,	[0]	−221·8				
	[−30]	−188·5	$r = -221{\cdot}8 + 121{\cdot}7\,(\theta - {\cdot}0005)^2$	[−121·1]	326·7	322·0
	[−110]	226·6				
γ_1,	[0]	23·8				
	[40]	97·5	$r = 23{\cdot}7 + 137{\cdot}89\,(\theta + {\cdot}03365)^2$	[83]	326·7	326·6
	[80]	305·5				
γ_2,	[0]	23·8				
	[−40]	79·0	$r = 23{\cdot}8 + 112{\cdot}94\,(\theta + {\cdot}00105)^2$	[−93·4]	326·7	324·3
	[−80]	244·2				
δ_1,	[0]	148·9				
	[30]	185·1	$r = 148{\cdot}9 + 131{\cdot}32\,(\theta + {\cdot}00140)^2$	[66·4]	326·7	325·7
	[60]	293·3				
δ_2,	[0]	148·9				
	[−30]	184·0	$r = 148{\cdot}9 + 122{\cdot}78\,(\theta + {\cdot}01116)^2$	[−68·15]	326·7	325·9
	[−60]	286·5				

III. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·3	320·6	1219·2	21·6	14·6	·0107	·4383	·7813	·561
β, 21·1		359·5	29·8	12·7	·0146	·7669	1·0052	·763
γ, 21·1		197·8	34·7	10·9	·0172	1·0241	1·2305	·832
δ, 21·4		121·5	38·3	9·1	·0190	1·3206	1·5911	·830
		79·0	40·6	7·8	·0201			
		53·0	41·7	6·5	·0207			
		35·4	41·7	5·3	·0207			
		24·1	39·7	4·1	·0198			
		16·1	41·0	3·6	·0203			
		10·8	43·2	3·1	·0214			

					o	c
β_1,	[0]	− 38·7				
	[60]	78·0	$r = -39{\cdot}6 + 127{\cdot}38\,(\theta - {\cdot}08595)^2$	[101·7]	320·6	323·7
	[90]	241·1				
β_2,	[0]	− 38·7				
	[−60]	106·6	$r = -39{\cdot}5 + 114{\cdot}58\,(\theta + {\cdot}08194)^2$	[− 96·5]	320·6	317·9
	[−90]	273·5				
γ_1,	[0]	122·8				
	[30]	157·6	$r = 122{\cdot}8 + 123{\cdot}11\,(\theta + {\cdot}00819)^2$	[71·83]	320·6	318·8
	[60]	259·9				
γ_2,	[0]	122·8				
	[−30]	156·8	$r = 122{\cdot}8 + 119{\cdot}83\,(\theta + {\cdot}00907)^2$	[−72·75]	320·6	318·7
	[−60]	256·6				
δ_1,	[0]	199·5				
	[20]	214·7	$r = 199{\cdot}5 + 126{\cdot}72\,(\theta - {\cdot}00279)^2$	[55·9]	320·6	319·4
	[40]	260·8				
δ_2,	[0]	199·5				
	[−20]	214·2	$r = 199{\cdot}5 + 125{\cdot}08\,(\theta - {\cdot}00610)^2$	[−56·95]	320·6	321·5
	[−40]	259·4				

IV. QUAD. MASS.

	N	R	H	C	D	T	A_1	A_2	e
	21·45	307·0	1219·2	20·0	15·3	·0104	·4142	·7050	·588
β,	21·2		394·6	27·3	13·0	·0142	·7076	·9088	·779
γ,	21·3		212·8	34·0	11·3	·0177	·9725	1·1813	·823
δ,	21·5		128·0	36·9	9·6	·0192	1·2052	1·5409	·782
			80·3	40·0	8·1	·0209			
			54·0	40·8	7·0	·0213			
			36·5	42·0	5·6	·0219			
			24·7	42·0	4·6	·0219			
			16·4	45·0	4·0	·0235			
			11·2	45·0	3·3	·0235			
				47·9	2·8	·0250			
				50·3	2·1	·0262			
				54·0	1·7	·0282			
				54·4	1·2	·0284			

				o	c
β_1, [0]	− 88·3				
[80]	150·0	$r = -88{\cdot}4 + 126{\cdot}72\,(\theta - {\cdot}0246)^2$	[102·33]	307	304·7
[100]	286·8				
β_2, [0]	− 88·3				
[− 80]	157·0	$r = -88{\cdot}5 + 119{\cdot}17\,(\theta + {\cdot}0389)^2$	[101·63]	307	303
[− 100]	290·9				
γ_1, [0]	93·8				
[30]	127·0	$r = 93{\cdot}8 + 126{\cdot}07\,(\theta - {\cdot}01046)^2$	[75·1]	307	306·9
[60]	229·5				
γ_2, [0]	93·8				
[− 30]	127·9	$r = 93{\cdot}8 + 122{\cdot}13\,(\theta + {\cdot}00488)^2$	[− 75·15]	307·0	305·4
[− 60]	229·0				
δ_1, [0]	179·3				
[20]	194·9	$r = 179{\cdot}3 + 125{\cdot}08\,(\theta + {\cdot}00401)^2$	[57·1]	307	304·5
[40]	241·0				
δ_2, [0]	179·3				
[− 20]	194·0	$r = 179{\cdot}3 + 126{\cdot}40\,(\theta - {\cdot}00802)^2$	[− 57·93]	307	306·5
[− 40]	239·5				

12/6/91. NEW NATIVE INDIA-RUBBER.

I. SINGLE MASS.

	N	R	H	C	D	T	A_1	A_2	e
	22·7	265·5	750·0	28·4	21·3	·018	·4317	·4866	·8872
ϵ,	22·7		501·2	29·5	18·1	·019	·5441	·6017	·9042
			343·8	30·6	15·9	·0195	·6358	·7167	·8871
			240·7	31·7	13·7	·020	·7550	·8627	·8751
			165·7	33·3	11·6	·021	·9004	1·0514	·8563
			110·5	35·4	10·0	·023	1·1403	1·2892	·8845
			74·2	36·0	8·3	·023			
			49·5	39·1	7·1	·025			
			33·2	40·8	6·0	·026			
			22·1	42·3	5·2	·027			
			14·7	45·1	4·5	·029			
			9·2	46·0	3·7	·029			
			5·7	48·6	3·0	·031			
			3·4	51·0	2·3	·033			

N	R	H	C	D	T	A_1	A_2	e
		2·2	48·6	1·6	·031			
		1·3	48·6	1·1	·031			
		1·0	48·6	·9	·031			
		·7	48·6	·6	·031			

				o	c
ϵ_1, [0]	99·2				
[30]	140·1	$r = 99{\cdot}2 + 148{\cdot}39\,(\theta + {\cdot}0014)^2$	[40]	171·8	171·8
[60]	262·3				
ϵ_2, [0]	99·2				
[− 30]	135·2	$r = 99{\cdot}2 + 131{\cdot}98\,(\theta - {\cdot}0014)^2$	[− 40]	163	163·3
[− 60]	243·5				

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·8	274·5	750·0	29·9	21·6	·018	·4581	·5131	·8928
ϵ, 21·8		508·0	31·8	18·3	·019	·5860	·6473	·9052
		347·8	33·0	15·8	·0195	·6873	·7646	·8989
		244·6	34·2	13·5	·020	·8064	·9163	·8800
		167·3	36·0	11·8	·021	·9833	1·1086	·8870
		112·2	37·9	10·1	·023			
		75·3	39·5	8·5	·024			
		50·2	41·3	7·5	·025			
		33·5	43·1	6·4	·026			
		22·1	45·8	5·4	·027			
		14·5	48·1	4·6	·029			
		9·0	49·5	3·7	·0295			
		5·7	50·9	2·9	·030			
		3·7	50·9	2·3	·030			
		2·7	50·9	1·9	·030			
		1·7	50·9	1·3	·030			
		1·2	50·9	·9	·030			

				o	c
ϵ_1, [0]	108·0				
[30]	145·5	$r = 108 + 136{\cdot}24\,(\theta + {\cdot}0010)^2$	[40]	175·0	175·0
[60]	257·8				
ϵ_2, [0]	108·0				
[− 30]	143·5	$r = 108 + 122{\cdot}46\,(\theta - {\cdot}0005)^2$	[− 20]	123·7	122·9
[− 60]	246·2				

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·2	292·1	750·0	37·6	26·1	·020	·4956	·5528	·897
ϵ, 21·8		536·8	40·9	23·1	·022	·5715	·6590	·867
		399·3	41·0	20·4	·022	·6590	·7220	·913
		303·8	42·9	18·3	·023	·7632	·8337	·915
		232·9	44·0	16·3	·024	·8561	·9270	·923
		177·3	45·5	14·7	·025	·9556	1·0831	·882
		133·7	46·6	13·2	·025			
		100·0	48·1	11·3	·026			
		73·7	50·1	10·5	·027			
		54·7	52·6	9·2	·0286			
		40·2	52·6	8·0	·0286			

N	R	H	C	D	T	A_1	A_2	e
		29·3	52·6	6·9	·0286			
		22·2	52·6	6·0	·0286			
		16·2	52·6	5·2	·0286			
		11·5	52·6	4·4	·0286			
		8·2	51·7	3·6	·028			
		5·7	52·0	3·0	·028			
		4·0	51·1	2·1	·028			
		2·9	50·2	1·5	·027			
		2·0	48·3	1·1	·026			
		1·6	44·7	0·6	·024			
		1·0	37·6	·4	·020			
		·8	34·2		·018			

ϵ_1, [0] 59·5 — *o* *c*
[30] 99·1 — $r = 59{\cdot}6 + 133{\cdot}95\,(\theta + {\cdot}0209)^2$ — [74·4] 292·1 292·5
[60] 212·1

ϵ_2, [0] 59·5
[– 30] 94·0 — $r = 59{\cdot}5 + 125{\cdot}74\,(\theta)^2$ — [– 77·6] 292·1 290·1
[– 60] 197·5

IV. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·8	313	750·0	39·5	26·4	·021	·5165	·5879	·879
ϵ, 21·9		539·2	41·7	23·3	·022	·6120	·6681	·916
		401·7	44·1	20·8	·023	·6997	·7879	·888
		305·9	46·2	18·7	·024	·8012	·8795	·911
		237·9	47·4	16·7	·025	·9244	1·000	·924
		180·8	49·0	15·0	·026	1·0441	1·1504	·908
		137·2	51·0	13·6	·027			
		103·4	52·7	12·0	·028			
		77·7	53·8	10·6	·028			
		58·1	56·0	9·5	·029			
		43·5	58·0	8·6	·030			
		32·3	59·5	7·5	·031			
		23·9	61·3	6·5	·032			
		17·3	61·0	5·8	·032			
		13·0	61·0	4·8	·032			
		9·5	61·0	3·9	·032			
		6·9	61·8	3·5	·032			
		4·9	62·6	2·8	·033			
		3·4	63·1	2·2	·033			
		2·3	63·0	1·7	·033			
		1·6	62·9	1·2	·033			
		1·2	60·9	·7	·032			
		·8	50·8	·4	·026			
		·5	48· ?	·3	·025			

ϵ_1, [0] 78·0 — *o* *c*
[30] 116·1 — $r = 78 + 135{\cdot}59\,(\theta + {\cdot}0066)^2$ — [75·2] 313 314
[60] 228·5

ϵ_2, [0] 78·0
[– 30] 114·5 — $r = 78 + 127{\cdot}38\,(\theta + {\cdot}01185)^2$ — [– 77] 313 312
[– 60] 220·9

22/6/91. New Vulcanised India-Rubber.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·0	269·2	914·4	25·3	18·4	·0155	·4262	·5325	·800
δ, 22·4		459·4	26·7	13·8	·0163	·5844	·7150	·817
		252·7	27·0	10·8	·0165	·7627	·9331	·817
		151·3	27·7	8·5	·0170	·9833	1·1918	·825
		90·8	28·4	6·7	·0174			
		53·7	29·1	5·2	·0178			
		31·1	29·1	4·0	·0178			
		18·0	30·0	3·1	·0184			
		10·4	30·9	2·5	·0189			
		6·1	31·1	2·0	·0190			
		3·6	31·1	1·4	·0190			
		2·0	31·1	1·0	·0190			

				o	c
δ_1, [0]	117·6				
[20]	133·1	$r = 117{\cdot}6 + 141{\cdot}17\,(\theta - {\cdot}0174)^2$	[60·5]	269·2	269·8
[40]	183·0				
δ_2, [0]	117·6				
[− 20]	134·5	$r = 117{\cdot}6 + 132{\cdot}63\,(\theta + {\cdot}00645)^2$	[− 61·15]	269·2	270·9
[− 40]	183·7				

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·2	283·2	914·4	26·1	18·5	·0146	·4434	·5438	·815
δ, 22·4		464·5	27·2	13·9	·0152	·5961	·7400	·805
		260·3	28·5	11·0	·0160	·7921	·9675	·819
		155·3	29·0	8·7	·0163	1·0082	1·2349	·816
		93·4	29·9	6·9	·0168			
		56·6	31·0	5·5	·0174			
		33·2	31·5	4·2	·0177			
		18·9	32·1	3·4	·0180			
		10·7	33·5	2·5	·0188			
		5·9	34·4	2·0	·0193			
		3·5	35·2	1·5	·0198			
		2·0	36·8	1·1	·0207			

				o	c
δ_1, [0]	128·4				
[20]	146·1	$r = 128{\cdot}4 + 142{\cdot}81\,(\theta + {\cdot}00296)^2$	[59·55]	283·2	283·4
[40]	198·6				
δ_2, [0]	128·4				
[− 20]	144·1	$r = 128{\cdot}4 + 131{\cdot}65\,(\theta - {\cdot}00401)^2$	[− 62·2]	283·2	284·6
[− 40]	191·9				

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
22·0	299·7	914·4	33·9	23·6	·0186	·4455	·5704	·781
δ, 21·7		505·1	37·4	18·8	·0206	·6009	·7655	·785
		305·9	38·8	15·3	·0212	·7790	·9523	·818
		193·7	39·8	12·4	·0219	·9573	1·1875	·806
		124·1	40·7	10·1	·0224			
		79·8	42·2	8·0	·0232			
		50·0	42·2	6·6	·0232			

N	R	H	C	D	T	A_1	A_2	e
		31·4	43·6	5·0	·0240			
		19·5	44·4	4·1	·0244			
		12·2	44·6	3·3	·0246			
		7·6	44·6	2·5	·0246			
		4·6	44·6	1·8	·0246			
		2·7	44·6	1·3	·0246			
		1·6						

					o	c
δ_1,	[0]	106·4				
	[20]	122·0	$r = 106{\cdot}4 + 132{\cdot}96\,(\theta - {\cdot}00645)^2$	[70]	299·7	302·7
	[40]	170·0				
δ_2,	[0]	106·4				
	[− 20]	122·5	$r = 106{\cdot}4 + 126{\cdot}40\,(\theta + {\cdot}00785)^2$	[− 70·4]	299·7	299·7
	[− 40]	169·4				

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·3	317·7	914·4	40·2	28·2	·0211	·4621	·6340	·729
δ, 22·3		486·3	47·0	23·7	·0248	·6290	·8021	·784
		293·3	51·0	19·5	·0268	·7974	1·0035	·795
		183·4	54·1	16·0	·0285	1·0355	1·2527	·827
		117·5	55·5	12·9	·0292	1·2783	1·5587	·820
		75·2	56·9	10·5	·0300			
		48·7	57·8	8·7	·0304			
		31·5	59·5	6·7	·0313			
		19·9	59·5	5·2	·0313			
		12·3	60·5	4·0	·0318			
		7·4	61·9	3·0	·0326			
		4·6	62·0	2·2	·0326			
		2·7	62·0	1·5	·0326			
		1·7	62·0	1·1	·0326			

					o	c
δ_1,	[0]	134·8				
	[20]	151·1	$r = 134{\cdot}8 + 140{\cdot}51\,(\theta - {\cdot}00837)^2$	[66·5]	317·7	321·2
	[40]	201·6				
δ_2,	[0]	134·8				
	[− 20]	152·0	$r = 134{\cdot}8 + 134{\cdot}60\,(\theta + {\cdot}00854)^2$	[− 66·25]	317·7	317·2
	[− 40]	202·0				

16/6/91. Cork.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·5	272·3	1219·2	14·4	9·9	·0089 ?	·3640	·6732	·541
β, 22·0		250·0	14·1	5·2	·0087	·7664	1·3230	·579
		71·4	14·1	3·0	·0087			
		24·5	14·7	1·7	·0091			
		8·9	15·4	1·2	·0095			

					o	c
β_1,	[0]	23·0				
	[30]	78·2	$r = 21{\cdot}7 + 148{\cdot}72\,(\theta + {\cdot}0924)^2$	[69·1]	272·3	272·4
	[60]	214·9				
β_2,	[0]	23·0				
	[− 30]	50·2	$r = 22{\cdot}8 + 116{\cdot}22\,(\theta - {\cdot}03836)^2$	[− 85·25]	272·3	267·0
	[− 60]	141·1				

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·15	286·8	1219·2	16·0	10·3	·0093	·3959	·7341	·539
β, 21·9		252·9	15·6	5·4	·0090	·8156	1·4176	·575
		71·5	15·7	3·2	·0091			
		25·1	16·3	1·9	·0094			
		9·4	16·5	1·3	·0096			
		3·5	17·6	·8	·0102			

				o	c
β_1, [0]	34·3				
[30]	76·9	$r = 34{\cdot}3 + 148{\cdot}06\,(\theta + {\cdot}0129)^2$	[74·4]	286·8	288·6
[60]	200·7				
β_2, [0]	34·3				
[− 30]	65·0	$r = 34{\cdot}3 + 113{\cdot}59\,(\theta - {\cdot}00384)^2$	[− 84·83]	286·8	282·0
[− 60]	158·0				

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·15	296·5	1219·2	29·1	15·7	·0156	·4032	·9884	·408
β, 21·4		179·8	27·4	6·7	·0147	·9977	1·9458	·513
		44·5	26·1	3·6	·0140			
		14·0	25·6	2·2	·0137			
		4·9	28·3	1·4	·0151			

				o	c
β_1, [0]	116·1				
[30]	151·5	$r = 116{\cdot}1 + 130{\cdot}66\,(\theta - {\cdot}00732)^2$	[67·5]	296·5	296·3
[60]	258·6				
β_2, [0]	116·1				
[− 30]	148·9	$r = 116{\cdot}1 + 119{\cdot}50\,(\theta)^2$	[− 70·33]	296·5	296·2
[− 60]	247·3				

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·45	321·7	1219·2	52·1	24·7	·0261	·4245	1·2218	·347
β, 21·3		135·1	54·2	10·0	·0271	1·2550	2·7450	·457
		26·4	46·2	4·4	·0231			
		6·8	46·9	2·5	·0235			

				o	c
β_1, [0]	186·4				
[20]	202·0	$r = 186{\cdot}04 + 128{\cdot}4\,(\theta)^2$	[58·75]	321·7	321·0
[40]	248·8				
β_2, [0]	186·4				
[− 20]	201·5	$r = 186{\cdot}4 + 120{\cdot}65\,(\theta - {\cdot}00663)^2$	[− 60·6]	321·7	319·6
[− 40]	246·0				

24/6/91. Cork.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·3	273·1	2286·0	24·1	20·5	·0148	·2568	·5844	·439
γ, 22·3		373·6	19·7	7·5	·0121	·6322	1·2009	·526
		88·6	17·2	3·7	·0105			
		29·7	16·9	2·1	·0104			
		11·4	17·4	1·4	·0107			
		4·6	17·8	1·0	·0109			

				o	c
γ_1, [0]	184·2				
[20]	202·1	$r = 184{\cdot}2 + 142{\cdot}15\,(\theta + {\cdot}00610)^2$	[45·1]	273·1	273·9
[40]	254·7				
γ_2, [0]	184·2				
[−20]	199·4	$r = 184{\cdot}2 + 129{\cdot}02\,(\theta - {\cdot}00558)^2$	[−30]	218·7	218·8
[−40]	246·0				

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·23	289·2	2286·0	26·1	21·1	·0151	·2754	·6200	·444
γ, 22·1		390·0	22·3	8·0	·0129	·6590	1·3079	·504
		91·5	18·8	3·8	·0108			
		30·3	18·5	2·3	·0107			
		11·5	19·1	1·6	·0110			
		4·7	20·4	1·0	·0118			

				o	c
γ_1, [0]	198·1				
[20]	214·8	$r = 198{\cdot}1 + 138{\cdot}87\,(\theta - {\cdot}00209)^2$	[46·7]	289·2	289·7
[40]	265·3				
γ_2, [0]	198·1				
[−20]	214·2	$r = 198{\cdot}1 + 130{\cdot}66\,(\theta + {\cdot}00192)^2$	[−30]	234·0	234·3
[−40]	262·1				

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·8	306·0	2286·0	38·6	29·2	·0206	·2908	·7463	·390
γ, 21·8		312·0	41·6	12·2	·0222	·7776	1·6697	·466
		64·3	33·5	4·9	·0179			
		18·0	31·8	2·7	·0170			
		5·9	31·8	1·6	·0170			
		1·8	36·1 ?	1·1	·0193?			

				o	c
γ_1, [0]	242·4				
[20]	257·5	$r = 242{\cdot}4 + 131{\cdot}65\,(\theta - {\cdot}01011)^2$	[30]	277·1	277·1
[40]	304·7				
γ_2, [0]	242·4				
[−20]	258·0	$r = 242{\cdot}4 + 128{\cdot}04\,(\theta)^2$	[−10]	246·3	246·3
[−40]	304·7				

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·1	323·8	2286·0	45·5	33·9	·0233	·3102	·9163	·3385
β, 22·0		232·6	72·1	17·8	·0369	·9244	2·1283	·4743
		42·0	62·3	6·8	·0319			
		8·4	57·1	3·0	·0292			

				o	c
β_1, [0]	91·0				
[30]	126·5	$r = 91{\cdot}0 + 134{\cdot}93\,(\theta - {\cdot}01064)^2$	[75·5]	323·8	321·6
[60]	236·0				
β_2, [0]	91·0				
[−30]	128·0	$r = 91 + 132{\cdot}30\,(\theta + {\cdot}0052)^2$	[−40]	156·1	156·4
[−60]	237·6				

26/6/91. VULCANISED INDIA-RUBBER.

I. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·5	310·8	2438·4	26·4	29·8	·0143	·2836	·3578	·793

II. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
21·9	300·0	2438·4	26·0	29·7	·0142	·2733	·3518	·777

III. DOUBLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·05	285·7	2438·4	26·0	31·1	·0151	·2586	·3457	·748

IV. QUAD. MASS.

N	R	H	C	D	T	A_1	A_2	e
22·05	270·1	2438·4	25·6	35·4	·0157	·2348	·3275	·717

3/7/91. VULCANITE.

I. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·35	271·4	1219·2	1·6	1·2	·0010	·3620	·7002	·517
β, 22·3		259·3	1·9	1·1	·0012	·7265	1·3663	·532
		72·7	2·2	·5	·0014			
		31·2	2·6	·5	·0016			
		16·8	2·9	·3	·0018			
		9·8	3·2	·3	·0020			
		5·9	2·8	·2	·0017			
		3·5	3·2	·2	·0020			
		2·0						

				o	c
β_1, [0]	12·4				
[30]	59·8	$r = 12·1 + 148·72\,(\theta + ·04254)^2$	[73·4]	271·4	272·6
[60]	188·7				
β_2, [0]	12·4				
[− 30]	44·0	$r = 12·4 + 122·13\,(\theta - ·01464)^2$	[− 83·4]	271·4	265·9
[− 60]	142·6				

II. DOUBLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·4	279·1	1219·2	2·1	2·3	·0013	·3561	·5250	·678
γ, 22·5		492·3	2·4	1·6	·0014	·5372	·9424	·570
		151·7	2·8	1·0	·0017			
		48·2	3·1	·6	·0019			
		23·0	3·6	·4	·0022			
		11·5	4·2	·3	·0025			

				o	c
γ_1, [0]	127·5				
[20]	144·0	$r = 127·5 + 141·50\,(\theta - ·0075)^2$	[59·7]	279·1	278·9
[40]	195·0				
γ_2, [0]	127·5				
[− 20]	144·7	$r = 127·5 + 136·24\,(\theta - ·00401)^2$	[60·0]	279·1	275·8
[− 40]	195·1				

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
23·35	299·3	2438·4	1·9	1·8	·0011	·2586	·4986	·519
γ, 23·2		542·8	2·1	1·4	·0012	·5486	·8601	·638
		172·3	2·4	1·0	·0014			
		57·0	3·3	·5	·0019			
		25·3	3·7	·4	·0022			
		11·4	4·0	·3	·0023			

				o	c
γ_1, [0]	127·7				
[30]	167·5	$r = 127{\cdot}7 + 150{\cdot}36\,(\theta - {\cdot}009070)^2$	[61·75]	299·3	299·9
[60]	289·7				
γ_2, [0]	127·7				
[− 30]	168·0	$r = 127{\cdot}7 + 143{\cdot}14\,(\theta + {\cdot}0136)^2$	[− 62·4]	299·3	301·7
[− 60]	286·8				

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·85	308·7	1219·2	3·4	2·7	·0019	·3805	·7673	·496
β, 22·5		291·0	3·3	1·1	·0018	·7841	1·7090	·459
		58·0	5·2	·9	·0029			
		14·7	9·6	·9	·0053			
		4·8	9·6	·5	·0053			
		1·7	9·6	·3	·0053			

				o	c
β_1, [0]	17·6				
[30]	66·0	$r = 17{\cdot}3 + 151{\cdot}35\,(\theta + {\cdot}0436)^2$	[77·5]	308·7	312·3
[60]	197·4				
β_2, [0]	17·6				
[− 30]	47·3	$r = 17{\cdot}4 + 125{\cdot}41\,(\theta - {\cdot}03487)^2$	[− 88·2]	308·7	301·2
[− 60]	145·7				

V. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·75	321·1	2438·4	2·3	1·5	·0012	·2867	·8878	·323
β, 22·5		272·1	2·6	0·8	·0014	·8466	1·8040	·469
		53·9	5·0	0·8	·0027			
		13·3	8·5	0·8	·0045			
		4·5	8·4	0·4	·0045			

				o	c
β_1, [0]	49·0				
[30]	90·3	$r = 49 + 142{\cdot}81\,(\theta + {\cdot}0143)^2$	[78·7]	321·1	324
[60]	210·0				
β_2, [0]	49·0				
[− 30]	83·6	$r = 49 + 132{\cdot}63\,(\theta - {\cdot}01255)^2$	[− 82·6]	321·1	319·8
[− 60]	191·0				

10/7/91. Lead.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·55	279·0	1219·2	2·5?	1·4?	·0015?	·3640	·8230?	·442?

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
23·35	295·0	1219·2						
		120·5	1·8	0·8	·0011			
		21·1	2·0	0·4	·0012			

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
22·55	304·2	1219·2	1·7	1·3	·0009	·3899	1·0355	·3765
		158·6	2·6	·7	·0014			
		17·0	2·6	·4	·0014			

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
23·0	325·3	1219·2	3·0	1·4	·0016	·4265	1·7461	·244
		84·7						
		9·4						

V. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
23·0	317·8	2438·4	4·5	2·0	·0024	·2773	1·9375	·143

29/6/91. Plane Tree.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·1	269·6	1219·2	2·0	1·7	·0012	·3719	·7590	·490
β, 21·0		215·3	2·8	1·1	·0016	·8785	1·6977	·517
		45·0	2·6	·6	·0015			
		15·4	2·6	·3	·0015			
		7·3	3·3	·2	·0019			

β_1, [0]	54·6			*o*	*c*
[30]	89·3	$r = 54{\cdot}6 + 130{\cdot}66\,(\theta - {\cdot}00802)^2$	[73·9]	269·6	269·2
[60]	195·6				
β_2, [0]	54·6				
[−30]	84·7	$r = 54{\cdot}6 + 110{\cdot}64\,(\theta - {\cdot}00209)^2$	[−80]	269·6	269·6
[−60]	175·5				

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·3	277·6	1219·2	1·1		·0007	·3640	·7618	·478
β, 21·7		229·9	2·6	1·3	·0016	·8273	1·5340	·439
		54·1	2·3	·7	·0014			
		17·0	3·0	·4	·0018			
		7·9	3·1	·2	·0019			

β_1, [0]	47·7			*o*	*c*
[30]	85·3	$r = 47{\cdot}7 + 134{\cdot}93\,(\theta + {\cdot}00436)^2$	[74·25]	277·6	275·8
[60]	196·9				
β_2, [0]	47·7				
[−30]	82·0	$r = 47{\cdot}7 + 123{\cdot}77\,(\theta + {\cdot}00296)^2$	[−78]	277·6	278·1
[−60]	184·2				

III. Double Mass.

	N	R	H	C	D	T	A_1	A_2	e
	22·5	288·7	1219·2	2·2	1·9	·0013	·3679	·8754	·420
β,	22·5		198·6	2·9	1·2	·0017	·9004	1·7251	·522
			48·8	3·4	·8	·0020			
			15·2	5·1	·6	·0030			
			5·5	8·5	·5	·0050			

					o	c
β_1,	[0]	90·6				
	[30]	128·0	$r = 90·6 + 141·83\,(\theta - ·00994)^2$	[68·4]	288·7	289·3
	[60]	243·2				
β_2,	[0]	90·6				
	[− 30]	128·0	$r = 90·6 + 134·60\,(\theta + ·00349)^2$	[− 69·4]	288·7	289·2
	[− 60]	239·3				

IV. Single Mass.

	N	R	H	C	D	T	A_1	A_2	e
	22·2	299·9	2438·4	...	1·0	...	·2830	·6494	·436
γ,	21·1		408·8	2·5	1·4	·0014	·6720	1·2846	·523
			90·7	2·1	·9	·0012			
			25·0	2·9	·5	·0016			
			11·1	3·5	·3	·0019			
			5·2	4·3	·2	·0024			

					o	c
γ_1,	[0]	210·0				
	[20]	225·2	$r = 210·0 + 138·21\,(\theta - ·00174)^2$	[47·1]	299·9	303·0
	[40]	274·1				
γ_2,	[0]	210·0				
	[− 20]	227·4	$r = 210 + 128·37\,(\theta + ·00174)^2$	[− 30]	247·9	245·4
	[− 40]	276·1				

V. Double Mass.

	N	R	H	C	D	T	A_1	A_2	e
	22·4	316·4	2438·4				·2943	·9067	·324
β,	21·4		255·4	3·6	1·4	·0019	·9131	1·7747	·515
			58·6	4·0	1·0	·0021			
			15·6	5·7	·7	·0030			
			5·0	11·0	·6	·0058			

					o	c
β_1,	[0]	61·2				
	[30]	95·8	$r = 61·2 + 129·02\,(\theta - ·00575)^2$	[80·75]	316·4	314·3
	[60]	201·2				
β_2,	[0]	61·2				
	[− 30]	95·2	$r = 61·2 + 119·83\,(\theta + ·00907)^2$	[− 83]	316·4	315·8
	[− 60]	195·0				

VI. Quad. Mass.

	N	R	H	C	D	T	A_1	A_2	e
	21·9	313·4	2438·4	5·2 ?	3·3 ?	·0027 ?	·2867	1·1263	·255
β,	22·4		126·1	3·2	·9	·0017	1·2364	3·3122	·374
			17·4	9·0	·8	·0047			
			3·2	9·5	·4	·0050			

β_1, [0]	187·0			o	c
[20]	203·8	$r = 187 + 138{\cdot}21\,(\theta)^2$	[54·6]	313·4	312·5
[40]	254·3				
β_2, [0]	187·0				
[− 20]	203·8	$r = 187 + 134{\cdot}27\,(\theta + {\cdot}00453)^2$	[− 55·2]	313·4	312·8
[− 40]	253·3				

6/7/91. Steel.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·3	275·0	1219·2	1·1	1·6	·0007	·3502	·7292	·343
		252·5	1·5	1·0	·0009	·7590	1·6709	·454
		50·4	2·0	·5	·0012			
		14·5	2·7	·3	·0016			

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·0	278·6	1219·2	·9	1·5	·0005	·3939	·8012	·492
		253·9	1·6	·9	·0009	·8079	1·7113	·472
		51·6	1·9	·3	·0011			
		15·0	2·2	·1	·0012			

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·75	284·3	1219·2	1·7	2·0	·0010	·3620	·7028	·515
		286·6	1·9	·9	·0011	·7346	1·4229	·516
		68·3	2·4	·6	·0014			
		19·2	3·3	·5	·0019			

IV. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·8	292·5	2438·4	0·9	1·2	·0005	·2679	·5716	·469
		385·0		1·1		·6745	1·6088	·419
		57·7	2·2	·4	·0012			
		11·3	3·5	·2	·0019			

V. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·55	297·5	2438·4	1·8	1·6	·0010	·2726	·7490	·364
		309·0	1·5	·8	·0008	·7813	1·6755	·466
		62·5	2·5	·4	·0014			
		10·2	4·6	·3	·0025			

VI. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·25	303·5	1219·2	3·1	2·1	·0017	·3799	1·1145	·341
		118·7						
		4·9						

VII. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·55	324·7	2438·4						
		83·5						
		6·7						

8/7/91. GLASS.

I. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·5	279·7	68·0	2·0	·5	·0012	1·4882	5·0504	·294
		6·1						

II. SINGLE MASS.

R	H
282·8	609·6
	73·8
	11·6

III. SINGLE MASS.

H
1219·2
124·6
19·1

7/11/91. VULCANISED INDIA-RUBBER. (SINGLE MASS.)

I. FLAT BASE.

N	R	H	C	D	T	A_1	A_2	e
21·4	277	1066	22	17·3	·0127	·3899	·4791	·814
		624·2	22·8	14·3	·0132	·4986	·6168	·808
		371·2	24	11·6	·0138	·6581	·8040	·818
		221·3	25	9·1	·0144			
		130·5	25·9	7·2	·0149			
		77·5	27·2	5·8	·0157			
		45·5	28·1	4·4	·0162			
		26·1	29·0	3·5	·0167			
		15·0	30·0	2·9	·0173			
		8·0	31·2	2·1	·0180			

II. FLAT BASE.

N	R	H	C	D	T	A_1	A_2	e
21·65	283	1066	22·2	17·7	·0127	·3939	·4942	·797
		...	23·9	14·7	·0136	·4986	·6358	·784
		384·6	24·7	12·0	·0141	·6519	·8391	·777
		230·0	26·5	9·4	·0151			
		136·3	27·1	7·3	·0155			
		81·5	28·2	6·0	·0161			
		48·0	29·0	4·9	·0166			
		27·7	30·7	4·0	·0175			
		16·0	32·0	3·0	·0183			
		9·0	32·5	2·1	·0186			

III. RIDGED BASE.

N	R	H	C	D	T	A_1	A_2	e
21·8	283	1066	34·9	26·2	·0201	·4122	·5195	·793
		559·6	37·9	21·2	·0218	·5430	·6681	·813
		338·5	39·0	17·3	·0224	·6916	·8481	·815
		207·8	40·8	14·3	·0235			
		128·0	41·7	11·2	·0240			
		78·8	41·7	8·9	·0240			
		48·0	43·0	7·0	·0247			
		29·0	44·0	5·6	·0253			
		17·0	44·3	4·1	·0255			

IV. Ridged Base.

N	R	H	C	D	T	A_1	A_2	e
21·6	296·5	1066	35·5	27·0	·0193	·4142	·5250	·789
		604·5	39·9	22·2	·0217	·5430	·6873	·790
		363·0	41·5	18·0	·0226	·7062	·8770	·805
		221·3	43·8	14·5	·0238			
		135·5	44·9	11·9	·0244			
		81·9	46·7	9·1	·0254			
		50·3	46·7	7·3	·0254			
		30·1	47·2	5·7	·0257			
		17·2	48·6	4·6	·0264			

13/7/92. Vulcanised India-Rubber. (Single Mass.)

N	R	H	C	D	T	A_1		A_2		e
						o	*c*	*o*	*c*	
19·783	333·3	1000	27·8	16·5	·0123	·5206	·5098	·6556	·6488	·794
		592·8	29·1	13·3	·0129	·6728	·6617	·8069	·8138	·834
β, 20·05		354·6	30·3	10·9	·0134	·8511	·8356	1·0680	1·0540	·797
γ, 20·54		213·2	31·6	8·9	·0140	1·0756	1·0776	1·3151		·818
δ, 20·53		129·1	32·8	7·2	·0145	1·4097		1·7205		·819
ϵ, 20·78		74·9	34·1	5·6	·0151	1·8040		2·3314		·774
		42·6	35·9	4·5	·0159	2·4142		3·0656		·788
		23·3	37·7	3·5	·0167	3·2540		4·3373		·750
		12·6	39·3	2·5	·0174	4·3433		6·1066		·711
		6·1	40·3	1·7	·0178					
		2·8	42·1	1·3	·0186					
		1·3	42·9	0·8	·0190					

					o	*c*
β_1,	[0]	− 259·7		[120]	+ 242·0	236·7
	[30]	− 228·0	$r = -259{\cdot}7 + 112{\cdot}33\,(\theta + {\cdot}0077)^2$	[130·57]	+ 333·3	327·6
	[60]	− 134·7				
β_2,	[0]	− 259·7		[− 120]	+ 207·0	208·8
	[− 30]	− 230·5	$r = -259{\cdot}7 + 106{\cdot}88\,(\theta - {\cdot}0009)^2$	[− 135·05]	333·3	333·7
	[− 60]	− 142·7				
γ_1,	[0]	− 21·9		[70]	161·0	160·8
	[60]	+ 113·0	$r = -21{\cdot}9 + 118{\cdot}34\,(\theta + {\cdot}0208)^2$	[97·95]	333·3	332·4
	[90]	277·8				
γ_2,	[0]	− 21·9		[− 70]	137·6	137·1
	[− 60]	+ 94·0	$r = -21{\cdot}9 + 111{\cdot}81\,(\theta - {\cdot}0288)^2$	[− 103·85]	333·3	333·7
	[− 90]	243·9				
δ_1,	[0]	120·5				
	[30]	153·0	$r = 120{\cdot}5 + 117{\cdot}62\,(\theta + {\cdot}0021)^2$	[76·9]	333·3	333
	[60]	250·0				
δ_2,	[0]	120·5		[− 45]	191·5	191·6
	[− 30]	152·5	$r = 120{\cdot}5 + 112{\cdot}33\,(\theta + {\cdot}0101)^2$	[− 78·3]	333·3	333·4
	[− 60]	246·1				
ϵ_1,	[0]	203·9				
	[20]	219·2	$r = 203{\cdot}9 + 120{\cdot}64\,(\theta + {\cdot}0072)^2$	[58·9]	333·3	333·2
	[40]	263·9				
ϵ_2,	[0]	203·9		[− 50]	291·6	290·7
	[− 20]	217·8	$r = 203{\cdot}9 + 114{\cdot}89\,(\theta - {\cdot}0037)^2$	[− 60·5]	333·3	331·1
	[− 40]	259·7				

[This was a single experiment, specially designed for the Nürnberg Exhibition.]

26/5/91. Unhammered Golf Ball. (Wood Block Unshod.)

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·7	240·7	1219·2	6·6	6·0	·00465	·3272	·5902	·555
		306·0	8·6	4·0	·00605	·6371	·9325	·683
		108·9	9·9	2·7	·00697	1·0110	1·5608	·648
		43·0	10·1	1·9	·00711			
		18·3	10·1	1·4	·00711			
		8·1	10·5	·9	·00739			
		3·8	11·6	·8	·00817			
		1·7						

II.

N	R	H	C	D	T	A_1	A_2	e
21·9	257·3	1219·2	7·3	6·3	·00464	·3504	·6050	·579
		337·4	9·5	4·3	·00603	·6140	·9896	·621
		118·6	10·8	2·8	·00686	1·1039	1·6022	·689
		47·3	11·1	1·9	·00705			
		20·5	11·2	1·3	·00711			
		9·5	11·4	·9	·00724			
		4·6	11·8	·6	·00750			
		2·3	11·8	·4	·00750			

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·9	273·5	1219·2	10·9	7·6	·00651	·3689	·7360	·501
		272·0	14·7	5·5	·00878	·7536	1·2916	·583
		98·8	17·0	4·0	·01016	1·2753	1·9774	·645
		40·1	17·7	2·6	·01058			
		17·9	17·9	1·8	·01070			
		8·4	17·9	1·2	·01070			
		4·0	18·6	·9	·01111			
		2·0	18·7	·7	·01117			

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·9	288·7	1219·2	15·6	10·1	·00883	·3819	·8430	·453
		233·0	22·6	7·7	·01279	·8391	1·4154	·593
		82·1	26·5	5·3	·01500	1·4200	2·2198	·640
		32·6	28·1	3·5	·01591			
		14·3	28·5	2·4	·01613			
		6·4	28·5	1·6	·01613			
		2·8	29·5	1·1	·01670			

28/5/91. Hammered Golf Ball. (Block Unshod.)

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·8	245·3	1219·2						
		378·0	7·8	4·1	·00517	·5902	·9163	·644
		144·3	9·0	2·7	·00597	·9358	1·3814	·677
		61·5	9·3	2·0	·00617			
		27·5	10·5	1·5	·00696			
		12·4	11·9	1·1	·00789			
		6·2	12·4	·9	·00822			
		2·9						

II.

N	R	H	C	D	T	A_1	A_2	e
21·6	254·3	1219·2	6·3	5·7	·00499	·3581	·5384	·665
		386·3	8·2	4·0	·00520	·6285	·9490	·662
		149·2	9·0	2·9	·00571	·9725	1·4388	·676
		63·4	10·1	2·2	·00640			
		29·9	10·2	1·5	·00647			
		14·2	11·0	1·2	·00697			
		6·7	11·7	·8	·00742			
		3·0						

III. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·6	272·5	1219·2	9·2	6·7	·00544	·3679	·6745	·545
		321·3	12·3	5·6	·00728	·6835	1·0486	·652
		132·6	13·3	3·9	·00787	1·0432	1·6085	·648
		59·8	14·7	2·7	·00870			
		28·0	15·6	2·1	·00923			
		13·7	16·3	1·6	·00964			
		6·9	17·5	1·2	·01035			
		3·5	18·2	1·0	·01077			
		1·6						

IV. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
22·6	288·1	1219·2	13·4	9·4	·00784	·3640	·7646	·476
		279·9	17·6	6·9	·01030	·7308	1·2505	·584
		108·1	20·1	4·6	·01177	1·1771	1·9596	·601
		44·0	22·0	3·2	·01288			
		18·9	23·8	2·6	·01393			
		9·0	23·0	1·6	·01346			
		4·3						
		2·0						

1/3/92. Hammered Golf Ball. (All Single Mass.)

I. (Steel Plate.)

N	R	H	C	D	T	A_1	A_2	e
21·75	263	1219·2	5·9	5·0	·00364	·3410	·5820	·586
		297·2	7·8	3·5	·0048	·6330	1·0176	·622
		105·0	9·0	2·5	·0056	1·0913	1·6865	·647
		39·2	9·5	1·6	·00586			
		15·8	10·9	1·2	·00617			
		6·9	11·1	0·9	·00685			
		2·7	11·4	·7	·00703			

II. (Steel Plate.)

N	R	H	C	D	T	A_1	A_2	e
23·2	273·0	1219·2	6·9	5·9 ?	·00438	·3551	·6009	·591
		354·2	8·6	3·7	·00545	·6627	1·0247	·647
		123·1	9·3	2·5	·00590	1·1132	1·6842	·661
		45·9	9·6	1·7	·00609			
		19·0	10·6	1·2	·00672			
		8·4	11·1	·9	·00704			
		3·7						

III. (WOOD.)

N	R	H	C	D	T	A_1	A_2	e
21·0	279·5	1219·2	5·8	4·3	·00325	·3462	·5774	·600
		383·0	7·7	3·5	·00432	·6208	·9691	·641
		131·9	8·3	2·1	·00465	1·0283	1·5911	·646
		49·4	8·9	1·6	·00499			
		20·1	9·5	1·1	·00533			
		8·6	9·8	·8	·00549			
		4·0						

IV. (WOOD.)

N	R	H	C	D	T	A_1	A_2	e
22·3	293·6	1219·2	6·4	4·7 ?	·00363	·4040	·7028	·575
		384·7	8·2	3·6 ?	·00465	·6644	1·0538	·630
		134·0	9·5	2·4	·00539	1·1028	1·6643	·663
		50·9	10·1	1·8	·00573			
		21·0	10·9	1·1	·00618			
		9·0	10·9	·9	·00618			
		4·1	10·9	·5	·00618			

V. (WOOD.)

N	R	H	C	D	T	A_1	A_2	e
22·6	306·5	1219·2	6·1 ?	4·2	·00336	·3939	·6656	·593
		390·7	8·4 ?	3·0 ?	·00462	·6758	1·2685	·533
		106·0	10·0	2·2	·00550	1·2647	1·9500	·649
		41·0	10·7	1·7	·00589			
		16·9	11·6	1·2	·00638			
		7·4	12·5	·9	·00688			
		3·2	14·4	·75	·00792			

VI. (STEEL PLATE.)

N	R	H	C	D	T	A_1	A_2	e
21·35	310·8	1219·2	8·0	5·5	·0041	·4204	·6681	·629
		381·8	10·8	4·0 ?	·00554	·7178	1·2572	·571
		102·7	12·3	2·5	·00631	1·3968	2·1742	·642
		37·0	12·7	1·7	·00652			
		15·6	13·7	1·1	·00703			
		6·6	14·8	·9	·00759			

16/3/92. UNHAMMERED GOLF BALL. (ALL SINGLE MASS.)
(STEEL PLATE.)

I.

N	R	H	C	D	T	A_1	A_2	e
22·45	275·4	1219·2	5·5	4·7	·00335	·3581	·6009	·596
		373·9	6·8	3·0	·00414	·6334	1·0000	·633
		128·0	8·6	2·1	·00523	1·0724	1·7217	·623
		45·5	8·6	1·3	·00523			
		17·4	9·0	0·9	·00548			
		6·8	10·0	·7	·00608			

II.

N	R	H	C	D	T	A_1	A_2	e
21·15	283·6	1219·2	5·7	4·8	·00317	·3819	·5914	·645
		420·2	7·3	3·5	·00406	·6249	1·0053	·622
		144·3	8·7	2·3	·00484	1·0488	1·7532	·598
		52·0	9·9	1·5	·00551			
		19·4	10·3	1·1	·00573			
		7·5	10·6	·8	·00590			

III.

N	R	H	C	D	T	A_1	A_2	e
22·2	291·3	1219·2	6·0	4·9 ?	·00341	·3726	·5716	·652
		433·4	7·8	3·5	·00444	·6192	1·0247	·604
		148·5	8·6	2·5	·00489	1·0428	1·6764	·622
		53·9	9·6	1·8	·00546			
		20·2	12·2	1·4	·00694			
		7·7	13·8	1·0	·00785			

IV.

N	R	H	C	D	T	A_1	A_2	e
20·8	299·0	1219·2	6·6	4·7	·00343	·3959	·6108	·648
		438·8	8·0	3·6	·00415	·6594	1·0649	·619
		153·8	9·5	2 6	·00493	1·0637	1·7603	·604
		55·2	10·3	1·7	·00535			
		20·7	11·6	1·4	·00602			
		8·5	12·9	·9	·00670			

V. (Wood.)

N	R	H	C	D	T	A_1	A_2	e
21·55	305·6	1219·2	6·6	5·0	·00347	·3919	·6469	·606
		426·2	8·2	3·3	·00431	·6586	1·0963	·601
		144·7	9·9	2·5	·00521	1·1370	1·7893	·635
		52·3	10·8	1·7	·00568			
		19·9	12·5	1·3	·00658			
		8·0	13·3	·9	·00700			

VI.

N	R	H	C	D	T	A_1	A_2	e
21·25	315·2	1219·2	6·6	4·4 ?	·00332	·4227	·6494	·651
		438·3	8·4	3·5	·00423	·6937	1·1048	·628
		153·0	9·6	2·5	·00483	1·1599	1·8572	·625
		56·1	10·4	1·6	·00523			
		21·5	10·8	1·1	·00543			
		8·7	12·0	0·7	·00604			

VII.

N	R	H	C	D	T	A_1	A_2	e
21·6	324·8	1219·2	6·6 ?	4·3 ?	·00328	·4327	·7220	·599
		447·3	8·8	3·8	·00437	·6958	1·1048	·630
		158·6	10·2	2·5	·00506	1·1566	1·8807	·615
		58·3	11·1	1·7	·00551			
		22·5	12·8	1·4	·00635			
		9·3	13·6	1·0	·00675			

VIII.

N	R	H	C	D	T	A_1	A_2	e
21·35	331·2	1219·2	6·7 ?	4·0 ?	·00322	·4418	·7063	·626
		445·2	8·9	3·6	·00428	·7225	1·0933	·661
		158·0	10·0	2·6	·00481	1·1785	1·8367	·642
		59·5	11·0	1·7	·00529			

5/4/92. Hammered Golf Ball on Hammered Golf Ball.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·37	260·9	1219·2	8·3	7·0	·00507	·3620	·5774	·627
		400·3	10·0	5·0	·00611	·6092	·9072	·672
		150·4	10·9	3·3	·00666	·9896	1·4550	·681
		57·5	12·3	2·5	·00752			
		24·1	13·4	1·7	·00819			
		10·5	12·2	·9	·00746			
		4·8	12·8	·6	·00782			

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·2	268·6	1219·2	8·0	7·4	·00494	·3310	·5200	·636
		458·2	9·7	5·4	·00598	·5362	·8332	·644
		173·2	11·3	4·0	·00697	·8894	1·3151	·676
		68·5	12·1	2·9	·00747			
		28·8	13·6	2·0	·00839			
		12·8	14·3	1·4	·00882			
		5·6	15·4	1·0	·00950			
		2·3	17·4	·7	·01074			

III. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·1	277·0	1219·2	8·9	7·5	·00530	·3696	·5543	·667
		467·8	10·3	5·3	·00613	·5766	·8682	·664
		178·0	12·0	3·8	·00714	·9358	1·3900	·673
		71·3	13·1	2·8	·00780			
		30·7	14·1	2·0	·00840			
		13·7	14·6	1·4	·00869			
		6·1	12·8	·8	·00762			

IV. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
22·6	285·2	1219·2	11·8	9·2	·00698	·3682	·5695	·646
		373·9	14·1	6·9	·00834	·6273	·8926	·702
		143·3	17·3	5·1	·01023	1·0064	1·4804	·680
		59·8	18·2	3·4	·01076			
		25·9	19·7	2·4	·01165			
		11·5 ?	20·2	1·7	·01196			
		5·0	21·6	1·2	·01277			

V. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
23·12	297·1	1219·2	12·1	9·9 ?	·00703	·3705	·5670	·653
		408·2	16·0	7·3	·00929	·6350	·9025	·704
		161·3	18·0	5·2	·01045	·9896	1·4770	·670
		68·0	19·5	3·6	·01132			
		29·9	20·8	2·6	·01208			
		13·3	21·4	1·8	·01243			
		5·9	20·5	1·2	·01190			
		2·9	22·2	0·9	·01289			

VI. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·5	312·2	1219·2	19·5	12·5 ?	·01002	·4215	·7248	·582
		286·6	26·5	9·3	·01362	·8069	1·1840	·682
		115·3	29·0	6·6	·01490	1·2746	1·9170	·665
		47·7	31·4	4·6	·01614			
		20·0	31·5	3·0	·01619			
		9·0	35·6	2·2	·01829			
		3·7	35·6	1·5	·01829			

24/3/92. Unhammered Golf Ball on Unhammered Golf Ball.

I. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·35	262·3	1219·2	8·3	7·1	·00528	·3424	·5206	·658
		419·5	9·6	5·0	·00610	·5774	·8214	·703
		161·6	11·0	3·6	·00699	·8988	1·3556	·663
		64·1	11·4	2·4	·00725			
		26·6	13·2	1·9	·00839			
		12·2	13·6	1·4	·00865			
		5·5	15·1	1·1	·00960			

II. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
22·05	270·3	1219·2	8·1	7·0	·00493	·3488	·5392	·647
		461·9	9·8	5·1	·00597	·5693	·8243	·691
		176·5	11·2	3·6	·00682	·9099	1·3352	·681
		69·1	12·3	2·6	·00749			
		30·5	13·0	1·9	·00791			
		13·5	13·3	1·3	·00810			
		6·1	13·6	0·9	·00828			

III. Single Mass.

N	R	H	C	D	T	A_1	A_2	e
21·4	278·2	1219·2	8·7	7·1	·00499	·3819	·5658	·675
		473·2	10·6 ?	5·4 ?	·00608	·5758	·8391	·686
		184·6	11·8	3·9	·00677	·9244	1·3238	·698
		73·6	12·5	2·5	·00718			
		31·7	14·1	2·0	·00809			
		14·3	13·8	1·3	·00792			
		6·3	15·1 ?	·8 ?	·00867			

IV. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
21·12	286·5	1219·2	12·7	9·2	·00699	·3819	·6342	·602
		381·2	15·8	6·8	·00869	·6745	·9657	·698
		156·1	18·1	5·2	·00996	1·0538	1·5014	·702
		66·7	19·5	3·6	·01073			
			19·7	2·4	·01084			
		12·6	21·0	1·7	·01155			
		5·7	21·0	1·2	·01155			

V. Double Mass.

N	R	H	C	D	T	A_1	A_2	e
22·0	297·1	1219·2	13·0	9·8	·00718	·3809	·5957	·639
		423·9	16·4	7·5	·00906	·6494	·9083	·715
		169·5	18·9	5·4	·01044	1·0088	1·4578	·692
		71·0	20·1	3·7	·01111			
		31·4	20·9	2·5	·01155			
		13·5	22·2	1·7	·01227			
		5·9	23·3	1·3	·01288			
		2·7	24·0	1·0	·01326			

VI. Quad. Mass.

N	R	H	C	D	T	A_1	A_2	e
21·9	313·6	1219·2	19·0	12·0	·00990	·4156	·7400	·562
		321·0	25·6	9·6	·01334	·7983	1·1263	·709
		136·3	28·8	7·1	·01501	1·1988	1·7321	·692
		58·9	31·0	4·9	·01615			
		25·9	31·9	3·2	·01662			
		11·6	34·8	2·3	·01813			
		5·1	35·2	1·5	·01834			

2/6/92. ECLIPSE BALL—STEEL PLATE.

I. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·25	273·8	1219·2	9·5	7·3	·00576	·3541	·6346	·558
			11·2	4·6	·00679	·6669	1·1504	·580
		107·0	12·2	3·0	·00740			
		38·1	14·0	2·0	·00850			
		13·9	14·0	1·3	·00850			

II. SINGLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·4	281·7	1219·2	9·8	7·3	·00582	·3696	·6656	·555
		333·8	11·5	4·7	·00682	·7107	1·1648	·610
		106·0	12·8	3·0	·00760			
		39·3	14·0	2·0	·00831			
		14·6	14·6	1·3	·00866			

ϵ_1, [0]	267·7			o	c
[6]	269·0	$r = 267{\cdot}7 + {\cdot}04444\,(\theta - {\cdot}5625)^2$	[18·48]	282	282
[12]	273·5				
ϵ_2, [0]	267·7				
[− 6]	269·4	$r = 267{\cdot}7 + {\cdot}04028\,(\theta + {\cdot}517)^2$	[18·38]	282	282·1
[− 12]	274·0				

III. DOUBLE MASS.

N	R	H	C	D	T	A_1	A_2	e
22·2	290·8	1219·2	14·0	9·9	·00798	·3676	·7146	·514
		291·9	17·8	6·4	·01014	·7590	1·3143	·577
		87·4	19·9	4·0	·01134			
		29·6	20·6	2·3	·01174			
		10·5	20·4	1·4	·01162			

IV. DOUBLE MASS.

N	R	H	C	D	T	A_1	A_2	e
21·55	300	1219·2	15·1	10·2	·00809	·4061	·7391	·549
		297·0	19·3	6·4	·01035	·8142	1·3865	·587
		88·8	20·9	4·0	·01120			
		30·4	22·8	2·5	·01222			
		10·7	24·2	1·7	·01297			
		3·5	26·0	1·0	·01394			

V. QUAD. MASS.

N	R	H	C	D	T	A_1	A_2	e
22·22	314·5	1219·2	19·7	12·2	·01039	·4115	·8746	·471
		241·2	27·4	8·0	·01445	·9179	1·6577	·554
		66·6 ?	30·6	4·5	·01613			
		20·5	34·3	2·9	·01808			
		6·2	36·8	1·5	·01940			

VI. QUAD. MASS.

N	R	H	C	D	T	A_1	A_2	e
22·6	324·3	1219·2	20·1	12·5	·01045	·4149	·8889	·467
		245·0	27·0	7·8	·01404	·9163	1·8094	·506
		63·6	32·0	4·5	·01664			
		19·7	35·5	2·8	·01846			
		5·8	36·8 ?	1·3 ?	·01913			

APPROXIMATE COEFFICIENTS OF RESTITUTION.

Successive Values of $e(1-m)$, calculated by the First Formula in the Paper. (The suffix to the number of the experiment indicates the mass, and the height of the first fall is quoted.)

7/4/91. Old V. I. R. 1219.

I_1	II_1	III_4	IV_4
·64	·67	·54	·57
·74	·74	·74	·73
·76	·76	·78	·78−
·77	·78−	·80	·79
·78	·78	·82	·82
·78	·79	·82−	·82+
·78+	·79	·83	·82
·79	·79	·83	·81
·78	·80	·82	·83
·77			
·79			
·78−			

12/6/91. New N. I. R. 750.

I_1	II_1	III_2	IV_2
·82	·82	·84	·85−
·83	·82	·86	·86
·84	·85	·87	·87
·83	·82	·87	·87
·82	·82	·87	·87
·82	·82	·86	·87
·81	·82	·86	·87
·82	·81	·86	·87+
·81	·81	·86	·87+
·81	·81	·85	·86
·79	·79	·85	·86
·79	·80−	·87	·86
·77	·80+	·85	·85
·80	·85	·84	·87
·74	·79	·84	·85
·88	·83	·84	·85
·82		·84	·84
		·84	·83
		·84	·82
		·89	·84

22/6/91. New V. I. R. 914·4.

I_1	II_1	III_2	IV_4
·71	·71	·74−	·73
·74	·75	·78	·78
·77	·77	·79+	·79
·77	·77	·80	·80
·75+	·77	·80	·80
·76	·77	·79	·80+
·76	·76−	·79	·80
·76	·75	·79	·79
·77	·75−	·79	·78
·76+	·77	·79	·77
·74	·75+	·78	·79
		·77	·77

13/7/92. 1000.

I_1
·77
·77
·77
·77
·76
·75
·74
·74
·70
·68
·68

16/6/91. Cork. 1219.

I_1	II_1	III_2	IV_4
·46	·45−	·38	·33
·53	·53	·49	·44
·59−	·59	·56	·50
·60	·61	·59	
	·61		

24/6/91. Cork. 2286.

I_1	II_1	III_2	IV_4
·40	·41	·32	·31+
·49	·49	·45	·42
·58	·58	·53−	·45
·63	·61	·57+	
·63	·64	·55	

3/7/91. Vulcanite.

1219.		2438.	1219.	2438.
I_1	II_2	III_2	IV_4	V_4
·46	·63	·47	·49	·33
·53	·55	·56	·45	·44
·65	·56	·58	·50	·49
·71	·69	·67	·57	·58
·76	·71	·67+	·59	
·78				
·78				
·76				

29/6/91. Plane.

1219.			2438.		
I_1	II_1	III_2	IV_1	V_2	VI_4
·42	·43	·40	·41	·32	·23
·46	·49	·49	·47	·48	·37
·58	·56	·56−	·52	·51	·43
·69	·69	·60	·66	·56	
			·68		

6/7/91. Steel.

1219.		2438.			1219.	2438
I_1	II_1	III_2	IV_1	V_2	VI_4	VII_4
·45	·45	·49	·39	·36	·31	·19
·42	·45	·49	·39−	·45	·20	·29
·53	·53	·52	·44	·40		

7/11/91. V. I. R.				Golf Ball, Wood Base.							
1066.				26/5/91. Unhammered.				28/5/91. Hammered.			
Flat Base.		Ridged Base.		1219.				1219.			
I_1	II_1	III_1	IV_1	I_1	II_1	III_2	IV_4	I_1	II_1	III_2	IV_4
·76	—	·72	·74	·50	·52	·47	·44	·55	·56	·51	·48
·77	—	·78	·77	·59	·59	·60	·59	·63	·62	·64	·60−
·77	·77	·78	·78	·63	·63	·63	·63	·65	·65	·67	·64
·77	·77	·78	·78	·65	·66	·67	·66	·67	·69−	·68	·65
·76	·77	·78	·78−	·66	·68	·69−	·66	·67	·69	·70	·69
·74	·77	·78	·78	·68	·70−	·69	·66	·70	·69−	·71	·69
·75	·76	·78−	·78−	·66	·70	·70		·69	·67	·71	·68
·75	·76	·77	·75								
·73	·75										

XC.

QUATERNION NOTES.

[*Proceedings of the Royal Society of Edinburgh, June* 4, 1888.]

(*a*) PROF. CAYLEY'S paper*, which was read at last meeting, reminded me of an old investigation which I gave only in brief abstract in our *Proceedings* for 1870 [*Anté*, No. XVIII.]. There is, unfortunately, a misprint† in the chief formula of transformation. In fact, we have quite generally, as a matter of quaternion analysis,

$$D_\sigma\nabla\sigma - \nabla D_\sigma\sigma = D_\sigma\nabla\sigma - (\nabla_1 D_{\sigma_1}\sigma + D_\sigma\nabla\sigma)$$

$$= -\nabla_1 D_{\sigma_1}\sigma = \nabla_1 S\sigma_1\nabla \,.\, \sigma$$

$$= (\nabla\sigma)^2 - S \,.\, \nabla_1\sigma_1\nabla \,.\, \sigma - \nabla\sigma \,.\, S\nabla\sigma + S\nabla\nabla_1 \,.\, \sigma_1\sigma.$$

The hydrokinetic equation is

$$D_\sigma\sigma = \nabla\left(P - \frac{p}{r}\right),$$

so that

$$V.\nabla D_\sigma\sigma = 0\,;$$

or, by the above transformation,

$$V.D_\sigma\nabla\sigma = V(\nabla_1 S\sigma_1\nabla \,.\, \sigma),$$

which is the equation treated by Cayley.

It is worthy of note that the right-hand member may be written as

$$V(\nabla\sigma)^2 - S \,.\, \nabla_1\sigma_1\nabla \,.\, \sigma - V\nabla\sigma \,.\, S\nabla\sigma \quad [\text{or as } V\nabla\sigma \,.\, S\nabla\sigma - S \,.\, \nabla_1\sigma_1\nabla \,.\, \sigma]$$

because

$$S\nabla\nabla_1 \,.\, V\sigma_1\sigma = 0 \text{ identically.}$$

* [*Collected Papers*, Vol. XIII., No. 890. *Note on the Hydrodynamical Equations.*]

† [Also an omission, corrected in the Reprint. The expression itself occurred to me while I was making the translation of v. Helmholtz' paper on *Vortex Motion* which appeared in *Phil. Mag.*, II., 1867. The multiform transformations of the expression $\nabla_1\nabla\sigma_1\sigma$ furnish a very interesting and instructive exercise in Quaternions. 1899.]

If we now introduce the equation of continuity

$$S\nabla\sigma = 0,$$

we have (as in the abstract referred to)

$$D_\sigma \nabla\sigma = -S \,.\, \nabla_1 \sigma_1 \nabla \,.\, \sigma = \delta_{\nabla\sigma}\sigma,$$

with the further result

$$-\nabla^2\left(P - \frac{v}{r}\right) = (\nabla\sigma)^2 + S\nabla\nabla_1 \,.\, S\sigma\sigma_1.$$

(*b*) The second note contains additions, of which

$$\iiint V \,.\, \nabla V\sigma\tau d\varsigma = \iint (\tau S\sigma U\nu - \sigma S\tau U\nu)\, ds$$

may be given as a specimen, to the paper on Quaternion Integrals printed in abstract as No. XXII. above. [One of the chief special applications, for which these formulæ were devised, was the comparison of integrals taken over the same finite closed surface. Thus for instance, even in the simple particular case cited, we have some remarkable equalities on the right from the mere assumption that σ and τ satisfy (in any of the infinite variety of ways possible) the condition

$$\nabla V\sigma\tau = \text{scalar},$$

or $$V\sigma\tau = \nabla v.$$ 1899.]

XCI.

OBITUARY NOTICE OF BALFOUR STEWART.

[*Proceedings of the Royal Society of London*, 1889.]

Dr Balfour Stewart was born in Edinburgh on November 1st, 1828, and died in Ireland on December 18th, 1887, having just entered his sixtieth year. He was educated for a mercantile profession, and in fact spent some time in Leith, and afterwards in Australia, as a man of business. But the bent of his mind towards physical science was so strong that he resumed his studies in Edinburgh University, and soon became assistant to Professor J. D. Forbes, of whose class he had been a distinguished member. This association with one of the ablest experimenters of the day seems to have had much influence on his career; for Forbes's researches (other than his Glacier work) were mainly in the departments of Heat, Meteorology, and Terrestrial Magnetism, and it was to these subjects that Stewart devoted the greater part of his life. In the classes of Professor Kelland, Stewart had a brilliant career; and gave evidence that he might have become a mathematician, had he not confined himself almost exclusively to experimental science.

In 1858, while he was still with Forbes, Stewart completed the first set of his investigations on Radiant Heat, and arrived at a remarkable extension of Prévost's "Law of Exchanges." His paper (which was published in the *Transactions of the Royal Society of Edinburgh*) contained the greatest step which had been taken in the subject since the early days of Melloni and Forbes. The fact that radiation is not a mere surface phenomenon, but takes place like absorption throughout the interior of bodies, was seen to be an immediate consequence of the new mode in which Stewart viewed the subject. Stewart's reasoning is, throughout, of an extremely simple character, and is based entirely upon the assumption (taken as an experimentally ascertained fact) that in an enclosure, impervious to heat and containing no source of heat, not only will the contents acquire the same temperature, but the radiation at all points and in all directions will ultimately become the same, in character and in intensity alike. It follows that the radiation is, throughout, that of a black body at the temperature of the enclosure. From this, by the simplest reasoning, it follows that the radiating and absorbing powers of any substance must be exactly proportional to one another (equal, in fact, if measured in proper units), not merely for the radiation as a whole, but for every definitely specified constituent of it. In Stewart's paper (as in those of the majority of young authors) there was a great deal of redundant matter, intended to show that his new views were compatible with all that had been previously known, and in consequence his work has been somewhat lightly spoken of, even by some competent judges. These allow that he succeeded in showing that equality of radiation and absorption is consistent with all that was known; but they refuse to acknowledge that he had proved it to be necessarily true. To such we would recommend a perusal of Stewart's article in the *Philosophical Magazine* (Vol. XXXV., 1863, p. 354), where they will find his own views about the meaning of his own paper. The only well-founded objection which has been raised to Stewart's proof applies equally to all proofs which have since been given, viz., in none of them is provision made for the peculiar phenomena of fluorescence and phosphorescence.

The subject of radiation, and connected properties of the luminiferous medium, occupied Stewart's mind at intervals to the very end of his life, and led to a number of observations and experiments, most of which have been laid before the Royal Society. Such are the "Observations with a Rigid Spectroscope," and those on the "Heating of a Disk by rapid Rotation in Vacuo," in which the present writer took part. Other allied speculations are on the connection between "Solar Spots and Planetary Configurations," and on "Thermal Equilibrium in an Enclosure containing Matter in Visible Motion."

From 1859 to 1870 Stewart occupied, with distinguished success, the post of Director of the Kew Observatory. Thence he was transferred to Manchester as Professor of Physics in the Owens College, in which capacity he remained till his death. His main subject for many years was Terrestrial Magnetism; and on it he wrote an excellent article for the recent edition of the *Encyclopædia Britannica*. A very complete summary of his work on this subject has been given by Schuster in the *Manchester Memoirs* (4th Series, Vol. I., 1888). In the same article will be found a complete list of Stewart's papers.

Among the separate works published by Stewart, his *Treatise on Heat*, which has already reached its fifth edition, must be specially mentioned. It is an excellent introduction to the subject, though written much more from the experimental than from the theoretical point of view. In the discussion of radiation, however, which is given at considerable length, a great deal of theoretical matter of a highly original character is introduced.

Of another work, in which Stewart took a great part, *The Unseen Universe*, the writer cannot speak at length. It has passed through many editions, and has experienced every variety of reception—from hearty welcome and approval in some quarters to the extremes of fierce denunciation, or of lofty scorn, in others. Whatever its merits or demerits it has undoubtedly been successful in one of its main objects, viz., in showing how baseless is the common statement that "Science is incompatible with Religion." It calls attention to the simple fact, ignored by too many professed instructors of the public, that human science has its limits; and that there are realities with which it is altogether incompetent to deal.

Personally, Stewart was one of the most lovable of men, modest and unassuming, but full of the most weird and grotesque ideas. His conversation could not fail to set one a-thinking, and in that respect he was singularly like Clerk-Maxwell. In 1870 he met with a frightful railway accident, from the effects of which he never fully recovered. He passed in a few months from the vigorous activity of the prime of life to grey-headed old age. But his characteristic patience was unruffled and his intellect unimpaired.

He became a Fellow of the Royal Society in 1862, and in 1868 he received the Rumford Medal.

His life was an active and highly useful one; and his work, whether it took the form of original investigation, of accurate and laborious observation, or of practical teaching, was always heartily and conscientiously carried out. When a statement such as this can be truthfully made, it needs no amplification.

XCII.

THE RELATION AMONG FOUR VECTORS.

[*Proceedings of the Royal Society of Edinburgh, March* 4, 1889.]

A SYSTEM of five points is completely determined by the vectors joining one of them with the other four. If α, β, γ be three of these, the fourth is necessarily

$$\delta = x\alpha + y\beta + z\gamma.$$

Hence any property characteristic of a group of five points will remain when x, y, z are eliminated. But we have

$$\begin{aligned} S\alpha\delta &= xS\alpha\alpha + yS\alpha\beta + zS\alpha\gamma, \\ S\beta\delta &= xS\beta\alpha + yS\beta\beta + zS\beta\gamma, \\ S\gamma\delta &= xS\gamma\alpha + yS\gamma\beta + zS\gamma\gamma, \\ S\delta\delta &= xS\delta\alpha + yS\delta\beta + zS\delta\gamma. \end{aligned}$$

Hence, at once, a determinant of the 4th order.

If we note that each term, as $S\beta\gamma$ for instance, can be written either as

$$\tfrac{1}{2}(\beta^2 + \gamma^2 - \overline{\beta - \gamma}^2) \text{ or as } -T\beta T\gamma \cos\widehat{\beta\gamma},$$

we see that the determinant may be written either in Dr Muir's form or as

$$0 = \begin{vmatrix} 1 & \cos\widehat{\alpha\beta} & \cos\widehat{\alpha\gamma} & \cos\widehat{\alpha\delta} \\ \cos\widehat{\beta\alpha} & 1 & \cos\widehat{\beta\gamma} & \cos\widehat{\beta\delta} \\ \cos\widehat{\gamma\alpha} & \cos\widehat{\gamma\beta} & 1 & \cos\widehat{\gamma\delta} \\ \cos\widehat{\delta\alpha} & \cos\widehat{\delta\beta} & \cos\widehat{\delta\gamma} & 1 \end{vmatrix}$$

which is the relation among the sides and the diagonals of a spherical quadrilateral. The method above can, of course, be extended to any number of points. One additional point introduces *three* new scalars to be eliminated, and *six* new scalar equations for the purpose.

(*Addition*—Read March 18.)

If we operate, as above, with any other four vectors, we have

$$\begin{vmatrix} S\alpha_1\alpha & S\alpha_1\beta & S\alpha_1\gamma & S\alpha_1\delta \\ S\beta_1\alpha & S\beta_1\beta & S\beta_1\gamma & S\beta_1\delta \\ S\gamma_1\alpha & S\gamma_1\beta & S\gamma_1\gamma & S\gamma_1\delta \\ S\delta_1\alpha & S\delta_1\beta & S\delta_1\gamma & S\delta_1\delta \end{vmatrix} = 0,$$

and the tensors are again factors of rows or columns. Thus, if $ABCD$, $abcd$, be any two spherical quadrilaterals,

$$\begin{vmatrix} \cos Aa & \cos Ab & \cos Ac & \cos Ad \\ \cos Ba & \cos Bb & \cos Bc & \cos Bd \\ \cos Ca & \cos Cb & \cos Cc & \cos Cd \\ \cos Da & \cos Db & \cos Dc & \cos Dd \end{vmatrix} = 0.$$

This has many curious particular forms; one, of course, being the former result, when the two quadrilaterals coincide. Another is when the quadrilaterals are "polar." Let a be the pole of AB, b of BC, &c., then

$$\cos Ab \cos Bc \cos Cd \cos Da - \cos Ac \cos Bd \cos Ca \cos Db = 0.$$

And numerous other relations can be obtained, with equal ease, by the same simple process.

Cayley's form of the expression connecting the distances, two and two, among five points in space is an immediate consequence of the *identity*

$$\Sigma x(\alpha - \theta)^2 = \Sigma x\alpha^2 - 2S\theta\Sigma x\alpha + \theta^2\Sigma x,$$

where α_1, α_2, &c., are n given vectors, θ any vector whatever, and x_1, x_2, &c., n undetermined scalars.

For, *provided that n is greater than* 4, we may always assume

$$\Sigma x = 0, \quad \Sigma x\alpha = 0,$$

which are equivalent to four *homogeneous* linear relations among the x's.

Let, then, $n = 5$, and write the above identity separately for each α, put in place of θ. Thus we have

$$\Sigma x(\alpha - \alpha_1)^2 = \Sigma x\alpha^2,$$
$$\Sigma x(\alpha - \alpha_2)^2 = \Sigma x\alpha^2,$$
$$\ldots\ldots\ldots\ldots = \ldots\ldots$$
$$\Sigma x(\alpha - \alpha_5)^2 = \Sigma x\alpha^2.$$

Take, with these, $$\Sigma x = 0,$$

and we obtain six linear equations from which to eliminate the five values of x. The result is, at once, A, B, C, D, E being the points,

$$\left|\begin{array}{cccccc} AA^2 & BA^2 & CA^2 & DA^2 & EA^2 & 1 \\ AB^2 & BB^2 & CB^2 & DB^2 & EB^2 & 1 \\ \cdots & \cdots & \cdots & \cdots & \cdots & \\ \cdots & \cdots & \cdots & \cdots & \cdots & \\ AE^2 & BE^2 & CE^2 & DE^2 & EE^2 & 1 \\ 1 & 1 & 1 & 1 & 1 & 0 \end{array}\right| \Sigma x\alpha^2 = 0.$$

As $\Sigma x\alpha^2$ may have *any* value, this is Cayley's expression*. An interesting variation of it is supplied by taking $\Sigma(x\iota) = 0$, instead of $\Sigma(x) = 0$, as the sixth equation.

* [*Collected Papers*, No. 1. Dr Muir's expression, mentioned above, is given in *Proc. R. S. E.*, XVI., p. 86. 1899.]

XCIII.

ON THE RELATION AMONG THE LINE, SURFACE, AND VOLUME INTEGRALS.

[*Proceedings of the Royal Society of Edinburgh, April* 1, 1889.]

THE fundamental form of the Volume and Surface Integral is

$$\iiint \nabla u d\varsigma = \iint U\nu u ds.$$

Apply it to a space consisting of a very thin transverse slice of a cylinder. Let t be the thickness of the slice, A the area of one end, and α a unit-vector perpendicular to the plane of the end. The above equation gives at once

$$V(\alpha\nabla)\, u \,.\, tA = t \int V \,.\, \alpha U\nu u dl,$$

where dl is the length of an element of the bounding curve of the section, and the only values of $U\nu$ left are parallel to the plane of the section and normal to the bounding curve. If now we put ρ as the vector of a point in that curve, it is plain that

$$V \,.\, \alpha U\nu = Ud\rho, \quad dl = Td\rho,$$

and the expression becomes (after division by t)

$$V(\alpha\nabla)\, uA = \int u d\rho.$$

By juxtaposition of an infinite number of these infinitely small directed elements, α (now to be called $U\nu$) being the normal vector of the area A (now to be called ds), we have at once

$$\iint V(U\nu\nabla)\, u ds = \int u d\rho,$$

which is the fundamental form of the Surface and Line Integral.

In fact, as the first of these expressions can be derived at once from the ordinary equation of "continuity," so the second is merely the particular case corresponding to displacements confined to a given surface.

XCIV.

QUATERNION NOTE ON A GEOMETRICAL PROBLEM.

[*Proceedings of the Royal Society of Edinburgh, June* 4, 1889.]

THE problem referred to is that of inscribing in a sphere a closed n-sided polygon, whose sides shall pass respectively through n given points which are not on the surface. Hamilton evidently regarded his solution of this question as a very tough piece of mathematics (see his *Life*, Vol. III. pp. 88, 426). In preparing the third edition of my *Quaternions*, I was led to a mode of treating this question which enables us to dispense with the brilliant feats of analysis which seem to be required in Hamilton's method.

[A sketch of his very curious analysis is given in § 250 of that work. § 250* gives the full text of my own process. As I have since found it to be needlessly prolix, it is considerably pruned down and concentrated in the present reprint. 1899.]

The quaternions which Hamilton employed were such as change the radius to one corner of the polygon into that to the next by a *conical* rotation. In the present Note I employ the quaternions which *directly* turn one side of the polygon to lie along the next. The sides, severally, are expressed as ratios of two of these successive quaternions.

Let ρ_1, ρ_2, &c., ρ_n be (unit) vectors drawn from the centre of the sphere to the corners of the polygon; $\alpha_1, \alpha_2, \ldots \alpha_n$, the points through which the successive sides are to pass. Then (by Euclid) we have n equations of the form

$$(\rho_{m+1} - \alpha_m)(\rho_m - \alpha_m) = 1 + \alpha_m^2 = A_m, \text{ suppose.}$$

These equations ensure that if the tensor of any one of the ρ's be unit, those of all the others shall also be units. Thus we have merely to eliminate $\rho_2, \ldots, \rho_n$; and then remark that (for the closure of the polygon) we must have

$$\rho_{n+1} = \rho_1.$$

That this elimination is possible we see from the fact already mentioned, which shows that the unknowns are virtually mere unit-vectors; while each separate equation contains *coplanar* vectors only. In other words, when ρ_m and α_m are given, ρ_{m+1} is determinate without ambiguity.

The general equation above may obviously be written as

$$\{\rho_{m+1} - \alpha_{m+1} - (\alpha_m - \alpha_{m+1})\}(\rho_m - \alpha_m) = 1 + \alpha_m^2 = A_m;$$

or, if we introduce the quaternion

$$q_{m-1} = (\rho_m - \alpha_m)(\rho_{m-1} - \alpha_{m-1}) \dots (\rho_1 - \alpha_1),$$

as

$$q_m = A_m q_{m-2} + \beta_m q_{m-1}.$$

Here

$$\beta_m = \alpha_m - \alpha_{m+1}$$

is one of the vector sides of the polygon whose corners are the assigned points. And the statement above as to the nature of the quaternions employed is expressed as

$$q_{m-1} = (\rho_m - \alpha_m) q_{m-2}.$$

Since we have

$$q_0 = \rho_1 - \alpha_1, \quad q_1 = (\rho_2 - \alpha_2)(\rho_1 - \alpha_1) = A_1 + \beta_1 q_0, \quad q_2 = A_2 q_0 + \beta_2 q_1, \text{ \&c.}$$

it is clear that the values of q are all *linear* functions of ρ_1, of the form

$$q_m = r_m + s_m \rho_1;$$

where r_m and s_m are definite functions of $\alpha_1, \alpha_2, \dots \alpha_m$ only.

Again, from

$$\rho_m - \alpha_m = \frac{q_{m-1}}{q_{m-2}},$$

we have

$$\rho_m = \frac{\alpha_m q_{m-2} + q_{m-1}}{q_{m-2}} = \frac{p_{m-2}}{q_{m-2}}, \text{ suppose.}$$

This gives at once, by the definition of p,

$$q_{m-1} = p_{m-2} - \alpha_m q_{m-2} = (\rho_m - \alpha_m) q_{m-2};$$

and, as an immediate consequence,

$$\begin{aligned} p_{m-1} = \alpha_{m+1} q_{m-1} + q_m &= \alpha_m q_{m-1} + A_m q_{m-2} = \alpha_m q_{m-1} + (1 + \alpha_m^2) q_{m-2} \\ &= q_{m-2} + \alpha_m p_{m-2} = -(\rho_m - \alpha_m) p_{m-2}. \end{aligned}$$

We now see at once that

$$q_{m-1} = (\rho_m - \alpha_m)(\rho_{m-1} - \alpha_{m-1}) \dots (\rho_2 - \alpha_2) q_0 = C(\rho_1 - \alpha_1),$$

$$p_{m-1} = (-)^{m-1} (\rho_m - \alpha_m)(\rho_{m-1} - \alpha_{m-1}) \dots (\rho_2 - \alpha_2) p_0 = (-)^{m-1} C(1 + \alpha_1 \rho_1).$$

Thus, finally,

$$\rho_{n+1} = \rho_1 = \frac{C + D\rho_1}{D - C\rho_1} = \frac{C\rho_1 - D}{D\rho_1 + C}, \text{ if } n \text{ be even}, \quad = \frac{C - D\rho_1}{D + C\rho_1} = \frac{C\rho_1 + D}{D\rho_1 - C}, \text{ if } n \text{ be odd} \dots (a),$$

C and D being quaternions to be calculated (as above) from the values of α. The two cases require to be developed separately.

Take first, the odd polygon:—then $\rho_1 D + \rho_1 C \rho_1 = C - D\rho_1$,

or

$$\rho_1 (d + \delta) + \rho_1 (c + \gamma) \rho_1 = c + \gamma - (d + \delta) \rho_1,$$

if we exhibit the scalar and vector parts of the quaternions C and D. Cutting out the parts which cancel one another, and dividing by 2, this becomes

$$d\rho_1 + S\delta\rho_1 + \rho_1 S\gamma\rho_1 - c = 0,$$

which, as ρ_1 is finite, divides itself at once into the two equations

$$S\gamma\rho_1 + d = 0, \quad S\delta\rho_1 - c = 0.$$

These planes intersect in a line which, by its intersections (if real) with the sphere, gives two possible positions of the first corner of the polygon.

For the even polygon we have

$$\rho_1 D - \rho_1 C\rho_1 = C + D\rho_1, \quad \text{or} \quad V\rho_1\delta - \rho_1 S\gamma\rho_1 - \gamma = 0;$$

which may be written $\quad V.\rho_1(\delta - V\gamma\rho_1) = 0, \quad \text{or} \quad \delta - V\gamma\rho_1 = x\rho_1.$

This equation gives $\quad \rho_1 = (x+\gamma)^{-1}(\delta + S\gamma\delta/x),$

where x is to be found from $\quad x^2 - \gamma^2 = S^2\gamma\delta/x^2 - \delta^2.$

The two values of x^2 have opposite signs. Hence there are two real values of x, equal and with opposite signs, giving two real points on the sphere. Thus *this* case of the problem is always possible.

[We might have arrived at equations (a), which involve the complete solution of the problem, by the following direct and simple process:—

Let ρ_{m-1}, ρ_m be any two successive corners of the polygon, α_{m-1} the point through which the corresponding side is to pass; we have at once

$$(\rho_m - \alpha_{m-1})(\rho_{m-1} - \alpha_{m-1}) = 1 + \alpha^2_{m-1},$$

or

$$\rho_m = \frac{\alpha_{m-1}\rho_{m-1} + 1}{\rho_{m-1} - \alpha_{m-1}}.$$

This is *general*, so that

$$\rho_{m+1} = \frac{\alpha_m\rho_m + 1}{\rho_m - \alpha_m} = \frac{(\alpha_m\alpha_{m-1} + 1)\rho_{m-1} - (\alpha_{m-1} - \alpha_m)}{(\alpha_{m-1} - \alpha_m)\rho_{m-1} + (\alpha_m\alpha_{m-1} + 1)}.$$

Note that, in these quaternion fractions, the coefficients of the linear expressions in ρ_{m-1}, above and below, are the *same pairs* of quantities, in direct and inverted order, viz.

$$\begin{matrix} \alpha_{m-1}, & 1 \\ 1\ , & -\alpha_{m-1} \end{matrix} \quad \text{and} \quad \begin{matrix} \alpha_m\alpha_{m-1} + 1, & -(\alpha_{m-1} - \alpha_m) \\ \alpha_{m-1} - \alpha_m\ , & \alpha_m\alpha_{m-1} + 1 \end{matrix}, \ \text{\&c.}$$

Their ostensible signs are, obviously, either alike above and unlike below, or unlike above and alike below, *alternately*.

Hence, as $\quad \rho_2 = \dfrac{\alpha_1\rho_1 + 1}{\rho_1 - \alpha_1}$ (signs *alike* above),

we have $\quad \rho_{n+1} = \rho_1 = \dfrac{C\rho_1 \pm D}{D\rho_1 \mp C},$

where the upper signs belong to the case of n odd, and the lower to n even. 1899.]

XCV.

NOTE APPENDED TO CAPTAIN WEIR'S PAPER "ON A NEW AZIMUTH DIAGRAM."

[*Proceedings of the Royal Society of Edinburgh, July* 15, 1889.]

[As Sir W. Thomson was unable personally to communicate Capt. Weir's paper to the Society, he asked me to add to it a Note on the principle of the new method.]

Capt. Weir's singularly elegant construction not only puts in a new and attractive light one of the most awkward of the formulæ of Spherical Trigonometry, but it practically gives in a single-page diagram the whole contents of the two volumes of Burdwood's *Azimuth Tables.* Further, it supplies a very interesting graphical plane construction of a function of three independent variables.

In the usual notation for spherical triangles, if A be the zenith, C the pole, and B a heavenly body (whose declination is δ), C is the hour-angle (h), b the colatitude $\left(\frac{\pi}{2}-\lambda\right)$, and A the supplement of the azimuth. Hence, from the formula

$$\cot a \sin b = \cot A \sin C + \cos b \cos C,$$

we have at once

$$\tan(\text{azimuth}) = \frac{\sin h}{\sin\lambda\cos h - \tan\delta\cos\lambda}.$$

Capt. Weir, in his diagram, virtually puts

$$\left.\begin{aligned} x &= \sin h \sec\lambda \\ y &= \cos h \tan\lambda \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

so that

$$\tan(\text{azimuth}) = \frac{x}{y - \tan\delta},$$

x and y being found by the intersection of the confocal conics

$$\frac{x^2}{\sec^2\lambda}+\frac{y^2}{\tan^2\lambda}=1, \text{ the latitude ellipse,}$$

and

$$\frac{x^2}{\sin^2 h}-\frac{y^2}{\cos^2 h}=1, \text{ the hour-angle hyperbola.}$$

The Amplitude is the value of the azimuth at rising or setting, so that the corresponding hour-angle is to be found from

$$\cos h+\tan\lambda\tan\delta=0.$$

With this value of h, equations (1) become

$$\left.\begin{aligned} x&=\sec\lambda\sqrt{1-\tan^2\lambda\tan^2\delta}\\ y&=-\tan^2\lambda\tan\delta\end{aligned}\right\}\quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2).$$

Elimination of δ gives, of course, the latitude ellipse as before. But elimination of λ gives, instead of the confocal hyperbola, the curve

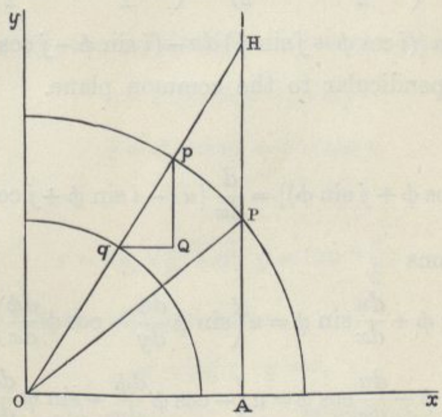
$$x^2+[y-\tfrac{1}{2}(\tan\delta-\cot\delta)]^2=\tfrac{1}{4}(\tan\delta+\cot\delta)^2,$$

or

$$x^2+(y+\cot 2\delta)^2=\operatorname{cosec}^2 2\delta,$$

which is a circle passing through the common foci of the ellipses and hyperbolas.

The construction of the "Diagram" by means of (1) is, theoretically, a very simple matter. Thus, take OA as unit length on the axis of x, and draw AP parallel to y. Make $AOP=\lambda$, and $yOH=h$. Draw the circles whose centre is O, and radii OP and AP respectively. Let OH meet them in p, q. From p and q draw lines parallel to Oy,



Ox, respectively. Their point of intersection, Q, belongs obviously to the ellipse λ, and to the hyperbola h. A somewhat similar, simple, construction can easily be given for the circle.

XCVI.

ON THE RELATIONS BETWEEN SYSTEMS OF CURVES WHICH, TOGETHER, CUT THEIR PLANE INTO SQUARES.

[*Proceedings of the Edinburgh Mathematical Society, November* 9, 1889. Vol. VII.]

If ρ be the vector of a corner of a square in one system, σ that in a system derived without inversion, we must obviously have

$$d\sigma = u\left(\cos\frac{\phi}{2} + k\sin\frac{\phi}{2}\right) d\rho \left(\cos\frac{\phi}{2} - k\sin\frac{\phi}{2}\right),$$

$$= u\{(i\cos\phi + j\sin\phi)\,dx - (i\sin\phi - j\cos\phi)\,dy\} \quad\text{.................(1)},$$

k being the unit-vector perpendicular to the common plane.

This requires that

$$\frac{d}{dy}\{u(i\cos\phi + j\sin\phi)\} = \frac{d}{dx}\{u(-i\sin\phi + j\cos\phi)\},$$

which gives the two equations

$$\frac{du}{dy}\cos\phi + \frac{du}{dx}\sin\phi = u\left(\sin\phi\frac{d\phi}{dy} - \cos\phi\frac{d\phi}{dx}\right),$$

$$\frac{du}{dy}\sin\phi - \frac{du}{dx}\cos\phi = u\left(-\cos\phi\frac{d\phi}{dy} - \sin\phi\frac{d\phi}{dx}\right),$$

or, in a simpler form,

$$\left.\begin{aligned} \frac{1}{u}\frac{du}{dx} &= \frac{d\phi}{dy} \\ \frac{1}{u}\frac{du}{dy} &= -\frac{d\phi}{dx} \end{aligned}\right\} \quad\text{.......................................(2).}$$

Eliminating ϕ and u separately, we have

$$\frac{d^2 \log u}{dx^2} + \frac{d^2 \log u}{dy^2} = 0,$$

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0.$$

Thus

$$\left.\begin{aligned} \log u = C_1 \\ \phi = C_2 \end{aligned}\right\} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

represent associated series of equipotential, and current, lines in two dimensions.

Assuming any lawful values for the members of (2) we obtain u and ϕ, and thence, by integration of (1), σ is given in terms of ρ.

Thus $$\sigma = i\xi + j\eta,$$

where ξ and η are known functions of x and y. From this x and y can be found in terms of ξ, η. Thus if

$$F_1(x, y) = A_1, \ F_2(x, y) = A_2 \quad\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

be a pair of sets of curves possessing the required property, we obtain at once another pair by substituting for x and y their values in terms of ξ, η. These may now be written as x, y, and the process again applied, and so on.

Thus, let the values of the pairs of equal quantities in (2) be 1, 0, respectively (which is obviously lawful), we have

$$u = \epsilon^x, \ \phi = y;$$

so that (1) becomes

$$d\sigma = \epsilon^x \{(i \cos y + j \sin y)\, dx - (i \sin y - j \cos y)\, dy\},$$

and $$\sigma = \epsilon^x (i \cos y + j \sin y)$$

or $$\xi = \epsilon^x \cos y, \ \eta = \epsilon^x \sin y.$$

From these we have

$$x = \log \sqrt{\xi^2 + \eta^2}, \ y = \tan^{-1}\frac{\eta}{\xi};$$

or, using polar coordinates for the derived series,

$$x = \log r, \ y = \theta.$$

[This is easily seen to be only a special case of (3) above.] Hence, by (4), another pair of systems satisfying the condition is

$$F_1(\log r, \theta) = A_1, \ F_2(\log r, \theta) = A_2.$$

This, of course, is only one of the simplest of an infinite number of solutions of the equation (1), which may be obtained with the greatest ease from (2).

If there is inversion, all that is necessary is to substitute ρ^{-1} for ρ, or $-\rho^{-1}d\rho\rho^{-1}$ for $d\rho$. But the necessity for this may be avoided by substituting for any pair of systems which satisfy the condition their electric image, which also satisfies it, and which introduces the required inversion.

The solution of this problem without the help of quaternions is interesting. Keeping as far as possible to the notation above, it will be seen that the conditions of the problem require that

$$\left(\frac{d\xi}{dx}dx + \frac{d\xi}{dy}dy\right)^2 + \left(\frac{d\eta}{dx}dx + \frac{d\eta}{dy}dy\right)^2 = u^2(dx^2 + dy^2)$$

whatever be the ratio $dx : dy$.

This gives at once

$$\left(\frac{d\xi}{dx}\right)^2 + \left(\frac{d\eta}{dx}\right)^2 = \left(\frac{d\xi}{dy}\right)^2 + \left(\frac{d\eta}{dy}\right)^2 = u^2,$$

$$\frac{d\xi}{dx}\frac{d\xi}{dy} + \frac{d\eta}{dx}\frac{d\eta}{dy} = 0.$$

From these the equations (2) can be deduced by introducing ϕ as an auxiliary angle.

XCVII.

ON THE IMPORTANCE OF QUATERNIONS IN PHYSICS*.

[*Philosophical Magazine, January*, 1890.]

My subject may usefully be treated under three heads, viz.:—

1. The importance of mathematics, in general, to the progress of physics.

2. The special characteristics required to qualify a calculus for physical applications.

3. How quaternions meet these requirements.

The question has often been asked, and frequently answered (one way or other) in the most decided manner:—Whether is experiment or mathematics the more important to the progress of physics? To any one who really knows the subject, such a question is simply absurd. You might almost as well ask:—Whether is oxygen or hydrogen the more necessary to the formation of water? Alone, either experiment or mathematics is comparatively helpless:—to their combined or alternate assaults everything penetrable must, some day, give up its secrets.

To take but one instance, stated as concisely as possible:—think of the succession of chief steps by which Electromagnetism has been developed. You had first the fundamental experiment of Oersted:—next, the splendid mathematical work of Ampère, which led to the building up of a magnet of any assigned description by properly coiling a conducting wire. But experiment was again required, to solve the converse problem:—and it was by one of Faraday's most brilliant discoveries that we learned how, starting with a magnet, to produce an electric current. Next came Joule and v. Helmholtz to show (the one by experiment, the other by analysis) the source of

* Abstract of an Address to the Physical Society of the University of Edinburgh, November 14, 1889. See the Author's Address to Section A at the British Association, 1871. [*Anté*, No. XXIII.]

the energy of the current thus produced:—in the now-a-days familiar language, why a powerful engine is required to drive a dynamo. Passing over a mass of important contributions mathematical and experimental, due to Poisson, Green, Gauss, Weber, Thomson, &c., which, treated from our present point of view, would furnish a narrative of extraordinary interest, we come to Faraday's *Lines of Force.* These were suggested to him by a long and patient series of experiments, but conceived and described by him in a form requiring only technical expression to become fully mathematical in the most exclusive sense of the word. This technical expression was given by Clerk-Maxwell in one of his early papers, which is still in the highest degree interesting, not only as the first step to his *Theory of the Electromagnetic Field,* but as giving by an exceedingly simple analogy the physical interpretation of his equations. Next, the narrative should go back to the establishment of the Wave-theory of Light:—to the mathematics of Young and Fresnel, and the experiments of Fizeau and Foucault. Maxwell's theory had assigned the speed of electromagnetic waves in terms of electrical quantities to be found by experiment. The close agreement of the speed, so calculated, with that of light rendered it certain that light is an electromagnetic phenomenon. But it was desirable to have special proof that there can be electromagnetic waves; and to measure the speed of propagation of such as we can produce. Here experiment was again required, and you all know how effectively it has just been carried out by Hertz. It is particularly to be noticed that the more important experimental steps were, almost invariably, suggested by theory—that is, by mathematical reasoning of some kind, whether technically expressed or not. Without such guidance experiment can never rise above a mere groping in the dark.

I have to deal, at present, solely with the mathematical aspect of physics; but I have led up to it by showing its inseparable connection with the experimental side, and the consequent necessity that every formula we employ should as openly as possible proclaim its physical meaning. In presence of this necessity we must be prepared to forego, if required, all lesser considerations, not excluding even such exceedingly desirable qualities as compactness and elegance. But if we can find a language which secures these to an unparalleled extent, and at the same time is transcendently expressive—bearing its full meaning on its face—it is surely foolish at least not to make habitual use of it. Such a language is that of Quaternions; and it is particularly noteworthy that it was invented by one of the most brilliant Analysts the world has yet seen, a man who had for years revelled in floods of symbols rivalling the most formidable combinations of Lagrange, Abel, or Jacobi. For him the most complex trains of formulæ, of the most artificial kind, had no secrets:—he was one of the very few who could afford to dispense with simplifications: yet, when he had tried quaternions, he threw over all other methods in their favour, devoting almost exclusively to their development the last twenty years of an exceedingly active life.

Everyone has heard the somewhat peculiar, and more than doubtful, assertion—*Summum jus, summa injuria.* We may, without any hesitation, make a parallel but more easily admitted statement:—*The highest art is the absence* (not, as Horace would have it, the *concealment*) *of artifice.* This commends itself to reason as well as to experience; but nowhere more forcibly than in the application of mathematics to physical science. The difficulties of physics are sufficiently great, in themselves, to tax the highest resources of human intellect; to mix them up with avoidable mathematical difficulties is unreason little short of crime. (To be obliged to evaluate a definite integral, or to solve a differential equation, is a necessity of an unpleasant kind, akin to the enforced extraction of a cube root; and here artifice is often requisite in our present state of ignorance: but its introduction for such purposes is laudable. It does for us the same kind of service which has been volunteered in the patient labour of the calculators of logarithmic tables. It is not of inevitable, but of gratuitous, complications that we are entitled to complain.) The intensely artificial system of Cartesian coordinates, splendidly useful as it was *in its day*, is one of the wholly avoidable encumbrances which now retard the progress of mathematical physics. Let any of you take up a treatise on the higher branches of hydrokinetics, or of stresses and strains, and then let him examine the twofold notation in Maxwell's *Electricity*. He will see at a glance how much expressiveness as well as simplicity is secured by an adoption of the mere notation, as distinguished from the processes, of quaternions. It is not difficult to explain the cause of this. But let us first take an analogy from ordinary life, which will be found to illustrate fairly enough some at least of the more obvious advantages of quaternions.

There are occasions (happily rare) on which a man is required to specify his name in full, his age, height, weight, place of birth, family history, character, &c. He may be an applicant for a post of some kind, or for a Life Policy, &c. But it would be absolutely intolerable even to mention him, if we had invariably to describe him by recapitulating all these particulars. They will be forthcoming when wanted; but we must have, for ordinary use, some simple, handy, and unambiguous method of denoting him. When we wish to deal with any of his physical or moral qualities, we can easily do so, because the short specification which we adopt in speaking of him is sufficient for his identification. It *includes* all his qualities. We all recognize and practise this in ordinary life; why should we outrage common-sense by doing something very different when we are dealing with scientific matters. especially in a science such as mathematics, which is purely an outcome of logic?

In quaternions, a calculus uniquely adapted to Euclidian space, this entire freedom from artifice and its inevitable complications is the chief feature. The position of a point (relative of course to some assumed origin) is denoted by a single symbol, which *fully* characterizes it, and depends upon length and direction alone, involving no reference whatever to special coordinates*. Thus we use ρ (say) in place of the Cartesian x, y, z, which are themselves dependent, for their numerical values, upon the particular scaffolding which we choose to erect as a (temporary) system of axes of reference. The distance between two points is

$$T(\rho - \rho'),$$

instead of the cumbrous Cartesian

$$\{(x-x')^2 + (y-y')^2 + (z-z')^2\}^{\frac{1}{2}}.$$

* Note here that though absolute *position* is an idea too absurd even for the majority of metaphysicians, absolute *direction* is a perfectly definite physical idea. It is one essential part of the first law of motion.

But the distance in question is fully symbolized as to direction as well as length by the simple form

$$\rho - \rho'.$$

If three conterminous edges of a parallelepiped be ρ, ρ', ρ'', its volume is

$$-S.\rho\rho'\rho''.$$

Even when advantage is taken of the remarkable condensation secured by the intensely artificial notation for determinants, Cartesian methods must content themselves with the much more cumbrous expression

$$\begin{vmatrix} x & y & z \\ x' & y' & z' \\ x'' & y'' & z'' \end{vmatrix}.$$

As we advance to higher matters, the Cartesian complexity tells more and more; while quaternions preserve their simplicity. Thus any central surface of the second degree is expressible by

$$S\rho\phi\rho = -1, \text{ or } T\phi^{\frac{1}{2}}\rho = 1;$$

while the Cartesian form develops into

$$Ax^2 + 2B''xy + A'y^2 + 2B'zx + 2Byz + A''z^2 = 1.$$

The homogeneous strain which changes ρ into ρ' is expressible by a single letter:—thus

$$\rho' = \psi\rho.$$

Its Cartesian form requires three equations,

$$x' = ax + by + cz,$$
$$y' = dx + ey + fz,$$
$$z' = gx + hy + iz.$$

These may be simplified, but only a little, by employing the notation for a matrix. To express in quaternions the conjugate strain, a mere dash is required: thus

$$\psi';$$

while with the artificial scaffolding we must write our three equations again, arranging the coefficients as below:—

$$\begin{matrix} a & d & g \\ b & e & h \\ c & f & i. \end{matrix}$$

If we now ask the question, What strain will convert the ellipsoid above into the unit sphere, the answer will be some time in coming from the ponderous Cartesian formulæ. The quaternion formula assigns it at once as $\phi^{\frac{1}{2}}$.

When Gauss gave his remarkable expression for the number of interlinkings of two endless curves in space, he had to print it as

$$\frac{1}{4\pi}\iint\frac{(x'-x)(dy\,dz'-dz\,dy')+(y'-y)(dz\,dx'-dx\,dz')+(z'-z)(dx\,dy'-dy\,dx')}{\{(x'-x)^2+(y'-y)^2+(z'-z)^2\}^{\frac{3}{2}}}.$$

What an immense gain in simplicity and intelligibility is secured when we are enabled to write this in the form

$$-\frac{1}{4\pi}\iint\frac{S.\overline{\rho-\rho_1}\,d\rho\,d\rho_1}{T\overline{\rho-\rho_1}^3},$$

or as

$$\frac{1}{4\pi}\int S.d\rho\int\frac{V\overline{\rho-\rho_1}\,d\rho_1}{T\overline{\rho-\rho_1}^3};$$

so that we instantly recognize in the latter factor the vector force exerted by unit current, circulating in one of the closed curves, upon a unit pole placed anywhere on the other; and thus see that the whole integral represents the work required to carry the pole once round its circuit.

Without as yet defining ∇, I shall take, as my final example, one in which it is involved. A very simple term, which occurs in connection with the strain produced by a given displacement of every point of a medium, is

$$S\alpha\nabla.S\beta\nabla_1.V\sigma\sigma_1.$$

Its Cartesian expression is, with the necessary specification,

$$\sigma=i\xi+j\eta+k\zeta,$$

made up of three similar terms of which it is sufficient to write one only, viz.,

$$(ab'-ba')\left(\frac{d\eta}{dx}\frac{d\zeta}{dy}-\frac{d\eta}{dy}\frac{d\zeta}{dx}\right)+(ca'-ac')\left(\frac{d\eta}{dz}\frac{d\zeta}{dx}-\frac{d\eta}{dx}\frac{d\zeta}{dz}\right)+(bc'-cb')\left(\frac{d\eta}{dy}\frac{d\zeta}{dz}-\frac{d\eta}{dz}\frac{d\zeta}{dy}\right).$$

Now, suppose this to be given as the x-coordinate of a point, similar expressions (formed by cyclical permutation) being written for the y and z coordinates. How long would it take you to interpret its meaning?

Look again at the quaternion form, and you see at a glance that it may be written

$$V(S\alpha\nabla.\sigma)(S\beta\nabla.\sigma),$$

in which its physical meaning is more obvious than any mere form of words could make it.

Or you may at once transform it to

$$-\tfrac{1}{2}S.(V\alpha\beta)\nabla\nabla_1.V\sigma\sigma_1,$$

which shows clearly why it vanishes when α and β are parallel.

I need not give more complex examples:—because, though their quaternion form may be simple enough (containing, say, 8 or 10 symbols altogether), even this unusually large blackboard would not suffice to exhibit more than a fraction of the equivalent Cartesian form.

Any mathematical method, which is to be applied to physical problems, must be capable of expressing not only space-relations but also the grand characteristics (so far as we yet know them) of the materials of the physical world. I have just briefly shown how exactly and uniquely quaternions are adapted to Euclidian space; we must next inquire how they meet the other requirements.

The grand characteristics of the physical world are:—Conservation of Matter, with absolute preservation of its identity; and Conservation of Energy, in spite of perpetual change of a character such as entirely to prevent the recognition of identity. The first of these is very simple, and needs no preliminary remarks. But the methods of symbolizing change are almost as numerous as are the various kinds of change. The more important of them employ forms of the letter D:—viz. d, ∂, D, Δ, δ, and ∇.

From our present point of view little need be said of Δ, which is the equivalent of $(D-1)$ or of $(\epsilon^{d/dx}-1)$, because the changes which it indicates take place by starts and not continuously. Good examples of problems in which it is required are furnished by the successive rebounds of a ball from a plane on which it falls, or by the motion of a light string, loaded at intervals with pellets.

Various modes of applying the symbol d are exemplified in the equation

$$dQ = \left(\frac{dQ}{dx}\right) dx + \left(\frac{dQ}{dy}\right) dy + \left(\frac{dQ}{dz}\right) dz.$$

In the terms dx, dy, dz, the symbol d stands for changes of value (usually small) of quantities treated as independent. In the term dQ it stands for the whole consequent change of a quantity which is a function of these independents. By the factors $\left(\frac{dQ}{dx}\right)$ &c. we represent the *rates* of increase of Q, per unit of length, in the directions in which x, y, z are respectively measured. The contrast between the native simplicity of the left-hand, and the elaborate artificiality of the right-hand, member of the equation, shows at once the need for improvement. To express the rate of change per unit of length in any other direction, we have to adopt the cumbrous expedient of introducing *three* direction-cosines, and the result is given in the form

$$l\frac{dQ}{dx} + m\frac{dQ}{dy} + n\frac{dQ}{dz}.$$

The above equation may be read as pointing out, *at any one instant*, how a function of position varies from point to point. To express the change, *at any one place*, from one instant to the next, we write in the usual notation

$$dQ = \left(\frac{dQ}{dt}\right) dt.$$

But if we have to express the changes, from instant to instant, of some property of a point, which is itself subject to an assigned change of position with time, we have to combine these expressions, and to indicate the relation of position to time. Thus we build up the complicated expression

$$dQ = \left(\frac{dQ}{dt}\right) dt + \left(\frac{dQ}{dx}\right) \frac{dx}{dt} dt + \left(\frac{dQ}{dy}\right) \frac{dy}{dt} dt + \left(\frac{dQ}{dz}\right) \frac{dz}{dt} dt.$$

Here the symbol ∂ is called in, to effect a slight simplification; and we go a little further in the same direction by putting u, v, w for

$$\frac{dx}{dt}, \quad \frac{dy}{dt}, \quad \frac{dz}{dt},$$

which are obviously the components of velocity of the point for which Q is expressed. Thus we write

$$\frac{\partial Q}{\partial t} = \left(\frac{dQ}{dt}\right) + u\left(\frac{dQ}{dx}\right) + v\left(\frac{dQ}{dy}\right) + w\left(\frac{dQ}{dz}\right).$$

Of course you all know this quite well; and you may ask why I thus enlarge upon it. It is to show you how completely artificial and unnatural are our recognized modes of expression.

Fresnel well said:—*La nature ne s'est pas embarrassée des difficultés d'analyse, elle n'a évité que la complication des moyens.* Why should we not attempt, at least, to imitate nature by seeking simplicity?

The notation δ, as commonly used, is (like the d in dQ above) quite unobjectionable. At least we cannot see how to simplify it further. Its effect is to substitute, for any one point of a figure or group, a proximate point in space, so that the figure or group of points undergoes slight, and generally continuous, but otherwise wholly arbitrary displacement and distortion. It thus appears that d and δ are entitled to take their places in a calculus, such as quaternions, where simplicity, naturalness, and direct intelligibility are the chief qualities sought. We have now to inquire how such expressions as

$$l\frac{dQ}{dx} + m\frac{dQ}{dy} + n\frac{dQ}{dz}$$

can be put in a form in which they will bear their meaning on their face.

It was for this purpose that Hamilton introduced his symbol ∇. No doubt, it was originally defined in the cumbrous and unnatural form

$$i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}.$$

But that was in the very infancy of the new calculus, before its inventor had succeeded in completely removing from its formulæ the fragments of their Cartesian shell, which were still persistently clinging about them. To be able to speak freely about this remarkable operator, we must have a name attached to it, and I shall speak of it as *Nabla**. We may define it in many ways, all independent of any system of coordinates. Thus we may give the definition

$$-S\alpha\nabla = d_\alpha,$$

meaning that, whatever unit-vector α may be, the resolved part of ∇ parallel to that line gives the rate of increase of a function, per unit of length, along it. From this we recover, at once, Hamilton's original definition:—thus

$$\nabla = -\alpha S\alpha\nabla - \beta S\beta\nabla - \gamma S\gamma\nabla = \alpha d_\alpha + \beta d_\beta + \gamma d_\gamma,$$

α, β, γ being *any* system of mutually rectangular unit-vectors.

But, preferably, we may define Nabla once for all by the equation

$$-Sd\rho\nabla = d,$$

where d has the meaning already assigned. The very nature of these forms shows at once that Nabla is an *Invariant*, and therefore that it ought not to be defined with reference to any system of coordinates whatever.

Either of the above definitions, however, shows at once that the effect of applying ∇ to any scalar function of position is to give its *vector-rate of most rapid change*, per unit of length.

Hence, when it is applied to a potential, it gives in direction and magnitude the force on unit mass; while from a velocity-potential it derives the vector velocity. From the temperature, or the electric potential, in a conducting body we get (employing the corresponding conductivity as a numerical factor) the vector flux of heat or of electricity. Finally, when applied to the left-hand member of the equation of a series of surfaces

$$u = C,$$

it gives the reciprocal of the shortest vector distance from any point of one of the surfaces to the next; what Hamilton called the vector of proximity.

If we form the square of Nabla directly from Hamilton's original definition, we find

$$\nabla^2 = -\left\{\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2\right\},$$

simply the negative of what has been called Laplace's Operator:—that which derives from a potential the corresponding distribution of matter, electricity, &c.

Thus Laplace's equation for spherical harmonics &c. is merely

$$\nabla^2 u = 0;$$

and, as $1/T(\rho - \alpha)$ is evidently a special integral, an indefinite series of others can be formed from it by operating with scalar functions of ∇, which are commutative

* Hamilton did not, so far as I know, suggest any name. Clerk-Maxwell was deterred by their vernacular signification, usually ludicrous, from employing such otherwise appropriate terms as *Sloper* or *Grader*; but adopted the word *Nabla*, suggested by Robertson Smith from the resemblance of ∇ to an ancient Assyrian harp of that name.

with ∇, such as $S\beta\nabla$, $\epsilon^{-S\gamma\nabla}$, &c. In passing, we may remark that if β be a unit vector, $il+jm+kn$, we have

$$-S\beta\nabla = l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}.$$

This is the answer to the question proposed a little ago.

The geometrical applications of Nabla do not belong to my subject, and they have been very fully given by Hamilton. But, for its applications to physical problems, certain fundamental theorems are required, of which I will take only three of the more important;—an analytical, a kinematical, and a physical one.

I. The analytical theorem is very simple, but it has most important bearings upon change of independent variables, and other allied questions in tridimensional space. Few of you, without the aid of quaternions or of immediately previous preparation, would promptly transform the independent variables in a partial differential equation from x, y, z to r, θ, ϕ:—and you would certainly require some time to recover the expressions in generalized (orthogonal) coordinates. But Nabla does it at once. Thus, let

$$\nabla_\sigma = i\frac{d}{d\xi} + j\frac{d}{d\eta} + k\frac{d}{d\zeta},$$

where
$$\sigma = i\xi + j\eta + k\zeta,$$

ξ, η, ζ being any assigned functions of x, y, z. Further, let

$$d\sigma = \phi d\rho,$$

where ϕ, in consequence of the above data, is a definite linear and vector function. Then, from the mere definition of Nabla,

$$Sd\sigma\nabla_\sigma = -d = Sd\rho\nabla,$$

which gives at once

$$S.\phi d\rho\nabla_\sigma = S.d\rho\phi'\nabla_\sigma = Sd\rho\nabla.$$

As $d\rho$ may have absolutely any direction, this is equivalent to

$$\phi'\nabla_\sigma = \nabla,$$

where ϕ' is the conjugate of ϕ.

II. The fundamental kinematical theorem is easily obtained from the consideration of the continuous displacement of the points of a fluid mass. (It is implied in the word "continuous" that there is neither rupture nor finite sliding.)

If σ be the displacement of the point originally at ρ, that of $\rho+d\rho$ is

$$\sigma + d\sigma = \sigma - Sd\rho\nabla.\sigma;$$

and thus the strain, in the immediate neighbourhood of the point ρ, is such as to convert $d\rho$ into

$$d\rho - Sd\rho\nabla.\sigma.$$

Thus the strain-function is $\quad \psi\tau = \tau - S\tau\nabla.\sigma.$

If this correspond to a linear dilatation e, and a vector-rotation ϵ, both being quantities whose squares are negligible, we must have

$$\psi\tau = (1 + e)\,\tau + V\epsilon\tau.$$

Comparing, we have
$$-S\tau\nabla \,.\, \sigma = e\tau + V\epsilon\tau,$$

from which at once (by taking the sum for any three directions at right angles to one another),

$$\nabla\sigma = -\Sigma(e) + 2\epsilon;$$

so that $S\nabla\sigma$ represents the compression,

and $\frac{1}{2}V\nabla\sigma$ „ „ vector-rotation,

of the element surrounding ρ.

By the help of these expressions we easily obtain the stress-function for a homogeneous isotropic solid, in terms of the displacement of each point, in the form

$$\phi\omega = -n\,(S\omega\nabla \,.\, \sigma + \nabla S\omega\sigma) - (c - \tfrac{2}{3}n)\,\omega S\nabla\sigma;$$

where n and c are, respectively, the rigidity and the resistance to compression; and $\phi\omega$ is the stress, per unit of surface, on a plane whose unit normal is ω.

III. The fundamental physical relation is that expressing conservation of matter, commonly called the equation of continuity. We have only to express symbolically that the increase of mass in a finite simply-connected space, due to a displacement, is the excess of what enters over what leaves the space. This gives at once

$$\iiint S\nabla\sigma d\varsigma = \iint SU\nu\sigma ds,$$

where $U\nu$ is unit normal drawn *outwards* from the bounding surface. If we put for σ the expression $u\nabla v$, where u and v are any two scalar functions of position, this becomes Green's Theorem.

If the space considered be imagined as bounded by two indefinitely close *parallel* surfaces, and by the normals at each point of a closed curve drawn on one of them, this is easily reduced * to the form of the line and surface integral

$$\iint S \,.\, U\nu\nabla\sigma ds = \int S\sigma d\rho.$$

The simplest forms of these equations are respectively

$$\iiint \nabla u d\varsigma = \iint U\nu u ds,$$

and
$$\iint V(U\nu\nabla)\, u ds = \int d\rho u,$$

where u is any scalar function of position. But it is clear from the mode in which it enters that u may be any quaternion. And it is easy to build on these an

* [*Anté*, No. XCIII. 1899.]

indefinite series of more complex relations. Thus, for instance, if σ and τ be any two vector-functions of ρ, we have

$$\iiint(\sigma\nabla^2\tau + K\nabla\sigma\,.\,\nabla\tau)\,d\varsigma = \iint\sigma U\nu\nabla\tau ds,$$

which has many important transformations. You will find it laborious, but alike impressive and instructive, to write this simple formula in Cartesian coordinates. It consists of four separate equations, containing among them 189 terms in all!

In the three relations just given we have the means of applying quaternions to various important branches of mathematical physics, where Nabla is indispensable. But I must confine myself to one example, so I will take very briefly the equations of fluid motion.

Let e be the density, and σ the vector-velocity, at the point ρ in a fluid. Consider the rate at which the density of a little portion of the fluid at ρ increases as it moves along. We have at once, for the equation of continuity,

$$\frac{\partial e}{\partial t} = eS\nabla\sigma;$$

which we may write, if we please, as

$$\frac{de}{dt} = S\nabla(e\sigma).$$

This is the result we should have obtained if we had considered the change of contents of a *fixed* unit volume in space. Next consider the rate at which the element gains momentum as it proceeds. We write at once, since momentum cannot originate or be destroyed by processes *inside* the element,

$$e\frac{\partial\sigma}{\partial t} = -e\nabla P + \iint\phi U\nu ds,$$

where P is the potential energy of unit mass at ρ, and $\phi U\nu$ is the stress-function due to pressure and viscosity. We have already had the form of this function; so that the equation transforms at once into

$$e\frac{\partial\sigma}{\partial t} = -e\nabla P - \nabla p - n(\nabla^2\sigma + \tfrac{1}{3}\nabla S\nabla\sigma);$$

which contains the three ordinarily given equations. Here n is the coefficient of viscosity, and the pressure p enters the equation in the form

$$cS\nabla\sigma.$$

To obtain v. Helmholtz's result as to vortex-motion, put $n = 0$, and we deduce for the rate of change of vector-rotation of an element, as it swims along,

$$\frac{\partial}{\partial t}V\nabla\sigma = V.\nabla V.\sigma V\nabla_1\sigma_1.$$

If the fluid be incompressible, this becomes

$$\frac{\partial}{\partial t} V\nabla\sigma = -S\,.\,\nabla_1\sigma_1\nabla\,.\,\sigma.$$

From either it is obvious that the rate of change of the vector-rotation vanishes where there is no rotation. But time forbids any further discussion of formulæ.

Hydrokinetics, as presented by Lagrange and Cauchy, was rather a triumph of mathematical skill than an inviting or instructive subject for the student. The higher parts of it were wrapped up in equations of great elegance, but of almost impenetrable meaning. They were first interpreted, within the memory of some of us, by Stokes and v. Helmholtz, after we know not what amount of intellectual toil. The magnificent artificers of the earlier part of the century were, in many cases, blinded by the exquisite products of their own art. To Fourier, and more especially to Poinsot, we are indebted for the practical teaching that a mathematical formula, however brief and elegant, is merely a step towards knowledge, and an all but useless one, until we can thoroughly read its meaning. It may in fact be said with truth that we are already in possession of mathematical methods, of the artificial kind, fully sufficient for all our present, and at least our immediately prospective, wants. What is required for physics is that we should be enabled at every step to feel intuitively what we are doing. Till we have banished artifice we are not entitled to hope for full success in such an undertaking. That Lagrange and Cauchy missed the import of their formulæ, leaving them to be interpreted some half-century later, is merely a case of retributive justice:—

"......neque enim lex aequior ulla
Quam necis artifices arte perire suâ."

Lagrange in the preface to that wonderful book, the *Mécanique Analytique*, says:—

"Les méthodes que j'y expose ne demandent ni constructions, ni raisonnemens géométriques ou mécaniques, mais seulement des opérations algébriques, assujéties à une marche régulière et uniforme."

But note how different is Poinsot's view:—

"Gardons-nous de croire qu'une science soit faite quand on l'a réduite à des formules analytiques. Rien ne nous dispense d'étudier les choses en elles-mêmes, et de nous bien rendre compte des idées qui font l'objet de nos spéculations."

No one can doubt that, in this matter, the opinion of the less famous man is the sound one. But Poinsot's remark must be confined to the analytical formulæ known to him. For it is certain that one of the chief values of quaternions is precisely this:—that no figure, nor even model, can be more expressive or intelligible than a quaternion equation.

XCVIII.

GLISSETTES OF AN ELLIPSE AND OF A HYPERBOLA.

[*Proceedings of the Royal Society of Edinburgh, December* 16, 1889.]

LAST summer, while engaged with some quaternion investigations connected with Dr Plarr's problem (the locus-boundary of the points of contact of an ellipsoid with three rectangular planes), I was led to construct the glissettes of an ellipse. I then showed to the Society a series of these curious curves, drawn in my laboratory by Mr Shand, who had constructed for the purpose a very true elliptic disc of sheet brass. I did not, at the time, think it necessary to print my paper; but, after the close of the session, I made the curious remark that precisely the same curves can be drawn each as a glissette of its own special hyperbola. This double mode of sliding generation of the same curve seems to possess interest. It is somewhat puzzling at first, since the ellipse turns completely round, while the hyperbola can only oscillate. But a little consideration shows the cause of the coincidence.

Let O be the origin, C any position of the centre of the ellipse, CA that of the major axis, and P the corresponding position of the tracing point. This does not require a figure.

Then it is easy to see that if ϕ be the inclination of OC to one of the guides, θ that of CA to the same, we have

$$\sqrt{a^2 \cos^2\theta + b^2 \sin^2\theta} = \sqrt{a^2 + b^2} \cos\phi.$$

But this gives $$\sqrt{a^2 \cos^2\phi - b^2 \sin^2\phi} = \sqrt{a^2 - b^2} \cos\theta,$$

which is the corresponding relation for the hyperbola. In fact the one equation is changed into the other by changing the sign of b^2, and interchanging the angles θ and ϕ.

Let the polar coordinates of the tracing point, referred to the centre of the ellipse and the major axis, be r, α, we obtain a position of P by the broken line OC, CP; their lengths being $\sqrt{a^2+b^2}$, r, and their inclinations to the guide ϕ, $\theta+\alpha$, respectively.

If we now turn the guides through an angle α, and use a hyperbola whose axes are to those of the ellipse respectively as $r : \sqrt{a^2-b^2}$; and consider the curve traced by a point Q in its plane, whose central polar coordinates are $\sqrt{a^2+b^2}$, $-\alpha$; the position of the point Q is given by the broken line OC', $C'Q$. Of these OC' is equal and parallel to CP, while $C'Q$ is equal and parallel to OC. Thus the points Q and P coincide.

In fact the motion of either is the resultant of two circular motions, one of which is complete (viz., θ, which has all values from 0 to 2π), the other reciprocating (viz., ϕ, which varies between $\sin^{-1}(b/\sqrt{a^2+b^2})$ and $\sin^{-1}(a/\sqrt{a^2+b^2})$). But, in the case of the ellipse, the centre has the reciprocating motion; while, in the hyperbola, it describes the complete circular path.

Mr Shand has constructed a hyperbolic disc, comprising a considerable portion of each of the branches of the curve, and it gives very fair glissettes. It is very curious to watch the proper point of the hyperbola gliding over the curve already traced by the ellipse. But this apparatus is not so easily managed as is the elliptic disc, so that the figures in Plate V were drawn by means of the latter, and reproduced on a diminished scale by photolithography.

To exhibit, by a few forms, as completely as possible the general nature of these glissettes, I selected a series of tracing points equidistant from the centre of the ellipse, and situated within and on the boundaries of the various regions, to each of which belongs a special form. For this purpose I traced the curve formed by successive positions of the instantaneous centre of rotation on the disc. The disc, with this curve on it, is represented in the upper central figure. The equation of the curve is

$$\frac{b^2x^2-a^2y^2}{b^2x^2+a^2y^2}=\frac{\sqrt{a^2+b^2}\,\sqrt{x^2+y^2}}{a^2-b^2}.$$

It is easily traced as follows. Draw the ellipse whose semiaxes parallel to x and y respectively are

$$\frac{a^2-b^2}{\sqrt{a^2+b^2}} \text{ and } \frac{a}{b}\,\frac{a^2-b^2}{\sqrt{a^2+b^2}};$$

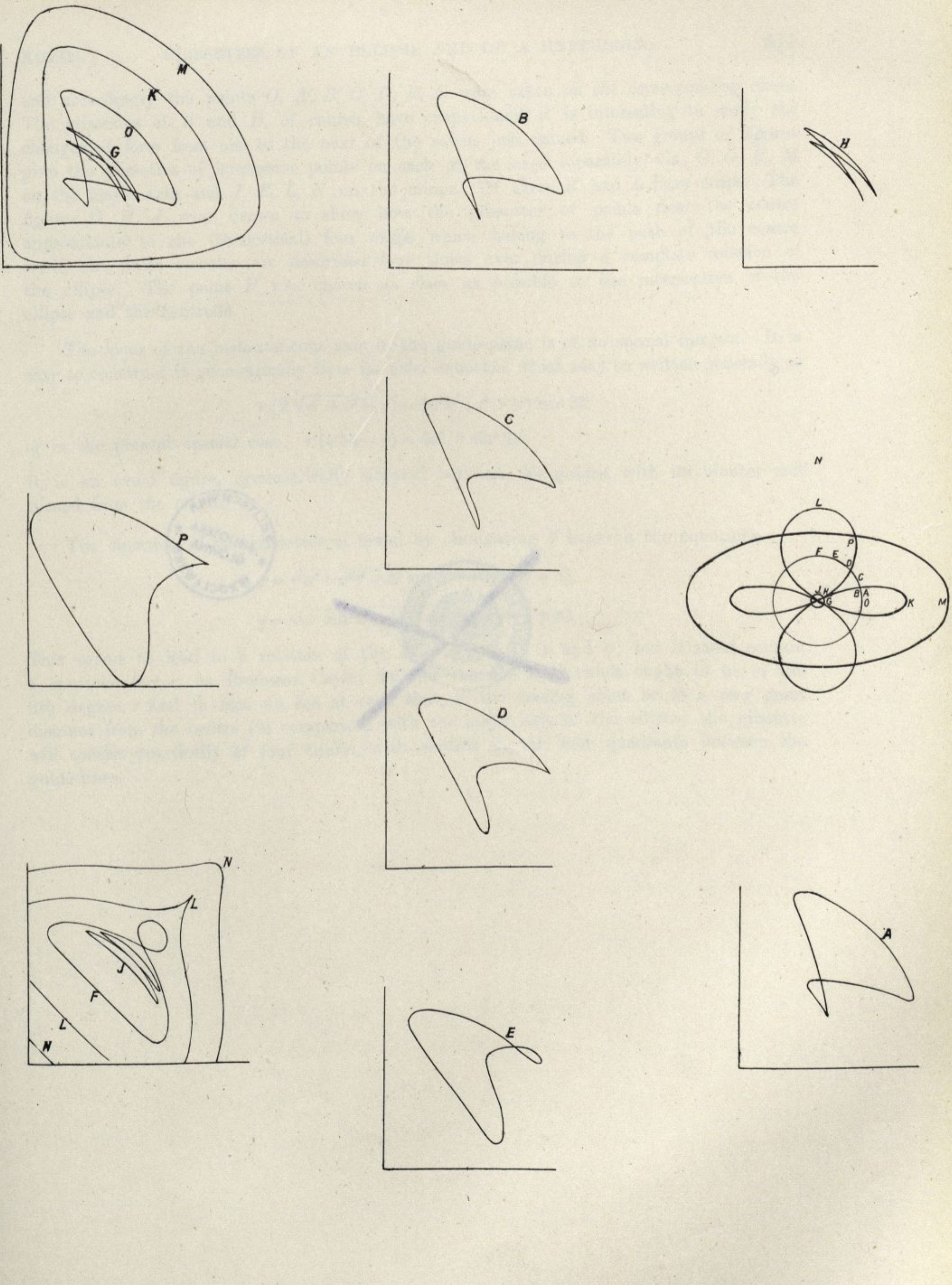
diminish every radius vector in proportion to the cosine of double the angle vector; and then diminish the ordinates in the ratio $b : a$, so that the ellipse itself becomes a circle.

In the disc from which the glissettes were drawn, a (rather more than a foot in length) was made double of b.

This equation suggested, as a useful distance of the tracing point from the centre, the quantity

$$\frac{a^2-b^2}{2\sqrt{a^2+b^2}};$$

Plate V



To face p. 310

and accordingly the points O, A, B, C, D, E, F were taken on the corresponding circle. The glissettes of B and D, of course, have cusps:—and it is interesting to study the changes of form from one to the next of the seven just named. Two groups of figures give the glissettes of successive points on each of the axes separately, viz., G, O, K, M on the major axis, and J, F, L, N on the minor. Of these K and L have cusps. The figures G, H, J were drawn to show how the glissettes of points near the centre approximate to the (theoretical) four cusps which belong to the path of the centre itself, the finite circular arc described four times over during a complete rotation of the ellipse. The point P was chosen as close as possible to the intersection of the ellipse and the centrode.

The locus of the instantaneous axis in the guide-plane is of no special interest. It is easy to construct it geometrically from its polar equation, which may be written generally as

$$r\,(2\sqrt{a^2+b^2}-r)=4a^2b^2/(a^2+b^2)\sin^2 2\theta,$$

or in the present special case $r\,(\sqrt{5}a-r)=4a^2/5\sin^2 2\theta.$

It is an ovoid figure, symmetrically situated between the guides, with its blunter end turned from the origin.

The equation of the glissettes is found by eliminating θ between the equations

$$x=\sqrt{a^2\cos^2\theta+b^2\sin^2\theta}+r\cos(\theta+\alpha),$$

$$y=\sqrt{a^2\sin^2\theta+b^2\cos^2\theta}+r\sin(\theta+\alpha).$$

This seems to lead to a relation of the 12th degree in x and y; but it must contain a spurious factor, as Professor Cayley informs me the final result ought to be of the 8th degree. And in fact we see at once that, if the tracing point be at a *very* great distance from the centre (in comparison with the major axis of the ellipse) the glissette will consist practically of four circles, with centres in the four quadrants between the guide-lines.

XCIX.

NOTE ON A CURIOUS OPERATIONAL THEOREM.

[*Proceedings of the Edinburgh Mathematical Society, January* 10, 1890.]

THE idea in the following note is evidently capable of very wide development, but it can be made clear by a very simple example.

Whatever be the vectors α, β, γ, δ, we have always

$$V\,.\,V\alpha\beta\,V\gamma\delta = \alpha S\,.\,\beta\gamma\delta - \beta S\,.\,\alpha\gamma\delta.$$

But vector operators are to be treated in all respects like vectors, provided each be always kept *before* its subject.

Let $$\sigma = i\xi + j\eta + k\zeta,$$

where ξ, η, ζ are functions of x, y, z; and let

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

as usual. Also let σ_1, ∇_1 be their values when x_1, y_1, z_1 are put for x, y, z.

Then by the first equation, attending to the rule for the place of an operator,

$$V\,.\,V\nabla\sigma V\nabla_1\sigma_1 = \nabla S\,.\,\sigma\nabla_1\sigma_1 - S(\nabla_1\sigma_1\nabla)\,\sigma.$$

If we suppose the operations to be completed, and *then* make $x_1 = x$, $y_1 = y$, $z_1 = z$, the left-hand member must obviously vanish. So therefore must the right.

That is:— $$\nabla S\,.\,\sigma\nabla_1\sigma_1 = S(\nabla_1\sigma_1\nabla)\,\sigma;$$

if when the operations are complete, we put $\sigma_1 = \sigma$, $\nabla_1 = \nabla$.

In Cartesian coordinates this is equivalent to three equations, of the same type. I write only one, viz.:—

$$\frac{d}{dx}\begin{vmatrix} \xi & \eta & \zeta \\ \frac{d}{dx_1} & \frac{d}{dy_1} & \frac{d}{dz_1} \\ \xi_1 & \eta_1 & \zeta_1 \end{vmatrix} = \begin{vmatrix} \frac{d}{dx_1} & \frac{d}{dy_1} & \frac{d}{dz_1} \\ \xi_1 & \eta_1 & \zeta_1 \\ \frac{d}{dx} & \frac{d}{dy} & \frac{d}{dz} \end{vmatrix}\xi,$$

if, *after* operating, we put $x_1 = x$, $\xi_1 = \xi$, &c., &c.

C.

NOTE ON RIPPLES IN A VISCOUS LIQUID.

[*Proceedings of the Royal Society of Edinburgh, March* 3, 1890.]

THE following investigation was made in consequence of certain peculiarities in the earlier results of some recent measurements of ripples by Prof. Michie Smith, in my Laboratory, which will, I hope, soon be communicated to the Society. These seemed to suggest that viscosity might have some influence on the results, as might also the film of oxide, &c., which soon gathers on a free surface of mercury. I therefore took account of the density, as well as of possible rigidity, of this surface layer, in addition to the surface tension which was the object of Prof. Smith's work. The later part of the paper, where Cartesian coordinates are employed, runs somewhat on the lines of an analogous investigation in Basset's *Hydrodynamics*. My original object, however, was different from his, as I sought the effects of viscosity on waves steadily maintained by means of a tuning-fork used as a current interruptor; not on waves once started and then left to themselves. Besides obtaining his boundary conditions in a singular manner, I think that in his § 521 Mr Basset has made an erroneous investigation of the effects of very great viscosity.

The stress-function in a viscous liquid may be obtained (*Anté*, No. XCVII., pp. 306–7) from that in an elastic solid, by substituting velocity for displacement; in the form

$$\phi\omega = -\mu\,(S\omega\nabla\,.\,\sigma + \nabla S\omega\sigma) - (c - \tfrac{2}{3}\mu)\,\omega S\nabla\sigma \qquad\qquad (1),$$

where, in order to include the part of the pressure which is not due to motion, we must write p instead of the quantity

$$cS\nabla\sigma.$$

Here σ is the vector velocity of an element at ρ, and μ is the coefficient of viscosity.

Hence, supposing the volume of the element to be unity, we have for the equation of motion

$$e\frac{\partial\sigma}{\partial t} = -\nabla(eP) + \iint\phi U\nu ds$$

$$= -\nabla(p + eP) - \mu(\nabla^2\sigma + \tfrac{1}{3}\nabla S\nabla\sigma),$$

where e is the density of the liquid, and P the potential energy of unit mass at ρ; and the double integral is taken over the surface of the element. This is a perfectly general equation, so we must proceed to the necessary limitations.

First. Let the displacements be so small that their squares may be neglected. Then we may write d for ∂.

Second. Let the liquid be incompressible; then

$$S\nabla\sigma = 0 \quad\text{.........(2).}$$

With these, the equation of motion becomes

$$e\frac{d\sigma}{dt} = -\nabla(p + eP) - \mu\nabla^2\sigma \quad\text{.........(3).}$$

Third. Let the motion be parallel to one plane, and we have

$$Sk\sigma = 0 \quad\text{.........(4).}$$

From (2) and (4) we have at once

$$\sigma = \nabla w \,.\, k \quad\text{.........(5),}$$

where w is a scalar function of $Vk\rho$.

Operate on (3) by $V.\nabla$, and substitute from (5), and we have

$$\left(e\frac{d}{dt} + \mu\nabla^2\right)\nabla^2 w = 0 \quad\text{.........(6).}$$

Fourth. Limit w to disturbances which diminish rapidly with depth. Here the problem has so far lost its generality that it is advisable to employ Cartesian co-ordinates, the axis of $x(i)$ being in the direction of wave-motion, and that of $y(j)$ vertically upwards. Then it is clear that a particular integral of (6) is

$$w = (A\epsilon^{ry} + B\epsilon^{sy})\,\epsilon^{(rx+nt)\iota} \quad\text{.........(7),}$$

where ι denotes $\sqrt{-1}$. The only conditions imposed on r, s, and n, are that the real parts of r and s, in so far as they multiply y, must be positive; and by (6)

$$\mu(s^2 - r^2) = en\iota \quad\text{.........(8).}$$

The speed of vertical displacement of the surface is

$$\dot\eta = -(Sj\sigma)_0 = (Si\nabla w)_0 = -r\iota(A + B)\,\epsilon^{(rx+nt)\iota} \quad\text{.........(9).}$$

From this, $\frac{d^2\dot\eta}{dx^2}$ and $\frac{d^4\dot\eta}{dx^4}$, which will be required below, are found by using the factors $-r^2$ and r^4.

The stress on the free surface (where $y = \eta$, a quantity of the order A) is, by (1),

$$(\phi j)_0 = -p_0 j - \mu (Sj\nabla . \sigma + \nabla Sj\sigma)_0 \quad \text{.............................(10)},$$

where, in p_0, we must include the effects of the tension T, and of the flexural-rigidity E, of the surface-film.

But, by (3) and (5), we have

$$\nabla (p + eP) = -\left(e \frac{d}{dt} + \mu\nabla^2\right) \nabla w . k;$$

so that as $$P = gy$$

we have $$dp + egdy = en\iota (r\iota dy - rdx) A \epsilon^{ry+(rx+nt)\iota}.$$

From this, by integrating, and introducing the surface conditions,

$$\Pi - p_0 = eg\eta - T\frac{d^2\eta}{dx^2} + E\frac{d^4\eta}{dx^4} + enA\epsilon^{(rx+nt)\iota}.$$

If we now substitute this in (10) and, for the boundary condition,* make

$$\frac{d}{dt}(\phi j)_0 = 0,$$

(omitting terms of the second degree in A and B), we have by means of (9) the two equations

$$R(A + B) - en^2 A + 2\mu rn\iota (rA + sB) = 0,$$

$$r^2 (A + B) + r^2 A + s^2 B = 0,$$

where, for shortness, $$R = egr + Tr^3 + Er^5 \quad \text{...(11)}.$$

Thus, finally, $$R - en^2 + 4\mu nr^2\iota + 4\frac{\mu^2}{e} r^3 (r - s) = 0 \quad \text{...............................(12)}.$$

This must be treated differently according as μ is small or great.

I. Let μ be small; and let n be given, and real. This is the case of the sustained waves in Prof. Smith's experiments.

The equation obtained by neglecting μ, viz.

$$R - en^2 = 0,$$

gives one, and only one, positive value of r, whose value is diminished (*i.e.*, the wave-length is increased) alike by surface tension and by surface-flexural-rigidity. Call it r_0, and let

$$r = r_0 + \mu\rho,$$

then by (12), keeping only terms of the first order in μ,

$$(ge + 3Tr_0^2 + 5Er_0^4)\rho + 4nr_0^2\iota = 0 \quad \text{.................................(13)}.$$

Thus ρ is a pure imaginary, and therefore the viscosity does not affect the length of the waves. It makes their amplitude diminish as they leave the source. (For the real

* W. Thomson, *Camb. and Dublin Math. Journal*, III. 89 (1848).

part of w belongs in this case, if we take n as positive, to waves travelling in the negative direction along x, and *vice versa*.) The factor for diminution of amplitude per unit distance travelled by the wave is

$$\epsilon^{-\mu\rho\iota}.$$

This expression gives very curious information as to the relative effects of viscosity on the amplitudes of long and short waves, when we suppose gravity, surface-tension, or surface-flexural-rigidity, *alone*, to be the cause of the propagation.

If the waves be started once for all, and allowed to die out, r is given and n is to be found. This is the first case treated by Mr Basset. If then $n = n_0$ be found from

$$en^2 = R,$$

we may put $$n = n_0 + \mu\nu.$$

By (12) we have, keeping only the first power of μ,

$$e\nu = 2r^2\iota,$$

which coincides with the result given in § 520 of Basset's *Treatise*.

II. Let μ be large. Suppose r to be given, a real positive quantity. Then, by (8), we may eliminate n from (12) and obtain

$$\frac{Re}{\mu^2} + (s^2 - r^2)^2 + 4r^2(s^2 - r^2) + 4r^3(r - s) = 0 \quad\text{.....................(14).}$$

The first term is very small, and the rest has the factor $s - r$. Omit the term which contains this factor twice, and we have

$$s(s - r) = -\frac{Re}{4r^2\mu^2} \quad\text{...................................(15).}$$

This has real positive roots if, and only if,

$$\mu^2 r^4 > Re,$$

and thus, by (8), when this condition is satisfied n is a pure imaginary, and there can be no oscillation. Of the two roots of (15) we must, in consequence of our assumption (that $(s - r)^2$ is negligible), choose that which is nearly equal to r. It might be fancied that, as this assumption leads to $B = -A$ very nearly, a new limitation would be introduced as regards the magnitude of η. But we have

$$\eta = -\frac{r}{n}(A + B)\,\epsilon^{(rx+nt)\iota} = -\frac{r}{n}A\left(1 - \frac{2r^2}{r^2 + s^2}\right)\epsilon^{(rx+nt)\iota}$$

$$= -A\frac{e\iota}{\mu r}\,\epsilon^{(rx+nt)\iota}, \text{ nearly.}$$

The wave-pattern, in this case, does not travel but subsides *in situ*, its amplitude diminishing according to the approximate factor

$$\epsilon^{-Rt/2\mu r^2}.$$

Thus, as was to be expected, the subsidence is slower as the friction is greater. Also, if gravitation is the sole cause of subsidence, the longer waves subside the faster;

while if the main cause be surface-tension, or surface-flexural-rigidity, the shorter waves subside the faster.

III. If there be a uniform film of oxide or dust, in *separate* particles which adhere to and move with the surface, we must add to the expression for surface-stress in (10) the term

$$-m(\dot{\sigma})_0 = -m\frac{d}{dt}(\nabla w)_0 k$$

$$= -m\{jrn(A+B) + in\iota(rA+sB)\}\,\epsilon^{(rx+nt)\iota},$$

where m is the surface-density of the film.

The equations for the elimination of A/B become

$$(R + mrn^2 - en^2 + 2\mu r^2 n\iota)A + (R + mrn^2 + 2\mu rsn\iota)B = 0,$$

$$\left(2r^2 - \frac{mnr\iota}{\mu}\right)A + \left(2r^2 + \frac{en\iota}{\mu} - \frac{mns\iota}{\mu}\right)B = 0;$$

so that instead of (12) we have

$$\{e - m(s-r)\}(R + mrn^2) = e^2n^2 - 4\mu r^2 ne\iota + 4\mu^2 r^3(s-r) - mn^2 es.$$

When μ and m are small, this is approximately

$$R + 2mrn^2 = en^2 - 4\mu r^2 n\iota.$$

There is no other term in the first power of m, independent of μ; so that, to this degree of approximation (which is probably always sufficient), the dust layer has no effect except to increase R. When there is no viscosity this increases the ripple-length (*i.e.*, diminishes r) for a given period of vibration.

When terms of the first degree in the viscosity are taken account of, the effect on n (for a given value of r) is merely to add to it the pure imaginary

$$2\mu r^2\iota/(e - 2mr),$$

whose value increases alike with m and with r.

Thus the period is not affected, but the surface layer aids viscosity in causing waves to subside as they advance.

This investigation above may be easily extended to the case in which a thin liquid layer is poured on mercury to keep its surface untarnished. The only difficulty is with respect to the relative tangential motion at the common surface of the liquids.

CI.

NOTE ON THE ISOTHERMALS OF ETHYL OXIDE.

[*Proceedings of the Royal Society of Edinburgh, July* 6, 1891.]

THE first three pressure-columns of the following little table were constructed from the elaborate data given by Drs Ramsay and Young in their important paper "On Evaporation and Dissociation," Part IV. (*Phil. Mag.*, May 1887). They give, in metres of mercury, the pressures required to confine one gramme of oxide of ethyl to various specified numbers of cubic centimetres, at temperatures near to that of the critical point.

v	193°·8	A	B	C
2	...	...	73·	72·9
2·3	38·6	...	38·55	38·3
2·4	34·	34·3	34·43	34·16
2·5	31·2	31·3	31·53	31·55
2·75	28·	28·1	28·24	28·41
3	...	27·7	27·42	27·45
3·3	...	27·2 +	27·19	27·3
3·7	...	27·2	27·19	27·2
4	...	27·2	27·20	27·2
5	27·	27·1	27·12	27·1
6	26·6	26·7	26·80	26·46
7	25·9	25·9	26·00	25·6
10	22·9	22·9	22·89	22·86
15	18·3	18·4	18·26	18·0
20	15·0	15·0	14·97	14·8
50	7·	7·	7·01	7·02
100	...	3·7	3·69	3·75
300	...	1·27	1·28	1·32

The values in the second column are taken directly from the paper referred to (Table I.), in which 193°·8 C. is regarded by the Authors as the critical temperature. Those in column A were calculated for temperature 194° C. from the pressures given in the same table for 195° C. and 200° C. (occasionally 210° or 220° C.). Those in column B were calculated, also for 194° C., from Table II. of Drs Ramsay and Young, which contains their "smoothed" values of the constants. Finally, column C has been computed from my own formula, in forms (given below) which are adapted to volumes greater and less than the critical volume, respectively. A glance at column B shows that, so far as the "smoothed" data are concerned, the critical point should be sought slightly *above* 194° C. For, at that temperature, the pressure has still distinctly a maximum and a minimum value, both corresponding to volumes between 3 and 5. Column A, calculated from the unsmoothed data, does not show this peculiarity. Hence I have assumed, as approximate data for the critical point,

$$\bar{t} = 194^\circ \text{ C.}, \ \bar{p} = 27{\cdot}2, \ \bar{v} = 4.$$

The last of these is, I think, probably a little too large; but we have the express statement of Drs Ramsay and Young that the true critical volume is about 4·06.

From their Table II., above referred to, I quote the first two lines below, giving (usually to only 3 significant figures) values of dp/dt at constant volume:—

v	2	2·5	3	4	5	10	20	50	100	300
$\frac{dp}{dt}$	1·60	0·92	·622	·414	·319	·133	·056	·019	·009	·0029
Calc.	...	...	·616	·426	·320	·131	·056	·019	·009	·0029
	1·65	0·90	·633	·405	...	...	...	...	...	...

The third and fourth lines are calculated respectively from the expressions

$$\left(0{\cdot}85 + \frac{6}{v+3}\right)\frac{1}{v}, \text{ and } \left(1{\cdot}2 + \frac{1{\cdot}05}{v-1{\cdot}5}\right)\frac{1}{v};$$

representing the coefficient of $(t-\bar{t})$ in my general formula

$$p = \bar{p}\left(1 - \frac{(v-\bar{v})^3}{v(v+\alpha)(v+\gamma)}\right) + R\left(1 + \frac{e}{v+\alpha}\right)\frac{t-\bar{t}}{v}.$$

Approximate values of the other constants are now easily obtained; and we have, for the critical isothermal, while the volume exceeds the critical value,

$$p = 27{\cdot}2\left(1 - \frac{(v-4)^3}{v(v+3)(v-0{\cdot}5)}\right).$$

In attempting to construct a corresponding formula for volumes lower than the critical range, I assumed 3·5 as an inferior critical volume, and obtained

$$p = 27{\cdot}2\left(1 - \frac{(v-3{\cdot}5)^3}{v^2(v-1{\cdot}5)}\right).$$

As will be seen by the numbers in column C above, which are calculated from them, these formulæ represent the experimental results very closely:—but I am not quite satisfied with the first of them, because the value (3), which it assigns to α, seems to be too large in comparison with $\bar{v}$. But, on the other hand, if we much reduce this value of α, the closeness of representation of dp/dt is much impaired. Again, the value $(-1{\cdot}5)$ which is assigned for α in the second of these formulæ is inconsistent with the fact that at 0° C. and 1 atm. the volume of one gramme is 1·4 c.c. nearly. But a very small change of α will entirely remove this objection, and will not perceptibly impair the agreement of the formula with experiment.

The general formula is applicable to temperatures considerably *under* that of the critical point, for volumes greater than 4. In fact Drs Ramsay and Young seem to assert that at *any* constant volume p is a linear function of t. But I think even their own experiments show that, for $v < 4$, there is diminution of the value of dp/dt as soon as the temperature falls below the critical value:—*i.e.*, as soon as we begin to deal with liquid alone. And certainly such is the result which theory would lead us to expect.

[It is curious to note that if, in my general formulæ (No. LXXX. above, p. 200), we assume

$$\alpha = \gamma,$$

we have
$$pv = E\left(1 + \frac{e}{v+\gamma}\right) - \frac{A-C}{v+\gamma} + \frac{eC}{(v+\gamma)^2};$$

and this leads to
$$p = \bar{p}\left(1 - \frac{(v-\bar{v})^3}{v\,(v+\gamma)^2}\right) + R\left(1 + \frac{e}{v+\gamma}\right)\frac{t-\bar{t}}{v};$$

with the condition
$$3\bar{v} + 2\gamma = R\bar{t}/\bar{p}.$$

This formula differs by want of one disposable constant from (C) of the paper referred to, but approximates much more closely to it than does either (A) or (B).]

CII.

NOTE APPENDED TO DR SANG'S PAPER, ON NICOL'S POLARIZING EYEPIECE.

[*Proceedings of the Royal Society of Edinburgh, November* 23, 1891.]

AT the very urgent request of the late Dr Sang, who regarded the above paper as one of his chief contributions to science, I brought before the Council of the Society the question of its publication. From the Minute-Book of the Ordinary Meetings, I find that it was read on the 20th February 1837, though it is not mentioned in the published *Proceedings* of that date. On 21st July 1891 the Council finally resolved that the paper should be printed in the *Proceedings* "if otherwise found desirable." The reasons in favour of printing it seem to outweigh those which may, readily enough, be raised against such a course.

The subject is one with which, except of course in its elements, I have long ceased to be familiar. But, from the imperfect examination which I have found leisure to make, I have come to the following conclusions.

The paper contains a very important suggestion which (one would have thought) should have been forthwith published, whatever judgment might be passed on the rest of the work:—viz., the proposal to construct the polariser of two glass prisms, separated by a thin layer, only, of Iceland spar. In view of the scarcity of this precious substance, such a suggestion was obviously of great value.

I am not sufficiently acquainted with the early history of the Nicol prism to be able to pronounce on the question of Dr Sang's claim to priority in the explanation of its action:—but he told me that he believed himself to have been the first to *demonstrate* that the separation effected was due to the total reflection of the ordinary ray*. And it is quite certain that, long subsequent to 1837, various very singular attempts at explanation have been given in print. The inventor, himself, seems to have thought that the effect of his instrument was merely to "increase the divergency" of the two rays.

The numerical error which Dr Sang has pointed out in Malus' work seems to have been a slip of the pen only, as the minutes and seconds of the angle in question are correctly given. He supplies no reference to the passage, but I find it in the list of calculated angles at p. 125 of the *Théorie de la Double Réfraction.* It cannot be a mere misprint, because the supplement is given along with the angle, and is affected by the corresponding error. But I do not think that Dr Sang's further remark is justified, as Malus not only gives the correct expression for the cosine of the angle in question, but seems to have employed in his subsequent calculations the inclination of the axis to a *face*, not to an *edge*, of the crystal:—and he gives the accurate numerical value of this quantity, as deduced from Wollaston's measure of the angle between two faces.

There is an altogether unnecessarily tedious piece of analysis in Dr Sang's investigation of the limits within which the prism works:—and it is so even although he shortens it by the introduction of the terribly significant clause "after repeated simplifications." I will give below what I consider to be a natural and obvious mode of dealing with the question (one which, besides, leads to some elegant results):—but I have reproduced Dr Sang's manuscript *as it was read,* for the circumstances of the present publication seem to require literal accuracy. Dr Knott has kindly verified for me the agreement of my final equation with that of Dr Sang.

Dr Sang's problem is equivalent to the following:—

A tangent is drawn to an ellipse from a point of a concentric circle; find when it subtends the greatest angle at the common centre.

Let the curves be

$$(x/\alpha)^2+(y/\beta)^2=1, \text{ and } x_1^2+y_1^2=\gamma^2, \text{ respectively.}$$

Then, if

$$x=\alpha\cos\phi,\quad y=\beta\sin\phi,$$

$$x_1=\gamma\cos\nu,\quad y_1=\gamma\sin\nu,$$

the condition of tangency is obviously

$$\frac{\cos\phi\cos\nu}{\alpha}+\frac{\sin\phi\sin\nu}{\beta}=\frac{1}{\gamma}.$$

Also, since the angle at O is to be a maximum,

$$\frac{d}{d\nu}\left\{\tan^{-1}\left(\frac{\beta}{\alpha}\tan\phi\right)-\nu\right\}=0.$$

* [See, however, a Note by Fox Talbot (*Proc. R. S. E.*, VII. 468; 15/5/71) which appears to settle this important matter of scientific history by reference to a paper published by him in 1834 (*Phil. Mag.*, IV. 289). 1899.]

Differentiating the first equation, and eliminating $d\phi/dv$ between the two, we get at once the remarkably simple relation

$$(\tan\phi)^3 = -\frac{\alpha}{\beta}\tan\nu \text{ ....(1).}$$

But we may put the first into the form

$$\frac{\cos\nu}{\alpha} + \frac{\sin\nu}{\beta}\tan\phi = \frac{1}{\gamma}\sec\phi,$$

or
$$\frac{(\cos\nu)^2}{\alpha^2} - \frac{1}{\gamma^2} + \frac{2\cos\nu\sin\nu}{\alpha\beta}\tan\phi + \left(\frac{(\sin\nu)^2}{\beta^2} - \frac{1}{\gamma^2}\right)(\tan\phi)^2 = 0 \text{ ....(2).}$$

The elimination of $\tan\phi$ between (1) and (2) is easily effected by multiplying (2) twice over by $\tan\phi$, using (1) after each operation. We thus avoid the radicals which make Dr Sang's work so complicated, and we have only to eliminate $\tan\phi$ and $(\tan\phi)^2$ among three equations of the first degree. The resulting equation is of the fourth degree in $(\sin\nu)^2$, but it contains the irrelevant factor

$$\frac{(\cos\nu)^2}{\alpha^2} + \frac{(\sin\nu)^2}{\beta^2}.$$

(Another method of effecting the elimination, while quite as simple as that just given, has the advantage of not introducing the irrelevant factor. Write for shortness

$$\frac{\cos\nu}{\alpha} = p, \quad \frac{\sin\nu}{\beta} = q,$$

and we have
$$p\cos\phi + q\sin\phi = \frac{1}{\gamma},$$

$$p(\sin\phi)^3 + q(\cos\phi)^3 = 0.$$

From the second of these, by the help of the first, we at once obtain

$$p\sin\phi + q\cos\phi = \frac{1}{\gamma}\cos\phi\sin\phi,$$

or
$$\frac{p}{\cos\phi} + \frac{q}{\sin\phi} = \frac{1}{\gamma}.$$

The following are immediate consequences:—obtained, respectively, by multiplying together the first and fourth of these equations, and by squaring and adding the first and third:—

$$p^2 + q^2 + \frac{pq}{\sin\phi\cos\phi} = \frac{1}{\gamma^2},$$

$$p^2 + q^2 + 4pq\sin\phi\cos\phi = \frac{1}{\gamma^2}\{1 + (\sin\phi\cos\phi)^2\}.$$

From these the final result may be written by inspection, in the form

$$p^2 + q^2 + \frac{4p^2q^2}{\frac{1}{\gamma^2} - p^2 - q^2} = \frac{1}{\gamma^2}\left(1 + \frac{p^2q^2}{\left(\frac{1}{\gamma^2} - p^2 - q^2\right)^2}\right),$$

or $$\left(p^2+q^2-\frac{1}{\gamma^2}\right)^3-4p^2q^2\left(p^2+q^2-\frac{1}{\gamma^2}\right)=\frac{p^2q^2}{\gamma^2},$$

which is obviously of the third degree in $(\sin \nu)^2$.)

[It is particularly interesting to compare these plane results with those of the corresponding space-problem as given by the obvious quaternion process. 1899.]

It is clear that there are other parts of Dr Sang's paper which might be greatly simplified by the use of an auxiliary angle; but it suffices to have shown the value of the method in the most complicated part of the investigation.

[*P.S.—Nov.* 23, 1891.—Mr R. T. Glazebrook has kindly given me a reference to *Comptes Rendus*, XCIX. 538 (1884), where M. E. Bertrand has suggested the employment of glass prisms separated by a thin layer of Iceland spar.]

CIII.

NOTE ON DR MUIR'S SOLUTION OF SYLVESTER'S ELIMINATION PROBLEM.

[*Proceedings of the Royal Society of Edinburgh, May* 2, 1892.]

THE following method of treating the question occurred to me while Dr Muir was reading his paper at the last meeting of the Society. It seems to throw some new and curious light on the intrinsic nature of the problem. I have confined myself to an exceedingly brief sketch, but it is clear that the proposed mode of treatment opens a wide field of interesting work.

Write the equations as

$$\frac{x^2}{A} - 2\frac{C'}{\sqrt{AB}} \cdot \frac{xy}{\sqrt{AB}} + \frac{y^2}{B} = 0, \text{ \&c.,}$$

or

$$\xi^2 - 2e_3\xi\eta + \eta^2 = 0, \text{ \&c.}$$

The two values of ξ/η, &c., are evidently reciprocals of one another. In fact, if we were to put

$$\xi/\eta = \tan\theta_3, \text{ \&c.,}$$

the equations might be written $\quad 1 - e_3 \sin 2\theta_3 = 0, \text{ \&c.}$

Since we have

$$\frac{\xi}{\eta} \cdot \frac{\eta}{\zeta} \cdot \frac{\zeta}{\xi} = 1,$$

while the values of the factors on the left are, respectively,

$$t_3 \text{ or } \frac{1}{t_3}, \quad t_1 \text{ or } \frac{1}{t_1}, \quad t_2 \text{ or } \frac{1}{t_2},$$

it is obvious that the fourth equation required for the elimination is

$$\frac{1}{(t_1t_2t_3)^6}(t_1t_2t_3-1)^2(t_1t_2-t_3)^2(t_2t_3-t_1)^2(t_3t_1-t_2)^2=0.$$

Put $T=t_1t_2t_3$, and this is

$$\frac{1}{T^8}(T-1)^2(T-t_1^2)^2(T-t_2^2)^2(T-t_3^2)^2=0.$$

Expanding and regrouping, the expression is easily transformed to

$$\frac{16}{T^4}\left\{1-\left(\frac{1+t_1^2}{2t_1}\right)^2-\left(\frac{1+t_2^2}{2t_2}\right)^2-\left(\frac{1+t_3^2}{2t_3}\right)^2+2\,\frac{1+t_1^2}{2t_1}\cdot\frac{1+t_2^2}{2t_2}\cdot\frac{1+t_3^2}{2t_3}\right\}=0,$$

or

$$\frac{16}{t_1^4t_2^4t_3^4}(1-e_1^2-e_2^2-e_3^2+2e_1e_2e_3)^2=0.$$

The factor in brackets is the square of the determinant

$$\begin{vmatrix} 1 & e_3 & e_2 \\ e_3 & 1 & e_1 \\ e_2 & e_1 & 1 \end{vmatrix},$$

and thus Dr Muir's result is reproduced when we insert the values of e_1, e_2, e_3 in terms of A, B, C, A', B', C'.

One interesting point of the transformation seems to be the breaking up of this determinant into the four factors above specified; so that the equation

$$\begin{vmatrix} 1 & (\sin 2\theta)^{-1} & (\sin 2\alpha)^{-1} \\ (\sin 2\theta)^{-1} & 1 & (\sin 2\beta)^{-1} \\ (\sin 2\alpha)^{-1} & (\sin 2\beta)^{-1} & 1 \end{vmatrix}=0$$

has for roots, as values of $\tan\theta$,

$$\tan\alpha\tan\beta,\quad \frac{1}{\tan\alpha\tan\beta},\quad \frac{\tan\alpha}{\tan\beta},\quad\text{and}\quad \frac{\tan\beta}{\tan\alpha}.$$

But the novelty and value of the process seem to lie in the mode in which the elimination is effected by mere general reasoning.

CIV.

NOTE ON THE THERMAL EFFECT OF PRESSURE ON WATER.

[*Proceedings of the Royal Society of Edinburgh, July* 18, 1892.]

I HAVE just seen in the *Comptes Rendus* (June 27) an account of some experiments, on this subject, made by M. Galopin in the laboratory of Professor Pictet. As the effects obtained by him seem to be somewhat greater than my own experiments had led me to expect, I was induced to repeat my calculations with the view of trying to account for the difference. Unfortunately, M. Galopin's work is confined to 500 atmospheres, a pressure which lies a little beyond the range of my experiments; so that no very trustworthy comparison can be made. M. Galopin's results have one advantage over those of the *direct* experiments of the same kind which I made, inasmuch as he was able to use ordinary thermometers, while I employed thermo-electric junctions, in measuring the rise of temperature by compression. But they have a corresponding disadvantage, in the fact that mine were obtained instantaneously (by means of a dead-beat galvanometer) and required no correction; while his had to be corrected for the heat-equivalent of his apparatus to an amount not easy to estimate with accuracy.

I had assured myself of the general accuracy of my own work by showing that three altogether independent modes of estimating the effect of pressure on the maximum density point of water gave closely concordant results:—viz. a lowering of that point by about 1° C. for every 50 atmospheres. These investigations were described to the Society in 1881—4, and appear (in abstract) in our *Proceedings*; more fully in the *Challenger Reports.* [See No. LXI. above.] One mode of determination was *direct* (a modification of Hope's experiment); the others were theoretical deductions, from the compressibility of water at different temperatures, and from the rise of temperature produced by compression, respectively. M. Amagat subsequently obtained a result very closely agreeing with mine as given above. His method differs from any of mine, for he seeks two temperatures, not very different, at which water has the same volume at the same pressure.

So far, I had been dealing with pressures of little more than 200 atmospheres. Higher pressures led to the result that the displacement of the maximum density point increases very much faster than does the pressure. For the terms in higher powers of the pressure begin to tell more and more; and another cause comes prominently into play, depending on the fact that water has a temperature of minimum compressibility (about 60° C. at ordinary pressures). This affects to a very much greater extent the lowering of the maximum density point by pressure than it affects the amount of heat developed by the compression. Both of these causes are indicated in my formulæ as contributing to such a result, but the small numerical factors of the terms which express them are not accurately known; and the calculation of the thermal effect of large pressures from data obtained by measuring compressibility at different temperatures is a very severe test of their accuracy. Besides, in giving a formula which exactly represented my determinations of the change of volume of water, under pressures from 150 to 450 atmospheres, and at temperatures 0° to 15° C., I expressly said that "it must not be extended, in application, much beyond" these limits. If, however, we venture to extend it to 500 atmospheres, it leads to the following expression, for the heating of water by the sudden application of that pressure,

$$\frac{t+3{\cdot}2}{26};$$

where t is the original temperature (C.) of the water operated on. In obtaining this result it is assumed, in accordance with Kopp's data, that the expansibility of water at ordinary temperatures and at atmospheric pressure is approximately $(t-4)/72{,}000$. Other experimenters make it somewhat greater. [If the maximum density point were lowered 1° for *every* 50 atmospheres, the heating by 500 atmospheres would be about $(t+1)/22$ only. Comparing this with the result above, we see how considerably the causes, alluded to, affect the calculated amount of heating.]

Now I find that M. Galopin's results may be represented very closely (from 0° to 10° C., which are his temperature limits) by the analogous expression

$$\frac{t+5}{25}.$$

The difference between the denominators of these expressions is not serious, and may depend upon the uncertainty of the assumed expansibility of water, or upon an over-correction of his results by M. Galopin. [He increases his observed data by 52 per cent. in consequence of the thermal capacity of his apparatus.] But the difference between the numerators seems to show once more that M. Galopin's data have been over-corrected, or that it was scarcely warrantable to extend the application of my formula so far as 500 atmospheres.

CV.

NOTE ON THE DIVISION OF SPACE INTO INFINITESIMAL CUBES.

[*Proceedings of the Royal Society of Edinburgh, December* 5, 1892.]

THE proposition that "the only series of surfaces which, together, divide space into cubes are planes and their electric images" presented itself to me twenty years ago, in the course of a quaternion investigation of a class of *Orthogonal Isothermal Surfaces* [No. XXV. above]. I gave a second version of my investigation in vol. IX. of our *Proceedings*. [No. XLIV. above.] Prof. Cayley has since referred me to Note VI., appended by Liouville to his edition of Monge's *Application de l'Analyse à la Géométrie* (1850), in which the proposition occurs, probably for the first time. The proof which is there given is very circuitous; occupying some eight quarto pages of small type, although the reader is referred to a Memoir by Lamé for the justification of some of the steps. But Liouville concludes by saying:—"l'analyse précédente qui établit ce fait important n'est pas indigne, ce me semble, de l'attention des géométres." He had previously stated that he had obtained the result "en profitant d'une sorte de hasard." As Liouville attached so much importance to the theorem, and specially to his proof of it, it may not be uninteresting if I give other modes of investigation. The first of them is merely an improved form of what I have already given in our *Proceedings*; the second (which is the real object of this note) seems to have secured nearly all the advantages which Quaternions can afford, in respect alike of directness, clearness, and conciseness. It is very curious to notice that much of this gain in brevity is due simply to the fact that the *Conjugate* of a certain quaternion is employed along with the quaternion itself in my later work; while I had formerly dealt with the reciprocal, and had, in consequence, to introduce from the first the tensor explicitly. The investigation should present no difficulties to anyone who has taken the sort of trouble to remember elementary quaternion formulæ which every tyro in integration has to take to fix in his memory the values of $d \tan x$, $d \log \frac{x + \sqrt{x^2 - a^2}}{a}$, or $d \tan^{-1} x$, &c.

The only peculiarities of the question seem to be due to the contrast between the (apparently) great generality of the initial equation and the extremely restricted character of the sole solution. This will be abundantly evident from the discussions which follow, since it would almost appear as if the conditions arrived at were too numerous to be simultaneously satisfied. I find it very convenient to use a symbol ∂ $\left(\text{in the sense of } \frac{d}{dx}\right)$ to express rate of increase per unit of length. Thus

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}$$

may be written

$$\nabla = \alpha\partial_1 + \beta\partial_2 + \gamma\partial_3,$$

where α, β, γ are any rectangular unit-system.

The equation

$$d\sigma = uq^{-1}d\rho q \qquad (1)$$

(where u is a scalar, and q a versor, function of ρ) ensures that an element of space at σ corresponds to a *similar* element at ρ; so that the transformation from ρ to σ, or *vice versâ*, is from one mode of dividing space into infinitesimal cubes to another. [From the purely analytical quaternion point of view the question may be regarded as simply that of finding u and q as functions of ρ, so that the right-hand member may be a complete differential.] We have at once

$$S\alpha d\sigma = -Sd\rho\nabla \,.\, S\alpha\sigma = uS \,.\, d\rho q\alpha q^{-1},$$

whatever constant unit vector α may be. Thus

$$-\nabla S\alpha\sigma = uq\alpha q^{-1} \qquad (2).$$

A part, only, of the information given by this is contained in

$$V\nabla \,.\, uq\alpha q^{-1} = 0 \qquad (3),$$

or

$$\begin{aligned} V \,.\, q\alpha q^{-1}\frac{\nabla u}{u} &= V\nabla \,.\, q\alpha q^{-1} \\ &= V \,.\, (\nabla qq^{-1}q\alpha q^{-1} + q\alpha q^{-1}\nabla qq^{-1}) - 2S \,.\, q\alpha q^{-1}\nabla_1 \,.\, Vq_1q^{-1} \\ &= 2q\alpha q^{-1}S \,.\, \nabla qq^{-1} - 2S \,.\, q\alpha q^{-1}\nabla_1 \,.\, Vq_1q^{-1}. \end{aligned}$$

From the sum of the three equations of this form (each multiplied by its $q\alpha q^{-1}$) it appears at once that

$$S \,.\, \nabla qq^{-1} = 0 \qquad (4);$$

so that, as $q\alpha q^{-1}$ may be *any* unit vector,

$$\begin{aligned} V \,.\, \alpha\frac{\nabla u}{u} &= -2S\alpha\nabla_1 \,.\, q_1q^{-1} \\ &= 2\partial_1 qq^{-1} \qquad (5). \end{aligned}$$

From two of the three equations of this form we have

$$\partial_2\left(V\alpha\frac{\nabla u}{u}\cdot q\right)=\partial_1\left(V\beta\frac{\nabla u}{u}\cdot q\right);$$

whence

$$-V\cdot\gamma(\alpha\partial_1+\beta\partial_2)\frac{\nabla u}{u}=\frac{\nabla u}{u}S\gamma\frac{\nabla u}{u},$$

where the V is obviously superfluous; so that

$$\gamma\nabla\frac{\nabla u}{u}+\partial_3\frac{\nabla u}{u}=\frac{\partial_3 u}{u}\frac{\nabla u}{u}\;\text{..................................}(6).$$

There are, of course, three equations of this form, and they give by inspection

$$\frac{1}{u}\nabla\frac{\nabla u}{u}=\alpha\partial_1\frac{\nabla u}{u^2}=\beta\partial_2\frac{\nabla u}{u^2}=\gamma\partial_3\frac{\nabla u}{u^2}=\frac{1}{3}\nabla\frac{\nabla u}{u^2}\;\text{.........................}(7).$$

The first and last of these equals give

$$\nabla^2(u^{\frac{1}{2}})=0,$$

whose general solution is known to be

$$u^{\frac{1}{2}}=\Sigma\frac{m}{T(\rho-\epsilon)},$$

where m and ϵ are constants. The other members of (7) show that *one* term only of this Σ is admissible; so that, as no origin was fixed,

$$u^{\frac{1}{2}}=\frac{m}{T\rho}.$$

From the three equations (5) we get also

$$\nabla(uq)=0,$$

so that
$$q=U\rho\,.\,a,$$

a being any constant versor. Thus we have the complete solution. It gives by (1)

$$d\sigma=m^2a^{-1}\rho^{-1}d\rho\rho^{-1}a=-m^2a^{-1}d\rho^{-1}a,$$

so that σ is merely $-\rho^{-1}$, multiplied by a constant and subjected to a definite rotation.

But the following process is very much simpler. For we may get rid of the factor u, and greatly shorten the investigation, by writing the equation of condition in the form

$$d\sigma=Kqd\rho q\;\text{...}(1).$$

It gives at once
$$\partial_2\partial_1\sigma=\partial_2(Kq\alpha q)=\partial_1(Kq\beta q),$$

or
$$V\cdot\gamma(\nabla q-\gamma\partial_3 q)q^{-1}=0\;\text{.......................................}(a).$$

Multiplying by γ, and adding the three equations of this form, we have

$$-\nabla q q^{-1} = \frac{\nabla Tq}{Tq}, \quad \text{or} \quad \nabla . qTq = 0^*.$$

By the help of this we may write (a) as

$$\frac{\nabla Tq}{Tq} = \gamma\left(\frac{\partial_3 Tq}{Tq} - \frac{\partial_3 Uq}{Uq}\right),$$

so that
$$\nabla \frac{1}{Tq} . Uq = \gamma\partial_3 \frac{Uq}{Tq} = \beta\partial_2 \frac{Uq}{Tq} = \alpha\partial_1 \frac{Uq}{Tq} = \frac{1}{3}\nabla \frac{Uq}{Tq} \quad\text{......................}(b).$$

Thus, as the form of the three middle terms shows that their common value must be some *constant* quaternion,

$$d\frac{Uq}{Tq} = \Sigma\partial_1 \frac{Uq}{Tq}\,dx = d\rho a,$$

or
$$\frac{Uq}{Tq} = \rho a,$$

for we need not add a constant *vector* to ρ, and the form of the first of the five equal quantities above shows that no *quaternion* constant (except, of course, one of the form ϵa already referred to) can be added to the right-hand side.

Thus, finally, as before $\quad d\sigma = -a\rho^{-1}d\rho\rho^{-1}Ka.$

Though the methods employed in these two investigations are, at least at first sight, entirely different, it will be easily seen that the equations (7) and (b) to which they respectively lead are identical in meaning with one another, term by term. Yet the former shows two differentiations in every term, while the second appears to involve one only. Thus also, two distinct integrations were required in the first solution, while one sufficed for the second. But in the first, the tensor and versor of the quaternion were all along separated; in the second the quaternion itself was directly sought.

* [Note that $S\partial q q^{-1} = \partial Tq/Tq$. 1897.]

CVI.

NOTE ON ATTRACTION.

[*Proceedings of the Edinburgh Mathematical Society, February* 10, 1893. Vol. XI.]

It is well known (see Thomson and Tait, §§ 517, 518) that a spherical shell, whose surface-density is inversely as the cube of the distance from an external point, as well as a solid sphere whose density is inversely as the fifth power of the distance from an external point, are centrobaric. The centre of gravity is, in each case, the "image" of the external point.

To show that these express the same physical truth, we may of course recur to the method of electric images from which they were derived. But we may even more easily prove it by a direct process, for it is obviously only necessary to show that a thin shell, both of whose surfaces give the *same* image of an external point, has everywhere its thickness proportional to the square of the distance from that point.

Call O the object, and I the image, point; and draw any radius-vector IPQ, meeting the respective surfaces of the shell in P and Q. Then, ultimately,

$$OQ - OP = QP \cos OPI,$$

or, in the usual notation,

$$\delta\left(\frac{r}{e}\right) = \delta r \cos OPI,$$

whence (introducing the new factor r)

$$r^2 \frac{\delta e}{e^2} = \delta r \left(\frac{r}{e} - r \cos OPI\right) = \delta r \, OI \cos IOP.$$

But IOP is equal to the angle between IP and the normal at P, so that the thickness of the shell at P is

$$\delta r \cos IOP = \frac{r^2 \delta e}{OI \,.\, e^2}.$$

CVII.

ON THE COMPRESSIBILITY OF LIQUIDS IN CONNECTION WITH THEIR MOLECULAR PRESSURE.

[*Proceedings of the Royal Society of Edinburgh, March* 6, 1893.]

THAT liquids, if finitely compressible, must (at any one temperature) become steadily less compressible as the pressure is raised, seems to be obvious without any attempt at proof. Yet the assertion is even now generally made, mainly in consequence of an erroneous statement of Örsted, which has been supported by some comparatively recent investigations of Cailletet and others, that the compressibility of water (at any one temperature) is practically the same at all pressures not exceeding a few hundred atmospheres.

But in 1826 (*Phil. Trans.*, CVI.), Perkins had clearly established the fact that the compressibility of water at 10° C. diminishes:—rapidly at first, afterwards more and more slowly:—as the pressure is gradually raised. Perkins' estimate of his pressure-unit seems to have been considerably too small, so that his numerical data are not very trustworthy:—but this does not in the least invalidate the proof he gives of the gradual diminution of compressibility; for that depends of course upon relative, not upon absolute, values.

In the very earliest determinations which I made, some ten or twelve years ago, while examining the pressure-errors of the "Challenger" thermometers, this diminution of the compressibility of water was prominently shown:—and in 1888 I gave, as a fairly close approximation to the average compressibility for the first p atmospheres, the empirical expression

$$A/(B+p),$$

in which the constants depend on temperature only.

This, it will be observed, is in complete agreement with the form of the result of Perkins. I also found that the addition of common salt, to the water operated on, had the effect of increasing the constant B in this formula by a quantity proportional to the amount of salt added; A being practically unchanged, so long as the temperature was kept constant.

These considerations seemed to point to the quantity B as being at least closely connected with the internal molecular pressure (usually named after Laplace); and, speculative as the idea confessedly is, it seemed worthy of further development. Another argument in its favour is furnished by a consequence of the hypothesis. For it is easy to see that when the average compressibility of a substance can be represented by the expression above, the equation of its isothermals must have the form

$$(B+p)(v-\alpha)=C;$$

approximately that given by the kinetic theory of a gas, when it is regarded as an assemblage of hard spherical particles.

Nearly three years ago, while I was preparing for press the second edition of my text-book *Properties of Matter*, M. Amagat kindly gave me several unpublished numerical details of his magnificent experiments on the compressibility of water and ether. The following short table gives in its second column some of these results for water at 0° C.:—

Pressure.	Volume.	*a*		*b*		*c*	
1	1·00000	1·00000	0	1·00000	0	1·00000	0
501	·97668	·97664	+ 4	·97652	+16	·97657	+11
1001	·95645	·95662	−17	·95644	+ 1	·95652	− 7
1501	·93924	·93925	− 1	·93909	+15	·93916	+ 8
2001	·92393	·92405	−12	·92393	0	·92399	− 6
2501	·91065	·91064	+ 1	·91058	+ 7	·91062	+ 3
3001	·89869	·89870	− 1	·89873	− 4	·89875	− 6

The numbers in the columns *a*, *b*, *c* are volumes calculated respectively from the following formulæ for the average compressibility for p atmospheres:—

$$\frac{\cdot 30454}{6019+p},\quad \frac{\cdot 30}{5887+p},\quad \frac{\cdot 3015}{5933+p}.$$

The first was calculated from the data for 1, 1501, and 3001 atm.; the second from those for 1, 1001, and 2001 atm.; the third was obtained from them by interpolation. After the numbers in each column the difference "observed − calculated" is given. These are all small; and, especially in the case of formula *c*, the coincidence seems almost perfect throughout, for the differences have regular alternations of sign. But it is to be noticed that simultaneous increase, or diminution, of A and B by as much as 2 per cent. does not seriously affect the agreement of the formula with the results of experiment.

I have been for some time preparing to undertake an extended series of experiments on the compressibility of various aqueous solutions, with the view of finding (although by an exceedingly indirect and possibly questionable process) how the addition of a salt to water affects its internal pressure. But the recent publication of the final results of Amagat's experiments on the compression of water by pressures rising to 3000 atmospheres (more than six-fold the range attained in my own work) has led me to make a new series of calculations with the view of testing how far the above speculations, suggested by the results of pressures limited to some three tons' weight per square inch, are borne out by the results of pressures of twenty tons. The agreement, as will be seen, seems on the whole highly satisfactory; though, for a reason already given, and presently to be even more forcibly illustrated, the calculations are necessarily of a somewhat precarious character.

Thus we obtain from Amagat's paper (*Comptes Rendus*, January 9, 1893) the following determinations of the volume of water at 0° C., for additional pressures of 400 and 800 atmospheres:—

Pressure.	Table, No. 1.	Table, No. 2.
1	1·00000	1·00000
401	·98067	·98071
801	·96371	·96371

The pressures in Table 1 extend to 1000 atm. only, those in Table 2 to 3000 atm.

These give, respectively, for the average compressibility of water per atmosphere for the first p additional atmospheres, p ranging from 0 to 800,

$$\frac{0·296}{5725+p}, \quad \frac{0·3057}{5939+p},$$

whence the compressibility at ordinary pressure may be either

$$0·0000517 \text{ or } 0·00005147.$$

To enable us to choose between these formulæ we have the following comparison with the data for higher pressures in Amagat's second table:—

Pressure.	Amagat.	First formula.	Second formula.
1001	·95596	·95595	·95595
2001	·92367	·92337	·92299
3001	·89828	·89824	·89741

The first formula, therefore, represents with remarkable closeness the average compressibility of water at 0° C. for any range of pressure up to 3000 atmospheres; while the second obviously gives considerably too much compression at higher pressures. Yet there is but one numerical difference between the sets of data from which these two formulæ were derived, and that is merely a matter of four units in the fifth decimal place of the volume at 401 atmospheres! Thus very small inevitable errors in the data may largely affect the values of the constants in the formula.

The only certain method of overcoming this difficulty would be to work with pressures of the same order as B.

The expression which I gave in 1888 for the average compressibility per atmosphere at 0° C. was (*Challenger Report, Physics and Chemistry*, Vol. II., Part 4, p. 36; *anté*, No. LXI. p. 34),

$$\frac{0{\cdot}001863}{36+p},$$

the unit for p being 1 ton weight per square inch. To atmospheres (152·3 per ton weight per square inch) this is

$$\frac{0{\cdot}284}{5483+p},$$

giving 0·0000518 as the compressibility at ordinary pressures. This agrees closely with the first, and more accurate, of the two formulæ just given; and yet it was derived from data ranging up to 450 atmospheres only. I stated at the time that "probably both of the constants in this formula ought to be somewhat larger." This would make it still more closely agree with Amagat's results.

I have worked out the values of the quantities A and B for the ten special temperatures (from 0° to 48°·95 C. inclusive) in Amagat's table No. 2; taking for each temperature the data for pressures 1, 1501, and 3001 atmospheres. The resulting formulæ give results agreeing very fairly with the compressions given for 501, 1001, 2001, and 2501 atmospheres:—the agreement being in fact almost perfect for the two higher pressures, but the compression being (as a rule) slightly in defect for the lower pressures. M. Amagat himself has stated that his results for lower pressures are given more accurately in the series of experiments where the pressure was never very great, than in those where it was pushed to 3000 atm. In fact his manometer had to be made considerably less sensitive when very great pressure was employed. For the reasons just pointed out I cannot wholly trust these calculations, and therefore I think it unnecessary to give them here. But they agree (with only one exception, for 29°·43 C.) in a very remarkable manner in showing that the values of A and B steadily *increase* with rise of temperature up to about 40° C., and thence apparently diminish. That the value of A should at first steadily increase with rise of temperature was of course to be expected as a consequence of the known change of molecular structure if (in accordance with the supposed analogy of the kinetic gas formula above quoted) it represents the utmost fractional diminution of volume which can be produced by unlimited pressure. And Canton's old discovery, that rise of temperature involves diminution of compressibility, requires that B should at first increase more rapidly than does A. [This is not necessarily inconsistent with the commonly received statement that the surface-tension of water is, in all cases, diminished by rise of temperature.] The turning-point seems to be connected with the temperature of minimum compressibility, discovered by Pagliani and Vincentini.

The behaviour of water at ordinary temperatures is of such an exceptional character that we cannot feel certain that aqueous solutions may not show more than mere traces of it. In my projected experiments, therefore, I intend to employ at least three different solutions of each of the salts to be examined, one of them being only a little below saturation strength. The comparison of the results for solutions of very different strength may enable me to eliminate the effects of the peculiarities of the solvent.

As a contrast to the behaviour of water, above discussed, I give some results for sulphuric ether; also founded on data furnished to me three years ago by M. Amagat. These data were given to four decimal places only.

	0° C.		20°·2 C.	
Pressure.	Amagat.	Formula.	Amagat.	Formula.
1	1·0000	1·0000	1·0320	1·0320
501	·9468	·9498	·9673	·9722
1001	·9130	·9156	·9294	·9311
1501	·8884	·8885	·9018	·9018
2001	·8684	·8684	·8805	·8797
2501	·8522	·8524	·8630	·8624
3001	·8394	·8395	·8484	·8484

The agreement is not by any means so complete as in the case of water:—but it is probable that slight changes in the values of the constants may greatly improve it where defective, while otherwise scarcely interfering with it.

The formulæ for average compressibility employed were, respectively,

$$\frac{\cdot 2863}{2350+p} \text{ for } 0°, \text{ and } \frac{\cdot 3016}{2086+p} \text{ for } 20°\cdot 2.$$

(Note that calculation from the data, direct, gives 0·31126 as the value of A in the second of these, but this has to be divided by the volume at one atmosphere.) Here, according to the previous mode of interpretation, the Laplace-pressure is diminished, and the ultimate volume seems to be increased by rise of temperature, as was to be expected.

CVIII.

PRELIMINARY NOTE ON THE COMPRESSIBILITY OF AQUEOUS SOLUTIONS, IN CONNECTION WITH MOLECULAR PRESSURE.

[*Proceedings of the Royal Society of Edinburgh, June* 5, 1893.]

THE experiments referred to in my paper of March 6th (*anté*, No. CVII.) have been completed, but the results are by no means so exact as I hoped to make them. There was great difficulty in procuring the small bore tubes for the piezometers, and thus I had to employ them without previous calibration, as the solutions to be experimented on had already been prepared, and their densities determined at definite temperatures. Delay might have led to evaporation. When I proceeded to the calibration, after completing a large series of experiments, I was greatly annoyed to find that the bores of many of the tubes were by no means uniform. This accounts for the fact that my experiments, though fairly concordant, are not sufficiently so to afford more than a very strong probability in favour of the general result of the inquiry. For this reason I have described my paper as a Preliminary Note.

The idea I sought to develop was of the following nature. I had found that the average compressibility of water, at any one temperature, could be well represented by the simple formula

$$\frac{A}{B+p},$$

where p is the range of pressure through which the compressibility is measured; A and B being functions of temperature. But I also found that for aqueous solutions of common salt, of different strengths, and at the same temperature as the water, the formula was altered to

$$\frac{A}{B+s+p};$$

where A and B were as before, and s was proportional to the weight of salt dissolved in 100 of water. In particular that, when 1 ton weight per square inch (152·3 atmospheres) is the pressure unit, s is nearly the weight of salt in 100 of water.

Theoretical speculations (given at some length in my *Report on some of the Physical Properties of Water, anté*, No. LXI.) led me to look on the B, and the $B+s$, of these formulæ as being connected with the molecular pressure in the liquid, and I developed one application of them, relating to the maximum density points of various solutions of common salt.

The present series of experiments was conducted precisely as were the earlier ones, but unfortunately many of the piezometers (of which a large number were required in order that several solutions should be operated on at the same time) were new, and (as I afterwards found) faulty. The selection of the salts was undertaken by Dr Crum Brown, and the solutions were made and the density determinations effected in his Laboratory by Mr A. F. Watson.

I give these at once, as they have intrinsic value altogether apart from my work and my hypothesis.

In the following table the letters S and W stand for the masses of salt, and of water, respectively. Mr Watson remarks that the error in the numbers of the first column, from which the second was calculated, does not exceed 1 in 1000. The error in the densities does not exceed unit in the fourth decimal place.

$100\frac{S}{S+W}$	$100\frac{S}{W}$	Temp. C.	Sp. Gr.	Temp. C.	Sp. Gr.
Potassium Iodide—					
14·538	17·011	5°·5	1·1197	13°·5	1·1179
9·302	10·256	5°·6	1·0737	12°·2	1·0727
4·313	4·507	5°·4	1·0329	12°·0	1·0323
Potassium Ferrocyanide—					
14·089	16·399	5°·5	1·0987	13°·5	1·0967
9·411	10·389	6°·3	1·0620	12°·1	1·0610
4·753	4·990	6°·0	1·0328	11°·4	1·0322
Ammonium Sulphate—					
15·938	18·960	6°·8	1·0954	11°·2	1·0944
9·232	10·171	6°·3	1·0559	12°·7	1·0547
5·301	5·597	5°·7	1·0326	12°·1	1·0317
Magnesium Sulphate—					
13·836	16·058	6°·8	1·1489	11°·2	1·1479
9·508	10·507	5°·8	1·1005	13°·1	1·0990
5·869	6·235	5°·7	1·0614	12°·1	1·0602

	$100\frac{S}{S+W}$	$100\frac{S}{W}$	Temp. C.	Sp. Gr.	Temp. C.	Sp. Gr.
Barium Chloride—						
	13·798	16·006	5°·8	1·1366	11°·2	1·1354
	9·096	10·006	5°·8	1·0869	13°·1	1·0855
	4·585	4·805	5°·6	1·0423	12°·2	1·0416

To these may be added the following, due to Dr Gibson, from my *Challenger Report* referred to.

		0° C.	6° C.	12° C.
Sodium Chloride—				
	17·6358	1·138467	1·136040	1·133565
	13·3610	1·101300	1·099341	1·097244
	8·8078	1·067589	1·066144	1·064485
	3·8845	1·029664	1·028979	1·027935

Although I made at least two observations at each of the pressures 1, 2, and 3 tons, on each solution, in each of two piezometers, I publish in this Abstract nothing beyond some mean results at one temperature and for one pressure:—viz. 12° C. and 2 tons. These are fairly representative of the whole work. The columns of mercury used in calibration corresponded nearly with the parts of the tubes concerned in the measured compression at that pressure; and, on such lengths of tube, errors of measurement due to slight changes of temperature of the solution, &c., are comparatively insignificant.

The change of (unit) volume of water per ton at 12° C. and 2 tons is (by my former work)

$$\frac{0·2474}{36+2} = 0·00651.$$

If to the 36 in this expression be added the product of the quantity s below given for any one salt, multiplied by the percentage of the salt, we have the numbers in the column headed *Calc.* Those headed *Obs.* were obtained as stated above; and the agreement is on the whole satisfactory. The old determinations for common salt are included in the table, though they show rather less concordance than the others.

	$100\frac{S}{W}$	s	Obs.	Calc.
Sodium Chloride—				
	17·6	1·1	0·00428	0·00431
	13·4		472	470
	8·8		524	519
	3·9		594	585
Magnesium Sulphate—				
	16·06	1·0	450	457
	10·51		510	510
	6·23		555	559

	$100\frac{S}{W}$	s	Obs.	Calc.
Ammonium Sulphate—				
	18·96	0·77	0·00475	0·00470
	10·17		542	540
	5·5		575	580
Potassium Ferrocyanide—				
	16·4	0·62	512	513
	10·4		554	556
	5·0		605	602
Barium Chloride—				
	16·0	0·52	530	534
	10·0		573	573
	4·8		612	611
Potassium Iodide—				
	17·01	0·29	576	576
	10·25		602	603
	4·5		627	629

As stated in my previous note, my formula agrees extremely well with the recent determinations of Amagat, of compression of water up to 3000 atmospheres. But the values of A and B which I deduced from them (especially about 12° C.) are somewhat larger than mine, though they bear to one another nearly the same ratio. If I had used his value of B, the coincidences above would not have been sensibly impaired, but the values of s would have come out a little greater.

CIX.

ON THE COMPRESSIBILITY OF FLUIDS.

[*Proceedings of the Royal Society of Edinburgh, January* 15, 1894.]

THE recent publication of the full results of Amagat's magnificent experiments has led me to make further comparisons with the empirical formula (originally suggested by the graphs of my *Challenger* work) which I have on several occasions brought before the Society:—viz.

$$\frac{v_0 - v}{pv_0} = \frac{e}{\Pi + p}.$$

I find that Amagat's results, for a number of common liquids, from 1 to 3000 atm. may be *fairly* represented by substituting the following values of e and Π in the above formula:—

	0°	10°	20°	30°	40° C.
Ether	0·291	·296	·302	·310	·319
	2420	2240	2100	1980	1860
Ethylic Alcohol . .	0·274	·280	·281	·287	·288
	3230	3130	2970	2865	2700
Methylic „ . .	0·283	·290	·295	·302	.
	3240	3180	2990	2870	
Propylic „ . .	0·265	·271	·277	.	·274
	3510	3390	3200		2880
Bisulphide of Carbon .	0·286	·286	·291	·294	·299
	3970	3720	3560	3370	3190
Iodide of Ethyl . . .	.	0·288	.	·291	.
		3570		2920	

	0°	10°	20°	30°	40° C.
Chloride of Phosphorus .	.	0·278 3490	.	·293 2990	.
Acetone	0·284 3180	.	.	·298 2570	.

For the curiously exceptional case of water we have

0° C.	2°·1	4°·35	6°·85	10°·1	14°·25	20°·4	29°·43	40°·45	48°·85
0·303	·303	·307	·311	·313	·314	·314	·313	·327	·323
5940	6030	6220	6390	6560	6680	6830	6940	7520	7440

whence compressibility for low pressures,

0·0000511	503	493	486	478	470	459	449	434	434

The agreement with the experimental data would be somewhat closer if Π for any one temperature were (in accordance with theory) regarded as a quantity which increases with the compression produced.

For the present, as no definite theoretical basis has been assigned for it, the formula must be regarded merely as an exceedingly convenient mode of summarizing the experimental results; justified by the closeness of its general agreement with them.

On these numbers remark

First, that e is nearly the same for all the liquids in the table:—its lowest value being for propylic alcohol, and its highest for water. But the differences of these extremes from the mean of all are less than 7 per cent. Hence it seems that ordinary liquids, as a rule, would be reduced by infinite pressure to about 70 per cent. of their usual volume:—provided, of course, that the formula remains applicable for pressures immensely exceeding even the enormous ones applied by Amagat.

Second, e increases, as a rule, with rise of temperature. [But it does not appear to increase, in any case, so much as to make the ultimate volume diminish when temperature rises.]

Third. Except in the case of water, Π falls off rapidly with rise of temperature. This was, of course, to be expected from the increase of volume; and it is the chief cause of the increase of compressibility as given by the formula. But the value of Π does not seem to vary inversely as the square of the volume.

Fourth. In the exceptional case of water, Π increases steadily with rise of temperature, at least up to 40° C. This is the immediate cause of the diminution of compressibility given by the formula as the temperature is raised. But, so far as the present rough calculations go, Amagat's data would seem to make the temperature of minimum compressibility considerably lower than that assigned by Pagliani and Vincentini. [This may be due to the great range of pressure, or to the fact that the formula treats Π as a constant instead of taking account of its increase with compression.]

It is interesting to compare, with these, some (necessarily very rough) results fo a substance which requires considerable external pressure to keep it in the liquid state. It is shown that if the empirical formula, above, be true generally for any substance, it holds from *any* initial value of v_0*, provided that we give e and Π proper corresponding changes of value. The new Π is greater than the old by the pressure at the new v_0. The new e must be employed with the new initial volume to give the ultimate volume. The following data were calculated from Amagat's Tables 13 and 19 (*Ann. de Chimie*, XXIX., 1893). The first of the three volumes given for each temperature is that of the substance when just *wholly* liquefied by pressure. This comparison is by no means a fair one, for the range of volumes is very different alike in extent and in situation, for the different temperatures. And, from the extremely great compressibility of the liquid when *just* formed, we should expect to find the assumption of constant Π very far from the truth.

CARBONIC ACID.

Temp.	0° C.		10°		20°		30°
p	v						
34·4	·002145	44·4	·002338	56·4	·002609	70·7	·003282
500	1781	500	1826	500	1876	500	1926
1000	1656	1000	1685	1000	1716	1000	1748

From these we obtain the following sets of values of e and Π:—

0·335	0·373	0·424	0·527
420	276	170	48

The value of Π, calculated from the altered formula, has, in each case, been diminished by the corresponding initial value of p. We see that e increases with great rapidity as the temperature rises:—but the indicated ultimate volumes of carbonic acid, under infinite pressure, are not much affected thereby, being respectively

0·00143   147   150   155

where the unit is the volume of the gas at 0° C. and 1 atm.

The values of Π are, of course, small; and they diminish rapidly with rise of temperature. [The critical point is about 31°·35 C., which is but little above the highest temperature in the table.]

A fairer test than the above, from one point of view at least, might have been based upon Amagat's important Table 17, had it given data for (say) vol. = ·00225 at each temperature in addition to those at ·0025 and ·0020. I have done the best I could, by taking the nearest data directly given in Table 13. Here are a few of the results obtained.

* [Thus, from $\dfrac{v_0 - v}{v_0} = \dfrac{ep}{\Pi + p}$, and $\dfrac{v_0 - v_1}{v_0} = \dfrac{ep_1}{\Pi + p_1}$,

we have at once
$$\frac{v_1 - v}{v_1} = e\frac{v_0}{v_1}\left(\frac{p}{\Pi + p} - \frac{p_1}{\Pi + p_1}\right)$$
$$= e\frac{\Pi(p - p_1)}{(\Pi + \overline{1 - ep_1})\,\Pi + p} = e_1\frac{p - p_1}{\Pi_1 + (p - p_1)},$$

if we write $e_1 = \dfrac{\Pi e}{\Pi + \overline{1 - ep_1}}$, and $\Pi_1 = \Pi + p_1$. 1899.]

CARBONIC ACID.

	20° C.		30°		40°		50°	
	p	v						
	64·4	·0025	109	·0025	155	·0025	201	·0025
	150	·002173	200	·0022	225	·00228	300	·002255
	300	·002	384	·002	470·5	·002	560	·002
e	·2833		·2936		·3136		·3312	
Π	35·6		22·7		24·5		34·6	
Ult. Vol.	·001792		·001766		·001716		·001672	

Other deductions from Amagat's data are given, in considerable numbers:—from regions of the CO_2 diagram in which Π is respectively +, −, or even zero, the latter belonging of course to the conditions under which it behaves as a true gas. Thus, taking the data for volumes

0·01636, 0·013, and 0·01

we obtain the values of Π given in the first line of the table below. Here the substance was, throughout, at density less than the critical. The second line gives the corresponding results for a range of volumes which *includes* the critical volume:—viz.

0·00578, 0·00428, 0·00316.

The application of the formula to this series (where the part of the isothermal which is treated contains a point of contrary flexure) is obviously a matter rather of curiosity than of science.

Finally, the third line gives data for volumes all well under the critical volume:—viz.

0·00316, 0·00250, and 0·002.

VALUES OF Π FOR CO_2 (in Atmospheres).

Temp. 30° C.	35°	40°	50°	60°	70°	80°	90°	100°	198°
(58·5)		34·3	14·2	4·9	2·4	0·5	− 1·2	− 2·1	− 8
	− 73·5	− 75·5	− 77	− 78·6	− 80·5	− 80	− 81·1	− 80·3	− 80·5
[35·6]		− 38·6	− 43	− 42	− 46·5	− 47	− 46·5	− 46·5	

The single number in ( ) refers to vapour, that in [ ] to liquid; all the others to gas. The results for volumes greater than the critical volume are very interesting.

The rest of the paper deals with (unsuccessful) attempts to apply, to Amagat's data, the equation of Van der Waals:—viz.

$$\left(p + \frac{A}{v^2}\right)(v - \beta) = RT.$$

The arguments in consequence of which the constituent A/v^2 was originally introduced and, as I have elsewhere* endeavoured to show, incorrectly introduced, were specially based upon the properties of liquids, rather than of fluids in general; and it is therefore to be expected that the formula, if valid, should be specially applicable to liquids.

The most valuable characteristic of the equation above, in addition to its special merit of giving in certain cases three real values of v, and therefore, in a sense, representing the results of Andrews and the conclusions of J. Thomson, is its simplicity. But this simplicity depends essentially upon the understanding that A, β, and R are genuine *constants*; or, at least, may be treated as such through moderate ranges of volume:—as, for instance, in the compression of an ordinary liquid by 3000 atmospheres. The equation loses its value (from this point of view) entirely if, as has been suggested, β is a sort of adjustable constant! For if it be so, it ought to be expressed as a function of v, or of v and t, and then the simplicity of the whole is gone.

Selecting, as before, a set of three corresponding pairs of values of p and v for any one temperature, we form three equations which lead to a quadratic in A, when β and R are eliminated. This involves heavy numerical work, and the results are so much modified by very slight changes in the data (quite within the limits of experimental error) that I was fain to try the simpler process of *assuming* tentative values for A, and determining the other constants from them:—the equations being then linear. But I found that very wide ranges of tentative values of A seemed to suit the conditions, to the same (extremely rough) approximation. I could get nothing satisfactory. The reason is easily found by making a case in which the labour of calculation shall be, to a considerable extent, avoided. It is clear, from the numbers in the early part of this paper, that we may lawfully assume the existence of a liquid which, for some special (ordinary) temperature, shall give

$$\Pi = 2700 \text{ atm.}, \quad e = 0{\cdot}3.$$

With these numbers the calculation is very much simplified. For such a liquid, if its volume were 1 at atmospheric pressure, would be reduced to 25/28 by 1500 atm., and to 16/19 by 3000 atm. The quadratic to which Van der Waals' formula leads, is found to have imaginary roots!

The main cause of this totally-unexpected result seems to be the factor $1/v^2$ in the term corresponding to K. Its effect is to make K increase at a rate quite inconsistent with the experimental data, at least if the rest of the equation is to retain its present form. This is easily seen by taking the following roughly approximate values of $\frac{dp}{dt}$ for ether, at constant volume, which I obtained by a graphic process from Amagat's Table 29.

v	1	·95	·9	·85
$\left(\frac{dp}{dt}\right)$, v const.,	10	12	14·5	17
p at 0° C.	1	460	1250	2570.

* *Trans. R. S. E.*, XXXVI. ii. (1891). *Ante*, No. LXXX. See also Correspondence with Lord Rayleigh and Prof. Korteweg (*Nature*, XLIV. and XLV.) [Part of this has been given on p. 208 above. 1899.]

Since Van der Waals' equation gives, for constant volume,

$$\left(\frac{dp}{dt}\right)(v-\beta)=R,$$

we easily find the approximate values

$$\beta = 0\cdot63, \quad R = 3\cdot8\,;$$

and the complete formula is something like

$$\left(p+\frac{2804}{v^2}\right)(v-0\cdot63)=1037+3\cdot8t,$$

where t is temperature centigrade.

This cannot be very far wrong, so far at least as β and R are concerned, for it gives the following calculated values of $\frac{dp}{dt}$ (at the *four* selected volumes above) which are compared with the observed values:—

Obs.	10	12	14·5	17
Calc.	10·27	11·9	14·1	17·3.

But when we calculate the corresponding pressures and compare them with those observed, we have

Obs.	1	460	1250	2570
Calc.	1	134	379	833.

The differences between the numbers in each pair are due to the very rapid increase of the K term in the formula, for moderate diminutions of volume. The following comparison is instructive. The first numbers are calculated on the hypothesis that K is inversely as v^2. Those in the second line are the corresponding values of K required to make an approximate agreement between Amagat's data, and the (numerical) formula above:—

2804	3107	3462	3881
2803	2781	2591	2144.

Thus the requisite values of K diminish rapidly, instead of increasing, as the compression proceeds. In fact it would seem as if Van der Waals' equation gives impossible roots in precisely that limited region where experiment shows that real ones are to be found. I intend soon to examine the cause of this strange result from a purely mathematical point of view.

CX.

ON THE APPLICATION OF VAN DER WAALS' EQUATION TO THE COMPRESSION OF ORDINARY LIQUIDS.

[*Proceedings of the Royal Society of Edinburgh, June* 4, 1894.]

IN a paper, read for me to the Society in January last (*ante*, No. CIX.) I pointed out the difficulties I had met with in trying to reconcile Van der Waals' equation with Amagat's experimental data for common liquids, and I promised to recur to the question when the state of my health should permit. I now find that, as I had then only surmised, the constants in Van der Waals' equation necessarily become non-real when we try to adjust it to Amagat's data.

The proof of this assertion is very simple. Suppose the equation

$$\left(p+\frac{A}{v^2}\right)(v-\beta)=BT$$

to hold for any three pairs of values of p and v; say p and a, q and b, r and c. Eliminating BT among the three resulting equations, we have

$$\beta=\frac{\left(pa+\frac{A}{a}\right)-\left(qb+\frac{A}{b}\right)}{\left(p+\frac{A}{a^2}\right)-\left(q+\frac{A}{b^2}\right)}=\frac{\left(qb+\frac{A}{b}\right)-\left(rc+\frac{A}{c}\right)}{\left(q+\frac{A}{b^2}\right)-\left(r+\frac{A}{c^2}\right)}.$$

The values of A are therefore to be found from the quadratic

$$A^2\frac{(a-b)(b-c)(c-a)}{a^2b^2c^2}+A\Sigma\left\{p\frac{b-c}{b^2c^2}(ab-bc+ca)\right\}-\Sigma\{pq(a-b)\}=0.$$

Write, for brevity, $\quad P=p\dfrac{b-c}{b^2c^2},\quad Q=q\dfrac{c-a}{c^2a^2},\quad R=r\dfrac{a-b}{a^2b^2};$

so that one at least of P, Q, R is essentially negative, if p, q, r be all positive. The condition that the values of A shall be real is

$$[\Sigma \{P(ab - bc + ca)\}]^2 + 4\Sigma \{PQ(a-b)^2 c^2\} > 0.$$

But it is an obvious theorem of ordinary algebra, that, whatever be the quantities involved, the two expressions

$$(lx + my + nz)^2 + \{xy(l-m)^2 + yz(m-n)^2 + zx(n-l)^2\}$$

and

$$(x + y + z)(l^2x + m^2y + n^2z)$$

are absolutely identical except in form.

Hence the condition for real values of A is simply that

$$(P + Q + R)\{P(ab - bc + ca)^2 + Q(ab + bc - ca)^2 + R(-ab + bc + ca)^2\}$$

shall be positive:—*i.e.* that its factors shall have the same sign.

To compare with experiment, let us take $r = 1$ atm., $c = 1$; and find the relation between the values of p and q, the pressures when the volume is reduced to $a = 0{\cdot}9$, and $b = 0{\cdot}95$, respectively.

The factors of the above quantity are

$$-p\frac{0{\cdot}05}{(0{\cdot}95)^2} + q\frac{0{\cdot}1}{(0{\cdot}9)^2} - \frac{0{\cdot}05}{(0{\cdot}95)^2(0{\cdot}9)^2}$$

and

$$-p\frac{0{\cdot}05\,(0{\cdot}805)^2}{(0{\cdot}95)^2} + q\frac{0{\cdot}1\,(0{\cdot}905)^2}{(0{\cdot}9)^2} - \frac{0{\cdot}05\,(0{\cdot}995)^2}{(0{\cdot}95)^2(0{\cdot}9)^2},$$

or, quite approximately enough for our purpose,

$$-p + 2{\cdot}228q - 1{\cdot}234$$

and

$$-p + 2{\cdot}816q - 1{\cdot}886.$$

In the latter form each has been divided by the (essentially positive) multiplier of p; and, as p and q are each of the order 1000 atm., the last terms may usually be disregarded. Thus it appears that the values of A cannot be real if p/q lie between the approximate limits 2·23 and 2·82. But from Amagat's data we easily calculate the following sufficiently accurate values:—

Ratio of Pressures at 0° C. *for Volumes* 0·9 *and* 0·95.

Water.	Bisulphide of Carbon.	Methylic Alcohol.	Ethylic Alcohol.	Chloride of Ethyl.	Propylic Alcohol.	Ether.
2·51	2·61	2·65	2·65	2·69	2·71	2·73

[The values of q range from 458 atm. in the case of ether to 1166 atm. in that of water.] All of these ratios lie well within the limits of the region in which the constants of Van der Waals' equation are non-real; though they are, as a rule, nearer to the upper than to the lower limit.

But it is well to inquire what values A assumes at the limits of this region, when it has just become real. A rough calculation shows that when $p/q = 2{\cdot}23$ we have $A = -18{\cdot}1q$ (a *tension*); and for $p/q = 2{\cdot}82$, $A = 20q$. Outside these limits A has of course two values.

It thus appears that Van der Waals' equation becomes altogether meaningless except for liquids in which the compressibility alters very much with increase of pressure:— *i.e.* for substances which have *just* assumed the liquid form under considerable pressure. For, of course, under the lower limit we are dealing with substances naturally in a state of tension. As I said in my previous paper, this state of things is due mainly to the factor $1/v^2$ with which A (if taken as corresponding to my Π) is affected. There is little doubt that the Π term in my formula does increase as the volume is diminished, but much more slowly than in the inverse ratio of the square of the volume.

(Added 6/6/94.) It may be interesting to look at the above result from a different point of view, so as to find *why* it is impossible to reconcile the general equation of Van der Waals with the experiments of Amagat.

For this purpose let us take β as independent variable, and (using the same data as before) find the value of p/q. Eliminating BT and A, we obtain the equation

$$\Sigma \left\{ p \frac{b^2 - c^2}{b^2 c^2} (a - \beta) \left(\frac{bc}{b + c} - \beta \right) \right\} = 0;$$

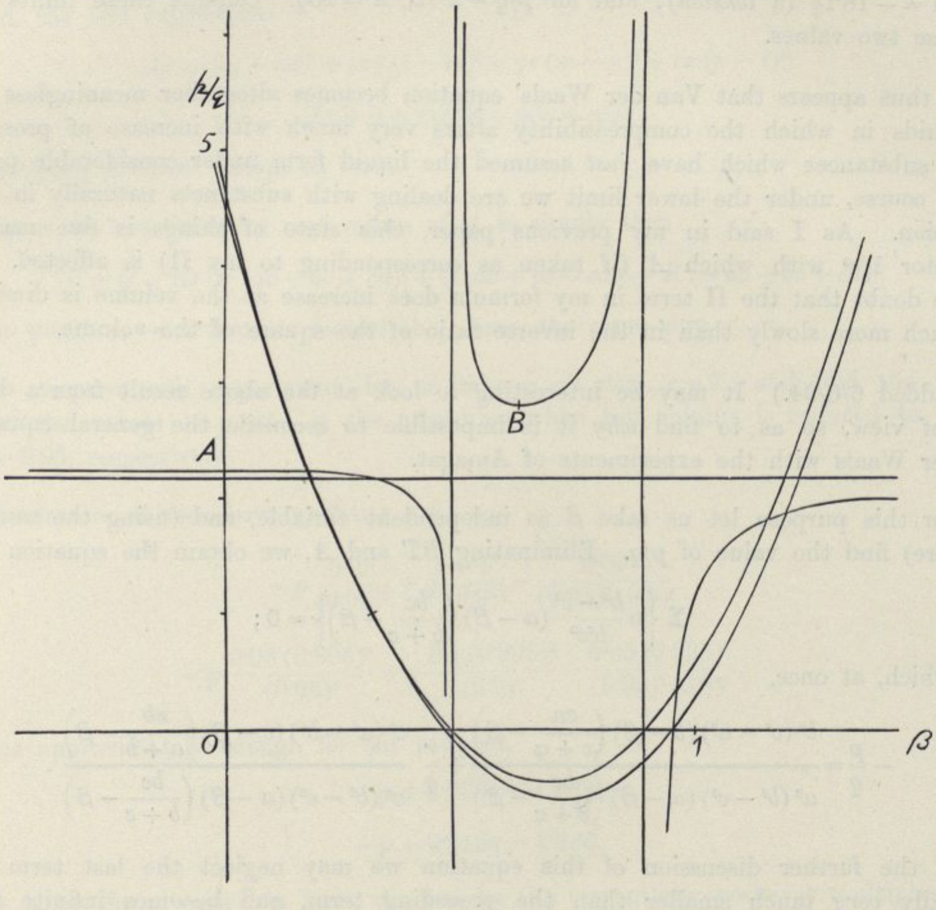
from which, at once,

$$-\frac{p}{q} = \frac{b^2 (c^2 - a^2)(b - \beta)\left(\dfrac{ca}{c + a} - \beta\right)}{a^2 (b^2 - c^2)(a - \beta)\left(\dfrac{bc}{b + c} - \beta\right)} + \frac{r}{q} \frac{c^2 (a^2 - b^2)(c - \beta)\left(\dfrac{ab}{a + b} - \beta\right)}{a^2 (b^2 - c^2)(a - \beta)\left(\dfrac{bc}{b + c} - \beta\right)}.$$

In the further discussion of this equation we may neglect the last term (which is usually *very* much smaller than the preceding term, and becomes infinite for the same values of β). Its only noticeable effect is to *slightly* alter the values of β for which p/q vanishes. We therefore have, to a quite sufficient approximation,

$$\frac{p}{q} = 2{\cdot}1712 \frac{(b - \beta)\left(\dfrac{ca}{c + a} - \beta\right)}{(a - \beta)\left(\dfrac{bc}{b + c} - \beta\right)},$$

where the literal factors have been retained in the more important portion. The value of p/q in terms of β is thus seen to be a numerical multiple of the ratio of the corresponding ordinates of two equal and similarly situated parabolas, whose vertices do not coincide. The first cuts the axis of x at b and $ca/(c + a)$, the second at a and $bc/(b + c)$, so that the second lies wholly within the first while y is negative. They intersect in the single point whose abscissa is $abc/(ab+bc+ca)$. These parabolas are shown in the cut below.



The values of p/q are the ordinates of the chief curve. This has three asymptotes:—two parallel to y, and cutting x at a and $bc/(b+c)$ respectively; and the third at a constant distance, 2·1712, from the axis of x. Its maximum ordinates are given by the equation

$$0 = \frac{d}{dx} \frac{(b-x)\left(\dfrac{ca}{c+a} - x\right)}{(a-x)\left(\dfrac{bc}{b+c} - x\right)},$$

or $$0 = (ab+bc+ca)\,x^2 - 2abcx.$$

Thus the maximum (at A in the cut) is on the axis of y; and the minimum (at B) corresponds to $x = 0{\cdot}6321$. Their values are 2·228 and 2·816 respectively; and the ordinate of the point of intersection of the construction-parabolas lies midway between them.

Thus, since the minimum numerically exceeds the maximum, the curve has no ordinate intermediate to these values; and therefore no selection of real constants can make Van der Waals' equation applicable to a liquid in which the pressure, required to reduce its volume by 10 per cent., exceeds that required for a 5 per cent. reduction, in any ratio between 2·228 and 2·816.

Moreover, in accordance with what has been said above about the term A/v^2, it is only while the ratio of pressures exceeds the higher of these limits that this term represents a pressure, and not a tension. For the graph of A/q in terms of β is easily seen to be a rectangular hyperbola whose asymptotes are parallel to the axes; cutting x at $bc/(b+c)$, and y at $b^2c^2/(b^2-c^2)$. The curve cuts x at b, and so its ordinates are positive from $bc/(b+c)$ to b, *only*.

CXI.

NOTE ON THE COMPRESSIBILITY OF SOLUTIONS OF SUGAR.

[*Proceedings of the Royal Society of Edinburgh, July* 18, 1898.]

IN continuation of former investigations of the alteration of compressibility of water, which is produced by dissolving various salts in it, I was led to imagine that some instructive results might be furnished by solutions such as those of sugar, whose bulk is nearly the sum of the bulks of their constituents:—for, in them, we might expect little change in compressibility from that of water itself; *i.e.* in accordance with my hypothetical formula, little change in the term regarded as representing the molecular pressure.

The following preliminary results have recently been obtained for me by Mr Shand, Nichol Foundationer, who employed the Fraser gun and the Amagat gauge procured for my "Challenger" work:—and a new set of piezometers of the same (Ford's) glass as that whose compressibility I had determined to be 0·0000026. These have been carefully gauged, but have not as yet been directly compared with those formerly employed.

The solutions experimented on were prepared, in Dr Crum Brown's Laboratory, by Mr W. W. Taylor, M.A., B.Sc., and contained respectively 5, 10, 15, 20 parts, by weight, of sugar to 100 of water. The temperature varied but slightly from 12°·4 C. during the whole course of the experiments.

Average Compressibility per Atmosphere, at 12°·4 C.

Sugar per 100 water	0	5	10	15	20
For first ton . . .	0·00004650	4430	4265	4109	3965
„ two tons . .	4520	4316	4160	4013	3875
„ three tons .	4410	4210	4065	3920	3789

The numbers in the first column were taken direct from the Plate in my second *Challenger* Report (*ante*, No. LXI.), 0·0000026 being (of course) added to each.

The Reciprocals of these numbers are, in order,

2151	2257	2344	2439	2522
2212	2317	2404	2492	2581
2268	2375	2460	2551	2640

Comparing with the formula, we see that these reciprocals should be, in the first column proportional to Π, $\Pi + 1$, $\Pi + 2$; in the second to $\Pi + 5x$, $\Pi + 1 + 5x$, $\Pi + 2 + 5x$; etc., where x is the increase of Π for 1 part sugar in 100 (by weight) of water.

The results are not very concordant, especially in the second and fifth columns (which seem to indicate some error in the gauging of the corresponding piezometers), but they are all fairly satisfied by taking

$$\Pi : 1 : x = 2151 : 58{\cdot}1 : 19{\cdot}2;$$

so that the actual value of Π appears to be 37 tons' weight per sq. inch.

Thus it appears that the effect of sugar is, weight for weight, barely one-third of that of common salt in reducing the compressibility of water; for, with common salt, $x = 1$ nearly.

CXII.

ON THE PATH OF A ROTATING SPHERICAL PROJECTILE.

[*Transactions of the Royal Society of Edinburgh*, Vol. XXXVII. June 5, and July 3, 1893.]

THE curious effects of rotation upon the path of a spherical projectile have been investigated experimentally by Robins and many others, of whom Magnus is one of the more recent. They have also been the subject of elaborate mathematical investigation, especially by Poisson, who has published a large treatise on the question*. For all that, we know as yet very little more about them than Newton did in 1666, when he made his famous experiments on what we now call dispersion. Writing to Oldenburg an account of these experiments in 1671–2†, he says:—

"Then I began to suspect whether the rays, in their trajection through the prism, did not move in curve lines, and according to their more or less curvity, tend to divers parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis-ball, struck with an oblique racket, describe such a curve line. For, a circular as well as a progressive motion being communicated to it by that stroke, its parts, on that side where the motions conspire, must press and beat the contiguous air more violently than on the other; and there excite a reluctancy and re-action of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient æther, on that side where the motions conspire, and thence be continually bowed to the other."

From this remarkable passage it is clear that Newton was fully aware of the effect of rotation in producing curvature in the path of a ball, also that it could be of sufficient amount to be easily noticed in the short flight of a tennis-ball; that he

* *Recherches sur le Mouvement des Projectiles dans l'Air.* Paris, 1839.

† *Isaaci Newtoni Opera quæ exstant Omnia* (Horsley), vol. IV. p. 297.

correctly described the direction of the deviation, and that he ascribed the effect to difference of air-pressure for which he assigned a cause. All that has since been done experimentally seems merely to have given various more or less striking illustrations of these facts, without any attempt to find how the deflecting force depends upon the velocities of translation and rotation: and I am not aware of any successful attempt to extend or improve Newton's suggestion of a theoretical explanation. It seems in fact to have been altogether unnoticed, perhaps even ignored.

Thus Robins*, writing some seventy years later than the date of Newton's letter, speaks of

"the hitherto unheeded effects produced by this resistance; for its action is not solely employed in retarding the motions of projectiles, but some part of it exerted in deflecting them from their course, and in twisting them in all kinds of directions from their regular track; this is a doctrine, which, notwithstanding its prodigious import to the present subject, hath been hitherto entirely unknown, or unattended to; and therefore the experiments, by which I have confirmed it, merit, I conceive, a particular description; as they are themselves too of a very singular kind."

Robins measured accurately, by means of thin screens placed across his range, the deviation (to right or left) of successive shots fired from a gun which could be exactly replaced in its normal position, after each discharge; and found that it increased much more rapidly than in simple proportion to the distance. Then he experimented successfully with a gun whose barrel was bent a little to the left near the muzzle, with the view of forcing a loose-fitting bullet to rotate by making it roll on one side of the bore. The bullet, of course, at first deviated a little to the left; but this was soon got over, and it then persistently curved away to the right. And he showed the effect of rotation very excellently by suspending a ball by two strings twisted together, so as to give rotation to it when it was made to vibrate as a pendulum. The plane of vibration rotated in the same sense as did the ball.

I have not had an opportunity of consulting, in the original, Euler's remarks on this question. The following quotations are taken from a retranslation† of his German version of Robins' work, but the statements they contain are so definite that the translator cannot be supposed to have misrepresented their meaning:—

"The cause which Mr Robins assigns for the uncertainty of the shot cannot be the true one, since we have indisputably proved, that it arises from the figure of the ball only." p. 313.

"if the ball has a progressive motion, we may, as has been already shown, consider it at rest, and the air flowing against it with the velocity of the ball's motion; for the force with which the particles of air act on the body will be the same in both cases." [Then follows an investigation.] . . . . . "hence this proposition appears

* *New Principles of Gunnery* (new edit.), 1805, p. 206. The paper referred to is stated to have been read to the Royal Society in 1747.

† "The true Principles of Gunnery investigated and explained, comprehending translations of Professor Euler's Observations, &c. &c." By Hugh Brown. London, 1277 (*sic*).

indisputably true; that a perfectly spherical body which, besides its progressive motion, revolves round its centre, will suffer the same resistance as if it had no such rotation. If, therefore, such a ball should receive two such motions in the cannon, yet its progressive motion in the air would be the very same as if it had no rotation." pp. 315–7.

Poisson's treatment of the subject is altogether unnecessarily prolix, and in consequence not very easily understood. It is sufficient to say that, like Euler, he rejects* Robins' explanation; and that his basis of investigation of the effects of rotation on the path of a homogeneous sphere really amounts to no more than this:—that, since friction is greater where the density of the air is greater, the front of the ball suffers greater friction than does the back. Thus there is a lateral force, which he shows to be very small, tending to deflect the ball as if it were rolling upon the air in front of it. As this is exactly the opposite of the effect described by Robins, I feared at first that I must have misunderstood Poisson's mathematics. But this feeling gave way to one of astonishment when I read further; for there can be no doubt of the meaning of the following passage which occurs in his comments on the investigation:—

"C'est ce que l'on peut aussi regarder comme évident *à priori*, si l'on considère que cette déviation est due à l'excès de la densité de l'air en avant du projectile, sur sa densité en arrière; excès qui donne lieu à un plus grand frottement du fluide, contre l'hémisphère antérieur, et à un moindre contre l'hémisphère postérieur . . . . il en résultera une force horizontale qui poussera ce point [the centre of inertia] dans le sens du plus grand frottement ou en sens contraire de la rotation à laquelle il répond, c'est-à-dire vers la gauche, quand les points de la partie antérieure du projectile tourneront de gauche à droite, et vers la droite, lorsqu'ils tourneront de droite à gauche." *Recherches*, &c., p. 119.

In fact, Poisson's elaborate investigation leads to no term, in the expression for the normal component of the force, which can have different values at corresponding points of the two front semihemispheres of the projectile:—and it is to a force of *this* nature that Newton's remarks and Robins' experiments alike point.

The paper of Magnus† commences with a historical sketch of the question, but it contains no reference to Newton. The author obviously cannot have read Robins' papers, for he mentions his work only once, and in the following altogether inadequate and unappreciative fashion:—

"Robins, der zuerst eine Erklärung dieser Abweichung in seinen *Principles of Gunnery* versucht hat, glaubte, dass die ablenkende Kraft durch die Umdrehung des Geschosses erzeugt werde, und gegenwärtig nimmt man dies allgemein an."

* Poisson, in fact, says of his own results:—"Néanmoins, d'après la composition de la formule qui exprime la déviation horizontale à la distance du canon où le boulet retombe sur le terrain, on reconnaît facilement que cette déviation ne peut jamais être qu'une très petite fraction de la longeur de la portée; en sorte que ce n'est pas au frottement de la surface du boulet contre la couche d'air adjacente et d'inégale densité, que sont dues principalement les déviations observées, ainsi que Robins et Lombard l'avait pensé." *Mémoire sur le Mouvement des Projectiles*, &c. *Comptes Rendus*, 5 Mars, 1838, p. 288.

† "Ueber die Abweichung der Geschosse," *Berlin Trans.*, 1852.

Had Magnus known of the experiments with the crooked gun-barrel and the rotating pendulum, he would surely have employed a stronger expression than "glaubte"! For Robins says (p. 208) of his own pendulum experiment:—

"it was always easy to predict, before the ball was let go, which way it would deflect, only by considering on which side the whirl would be combined with the progressive motion; for on that side always the deflecting power acted; as the resistance was greater here, than on the side where the whirl and progressive motion were opposed to each other."

This passage strongly resembles part of the extract already made from Newton's letter. But Robins justly adds (two words have been italicized)—

"This experiment is an *incontestible proof*, that, if any bullet, besides its progressive motion, hath a whirl round its axis, it will be deflected in the manner here described."

The one novelty in the experiments of Magnus (so far as *spherical* projectiles are concerned) consisted in blowing a stream of air against the rotating body, instead of giving it a progressive as well as a rotatory motion; thus, in fact, realizing the idea suggested by Euler in one of the quotations made above. He was thus enabled, by means of little vanes, to trace out in a very interesting and instructive manner the character of the relative motion of the air and the rotating body. This was a cylinder instead of a sphere, so the effects were greater and of a simpler character, but not so directly applicable to bullets. Otherwise, his experiments are merely corroborative of those of Robins.

But neither Robins nor Magnus gives any hint as to the form of the expression for the deflecting force, in terms of the magnitudes of the translatory and the rotatory speed. That it depends upon *both* is obvious from the fact that it does not exist when either of them is absent, however great the other may be.

1. For some time my attention has been directed to this subject by the singularly inconsistent results which I obtained when endeavouring to determine the resistance which the air offers to a golf-ball*. The coefficient of resistance which I calculated from Robins' data for iron balls, by introducing the mass and diameter of a golf-ball, was very soon found to be too small:—and I had grounds for belief that even the considerably greater value, calculated in a similar way from Bashforth's data, was also too small. Hence the reason for my attempts to determine its value, however indirectly. The roughness of the ball has probably considerable influence; and, as will be seen later, so possibly has its rotation. I collected, with the efficient assistance of Mr T. Hodge (whose authority on such matters, alike from the practical and the observational point of view, no one in St Andrews will question), a fairly complete set of data for the average characteristics of a really fine drive:—elevation at starting, range, time

* "The Unwritten Chapter on Golf," *Nature*, 22/9/87; and "Some Points in the Physics of Golf," *Ibid.*, 28/8/90, 24/9/91, 29/6/93. Also a popular article "Hammering and Driving," *Golf*, 19/2/92; where the importance of underspin is considered, mainly from the point of view of stability of motion of a projectile which is always somewhat imperfect as regards both sphericity and homogeneity.

of flight, position of vertex, &c. Assuming, as the definite result of all sound experiment from Robins to Bashforth*, that the resistance to a *spherical* projectile (whose speed is less than that of sound) varies nearly as the square of the speed, I tried to determine from my data the initial speed and the coefficient of resistance, treating the question as one of ordinary *Kinetics of a Particle.* We easily obtain, for a low trajectory, simple but sufficiently approximate expressions for the range, the time of flight, and the position of the vertex, in terms of the data of projection and the coefficient of resistance. If, then, we assume once for all an initial elevation of 1 in 4, the only disposable initial element is the speed of projection. Making various more or less probable assumptions as to its value, I found for each the corresponding coefficient of resistance which would give the datum range. Thus I obtained the means of calculating the time of flight and the position of the vertex of the path. The greater the assumed initial speed (short, of course, of that of sound) the larger is the coefficient of resistance required to give the datum range, and the more closely does the position of the vertex agree with observation; though it seems always considerably too near the middle of the path. But the calculated time of flight, which is greatest (for a given range) when there is no resistance, is always less than two-thirds of that observed:—while, for high speeds, and correspondingly high resistances, it is diminished to less than half the observed value. To make certain that this discrepancy was not due to the want of approximation in my equations, yet without the slightest hope of success in reconciling the various conflicting data, I made several calculations by the help of Bashforth's very complete tables, which carry the approximation as far as could be wished; but the state of matters seemed worse rather than better. It then became clear to me that it is impossible for a projectile to pursue, for so long a period as *six seconds*, a path of only 180 yards, no part of which is so much as 100 feet above the ground:—unless there be some cause at work upon it which can, at least partially, counteract the effect of gravity. The only possible cause, in the circumstances, is *underspin*:—and it must, therefore, necessarily characterise, to a greater or less degree, every fine drive. (And I saw at once that I had not been mistaken in the opinion, which I had long ago formed from observation and had frequently expressed, that the very longest drives almost invariably go off at a comparatively slight elevation, and are *concave upwards* for nearly half the range.) In *Nature* (24/9/91) I said:—

"it thus appears that . . . . . the rotation of the ball must play at least as essential a part in the grandest feature of the game, as it has long been known to do in those most distressing peculiarities called heeling, toeing, slicing, &c."

This conclusion, obvious as it seemed to myself, was vigorously contested by nearly all of the more prominent golfers to whom I mentioned it:—being generally regarded as a sort of accusation, implying that the best players were habitually guilty of something quite as disgraceful as heeling or toeing, even though its effects might be beneficial instead of disastrous. The physical cause of the underspin appears at once when we consider that a good player usually tries to make the motion of the club-head as nearly as possible horizontal when it strikes the ball from the tee, and that he stands a

* *On the Motion of Projectiles*, 2nd edn., London, 1890.

little behind the tee. Thus the club-head is moving at impact in a direction *not* perpendicular to the striking face; and, unless the ball be at once perfectly spherical and perfectly smooth, such treatment must give it underspin:—the more rapid the rougher are the ball and the face of the club. This is, simply, Newton's "oblique racket."

In fact, if the ball be treated as hard, and if the friction be sufficient to prevent slipping, there is necessarily a *maximum* elevation (about 34°) producible by a club moving horizontally at impact, however much "spooned" the face may be. This maximum is produced when the face of the club makes, with the sole, an angle of about 28°:—which is less than that of the most exaggerated "baffy" I have seen. This, taken along with the remark above (viz. that the longest drives usually go off at very small elevations), is another independent proof that there is considerable underspin.

Hence the practical conclusion, that the face of a spoon, if it is to do its *proper* work efficiently, ought to be as smooth as possible.

2. I next considered how to take account, in my equations, of the effects of the rotation; and it appeared to me most probable that this could be done, with quite sufficient approximation, by introducing a new force whose direction is perpendicular at once to the line of flight and to the axis of rotation of the ball:—concurrent in fact with the direction of rotatory motion of the foremost point of the surface. Various considerations tended to show that its magnitude must be at least nearly proportional to the speed of rotation and that of translation conjointly. Among these there is the simple one that its direction is reversed when either of these motions is reversed. This may be generalised; for if the vector axis, ϵ, be anyhow inclined to the vector of translation, α, the direction (why not then the magnitude also, to a constant multiplier *près*) of the deflecting force is given by $V\epsilon\alpha$. Another is that, as the resistance (*i.e.* the pressure) on the non-rotating ball is proportional to the square of the speed, the pressures on the two front semihemispheres of the rotating ball must be (on the average) proportional to $(v+e\omega)^2$ and $(v-e\omega)^2$ respectively:—where v is the speed of translation, ω that of rotation, and e a linear constant. The resultant of these, perpendicular to the line of flight, will obviously be perpendicular also to the axis of rotation, and its magnitude will be as $v\omega$. But I need not enumerate more arguments of this kind. In the absence of anything approaching to a complete theory of the phenomenon we must make some assumption, and the true test of the assumption is the comparison of its consequences with the results of observation or experiment. This I have attempted, with some success, as will be seen below.

3. Another associated question, of greater scientific difficulty but of less apparent importance to my work, was the expression for the rate of loss of energy of rotation by the ball. Is it, or is it not, seriously modified by the translation? But here I had what seemed strong experimental evidence to go on, afforded by the fact that I had often seen a sliced or heeled ball rotating rapidly when it reached the ground at the end of its devious course. This is, of course, what would be expected if the deflecting force were the only, or at least the principal, result of the rotation:—for, being always perpendicular to the direction of translation, it does no work. But, on

the other hand, if the *friction* on a rotating ball depends upon its rate of translation, the ball while flying should lose its spin faster than if its centre were at rest. This is a kind of information which might have been obtained at once from Magnus' experiments, but unfortunately was not.

4. As I felt that there was a good deal of uncertainty about the whole of these speculations, I resolved to consult Sir G. G. Stokes. I therefore, without stating any arguments, asked him whether my assumptions appeared to him to be sufficiently well-founded to warrant the expenditure of some time and labour in developing their consequences:—and I was much encouraged by his reply. For he wrote:—

"if the linear velocity at the surface, due to the rotation, is small compared with the velocity of translation, I think your suggestion of the law of resistance a reasonable one, and likely to be approximately true. This would make the deflecting force vary as $v\omega$. I think too that the resistance in the line of flight will vary nearly as v^2, irrespective of the velocity of rotation of the ball.

"As to the decrement of the energy of rotation, I think the second law which you suggested is likely to be approximately true. The linear velocity due to rotation, even at the surface where it is greatest, being supposed small, or at least tolerably small, compared with the velocity of translation, I think you are right in saying that the force acting laterally upon the ball will vary, at least approximately, as $v\omega$. If this acted through the centre, it would have no moment. But I think it will not act through the centre, though probably not far from it, so that it would have a moment varying as $v\omega$. Hence the decrement of angular velocity would vary as $v\omega$, and the decrement of energy of rotation as $\omega(-d\omega/dt)$, or as $\omega . v\omega$, or as $v\omega^2$, according to your second formula.

"However, I think the force at any point of the surface, of the nature of that which we have been considering, would act very approximately towards the centre, and therefore would have little moment, so that after all the moment of the force tending to check the rotation may depend rather on the spin directly than on its combination with the velocity of translation. But, if this be so, I doubt whether the diminution of rotation during the short time that the ball is flying is sufficient to make it worth while to take it into account."

5. For a first inquiry, and one of great consequence as enabling us to get at least general notions of the magnitude of the deflecting force, let us take the simple case of a ball, projected in a direction perpendicular to its axis of rotation, in still air, and not acted on by gravity. [This would be the case of a top or "pearie," with its axis vertical, travelling on a smooth horizontal plane.] Suppose, further, that the rate of rotation is constant. Then, in intrinsic coordinates, the equations of tangential and normal acceleration given by our assumptions are

$$\ddot{s} = -\dot{s}^2/a, \text{ and } \dot{s}^2/\rho = \dot{s}^2 \frac{d\phi}{ds} = k\omega\dot{s},$$

respectively. The second may be put in either of the forms

$$\dot{\phi} = k\omega, \text{ or } \frac{d\phi}{ds} = k\omega/\dot{s}.$$

The first shows that the direction of motion revolves uniformly; the second, that the curvature is inversely as the speed of translation. And, as the first equation gives

$$\dot{s} = V\epsilon^{-s/a},$$

the intrinsic equation of the path is evidently

$$\phi = \frac{k\omega a}{V}(\epsilon^{s/a} - 1),$$

if ϕ be measured from the initial direction of projection, and V be the initial speed. This is an endless spiral, which has an asymptote, but no multiple points, and whose curvature is

$$\frac{k\omega}{V}\epsilon^{s/a}.$$

It therefore varies continuously from nil, at negative infinite values of s, to infinity at positive infinite values. Any arc of the spiral has therefore precisely the character of the horizontal projection of the path of a sliced, toed, or heeled, golf-ball; for it is obvious at once that the curvature steadily increases with the diminishing speed of the ball, thus far justifying the assumptions made in forming the equations of motion. We have only to trace this spiral, once for all, to get the path for *any* circumstances of projection. For the asymptote is obviously parallel to

$$\phi = -\frac{k\omega a}{V} = -\alpha \text{ suppose.}$$

Measure ϕ from this direction, and the equation becomes

$$\phi = \alpha\epsilon^{s/a}.$$

a gives the length corresponding to unit in the figure; and α (which determines the point of it from which the ball starts) depends only upon a and the *ratio* of the spin to the initial speed. This, with ϕ/α and s/a interchanged, is the equation of the equiangular spiral, which would be the path if the resistance were directly as the speed.

6. This enables us to get an approximate idea of the possible value of $k\omega$ in the flight of a golf-ball. For if it be well sliced, its direction of motion when it reaches the ground is often at right angles to the initial direction, although the whole deviation from a straight path may not be more than 20 or 30 yards. Assume for a moment, what will be fully justified later, that in such a case we may have (say) $s = 480$ feet, $a = 240$ feet, and $V = 350$ foot-seconds. We see that

$$\frac{\pi}{2} = k\omega \times \frac{24}{35} \times 6{\cdot}4;$$

so that
$$k\omega = \frac{\pi}{8{\cdot}8} = 0{\cdot}357, \text{ nearly,}$$

gives a sort of average value, which may safely be used in future calculations. In the case just considered, the acceleration (at starting) due to the rotation, is $0{\cdot}357 \times 350$

or nearly four-fold that of gravity: *i.e.*, the initial deflecting force is four times the weight of the ball.

7. In trying to find the positions of the asymptote, and of the pole, of the spiral of § 5, I spent a good deal of time on integrals like

$$\int_0^\infty \frac{\sin\phi\, d\phi}{\alpha+\phi};$$

with the hope of adapting them to easy numerical calculation by transformation to others with finite limits, such as 0, $\pi/2$. Happily, I learned from Professor Chrystal that they had been tabulated by Mr J. W. L. Glaisher:—and from his splendid paper (*Phil. Trans.* 1870) I obtained at once all that I sought. In fact his $Si\phi$ and $Ci\phi$ are simply the x, y coordinates of this spiral (each divided by a); the axes being respectively the perpendicular from the pole on the asymptote, and the asymptote itself. Thus I traced at once, as shown in Plate VI. Fig. 1, the first three-quarters of a turn:—and the transformations I had already obtained enabled me to interpolate points when (after $\phi = 5$) those given in the tables were too distant from one another for sure drawing. Another help in completing the curve graphically is given by the fact that the tangent, at any point, makes with the asymptote the angle ϕ which belongs to the point. This spiral does not, perhaps, exhibit the courses of the two functions so clearly as do the separate curves given by Glaisher; but it certainly shows their mutual relation, and their maximum and minimum values, in a very striking manner.

The numbers, affixed to various points of the figured spiral, are (in circular measure) the corresponding values of ϕ, or (by the equations of § 5) they may be taken as proportional to the *times* of reaching these points by the moving ball, starting with infinite speed from an infinite distance.

8. Even in the plane problem of § 5, the introduction of the effects of a steady current of wind in the plane of motion complicates the equations in a formidable manner. Suppose ϕ be measured from the reversed direction of the wind, and let the speed of the wind be W. Then if U, with direction ψ, be the *relative* velocity of the ball with regard to the wind (for it is upon *this* that the resistance, and the deflecting force, depend), we have

$$U\cos\psi = W + \dot{s}\cos\phi,$$

$$U\sin\psi = \qquad \dot{s}\sin\phi;$$

and the equations of motion are

$$\ddot{s} = -\frac{U^2}{a}\cos(\phi-\psi) + kU\sin(\phi-\psi),$$

$$\frac{\dot{s}^2}{\rho} = \quad \frac{U^2}{a}\sin(\phi-\psi) + kU\cos(\phi-\psi);$$

where, once for all, we have written k for $k\omega$.

Putting v for $\dot{s}$, and eliminating t, these become

$$v\frac{dv}{ds} = -\frac{U}{a}(W\cos\phi + v) + kW\sin\phi,$$

$$v^2\frac{d\phi}{ds} = \frac{U}{a}W\sin\phi \qquad + k(W\cos\phi + v);$$

where, of course,

$$U^2 = W^2 + v^2 + 2Wv\cos\phi.$$

These equations reduce themselves at once to the simpler ones above treated, when we put $W=0$, and therefore $U=v$. As they stand they appear intractable, in general, except by laborious processes of quadrature. But while ϕ is small, *i.e.*, while the ball is advancing nearly in the wind's eye, they may be written approximately as

$$v\frac{dv}{ds} = -\frac{(W+v)^2}{a} + kW\phi,$$

$$v^2\frac{d\phi}{ds} = \frac{W+v}{a}W\phi + k(W+v).$$

From the first of these we see not only that the space-rate of diminution of speed is increased in the ratio $(W+v)^2/v^2$, which was otherwise obvious; but also that the rotation tends, in a feeble manner, to counteract this effect. From the second we see that the space-rate of change of direction is increased, not only by the factor $(W+v)/v$ in the term due to spin, but by a direct contribution from the resistance itself. The effect of a head-wind in producing upward curvature, even in a "skimmer," is well known; and we now see that it is, at first, almost entirely due to the underspin which, without being aware of it, long drivers necessarily give to the ball. As soon as $\sin\phi$ has, by the agency of the underspin, acquired a finite value, the direct resistance comes in to aid the underspin in further increasing it. We now see the true nature of the important service which (in the hands of a powerful player) the *nearly vertical* face of a driving putter renders against a strong wind. It enables him to give great translatory speed, with little elevation, and with just spin enough to neutralize, for the earlier part of the path, the effect of gravity.

9. Before I met with Robins' paper, I had tried his pendulum experiment in a form which gives the operator much greater command over the circumstances of rotation than does his twisting of two strings together. Some years ago, with a view to measuring the coefficient of resistance of air, even for high speeds, in the necessarily moderate range afforded by a large room, I had procured a number of spherical wooden shells, turned very thin. My object, at that time, was to make the mass as small as possible, while the diameter was considerable:—but, of course, the moment of inertia was also very small. So, when I fixed in one of them the end of a thin iron wire, the other end of which was fastened to the lower extremity of a vertical spindle which could be driven at any desired speed by means of multiplying gear, the wire suffered very little torsion, except at the moments of reversal of the spin. The pendulum vibrations of this ball showed almost perfect elliptic orbits, rotating about the centre in the same sense as did the shell:—and with angular velocity approximately

proportional to that of the shell. These two experimental results are in full accordance with the assumed law for the deflecting force due to rotation. For, the ordinary vector equation of elliptic motion about the centre is

$$\ddot{\sigma} = -m^2\sigma.$$

If the orbit rotate, with angular velocity Ω, about the vertical unit vector α, perpendicular to its plane, σ becomes

$$\rho = \alpha^{2\Omega t/\pi}\,\sigma.$$

Eliminate σ from these equations, and we have at once

$$\ddot{\rho} = -(m^2 - \Omega^2)\,\rho + 2\Omega\alpha\dot{\rho}.$$

The part of the acceleration which depends upon the motion of translation of the bob:—viz.

$$2\Omega\alpha\dot{\rho},$$

is proportional to the speed, and also to Ω, that is (by the results of observation) proportional to the rate of spin; and it is perpendicular alike to α and to the direction of translation. These statements involve the complete assumption above. The other part of the acceleration depends upon position alone, and must therefore be $-n^2\rho$, that of the non-rotating ball. Hence we see that

$$m^2 = n^2 + \Omega^2,$$

or the period in the rotating ellipse is always shortened:—whether the ball move round it in the sense of the spin or not. *This* test cannot be applied with any certainty in the experiment described above, for in general Ω is much less than n, so that m exceeds n by a very small fraction only of its value.

A very beautiful modification of this experiment consists in making the path of the pendulum bob circular, before it is set in rotation. Then rotation, in the same sense as the revolution, makes the orbit shrink and notably diminishes the period. Reverse the rotation; the orbit swells out, and the period becomes longer.

10. The equations of motion of a golf-ball, which is rotating about an axis perpendicular to its plane of flight, and moving in still air, are now easily seen to be

$$\ddot{s} = -\frac{\dot{s}^2}{a} - g\sin\phi,$$

$$\dot{\phi} = \quad k - \frac{g}{\dot{s}}\cos\phi.$$

The most interesting case of this motion is a "long drive," as it is called, where ϕ is always small, so long at least as it is positive; its utmost average value for the first two-thirds of the range being somewhere about 0·25. This applies up to, and about as much beyond, the point of contrary flexure. A little after passing that point, ϕ begins to diminish at a considerably greater rate than that at which it had previously increased.

A first approximation gives, as above,

$$\dot{s} = V\epsilon^{-s/a},$$

if we omit the term $g \sin \phi$ in the first equation. With this, the second equation gives at once, on integration,

$$\phi = \alpha + \frac{ka}{V}(\epsilon^{s/a} - 1) - \frac{ga}{2V^2}(\epsilon^{2s/a} - 1).$$

We might substitute this for $\sin \phi$ in the first equation, and so obtain a second, and now very close, approximation to the value of $\dot{s}$. But the result is far too cumbrous for convenient use in calculation. We will, therefore, be content for the present with the rudely approximate value of $\dot{s}$ written above.

Integrating again with respect to s, we have

$$\int_0^s \phi ds = \alpha s + \frac{ka^2}{V}\left(\epsilon^{s/a} - 1 - \frac{s}{a}\right) - \frac{ga^2}{4V^2}\left(\epsilon^{2s/a} - 1 - \frac{2s}{a}\right).$$

Now, for rectangular coordinates (x horizontal) and the same origin,

$$x = \int_0^s \cos \phi ds = \int_0^s \left(1 - \frac{\phi^2}{2} + \&c.\right) ds, \quad y = \int_0^s \sin \phi ds = \int_0^s \left(\phi - \frac{\phi^3}{6} + \&c.\right) ds;$$

so that, to the order of approximation we have adopted, the equation of the path is

$$y = \alpha x + \frac{ka^2}{V}\left(\epsilon^{x/a} - 1 - \frac{x}{a}\right) - \frac{ga^2}{4V^2}\left(\epsilon^{2x/a} - 1 - \frac{2x}{a}\right).$$

The only really serious defect in this approximation is the omission of $g \sin \phi$ in the first equation. This renders the value of s too large for the greater part of the path, and thus the value of y will be slightly too small up to the point of inflection, and somewhat too large up to (and some way beyond) the vertex of the path.

11. When this paper was first read to the Society, it contained a considerable number of details and sketches of the paths of golf-balls, based on three very different estimates of the constant of resistance:—respectively much less than, nearly equal to, and considerably greater than, that suggested by Bashforth's results. These details have just been printed in *Nature* (June 29), and I therefore suppress them here, replacing them by calculations based on experiments made *between* the two dates at the head of the paper. One important remark, suggested by the appearance of these curves, must, however, be made now. Whatever, from 180 to 360 feet, be assumed as the value of a, the paths required to give a range of 180 yards and a time of $6^s{\cdot}5$, have a striking family resemblance. So much do they agree in general form, that I do not think anything like an approximation to the true value of a could be obtained from eye-observations alone. We must, therefore, find a or V directly. Only the possession of a really trustworthy value of a, found by such means, would justify the labour of attempting a closer approximation than that given above. I have not as yet obtained the means of making any direct determinations of a, but I have tried to find its value indirectly; first, from experimental measures of V made some years ago by means of a ballistic pendulum; secondly, a few days ago, by (what comes nearly to the same

thing) measuring directly the speed of the club-head at impact, and thus determining the speed from the known coefficient of restitution of the ball. All of these experiments have been imperfect, mainly in consequence of the novelty of the circumstances and the feeling of insecurity, or even of danger, which prevented the player from doing his best. The results, however, seem to agree in showing that V is somewhat over 300 foot-seconds (say, for trial, 350) for a really fine drive. Taking the carry as 180 yards, and the time as 6^s, the value of a given by the formulæ above is somewhere about 240 feet. With these assumed data, the initial (direct) resistance to the ball's motion is sixteen-fold its weight. Bashforth's results for iron spheres, when we take account of the diameter and mass of a golf-ball, give about 280 feet as the value of a. The difference (if it really exist) may possibly arise from the roughness of the golf-ball, which we now see to be essential to long carry and to steady flight, inasmuch as the ball is enabled by it to take readily a great amount of spin, and to avail itself of that spin to the utmost. One of the arguments in § 2 above would give the resistance as proportional to $v^2 + e^2\omega^2$, instead of to v^2 simply.

12. We have thus all the data, except values of α and of k, required for the working out of the details of the path by means of the approximate x, y equation just given. The best course seems to be to assume values of α from 0·24 (according to Mr Hodge) down to zero; and to find for each the corresponding value of k which will make $y = 0$ for $x = 540$. This process gives the following values with $a = 240$, $V = 350$, as above:—

α	k	kV/g	$a \log kV/g$
0·24	0·182	2·00	166·3
0·12	0·246	2·69	237·5
0·0	0·309	3·37	291·6

It will be seen that the values of k are of the order pointed to by the behaviour of a sliced ball, though they are considerably less than that given in the example of § 6. This, of course, is a strong argument in favour of the present theory; for, even in the wildest of (unintentional) heeling, the face of the club is scarcely so much inclined to its direction of motion as it is in good, ordinary, driving with a grassed club. (Slicing is very much less susceptible of accurate quantitative estimation by means of eye-observations.) The third column gives the ratio of the initial deflecting force to the weight of the ball. As this is more than unit in each of the three cases, all these paths are at first concave upwards. The numbers in the fourth column indicate (in feet) the distance along the range from the origin to the point of inflexion.

The approximate equation of the first of these paths is

$$y = 57{\cdot}6\,\frac{x}{a} + 30{\cdot}05\left(\epsilon^{x/a} - 1 - \frac{x}{a}\right) - 3{\cdot}76\left(\epsilon^{2x/a} - 1 - \frac{2x}{a}\right).$$

The abscissa of the maximum ordinate is given by

$$0 = 57{\cdot}6 + 30{\cdot}05\,(\epsilon^{x/a} - 1) - 7{\cdot}52\,(\epsilon^{2x/a} - 1),$$

which leads to $\epsilon^{x/a} = 4{\cdot}93$, whence $x = 384$ nearly.

The vertex is therefore at 0·71 of the range.

13. Under exactly the same circumstances, *had there been no rotation*, the equation of the path would have been

$$y = 57{\cdot}6\,\frac{x}{a} - 3{\cdot}76\left(\epsilon^{2x/a} - 1 - \frac{2x}{a}\right).$$

This gives for $y = 0$, $\quad x = 1{\cdot}71a = 410$ feet only.

The position of the vertex is given by

$$0 = 57{\cdot}6 - 7{\cdot}52\,(\epsilon^{2x/a} - 1);$$

so that $\quad x = 258$ feet, nearly.

In this case the vertex is at 0·63 of the range, only, and the time of flight is $3^{s}{\cdot}1$.

We have here, in consequence of a very moderate spin only, (in fact about half of that given by a good slice), all other initial circumstances being the same, an exceedingly well-marked difference in character between the two paths, as well as notable differences in range, and time of flight. Thus, while a player who gives no spin has (say) a carry of 136 yards only; another, who gives the *same* initial speed and inclination of path but *also* a very moderate amount of spin, accomplishes 180 yards with ease; his ball, in fact, remaining twice as long in the air.

14. For the sake of further illustration, let us consider the course by which the ball, sent off at the same inclination, but without rotation, may be forced by mere initial speed to have a range of 540 feet. Here the condition for V is

$$0 = 129{\cdot}6 - 8\left(\frac{240}{V}\right)^2 84{\cdot}5,$$

so that the requisite speed is 548 foot-seconds; an increase of 56 per cent., involving about 2·5-fold energy of translation, which I take to be entirely beyond the power of any player. And the time of flight is reduced to $3^{s}{\cdot}7$ only, a rapidity of execution never witnessed in so long a carry. The initial resistance in this case rises to nearly forty-fold the weight of the ball. The equation of the path is

$$y = 57{\cdot}6\,\frac{x}{a} - 1{\cdot}54\left(\epsilon^{2x/a} - 1 - \frac{2x}{a}\right),$$

and the vertex is at 355, or about two-thirds of the range, only.

15. Fig. 2 shows the three paths just described, which start initially in the same direction; the uppermost is that with speed 350 and moderate spin. The lowest has the same speed, but no spin. The intermediate course, also, has no spin, but the initial speed is 548 to enable it to have a range of 540 feet. Thus the two upper paths in this figure are characteristic of the two modes of achieving a long carry:—viz. skill, and brute force, respectively. In fig. 3 the first of these paths is repeated, and along with it are given the corresponding trajectories with the same initial speed

350, but with inclinations of 0·12 and 0·0 respectively, and with the values of k, given above, which are required to secure the same common range. [To increase this range from 180 to 250 yards, even in the lowest and thus least advantageous path where there is no initial elevation, all that is required is to raise the value of kV (the initial acceleration due to rotation) from 108 to 219; *i.e.* practically to double it. V might, perhaps, be increased by from 25 to 30 per cent. by a greatly increased effort in driving:—but k is much more easily increased. A carry of 250 yards, in still air, is therefore quite compatible with our data, even if there be no initial elevation. It can be achieved, for instance, if V is 400 foot-seconds, and k about 50 per cent. greater than that which we have seen is given by a good slice. Of course it will be easier of attainment if the true value of a is greater than 240 feet. When there is no rotation there must be initial elevation; and, even if we make it as great as 1 in 4, the requisite speed of projection for a carry of 250 yards would be 1120 feet per second, or about that of sound.] Each of the curves has its vertex marked, and also its point of inflexion, when it happens to possess one. Fig. 4 gives a rough, conjectural, sketch of the probable form of the path if, other things being the same, the spin could be very greatly increased. As I do not see an easy way to a moderately approximate solution of this problem, either by calculation or by a graphic process, I intend to attempt it experimentally. I am encouraged to persevere in this by the fact that in one of the few trials which I have yet made, with a very weak bow, I managed to make a golf-ball move *point blank* to a mark 30 yards off. When the string was adjusted round the middle of the ball, instead of catching it lower, the droop in that distance was usually about 8 feet. With a more powerful bow, and with one of the thin wooden shells I have mentioned above, the circumstances will be very favourable for a path with a kink in it.

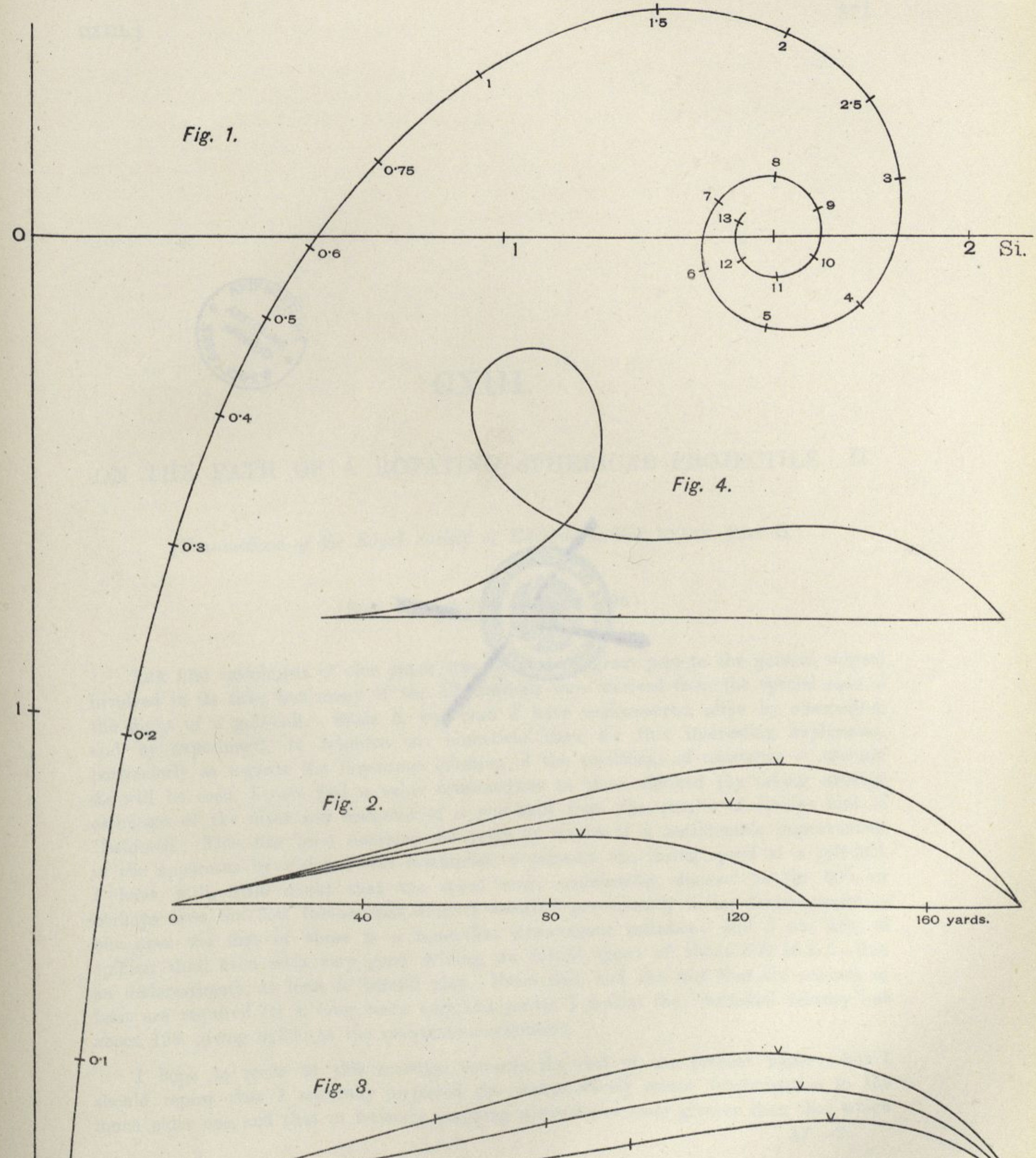


Fig. 1.
1·5
2
2·5
1
0·75
3
8
7
9
13
0
1
2
Si.
0·6
12
10
6
11
4
0·5
5
0·4
0·3
1
0·2
0·1
2
-Ci.
Fig. 4.
Fig. 2.
0
40
80
120
160 yards.
Fig. 3.

CXIII.

ON THE PATH OF A ROTATING SPHERICAL PROJECTILE. II.

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(Read 6th and 20th January, 1896.)

THE first instalment of this paper was devoted in great part to the general subject involved in its title, but many of the illustrations were derived from the special case of the flight of a golf-ball. Since it was read I have endeavoured, alike by observation and by experiment, to improve my numerical data for this interesting application, particularly as regards the important question of the coefficient of resistance of the air. As will be seen, I now find a value intermediate to those derived (by taking average estimates of the mass and diameter of a golf-ball) from the results of Robins and of Bashforth. This has been obtained indirectly by means of a considerable improvement in the apparatus by which I had attempted to measure the initial speed of a golf-ball. I have, still, little doubt that the speed may, *occasionally,* amount to the 300, or perhaps even the 350, foot-seconds which I assumed provisionally in my former paper:—but even the first of these is a somewhat extravagant estimate: and I am now of opinion that, even with very good driving, an initial speed of about 240 is not often an underestimate, at least in careful play. From this, and the fact that six seconds at least are required for a long carry (say 180 yards), I reckon the "terminal velocity" at about 108, giving $v^2/360$ as the resistance-acceleration.

I hope to recur to this question towards the end of the present paper:—but I should repeat that I naturally preferred the comparatively recent determination to the much older one, and that in formerly assuming a resistance even greater than that which

Bashforth's formula assigns, I was to some extent influenced by the consideration of the important effects of roughening or hammering a golf-ball. For I fancied that this might increase the direct resistance, as well as the effects due to rotation, by the better grip of the air which it gives to the ball. [See last sentence of § 11. Of course the assumption of increased coefficient of resistance *required* a corresponding increase of the estimate of initial speed.] The time of describing 180 yards *horizontally, i.e.*, when gravity is not supposed to act, if the initial speed is 240 and the "terminal velocity" 108, is about $5^{s}\cdot2$; and this has to be increased by at least 1^{s}, if we allow for the curvature of the path and the effect of gravity. I have employed this improved value of the coefficient of resistance in all the calculations which have been made since I obtained it. But various considerations have led me to the conclusion that the resistance, towards the end of the path, may be somewhat underrated because of the assumption that it is, throughout, proportional to the square of the speed. This point, also, will be referred to later, as I wish to make at once all the necessary comments and improvements on the part already published.

Though the present communication is thus specially devoted to some curious phenomena observed in the game of golf, it contains a great deal which has more extended application:—to which its results can easily be adapted by mere numerical alterations in the data. Therefore I venture to consider its subject as one suitable for discussion before a scientific Society.

In my short sketch of the history of the problem I failed to notice either of two comparatively recent papers whose contents are at least somewhat closely connected with it. These I will now very briefly consider.

The first is by Clerk-Maxwell* "*On a particular Case of the Descent of a Heavy Body in a Resisting Medium.*" The body is a flat rectangular slip of paper, falling with its longer edges horizontal. It is observed to rotate about an axis parallel to these edges, and to fall in an oblique direction. The motion soon becomes approximately regular; and the deflection of the path from the vertical is to the side towards which the (temporarily) lower edge of the paper slip is being transferred by the rotation. [When the rectangle is not very exact, or the longer edges not quite horizontal, or the slip slightly curved, the appearance, especially when there is bright sunlight, is often like a spiral stair-case.] Maxwell examines experimentally the distribution of currents, and consequently of pressure, about a non-rotating plane upon which a fluid plays obliquely; and shows that when the paper is rotating the consequent modification of this distribution of pressure tends to maintain the rotation. The reasoning throughout is somewhat difficult to follow, and the circumstances of the slip are very different from those of a ball:—but the direction of the deflection from the unresisted path is always in agreement with the statement made by Newton.

Much more intimately connected with our work is a paper by Lord Rayleigh† "*On the Irregular Flight of a Tennis Ball,*" in which the "true explanation" of the

* *Cambridge and Dublin Mathematical Journal*, IX. 145 (1854).

† *Messenger of Mathematics*, VII. 14 (1878).

curved path is attributed to Prof. Magnus. The author points out that, in general, the statement that the pressure is least where the speed is greatest, is true only of perfect fluids unacted on by external forces; whereas in the present case the whirlpool motion is directly due to friction. But he suggests the idea of short blades projecting from the ball, the pressure on each of which is shared by the contiguous portion of the spherical surface. Here we have practically Newton's explanation—*i.e.* the "pressing and beating of the contiguous air." Lord Rayleigh's paper contains an investigation of the form of the stream-lines when a perfect fluid circulates (without molecular rotation) round a cylinder, its motion at an infinite distance having uniform velocity in a direction perpendicular to the axis of the cylinder. And it is shown that the resultant pressure, perpendicular to the general velocity of the stream, has its magnitude proportional alike to that velocity and to the velocity of circulation. [There are some comments on this paper, by Prof. Greenhill, in the ninth volume of the journal referred to.]

In the *Beiblätter zu d. Ann. d. Phys.* (1895, p. 289) there appears a somewhat sarcastic notice of my former paper. The Reviewer, evidently annoyed at my remarks on Magnus' treatment of Robins, which he is unable directly to controvert, refers to Hélie, *Traité de Balistique,* as containing an anticipation of my own work. I find nothing there beyond a very small part of what was perfectly well known to Newton and Robins; except a few of the more immediately obvious mathematical consequences, deduced from the hypothesis (for which no basis is assigned, save that it is the simplest possible) that the transverse deflecting force due to rotation is proportional to the first power of the translational speed.

In the present article I give first a brief account of my recent attempts to determine the initial speed of a golf-ball, and consequently to approximate to the coefficient of v^2 in the assumed expression for the resistance.

Next, instead of facing the labour of the second approximation (suggested in § 10) to the solution of the differential equations, I have attempted by mere numerical calculation to take account of the effect of gravity on the speed of the projectile, and have thus been enabled to give improved, though still rough, sketches of the form of the trajectory when it is not excessively flat. This process furnishes, incidentally, the means of finding the time of passage through any arc of the trajectory.

Third, I treat of the effects of wind, regarded as a uniform horizontal translation of the atmosphere parallel, or perpendicular, to the plane of the path.

Finally, recurring to the limitation of a very flat trajectory, I have treated briefly the effects of gradual diminution of spin during the flight. This loss is shown to be inadequate to the explanation of the unexpectedly small inclination of the calculated path when the projectile reaches the ground. Hence some other mode of accounting for its nearly vertical fall is to be sought, and it is traced to the rapid diminution of the resistance (assigned by Robins' law) when the speed has been greatly reduced.

Determination of Initial Speed.

16. The bob of my new ballistic pendulum was a stout metal tube, some 3 feet long, suspended horizontally, near the floor, by two parallel pieces of clock-spring about 2·5 feet apart, and 8·63 feet long. On one end of the tube was fixed transversely a circular disc, 1 foot in diameter, covered with a thick layer of moist clay into which the ball was driven from a distance of 4 feet or so. The whole bob had a mass of about 33 lbs.; and, in the most favourable circumstances, its horizontal displacement was about 3·5 to 4 inches. As the ball's mass is 0·1 lb., the average indicated speed was thus about 200 foot-seconds*. Though I had the assistance of two long drivers, whose habitual carry is 180 yards or upwards, the circumstances of the trials were somewhat unfavourable, for there was great difficulty in hitting the disc of clay centrally. The pendulum was suspended in an open door-way; and heavy matting was disposed all about the clay so as (in Robins' quaint language) "to avoid these dangers, to the braving of which in philosophical researches no honour is annexed"; so that the whole surroundings were absolutely unlike those of a golf-course. I therefore make an allowance of 20 per cent., and (as at present advised) regard 240 foot-seconds or something like it as a fair average value of the initial speed of a really well-driven ball:—while thinking it quite possible that, under exceptionally favourable circumstances, this may be increased by 20 or 30 per cent. at least. Now, it is certain that the time of flight is usually about six seconds when the range is about 180 yards:—considerably more for a very high trajectory, and somewhat less for a very flat one. As we have by § 5 the approximate formula

$$t = \frac{a}{V}(\epsilon^{s/a} - 1),$$

we may take $a = 360$ as a reasonable estimate. This number is possibly some 10 per cent. in error, but it is very convenient for calculation, and golf-balls differ considerably from one another in density as well as in diameter. With it the "terminal velocity" of a golf-ball is about 108 foot-seconds; intermediate to the values deduced from the formulæ of Robins and of Bashforth, which I make out to be 114 and 95 respectively.

* If l be the length (in feet) of the supporting straps, d the (small) horizontal deflection of the bob, its vertical rise is obviously $d^2/2l$, so that its utmost potential energy is

$$(M+m)\, gd^2/2l,$$

where M is its mass and m that of the ball. But, if V was the horizontal speed of the ball, that of bob and ball was $mV/(M+m)$. Equating the corresponding kinetic energy to the potential energy into which it is transformed, we find at once $(M+m)\, gd^2/2l = m^2V^2/2\,(M+m)$, leading to the very simple expression

$$V = \frac{M+m}{m} d\sqrt{g/l}.$$

With the numerical values given in the text we easily find that this is equivalent to

$$V = 331\frac{D}{12}1{\cdot}93 = 53{\cdot}2D;$$

where V is, of course, in foot-seconds, but the deflection is now (for convenience) expressed in inches, and called D. Hence the numerical result in the text.

With this value of a, it is easy to see that air-resistance, alone, reduces the speed of a golf-ball to half its initial value in a path of 83 yards only. This is the utmost gain of range obtainable (other conditions remaining unchanged) by giving four-fold energy of propulsion. With the value (282) of a deduced from Bashforth's formula, this gain would have been 65 yards only! [So far for the higher speeds, but it is obvious from all ordinary experience of pendulums (with a golf-ball as bob) that slow moving bodies suffer greater resistance than that assigned by this law.]

In passing, I may mention that, on several occasions, I fastened firmly to the ball a long light tape, the further end being fixed (after all twist was removed) to the ground so that the whole was perpendicular to the direction of driving. After the 4-foot flight of the ball, the diameter at first parallel to the tape preserved its initial direction, while the tape was found twisted (in a sense corresponding to under-spin) and often through one or two *full* turns, indicating something like 60 or 120 turns per second. This is clearly a satisfactory verification of the present theory.

Numerical Approximation to Form of Path.

17. The differential equations of the trajectory were integrated approximately in § 10 by formally omitting the term in g in the first of them, that is so far as the speed is concerned. In other words:—by assuming that ϕ is always very small, or the path nearly horizontal throughout. It was pointed out that if the value of ϕ, thus obtained from the second, were substituted for $\sin\phi$ in the first, equation, we should be able to obtain a second approximation to the intrinsic equation of the path, amply sufficient for all ordinary applications. But the process, though simple enough in all its stages, is long and laborious:—and it is altogether inapplicable to the kinked path, discussed in § 15, which furnishes one of the most singular illustrations of the whole question.

The fact that one of my Laboratory students, Mr James Wood, had shown himself to be an extremely rapid and accurate calculator led me to attempt an approximate solution of the equations by means of differences:—treating the trajectory as an equilateral polygon of 6-foot sides, and calculating numerically the inclination of each to the horizon, as well as the average speed with which it is described. For we may write the differential equations in the form

$$\frac{1}{2}\frac{d(v^2)}{ds}+\frac{v^2}{a}=-g\sin\phi,$$

$$\frac{d\phi}{ds}=\frac{k}{v}-\frac{g}{v^2}\cos\phi,$$

and these involve approximately

$$v'^2-v^2+2\left(\frac{v^2}{a}+g\sin\phi\right)\delta s=0,$$

$$\phi'-\phi=\left(\frac{k}{v}-\frac{g}{v^2}\cos\phi\right)\delta s.$$

Thus we find, after a six-foot step, the new values

$$v'^2 = \left(1 - \frac{12}{a}\right) v^2 - 384 \sin \phi,$$

$$\phi' = \phi + \frac{6k}{v} - \frac{192 \cos \phi}{v^2}.$$

[If we take account of terms in $(\delta s)^2$, we find that we ought to write for $12/a$ the more accurate expression $12/a \,.\, (1 - 6/a)$. But this does not alter the *form* of the expression for v'^2. It merely increases by some 2 per cent. the denominator of the coefficient of resistance, of which our estimate is, at best, a very rough one; so that it may be disregarded. But the successive values of v^2 are all on this account too large; and thus the values of ϕ, in their turn, are sometimes increased, sometimes diminished, but only by trifling amounts. This is due to the fact that the change of ϕ depends upon terms having opposite signs; and involving different powers of v, so that their *relative* as well as their *actual* importance is continually changing. These remarks require some modification when k is such that ϕ may have large values, as for instance in the kinked path treated below. But I do not pretend to treat the question exhaustively, so that I merely allude to this source of imperfection of the investigation.]

Let, now, $a = 360$, $k = 1/3$, and suppose ϕ to be expressed in degrees. We have, to a sufficient approximation,

$$v'^2 = (v^2 - 400 \sin \phi)\left(1 - \frac{1}{30}\right),$$

$$\phi' = \phi + \frac{120}{v} - \frac{12000}{v^2} \cos \phi \left(1 - \frac{1}{30}\right),$$

and successive substitutions in these equations, starting from any assigned values of v and ϕ, will give us the corresponding values for the next side of the polygon, with the more recent estimate of the coefficient of resistance. See the two last examples in § 19 below, which lead to the trajectories figured as 5 and 6 in Plate VII.

Unfortunately, many of Mr Wood's calculations were finished before I had arrived at my new estimate of the value of a; but their results are all approximately representative of possible trajectories:—the balls being regarded as a little larger, or a little less dense, than an ordinary golf-ball; in proportion as the coefficient of resistance assumed is somewhat too great. And no difficulty arises from the assumption of too great an initial speed; for we may simply *omit* the early sides of the polygon, until we come to a practically producible rate of motion.

18. To discover how far this mode of approximation can be trusted, we have only to compare its consequences with those of the *exact* solution. For the intrinsic equation can easily be obtained in finite terms when there is no rotation. In fact,

by elimination of g between the differential equations of § 10, assuming $k=0$, we have at once the complete differential of the equation

$$\epsilon^{s/a} v \cos\phi = V\cos\phi_0 = V_0 \text{ suppose};$$

where it is to be particularly noticed that V_0 is the speed of the *horizontal component* of the velocity of projection, *not* the total speed. By means of this the second of the equations becomes

$$\frac{d\phi}{ds} = -\frac{g}{V_0^2}\epsilon^{2s/a}\cos^3\phi,$$

whence $$\frac{ag}{V_0^2}(\epsilon^{2s/a}-1) = \sec\phi_0\tan\phi_0 - \sec\phi\tan\phi + \log\frac{\sec\phi_0+\tan\phi_0}{\sec\phi+\tan\phi}.$$

The following fragments show the nature and arrangement of the results in one of the earlier of Mr Wood's calculated tables. Having assumed (for reasons stated in the introductory remarks above) that $a = 240$, I supplied him with the following formulæ:—

$$v'^2 = \left(1-\frac{1}{20}\right)v^2 - 400\sin\phi\,(1-0{\cdot}04),$$

$$\phi' = \phi - \frac{12000}{v^2}\cos\phi\,(1-0{\cdot}04),$$

and I took as initial data $V=300$, $\phi = 15°$; [whence, of course, $V_0^2 = 84{,}000$ nearly. This is required for comparison with the *exact* solution].

Working from these he obtained a mass of results from which I make a few extracts:—

$s/6$	v^2	v	$1/v$	$\Sigma(1/v)$	ϕ	$\sin\phi$	$\Sigma(\sin\phi)$	$\cos\phi$	$\Sigma(\cos\phi)$
1.	90,000	300	·00$\dot{3}$	·00$\dot{3}$	15°	·2588	·2588	·9659	·9659
2.	85,401	292·2	·00342	·00675	14·876	·2565	·5153	·9665	1·9324
3.	81,032	284·6	·00351	·01026	14·746	·2546	·7699	·9671	2·8995
	*		*		*		*		*
20.	33,045	181·8	·00550	·08666	11·028	·1914	4·6102	·9815	19·4569
21.	31,319	177·0	·00565	·09231	10·686	·1854	4·7956	·9826	20·4395
	*		*		*		*		*
40.	11,440	106·9	·00935	·23391	− 1·023	− ·0178	6·6163	·9998	39·3178
41.	10,875	104·3	·00959	·24350	− 2·030	− ·0355	6·5808	·9994	40·3172
	*		*		*		*		*
60.	5453	73·8	·01354	·46935	− 30·748	− ·5113	1·4677	·8595	58·3988
61.	5377	73·3	·01363	·48298	− 32·564	− ·5383	·9294	·8428	59·2416
	*		*		*		*		*

This table gives simultaneous values of s, v, and ϕ directly. t is obviously to be found by multiplying by 6 feet the numbers in column fifth; while by the same process we obtain rectangular coordinates, vertical and horizontal, from the eighth, and the last, columns respectively. Thus for instance we have simultaneously

s	v	t	ϕ	y	x
120	181·8	0^s·52	11°·028	27·66	116·74
240	106·9	1 ·404	− 1 ·023	39·69	235·9

(The trajectory is given as fig. 3 in the Plate, and will be further analysed in the next section of the paper.)

From the complete table we find that, in this case, ϕ is positive up to the 38th line inclusive, and then changes sign. It vanishes for $s = 233$ (approximately) after the lapse of 1^s·35. The rectangular coordinates of the vertex are about 230 and 40, and the speed there is reduced to 110. From the exact equation we find $s = 232$ for $\phi = 0°$. This single agreement is conclusive, since the earlier tabular values of s for a given value of ϕ ought to be somewhat in excess of the true values; while the later, and especially those for negative values of ϕ greater than 30° or so, should be somewhat too small:—*i.e.* the calculated trajectory has at first somewhat too little curvature, but towards the end of the range it has too much. It is easy to see that this is a necessary consequence of the mode of approximation employed:—look, for instance, at the fact that the initial speed is taken as constant through the first six feet. See also the remarks in § 17. On the whole, therefore, though the carry may possibly be a little underrated, the numerical method seems to give a very fair approximation to the truth. This admits of easy verification by the help of the value of $d\phi/ds$ last written, for it enables us to calculate the exact value of s for any assigned value of ϕ by a simple difference calculated from the result obtained from an assumed value.

19. Taking the method for what it is worth, the following are a few of the results obtained from it by Mr Wood. I give the numerical data employed, plotting the curves from a few of the calculated values of x and y. But I insert, at the side of each trajectory, marks indicating the spaces passed over in successive seconds. This would have been a work of great difficulty if we had adopted a direct process, even in cases where the intrinsic equation can be obtained exactly:—and it *must* be carried out when we desire to find the effects of wind upon the path of the ball.

Fig. 1 represents the path when $a = 240$ (properly 234), $V = 300$, $\phi_0 = 0°$, and $k = 1/3$. This will be at once recognised as having a very close resemblance to the path of a well-driven low ball. The vertex (at 0·76 of the range) and the point of contrary flexure are indicated. This trajectory does not differ very much from that given (for the same initial data) by the roughly approximate formula of § 10; which rises a little higher, and has a range of some ten yards greater. But the assumed initial speed, and consequently the coefficient of resistance, are both considerably too great.

In fig. 2 all the initial data are the same except k, which is now increased to 1/2:—*i.e.* the spin is 50 per cent. greater than in fig. 1. We see its effect mainly in the increased height of the vertex, and in the introduction of a second point of contrary flexure. A further increase of k will bring these points of contrary flexure nearer to one another, till they finally meet in the vertex, which will then be a cusp, a point of momentary rest, and *the path throughout will be concave upwards!* This is one of the most curious results of the investigation, and I have realized it with an ordinary golf-ball:—using a cleek whose face made an angle of about 45° with the shaft and was furnished with parallel triangular grooves, *biting downwards*, so as to ensure great under-spin. [The data for this case give extravagant results when employed in the formula of § 10. The vertex it assigns is 510 feet from the starting-point and at nearly 172 feet of elevation:—while the range is increased by 60 or 70 yards. And that formula can never give more than one point of contrary flexure. All this was, however, to be expected; since the formula was based on the express assumption that gravity has no direct effect on the speed of the projectile.]

Fig. 3 shows the result of dispensing altogether with initial rotation, while endeavouring to compensate for its absence by giving an initial elevation of 15°. This figure, also, will be recognised as characteristic of a well-known class of drives; usually produced when too high a tee is employed, and the player stands somewhat behind his ball. Notice, particularly, how much the carry and the time of flight are reduced, though the initial speed is the same. The slight underspin makes an extraordinary difference, producing as it were an unbending of the path throughout its whole length, and thus greatly increasing the portion above the horizon. But of course the pace of the ball, when it reaches the ground, is very much greater than in the preceding cases, it usually falls more obliquely, and it has no back-spin. On all these accounts we should expect to find that the "run" will in general be very much greater. Still, in consequence partly of the greater coefficient of resistance at low speeds, presently to be discussed, overspin (due to the disgraceful act called "topping") is indispensable for a really long run. In such a case the carry will, of course, be still further reduced, unless the initial elevation be very considerably increased. (Some of Mr Wood's numerical results, from which fig. 3 was drawn, were given in the preceding section.)

In fig. 4, a and V are as in fig. 1, but $k = 1$ and $\phi_0 = 45°$. Here we have the kink, of which a provisional sketch (closely resembling the truth) was given in the former instalment of the paper. I have not yet obtained it with a golf-ball, though as already stated I have got the length of producing the cusp above spoken of. But the kink can be obtained in a striking manner when we use as projectile one of the large balloons of thin india-rubber which are now so common. We have only to "slice" the balloon sharply downwards (in a nearly vertical plane) with the flat hand. This is a most instructive experiment, and its repetition presents no difficulty whatever. It is to be specially noticed that, in the particular kink sketched, there is a point of minimum speed somewhat beyond the vertex, and a point of maximum speed, both nearly in the same vertical with the point of projection. The first (where the speed is reduced to 58·7) is reached in a little more than two seconds, the other (where it has risen to 73·8) in rather more than four.

It may be interesting to give a few details of Mr Wood's calculations for this case:—selecting specially those near the points of maximum and minimum speed, and along with them those for closely corresponding elevations on the ascending side. Also near the vertex. The equations were

$$v_1^2 = v^2\left(1 - \frac{1}{20}\right) - 400 \sin\phi\,(1 - 0{\cdot}04)$$

$$\phi_1 = \phi + \frac{360}{v} - \frac{12000}{v^2}\cos\phi\,(1 - 0{\cdot}04)$$

$s/6$	v^2	v	$1/v$	$\Sigma(1/v)$	ϕ	$\sin\phi$	$\Sigma(\sin\phi)$	$\cos\phi$	$\Sigma(\cos\phi)$
1.	90000	300	·00$\dot{3}$	·00$\dot{3}$	45°	·7071	·7071	·7071	·7071
	*		*		*		*		*
23.	24582	156·8	·00638	·10693	78°·72	·9807	19·6186	·1956	11·3075
	*		*		*		*		*
41.	5583	74·7	·01359	·27640	145°·3	·5693	35·8751	·8221	6·2814
	*		*		*		*		*
44.	4278	65·4	·01529	·32038	166°·46	·2343	36·9422	− ·9722	3·4951
45.	3974	63·0	·01586	·33624	174°·58	·0944	37·0366	− ·9955	2·4996
46.	3739	61·1	·01636	·35260	183°·16	− ·0553	36·9813	− ·9981	1·5015
	*		*		*		*		*
48.	3475	59·0	·01697	·38630	201°·3	− ·3633	36·4078	− ·9317	− ·5921
49.	3441	58·7	·01704	·40334	210°·5	− ·5075	35·9003	− ·8616	− 1·4537
50.	3464	58·9	·01700	·42034	219°·5	− ·6363	35·2640	− ·7714	− 2·2251
	*		*		*		*		*
67.	5434	73·7	·01357	·67179	313°·1	− ·7302	20·0274	·6833	− ·3162
68.	5443	73·8	·01355	·68534	316°·5	− ·6880	19·3394	·7258	+ ·4096
69.	5435	73·7	·01357	·69891	319°·9	− ·6446	18·6948	·7646	+ ·1742
	*		*		*		*		*

The following data belong to the last elements for which the calculations were made:—

80.	4374	66·1	·01512	·85485	352°·9	− ·1224	14·6898	·9925	11·2602
81.	4202	64·8	·01542	·87027	355°·8	− ·0732	14·6166	·9973	12·2575

As the last five values of ϕ have been increasing steadily by nearly 3° for each element, it is clear that the direction of motion again rises above the horizontal; but whether the path has next a point of contrary flexure, or another kink, can only be found by carrying the calculation several steps further. [The second kink is very unlikely, as the speed is so much reduced at the point where the calculations were arrested. Mr Wood has gone to Australia, and I had unfortunately told him to stop the numerical work in this particular example as soon as he found that $\Sigma(\cos\phi)$, after becoming negative, had recovered its former maximum (positive) value.]

The trajectories represented in figs. 5 and 6 may be taken as fairly representative of ordinary good play by the two classes of drivers. For we have in both $a = 360$, $V = 200$. These are the new data, representing (as above explained) the best information I have yet acquired. In fig. 5 $k = 1/3$, $\phi_0 = 10°$; but in fig. 6 $k = 0$, $\phi_0 = 15°$. In spite of its 50 per cent. greater angle of initial elevation, the carry of the non-rotating projectile is little more than half that of the other:—and it takes only one-third of the time spent by the other in the air. But the contrast shows how much more important (so far as carry is concerned) is a moderate amount of underspin than large initial elevation. And we can easily see that initial elevation, which is always undesirable (unless there is a hazard close to the tee) as it exposes the ball too soon to the action of the wind where it is strongest, may be entirely dispensed with. This point is discussed in next section.

On account of their intimate connection with actual practice, I give a few of the numerical results for these two closely allied yet strongly contrasted cases, belonging to two different classes of driving:—choosing sides of each polygon passed at intervals of about 1^s, as well as those near the vertices and the point of contrary flexure. The formulæ for these cases are those given at the end of § 17 above:—the second term in the expression for ϕ' being omitted for the latter of the two trajectories.

For Fig. 5.

$s/6$	v^2	v	$1/v$	$\Sigma(1/v)$	ϕ	$\sin\phi$	$\Sigma(\sin\phi)$	$\cos\phi$	$\Sigma(\cos\phi)$
1.	40,000	200	·00500	·00500	10°	·1736	·1736	·9848	·9848
	*			*	*		*		*
25.	15,497	124·5	·00803	·16549	17·552	·3015	6·2345	·9534	25·2200
	*			*	*		*		*
39.	8,216	90·6	·01103	·29869	19·789	·3388	10·7983	·9410	38·4544
	*			*	*		*		*
42.	7,042	83·9	·01192	·33353	19·665	·3366	11·8116	·9417	41·2783
	*			*	*		*		*
54.	3,511	59·3	·01687	·50626	13·611	·2354	15·3925	·9719	52·7246
	*			*	*		*		*
61.	2,387	48·9	·02046	·63904	1·727	·0303	16·3078	·9996	59·6508
62.	2,296	47·9	·02088	·65992	− 0·675	− ·0120	16·2958	·9999	60·6507
	*			*	*		*		*
70.	2,249	47·4	·02109	·83155	− 21·807	− ·3714	14·5533	·9285	68·4117
	*			*	*		*		*
79.	3,157	56·2	·01780	1·00513	− 35·890	− ·5862	9·9647	·8103	76·1309
	*			*	*		*		*
89.	4,338	65·9	·01519	1·16748	− 40·840	− ·6538	3·6521	·7566	83·8830
	*			*	*		*		*
94.	4,853	69·7	·01436	1·24081	− 41·548	− ·6633	0·3507	·7484	87·6381

For Fig. 6.

1.	40,000	200	·00500	·00500	15°	·2588	·2588	·9659	·9659
	*			*	*		*		*
26.	16,035	126·6	·00790	·16507	3·523	·0613	4·5617	·9981	25·5497
	*			*	*		*		*
30.	13,940	118·1	·00847	·19809	0·472	·0082	4·6769	·9999	29·5476
31.	13,472	116·1	·00861	·20670	− 0·360	− ·0064	4·6705	·9999	30·5475
	*			*	*		*		*
44.	9,147	95·6	·01046	·33189	− 13·854	− ·2393	3·0442	·9709	43·4147
	*			*	*		*		*
52.	7,850	88·6	·01129	·41952	− 24·208	− ·4099	·3650	·9121	50·9412

I regret that Mr Wood was obliged to give up his calculations before he had worked out more than about a third of the requisite rows of figures for a trajectory differing initially from fig. 5 in the *sole* particular $\phi = 5°$ instead of 10°. This would have been still more illustrative than fig. 5 as a contrast with fig. 6. But a fairly approximate idea of its form is obtained by taking the earlier part of fig. 5, regarded as having the dotted line for its base. See a remark in § 22 below, which *nearly* coincides with this.

Effect of Wind.

20. So far, we have supposed that there is no wind. But with wind the conditions are usually very complex, especially as the speed of the wind is generally much greater at a little elevation than *close* to the ground. Hence I must restrict myself to the case of uniform motion of the air in a horizontal direction. We have in such a case merely to trace, by the processes already illustrated, *the path of the ball relatively to the air;* and thence we easily obtain the path relatively to the earth. Here, of course, it is absolutely necessary to calculate the time of passing through each part of the trajectory relative to the air. If the wind be in the plane of projection, and its speed U, the *relative* speed with which the ball starts has horizontal and vertical components $V \cos \alpha - U$, and $V \sin \alpha$, respectively. Thus, relatively to the moving air, the angle of elevation is given by

$$\tan \alpha' = \frac{V \sin \alpha}{V \cos \alpha - U},$$

and the speed is

$$V' = \sqrt{V^2 - 2UV \cos \alpha + U^2}.$$

The relative trajectory, traced from these data, must now have each of its points displaced forwards by the distance, Ut, through which the air has advanced during the time, t, required to reach that point in the relative path. Of course, for a head-wind, U is negative; and the points of the relative trajectory must be displaced backwards.

Figs. 7, 8, 9 illustrate in a completely satisfactory manner, though with somewhat exaggerated speeds and coefficient of resistance, the results of this process. Mr Wood had calculated for me the path in still air, with $a = 288$ (or, rather, 282), $V = 300$, $\phi = 6°$, $k = 1/3$. Since the time of reaching each point in this path had been incidentally calculated, it had only to be multiplied by 25, and subtracted from the corresponding abscissa, in order to give the actual path when the speed of the head-wind is about 17 miles an hour, and the initial speed about 275. (The exact values of this and of the actual angle of projection must be calculated by means of the preceding formulæ:—but they are of little consequence in so rough an illustration as the present, especially as ϕ_0 and U/V are both small.) The corresponding trajectory is shown in fig. 7. If we use the same relative path for wind of 25·5 miles per hour, the actual initial speed must be about 262·5, and the true path is fig. 8. Finally, fig. 9 gives the result with actual initial speed 250, and head-wind blowing at 34 miles an hour. Here, again, a kink is produced in the actual path, but it is due to a completely different cause from that of fig. 4. And it is specially to be noted how much the vertex is displaced towards (and even beyond) the end of the range.

21. It is not necessary to figure the result of a following wind, for such a cause merely lengthens the abscissæ in a steadily increasing ratio, and makes the carry considerably longer, while placing the vertex more nearly midway along the path. But it is well to call attention to a singularly erroneous notion, very prevalent among golfers, viz., that a following wind *carries the ball onwards!* Such an idea is, of course, altogether absurd, except in the extremely improbable case of wind moving faster than the actual initial speed of the ball. The true way of regarding matters of this kind is to remember that there is always resistance while there is relative motion of the ball and the air, and that it is less as that relative motion is smaller; so that it is reduced throughout the path when there is a following wind.

Another erroneous idea, somewhat akin to this, is that a ball rises considerably higher when driven against the wind, and lower if with the wind, than it would if there were no wind. The difference (whether it is in excess or in defect will depend on the circumstances of projection, notably on the spin) is in general very small; the often large apparent rise or fall being due mainly to perspective, as the vertex of the path is brought considerably nearer to, or further from, the player.

These approximations to the effect of wind are, as a rule, very rough; because in the open field the speed of the wind usually increases in a notable manner up to a considerable height above the ground, so that the part of the path which is most affected is that near the vertex. But the general character of the effect can easily be judged from the examples just given.

When the wind blows directly across the path, the same process is to be applied. It is easy to see that the trajectory is no longer a plane curve; and also that, in every case, the carry is increased. But, in general, "allowance is made for the wind," *i.e.* the ball is struck in such a direction as to make an obtuse angle with that of the wind, more obtuse as the wind is stronger. In this case the carry must invariably be shortened. But without calculation we can go little beyond general statements like these.

Effect of Gradual Diminution of Spin.

22. In my former paper I assumed, throughout, that the spin of the ball remains practically unchanged during the whole carry. That this is not far from the truth, is pretty obvious from the latter part of the career of a sliced or a heeled ball. If, however, in accordance with § 4, we assume *it* also to fall off in a geometric ratio with the space traversed:—an assumption which is probable rather than merely plausible; so long, at least, as we neglect the part of the loss which would occur even if the

ball had no translatory speed:—the equations of § 10 require but slight modification. For we must now write, instead of k,

$$k\epsilon^{-s/b}.$$

The time rate at which this falls off is proportional to itself and to v, directly, and to b inversely.

If we confine ourselves to the very low trajectories which are now characteristic of much of the best driving, we may neglect (as was provisionally done in § 10) the effect of gravity on the speed of the ball, and write simply

$$v = V\epsilon^{-s/a}.$$

Thus the approximate equation of the path becomes

$$\frac{dy}{dx} = \alpha + \frac{ka'}{V}(\epsilon^{x/a'} - 1) - \frac{ga}{2V^2}(\epsilon^{2x/a} - 1).$$

Here

$$\frac{1}{a'} = \frac{1}{a} - \frac{1}{b};$$

and finally $$y = \alpha x + \frac{ka'^2}{V}(\epsilon^{x/a'} - 1 - x/a') - \frac{ga^2}{4V^2}(\epsilon^{2x/a} - 1 - 2x/a),$$

where α is always very small, perhaps even negative; and may, at least for our present purpose, be neglected. Its main effect is to elevate, or depress, each point of the path by an amount proportional to the distance from the origin; and thus (when positive) it enables us to obtain a given range with less underspin than would otherwise be required.

23. For calculation it is very convenient to begin by forming tables of values of the functions

$$f(p) = \frac{\epsilon^p - 1}{p}, \text{ and } F(p) = \frac{\epsilon^p - 1 - p}{p^2} = \frac{f(p) - 1}{p};$$

for values of p at short intervals from 0 to 3 or so. [Note that the same tables are adaptable to negative values of p, since we have, obviously,

$$f(-p) = \epsilon^{-p} f(p), \text{ and } F(-p) = \epsilon^{-p}(f(p) - F(p))].$$

These we will take for granted. We may now write

$$y = \frac{x^2}{V^2}(kVF(x/a') - gF(2x/a))$$

$$\frac{dy}{dx} = \frac{x}{V^2}(kVf(x/a') - gf(2x/a)),$$

$$\frac{d^2y}{dx^2} = \frac{1}{V^2}(kV\epsilon^{x/a'} - g\epsilon^{2x/a}).$$

The range, and the horizontal distances of the vertex and of the point of contrary flexure, respectively, are given by the values of x which make the second factors vanish:—and it is curious to remark that (to the present rough approximation, of course, and for given values of a and a') these depend only upon the value of kV/g, *i.e.* the initial ratio of the upward to the downward acceleration. Thus so far as the *range* is concerned, the separate values of k and V are of no consequence, all depends on their product. But it is quite otherwise as regards the flatness of the trajectory, for the maximum height is inversely as the square of V. Of course we must remember that one indispensable condition of the approximation with which we are dealing is that the trajectory shall be very flat; and thus, if the range is to be considerable, V cannot be small, and (also of course) k cannot be very large. We have already seen how to obtain a fairly approximate value of a (say 360), but b presents much greater difficulty. We may, therefore, assume for it two moderate, and two extreme values, and compare the characteristics of the resulting paths. If b be infinite, we have the case already treated, in which the spin does not alter during the ball's flight; while, if b be less than a, the spin dies out faster than does the speed and we approximate (at least in the later part of the path) to the case of no spin. Hence we may take for the values of b the following:—∞, 900, 360, and 180:—so that a' has the respective values 360, 600, ∞, and -360. Let the carry ($\bar{x}$) be, once for all, taken as 180 yards. Then, for $y=0$, we must have $2\bar{x}/a=3$; and the respective values of $\bar{x}/a'$ are 1·5, 0·9, 0, and $-1·5$. With these arguments the values of F are, in order,

$$1·7873;\ 0·8807,\ 0·6908,\ 0·5,\ \text{and}\ 0·3258;$$

so that we have the following approximate values of the ratio kV/g

$$2·03,\ 2·59,\ 3·57,\ 5·49.$$

The first two require a moderate amount of spin, only, if we take 240 as the initial speed.

The approximate position of the vertex (x_0) of the first of these paths is given by

$$f(2x_0/a)=2·03\,f(x_0/a),\quad \text{or}\quad \epsilon^{x_0/a}=3·06,\quad (x_0/a=1·1184)$$

whence $x_0=402·6$, or about three-fourths of the carry.

The corresponding value of y is about 27 feet.

The point of contrary flexure is at $\epsilon^{x/a}=2·03$, so that $x_1=255$, and the value of $\dfrac{dy}{dx}$ there has its maximum, about 0·07 only.

In the other three paths above, the maximum ordinate and the maximum inclination both increase with the necessarily increased value of k, while the vertex and the point of inflexion both occur earlier in the path. The approximate time of flight, in all, is a little over five seconds. The paths themselves are shown, much foreshortened, in figs. 10, 11, 12, 13, where the unit of the horizontal scale is 3·6 times that of the vertical. This is given with the view of comparing and contrasting them. Fig. 14 shows the first, and flattest, of these paths in its proper form. It is clearly a fair approximation to the actual facts; and when we compare it with the others, as in the foreshortened figures, we see that the assumption of constant spin (§ 4) is probably not far from the truth. For, in the great majority of cases of drives of this character, there is observed to be very little run:—and this can be accounted for only on the assumption that there is considerable underspin left at the pitch. But it is also clear that the falling off of the spin produces comparatively little increase of the obliquity of impact on the ground, even in the exaggerated form in which these paths are drawn. Their actual inclinations to the ground have tangents about 0·49, 0·66, 0·78, and 1·08 respectively. The last, and greatest, of these angles is just over 45°.

24. It is interesting to compare this set of data, and their consequences, with those of §§ 11, 14, 15. The latter were in fair agreement with many of the more easily observed features of a good drive, but they gave too high a trajectory. The new measure of initial speed, and the consequent reduction of the estimated value of the coefficient of resistance, have led to results more closely resembling the truth.

But in all, as we have seen, there is one notable defect. The ball comes down too obliquely, and this is the case more especially when the carry is a long one, and the ball's speed therefore much reduced. I was at first inclined to attribute this to my having assumed the spin to remain constant during the whole flight. This was my main reason for carrying out the investigations described in §§ 22 *sq.* But these give little help, as we have just seen, and I feel now convinced that the defect is due chiefly to the assumption that the resistance is *throughout* proportional to the square of the speed. I intend to construct an apparatus on the principle described in § 16 above, but of a much lighter type, to measure the resistance for speed of 30 feet-seconds or so, downwards. But I shall probably content myself with verifying, if I can, the idea just suggested; leaving to some one who has sufficient time at his disposal the working out of the details when the resistance is proportional (towards the end of the path) to the speed directly, or to a combination of this with the second power. The former is considerably more troublesome than Robins' law; and a combination of the two may probably be so laborious as to damp the ardour of any but a genuine enthusiast. The possibility that the law of resistance may change its form for low speeds (*i.e.*, towards and beyond the vertex of the path) throws some doubt upon the accuracy of the determination of the coefficient of resistance from the range, the time of flight, and the initial speed. But, at present, I have no means of obtaining a more accurate approximation.

25. The whole of this inquiry has been of a somewhat vague character, but its value is probably enhanced, rather than lessened, in consequence. For the circumstances can never be the same in any two drives, even if they are essentially good ones, and made by the same player. To give only an instance or two of reasons for this:—Two balls of equal mass may have considerably different coefficients of resistance in consequence of an apparently trifling difference of diameters, or of the amount or character of the hammering:—or they may have very different amounts of resilience, due to comparatively slight differences of temperature or pressure during

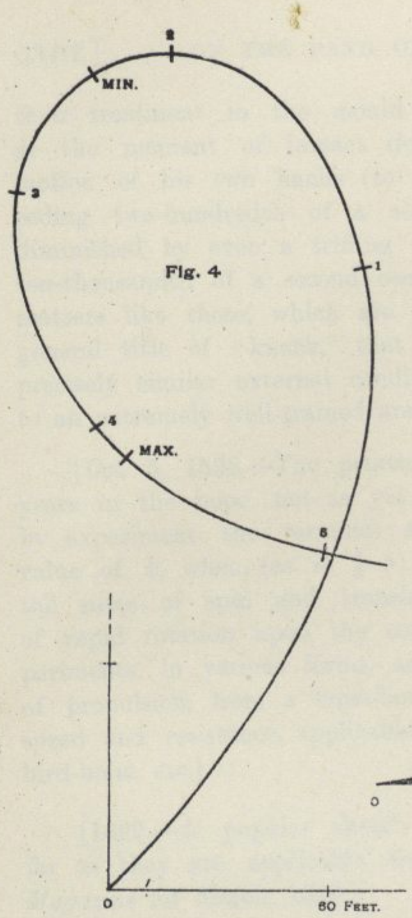


Fig. 4
MIN.
MAX.
0
60 FEET.

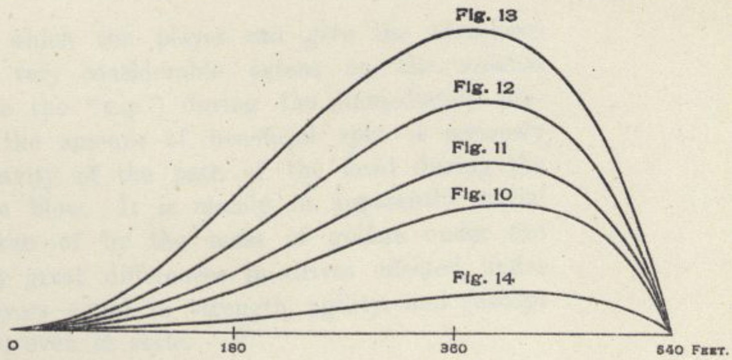


Fig. 13
Fig. 12
Fig. 11
Fig. 10
Fig. 14
0
180
360
540 FEET.

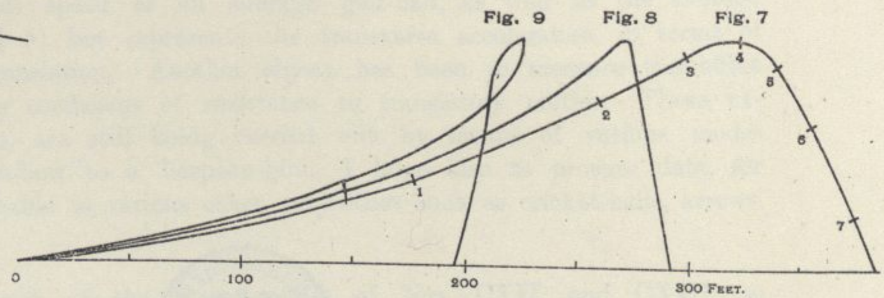


Fig. 9
Fig. 8
Fig. 7
0
100
200
300 FEET.

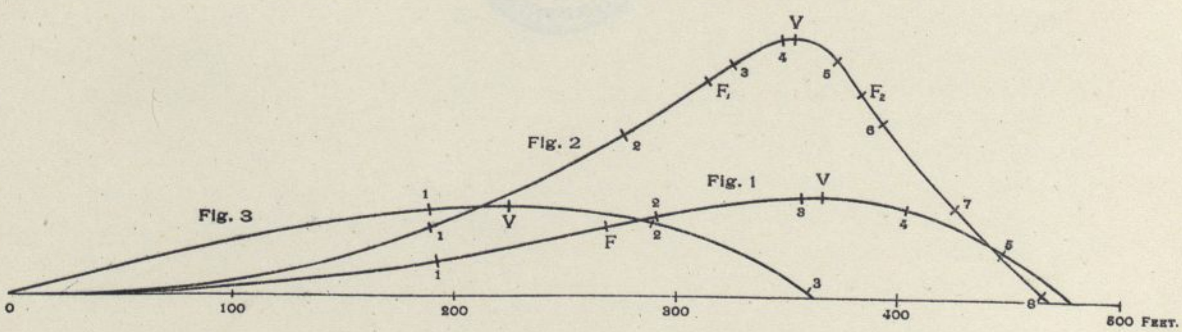


Fig. 2
Fig. 1
Fig. 3
V
F
0
100
200
300
400
500 FEET.

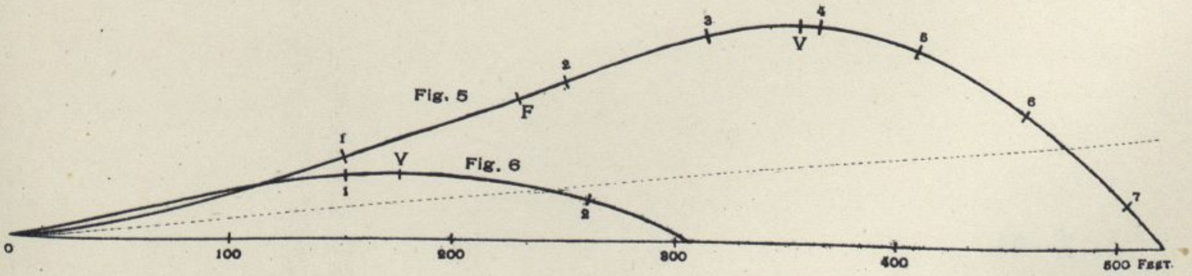


Fig. 5
Fig. 6
V
F
0
100
200
300
400
500 FEET.

their treatment in the mould. The pace which the player can give the club-head at the moment of impact depends to a very considerable extent on the *relative* motion of his two hands (to which is due the "nip") during the immediately preceding two-hundredth of a second, while the amount of beneficial spin is seriously diminished by even a trifling upward concavity of the path of the head during the ten-thousandth of a second occupied by the blow. It is mainly in apparently trivial matters like these, which are placidly spoken of by the mass of golfers under the general title of "knack," that lie the very great differences in drives effected, under precisely similar external conditions, by players equal in strength, agility, and (except to an extremely well-trained and critical eye) even in style.

[*Oct.* 5, 1898.—The printing of this paper has been postponed for nearly three years in the hope, not as yet realised, that I might be able to determine accurately by experiment the terminal speed of an average golf-ball, as well as the average value of k, when (as in § 5) $k\omega v$ represents the transverse acceleration, in terms of the rates of spin and translation. Another object has been to measure the effect of rapid rotation upon the coefficient of resistance to translatory motion. These experiments, in various forms, are still being carried out by means of various modes of propulsion, from a cross-bow to a harpoon-gun. I hope also to procure data, for speed and resistance, applicable to various other projectiles such as cricket-balls, arrows, bird-bolts, etc.]

[1899.—A popular sketch of the main results of Nos. CXII. and CXIII., so far as they are applicable to the game of Golf, will be found in the *Badminton Magazine* for March, 1896.]

CXIV.

NOTE ON THE ANTECEDENTS OF CLERK-MAXWELL'S ELECTRO-DYNAMICAL-WAVE-EQUATIONS.

[*Proceedings of the Royal Society of Edinburgh, April* 2, 1894.]

THE first obvious difficulty which presents itself, in trying to derive Clerk-Maxwell's equations from those of the elastic-solid theory, appears in the fact that the latter, being linear, do not impose any relations among simultaneous disturbances. Thus, for instance, they indicate no reason for the associated disturbances which, in Maxwell's theory, constitute a ray of polarised light. Hence it appears that we must look on the vectors of electric and magnetic force, if they are to be accounted for on ordinary dynamical principles, as being necessary concomitants, qualities, or characteristics of one and the same vector-disturbance of the ether, and not themselves primarily disturbances. From this point of view the disturbance, in itself, does not correspond to light, and may perhaps not affect any of our senses. And the very form of the elastic equation at once suggests any number of sets of two concomitants of the desired nature, which are found to be related to one another in the way required by Maxwell's equations.

For the moment, as sufficiently illustrating the essential point of the above remarks, I confine myself to disturbances, in the free ether, such as do not involve change of volume. The elastic equation is

$$\ddot{\theta} = -a^2\nabla^2\theta,$$

with the limiting condition $$S\nabla\theta = 0^*.$$

[Had not this condition been imposed, the dynamical equation would have involved, on the right, the additional term

$$(a^2 - b^2)\nabla S\nabla\theta.]$$

* Stokes, "On the Dynamical Theory of Diffraction," *Camb. Phil. Trans.*, IX. (1849).

From any vector satisfying these equations let us derive (by means of the operators d/dt and $a\nabla$, which are the only ones occurring in the equation of motion) the concomitants

$$\epsilon = \dot{\theta}, \quad \mu = -a\nabla\theta;$$

or

$$\epsilon = \ddot{\theta}, \quad \mu = -a\nabla\dot{\theta}, \text{ \&c., \&c.,}$$

and we have between them Clerk-Maxwell's equations

$$\dot{\epsilon} = a\nabla\mu, \quad \dot{\mu} = -a\nabla\epsilon,$$

with the conditions

$$S\nabla\epsilon = 0, \quad S\nabla\mu = 0.$$

The extension to dielectrics, whether they be isotropic or not, is obtained at once:— and it secures (in the latter case) all the simplicity which Hamilton's linear and vector function affords. Thus the properties of double refraction, wave-surfaces, &c., follow almost intuitively.

When we come to conducting bodies, we have to introduce further conditions. But I do not enter on these at present, as the problem is essentially altered in character. Nor do I, for the moment, discuss the bearing of the above notions upon the profound question of the possible *nature* of electricity and of magnetism.

There is a sort of analogy to the above, in the case of sound. For it is not the (vector) disturbance of the air which affects the sense of hearing, but the (scalar) concomitant change, or rate of change, of density.

Thus, possibly, the widely different results obtained by observers of the alteration of plane of polarisation in diffracted light, may *all* really be in accordance with Stokes' splendid investigation:—if we look upon light as an effect produced by the concomitants of the ether disturbance, and not directly by the ether disturbance itself.

CXV.

ON THE ELECTRO-MAGNETIC WAVE-SURFACE.

[*Proceedings of the Royal Society of Edinburgh, April* 2, 1894.]

WE may write the electro-magnetic equations of Clerk-Maxwell as

$$\phi\dot{\theta}_1 = V\nabla\theta_2, \quad \psi\dot{\theta}_2 = -V\nabla\theta_1.$$

For plane waves, running with normal velocity $v\alpha = -\mu^{-1}$, we have

$$\theta_1 = \epsilon f(vt + S\alpha\rho), \quad \theta_2 = \eta f(vt + S\alpha\rho),$$

whence at once $\qquad \phi\epsilon = V\mu\eta, \quad \psi\eta = -V\mu\epsilon,$

so that $\qquad S\mu\phi\epsilon = 0, \quad S\mu\psi\eta = 0.$

[For the moment, we assume that ϕ and ψ are self-conjugate, so that a linear function of them is also self-conjugate. And we employ the method sketched in Tait's *Quaternions*, §§ 438-9.]

We have $\qquad n\psi^{-1}\phi\epsilon = V\psi\mu\psi\eta = -V . \psi\mu V\mu\epsilon,$

or $\qquad \psi\mu S\epsilon\psi\mu = n\phi\epsilon + S\mu\psi\mu . \psi\epsilon = \varpi\epsilon,$ say.

Thus we have, to determine μ, the single scalar equation

$$S . \mu\phi\varpi^{-1}\psi\mu = S\mu(n\psi^{-1} + S\mu\psi\mu . \phi^{-1})^{-1}\mu = 0 \quad\ldots\ldots\ldots\ldots (a).$$

This is the index-surface, and the form of ϖ shows that it has two sheets:— *i.e.*, there are two values of $T\mu$ for each value of $U\mu$.

The tangent plane to the wave is

$$S\mu\rho = -1 \quad\ldots\ldots\ldots\ldots (b).$$

To shorten our work, introduce in place of ϵ the auxiliary vector

$$\tau = \varpi^{-1}\psi\mu = \epsilon / S\epsilon\psi\mu,$$

so that $$\psi\mu = n\phi\tau + S\mu\psi\mu \,.\, \psi\tau \quad\ldots\ldots\ldots\ldots(c).$$

(a) may now be written $$S\mu\phi\tau = 0 \quad\ldots\ldots\ldots\ldots(a).$$

Hence (c) gives, by operating with $S.\mu$, $S.\tau$, and $S.\psi^{-1}\rho$,

$$S\mu\psi\tau = 1 \quad\ldots\ldots\ldots\ldots(1),$$

$$1 = nS\tau\phi\tau + S\mu\psi\mu\, S\tau\psi\tau \quad\ldots\ldots\ldots\ldots(2),$$

$$-1 = nS\rho\psi^{-1}\phi\tau + S\tau\rho\, S\mu\psi\mu \quad\ldots\ldots\ldots\ldots(3).$$

These preliminaries being settled, we must find the envelope of (b) subject to the sole condition (a). We have at once by differentiation

$$S\rho d\mu = 0, \text{ and } Sd\mu\,(\phi\tau - \psi\mu S\tau\phi\tau) = 0,$$

so that $$x\rho = \phi\tau - \psi\mu S\tau\phi\tau \quad\ldots\ldots\ldots\ldots(d).$$

Treat this with the three operators used before, and we have respectively

$$x = S\mu\psi\mu\, S\tau\phi\tau \quad\ldots\ldots\ldots\ldots(4),$$

$$S\tau\rho = 0 \quad\ldots\ldots\ldots\ldots(5),$$

$$xS\rho\psi^{-1}\rho = S\rho\psi^{-1}\phi\tau + S\tau\phi\tau \quad\ldots\ldots\ldots\ldots(6).$$

By means of (5), (3) becomes $-1 = nS\rho\psi^{-1}\phi\tau$,

so that (6) takes the form $$xS\rho\psi^{-1}\rho = -\frac{1}{n} + S\tau\phi\tau \quad\ldots\ldots\ldots\ldots(6).$$

Substitute for $\psi\mu$ in (d) its value in terms of τ from (c); and x becomes, by (4) and (6), a factor of each term; so that

$$\rho = -nS\rho\psi^{-1}\rho \,.\, \phi\tau - \psi\tau \quad\ldots\ldots\ldots\ldots(d).$$

Eliminating τ between this and (5), we have finally

$$S.\rho\,(\psi + nS\rho\psi^{-1}\rho \,.\, \phi)^{-1}\rho = 0.$$

(Equation (2) above, has not been, so far, required:—but it is necessary if we desire to find the values of $S\mu\psi\mu$ and other connected quantities.)

It is obvious that, if we had originally eliminated ϵ instead of η, we should have obtained the (apparently) different form

$$S.\rho\,(\phi + mS\rho\phi^{-1}\rho \,.\, \psi)^{-1}\rho = 0.$$

It is an interesting example in the treatment of linear and vector functions to transform one of these directly into the other. (Tait's *Quaternions*, § 183.)

CXVI.

ON THE INTRINSIC NATURE OF THE QUATERNION METHOD.

[*Proceedings of the Royal Society of Edinburgh, July* 2, 1894.]

My title is purposely ambiguous, because it has to represent two things:—I intend to treat not only of what a quaternion really is, but also of its self-containedness, or independence.

Professor Cayley has just stated* that "while coordinates are applicable to the whole science of geometry, and are the natural and appropriate basis and method in the science, Quaternions seem to me a particular and very artificial method for treating such parts of the science of three-dimensional Geometry as are most naturally discussed by means of the rectangular coordinates x, y, z."

On this I would remark as follows:—

1. I have always maintained that it is not only not a reproach to, but one of the most valuable characteristics of, Quaternions that they are uniquely adapted to tridimensional space. In my Address to Section A, at the British Association Meeting in 1871 (No. XXIII. above), I said:—

"It is true that, in the eyes of the pure mathematician, Quaternions have one grand and fatal defect. They cannot be applied to space of n dimensions, they are contented to deal with those poor three dimensions in which mere mortals are doomed to dwell, but which cannot bound the limitless aspirations of a Cayley or a Sylvester. From the physical point of view this, instead of a defect, is to be regarded as the greatest possible recommendation. It shows, in fact, Quaternions to be a special instrument so constructed for application to the *Actual* as to have thrown overboard

* "Coordinates versus Quaternions," *Proc. R.S.E.*, July 2, 1894; or *Collected Papers*, No. 962.

everything which is not absolutely necessary, without the slightest consideration whether or no it was thereby being rendered useless for applications to the *Inconceivable.*"

2. Whether Quaternions are to be regarded as artificial, or the reverse, will obviously depend wholly upon what is to be understood by the term Quaternions. This forms the main object of the present paper.

3. Though the passage quoted above contains no statement as to the relative merits of Quaternions, and Coordinates, as instruments (in the region which is common to them), it is clear from other passages in his paper that Prof. Cayley holds that Quaternions are, at best, superfluous:—he allows that they enable us to effect great abbreviations, but he insists that, to be applied or even understood, they must be reconverted into the x, y, z elements of which they are, in his view, necessarily composed.

But their Inventor himself, who certainly devoted vastly more time and attention to Quaternions than it can have been possible for Prof. Cayley to devote, took a very different view of the matter:—

"It is particularly noteworthy that [Quaternions were] invented by one of the most brilliant Analysts the world has yet seen, a man who had for years revelled in floods of symbols rivalling the most formidable combinations of Lagrange, Abel, or Jacobi. For him the most complex trains of formulæ, of the most artificial kind, had no secrets:—he was one of the very few who could afford to dispense with simplifications: yet, when he had tried Quaternions, he threw over all other methods in their favour, devoting almost exclusively to their development the last twenty years of an exceedingly active life."

It will be gathered from what precedes that, in my opinion, the term Quaternions means one thing to Prof. Cayley and quite another thing to myself:—thus

To Prof. Cayley Quaternions are mainly a Calculus, a species of Analytical Geometry; and, as such, *essentially* made up of those coordinates which he regards as "the natural and appropriate basis of the science." They artfully conceal their humble origin, by an admirable species of packing or folding:—but, to be of any use, they

———doubly dying, must go down
To the vile dust from whence they sprung!

To me Quaternions are primarily a mode of representation:—immensely superior to, but of essentially the same kind of usefulness as, a diagram or a model. They *are*, virtually, the thing represented: and are thus antecedent to, and independent of, coordinates: giving, in general, all the main relations, in the problem to which they are applied, without the necessity of appealing to coordinates *at all*. Coordinates may, however, easily be *read into* them:—when anything (such as metrical or numerical detail) is to be gained thereby. Quaternions, in a word, *exist* in space, and we have only to recognize them:—but we have to *invent* or *imagine* coordinates of all kinds. The grandest characteristic of Quaternions is their transparent intelligibility. They give the spirit, as it were, leaving the mere letter aside, until or unless, it seems necessary to attend to that also. In this respect they give a representation analogous to the real image of a planet in the focus of an object-glass or mirror:—all that is obtainable is *there*, and you may apply your microscopes and micrometers to it if you please. But, theoretically at least, you may dispense with them and have recourse to your eyes and your yard-stick alone, if you increase your focal length, and along with it the aperture, of your object-glass sufficiently. Of course Newton's "most serene and quiet air" would be indispensable. For the development of this feature of my subject, and for illustrative examples, I refer to the B. A. Address above cited; and to the Address to the Edinburgh University Physical Society (No. XCVII. above), alluded to by Prof. Cayley.

To those who have read Poe's celebrated tale, *The Purloined Letter*, it will be obvious that the contrast between these two views of Quaternions is even greater than that between the Parisian Police and M. Dupin himself, though of very much the same kind.

There was a time, in their early history, when Professor Cayley's view of Quaternions was not merely a correct one, it was the *only* possible one. But, though the name has not been altered, the thing signified has undergone a vital change. To such an extent, in fact, that we may almost look upon the Quaternion of the latter half of this century as having, from at least one point of view, but little relation to that of the seven last years of the earlier half.

Hamilton's extraordinary *Preface* to his first great book shows how from Double Algebras, through Triplets, Triads, and Sets, he finally reached Quaternions. This was the genesis of the Quaternion of the forties, and the creature then produced is still essentially the Quaternion of Professor Cayley. It is a magnificent analytical conception; but it is nothing more than the full development of the system of imaginaries i, j, k; defined by the equations

$$i^2 = j^2 = k^2 = ijk = -1,$$

with the associative, but *not* the commutative, law for the factors. The novel and splendid points in it were the treatment of all directions in space as essentially alike in character, and the recognition of the unit vector's claim to rank also as a quadrantal versor. These were indeed inventions of the first magnitude, and of vast importance. And here I thoroughly agree with Prof. Cayley in his admiration. Considered as an analytical system, based throughout on pure imaginaries, the Quaternion method is elegant in the extreme. But, unless it had been *also* something more, something very different and much higher in the scale of development, I should have been content to admire it:—and to pass it by.

It has always appeared to me that, magnificent as are Hamilton's many contributions to mathematical science:—his Fluctuating Functions, and his Varying Action, for instance:—nothing that he (or indeed any other man) ever did in such matters can be regarded as a higher step in pure reasoning than that which he took when he raised Quaternions from the comparatively low estate of a mere system of *Imaginaries* to the proud position of an Organ of Expression; giving simple, comprehensive, and (above all) transparently intelligible, embodiment to the most complicated of *Real* geometrical and physical relations. *From the most intensely artificial of systems arose, as if by magic, an absolutely natural one!*

Most unfortunately, alike for himself and for his grand conception, Hamilton's nerve failed him in the composition of his first great Volume. Had he then renounced, for ever, all dealings with i, j, k, his triumph would have been complete. He spared Agag, and the best of the sheep, and did not utterly destroy them! He had a paternal fondness for i, j, k; perhaps also a (not unnatural) liking for a meretricious title such as the mysterious word *Quaternion;* and, above all, he had an earnest desire to make the utmost return in his power for the liberality shown him by the authorities of Trinity College, Dublin. He had fully recognized, and proved to others, that his i, j, k were mere excrescences and blots on his improved method:—but he unfortunately considered that their continued (if only partial) recognition was indispensable to the reception of his method by a world steeped in Cartesianism! Through the whole compass of each of his tremendous volumes one can find traces of his desire to avoid even an allusion to i, j, k; and, along with them, his sorrowful conviction that, should he do so, he would be left without a single reader. There can be little doubt that, by thus taking a course which he *felt* to be far beneath the ideal which he had attained, he secured for Quaternions at least the temporary attention of mathematicians. But there seems to me to be just as little doubt that in so doing he led the vast majority of them to take what is still Professor Cayley's point of view; and thus, to regard Quaternions as (apparently at least) obnoxious to his criticisms. And I further believe that, *to this cause alone,* Quaternions owe the scant favour with which they have hitherto been regarded.

[I am quite aware that, in making such statements, I inferentially condemn (to some extent, at least) the course followed in my own book. But, since my relations with Hamilton in the matter have been alluded to more than once, and alike incompletely and incorrectly, by Hamilton's biographer, I may take this opportunity of making a slight explanation, not perhaps altogether uncalled for. That Hamilton can altogether have forgotten the permission (limited as it was) which he had given me, when, a little later, I proposed to avail myself of it (*strictly within the limits imposed*), seems incredible. Mr Graves should either have let the matter alone, or have gone into much greater detail about it. As it stands, he virtually represents Hamilton as being unaccountably capricious. The following extract from the letter (of date July 10, 1859) in which Hamilton gave his sanction to my writing a book on the subject, speaks for itself. I had, of course, no rights in the matter:—and I cheerfully submitted to the restrictions he imposed on me; especially as I understood that he expressly (and most justly) desired to be the first to give to the world his system in its vastly improved form.

"[2.] If I shall go on to speak of my views, wishes, or feelings, on the subject of future publication, I request you beforehand to give to any such expression of mine your most indulgent construction; and not to attribute to me any jealousy of you, or any wish to interfere, in any way, with your freedom, as Author and as Critic.

[3.] If we were altogether strangers, I could have no right to address you on such a subject at all. [Here follow, as an example, some allusions (which need not be quoted) to a then recent pamphlet of Möbius, dealing with the Associative Principle in Quaternion Multiplication.] But between you and me, the case is perhaps not exactly similar; as we have so freely corresponded, and as you are an Author in the same language, and of the same country:—England, Scotland, and Ireland, being here held to have their sons compatriots.

[4.] To Möbius's excellent Pamphlet, it is likely that I may return. Meanwhile I trust that it cannot be offensive to you, if I confess,—what indeed your No. 38 encourages me to state,—that in any such future publication on the Quaternions as you do me the honour to meditate, I should *prefer* the *establishment* of 'PRINCIPLES' being left, for some time longer,—say even 2 or 3 years,—in my own hands. Open to improvement as my treatment of them confessedly is, I wish that improvement, at least to some extent, to be made and published by myself. Briefly, I should like (I own it) that no book, so much more attractive to the mathematical public than any work of mine, as a book of yours is likely to be, should have the appearance of laying a 'FOUNDATION': although the richer the 'SUPERSTRUCTURE,' on a previously laid foundation, may be, the better shall I be pleased. I think, therefore, that you may be content to *deduce* the Associative Law, from the *rules* of i, j, k; leaving it to me to consider and to *discuss* whether it might not have been a fatal *objection* to these *rules*, if they had been found to be *inconsistent* with that PRINCIPLE.

. . . .

[7.] For calculation, you know, the *rules* of i, j, k are a *sufficient basis*, although of course we have continual need for transformations, such as

$$V\gamma V\beta\alpha = \alpha S\beta\gamma - \beta S\gamma\alpha,$$

which may at last be reduced to *consequences* of those rules; and also require some *Notation*, such as S, V, K, T, U, which I have been glad to find that you are willing, *at least for the present*, to retain and to employ. But my peculiar turn of mind makes me dissatisfied without seeking to go deeper into the *philosophy* of the whole subject, although I am conscious that it will be imprudent to attempt to gain any lengthened hearing for my reflections. In fact I hope to get much more rapidly on to *rules* and *operations*, in the MANUAL than in the LECTURES; although I cannot consent to neglect the occasion of developing more fully my *conception* of the MULTIPLICATION OF VECTORS, and of seeking to establish such mult[iplication] as a much *less arbitrary* process, than it may seem to most readers of my former book to be."

I do not now think that Hamilton, with the "peculiar turn of mind" of which he speaks, could ever, *in a book*, have conveyed adequately to the world his new conception of the Quaternion. I got it from him by correspondence, and in conversation. When he was pressed to answer a definite question, *and could be kept to it*, he replied in ready and effective terms, and no man could express *vivâ voce* his opinions on such subjects more clearly and concisely than he could:—but he perpetually planed and re-polished his printed work at the risk of attenuating the substance: and he fatigued and often irritated his readers by constant excursions into metaphysics. One of his many letters to me gave, in a few dazzling lines, the whole substance of what afterwards became a Chapter of the *Elements;* and some of his *shorter* papers in the *Proc. R. I. A.* are veritable gems. But these were dashed off at a sitting, and were not planed and repolished.

Should I be called upon, in the future, to produce a fourth edition of my book, the Chapter which Prof. Cayley so kindly furnished for the third edition will probably preserve by far the greater part of the allusions to i, j, k (except, of course, the necessary introductory and historical ones) which it will contain.]

In the sense above explained, I consider Prof. Cayley's remarks to be so far warranted, hard to bear though some of them undoubtedly are. But the Quaternion, when it is regarded from the true point of view, is seen to be untouched, in fact unassailable, by any criticism based upon such grounds as reference to coordinates. It occupies a region altogether apart. To compare it to a pocket map is to regard it as a mere artificial mode of wrapping up and concealing the i, j, k or the x, y, z which are supposed to be its ultimate constituents. To be of any use it must be unfolded, and its neatly hidden contents turned out. But, from my point of view, this comparison is entirely misleading. The quaternion exists, as a space-reality, *altogether independent of* and *antecedent to* i, j, k or x, y, z. *It* is the natural, *they* the altogether artificial, weapon. And I venture further to assert (1) that if Descartes, or some of his brilliant contemporaries, had recognised the quaternion, (and it is quite conceivable that they might have done so), science would have then advanced with even more tremendous strides than those which it has recently taken; and (2) that the wretch who, under such conditions, had ventured to introduce i, j, k, would have been justly regarded as a miscreant of the very basest and most depraved character: possibly subjected to "brave punishments," the *peine forte et dure* at the very least! In a word, Hamilton INVENTED the Quaternion as Prof. Cayley sees it; he afterwards DISCOVERED the Quaternion as I see it.

If Quaternions are to be compared to a map, at all, they ought to be compared to a *contoured* map or to a model in relief, which gives not only all the information which can be derived from the ordinary map but something more:—something of the very highest importance as regards the features of a country.

A much more natural and adequate comparison would, it seems to me, liken Coordinate Geometry (Quadriplanar or ordinary Cartesian) to a steam-hammer, which an expert may employ on any destructive or constructive work *of one general kind*, say the cracking of an egg-shell, or the welding of an anchor. But you must have your expert to manage it, for without him it is useless. He has to toil amid the heat, smoke, grime, grease, and perpetual din of the suffocating engine-room. The work has to be brought to the hammer, for it cannot usually be taken to its work. And it is not, in general, transferable; for each expert, as a rule, knows, fully and confidently, the working details of his own weapon only. Quaternions, on the other hand, are like the elephant's trunk, ready at *any* moment for *anything*, be it to pick up a crumb or a field-gun, to strangle a tiger, or to uproot a tree. Portable in the extreme, applicable anywhere:—alike in the trackless jungle and in the barrack square:—directed by a little native who requires no special skill or training, and who can be transferred from one elephant to another without much hesitation. Surely this, which adapts itself to its work, is the grander instrument! But then, *it* is the natural, the other the artificial, one.

The naturalness of Quaternions is amply proved by what they have effected on their first application to well-known, long threshed-out, plane problems, such as seemed particularly ill-adapted to treatment by an essentially space-method. Yet they gave, at a glance, the kinematical solution (perfectly obvious, no doubt, *when found*) of that problem of Fermat's which so terribly worried Viviani! And, without them, where would have been even the Circular Hodograph, with its marvellous power of simplifying the elementary treatment of a planet's orbit? I could give many equally striking instances.

As to the necessity, in modern mathematical physics, for *some* substitute for what I must (with all due deference to Prof. Cayley) call the cumbersome, unnatural, and unwieldy mechanism of coordinates, I have elsewhere fully expressed my own opinion, and need not repeat it.

Of course it will be obvious from what precedes that I adhere to every word of the first extract which Prof. Cayley has made from my original *Preface.*

The phrase which he afterwards extracts for comment:—"such elegant trifles as Trilinear Coordinates":—seems somewhat too sweeping, and I should certainly hesitate to use it without qualification. But the context shows that, in my *Preface*, it was used to characterize the so-called "Abridged Notation" which had then been for some years introduced into Cambridge reading and examinations, not at all because of its superiority in completeness to the ordinary x, y system:—and therefore not on scientific grounds:—but mainly for the purpose of "aggravating" students, whether in the lecture-room or in the Senate House, at very small additional labour on the part of the lecturer or the examiner. But I made no reference whatever to Quadriplanar Coordinates; for which I feel all due respect, not altogether free from an admixture of wholesome awe!

CXVII.

SYSTEMS OF PLANE CURVES WHOSE ORTHOGONALS FORM A SIMILAR SYSTEM.

[*Proceedings of the Royal Society of Edinburgh, May* 6, 1895.]

(*Abstract.*)

WHILE tracing the lines of motion and the meridian sections of their orthogonal surfaces for an infinite mass of perfect fluid disturbed by a moving sphere:—the question occurred to me, "When are such systems similar?" In the problem alluded to the equations of the curves are, respectively,

$$(r/a)^2 = \cos\theta, \text{ and } (r/b)^{\frac{1}{2}} = \sin\theta.$$

It was at once obvious that any sets of curves such as

$$(r/a)^m = \cos\theta \text{ and } (r/b)^{\frac{1}{m}} = \sin\theta$$

are orthogonals. But they form *similar* systems only when

$$m^2 = 1.$$

Hence the only sets of similar orthogonal curves, having equations of the above form, are (*a*) groups of parallel lines and (*b*) their electric images (circles touching each other at one point). As the electric images of these, taken from what point we please, simply reproduce the same system, I fancied at first that the solution must be unique:—and that it would furnish an even more remarkable example of limitation than does the problem of dividing space into infinitesimal cubes. (See No. CV. above.)

But I found that I could not prove this proposition; and I soon fell in with an infinite class of orthogonals having the required property. These are all of the type

$$r\frac{d\theta}{dr} = (\tan\theta)^{2m+1} \quad \text{..........(1)},$$

which includes the straight lines and circles already specified. The next of these in order of simplicity among this class is

$$r = a\epsilon^{\frac{1}{2\cos^2\theta}}\cos\theta,$$

with

$$r = b\epsilon^{\frac{1}{2\sin^2\theta}}\sin\theta.$$

In order to get other solutions from any one pair like this, we must take its electric image from a point whose vector is inclined at $\pi/4$ or $3\pi/4$ to the line of reference. For such points alone make the images similar. And a peculiarity now presents itself, in that the new systems are not directly superposable:—but each is the perversion of the other.

If we had, from the first, contemplated the question from this point of view, an exceedingly simple pair of solutions would have been furnished at once by the obviously orthogonal sets of logarithmic spirals

$$r = a\epsilon^{\theta}, \quad r = b\epsilon^{-\theta};$$

and another by their electric images taken from any point whatever. The groups of curves thus obtained form a curious series of spirals, all but one of each series being a continuous line of finite length whose ends circulate in opposite senses round two poles, and having therefore one point of inflection. The excepted member of each series is of infinite length, having an asymptote in place of the point of inflection. This is in accordance with the facts that:—a point of inflection can occur in the image only when the circle of curvature of the object curve passes through the reflecting centre, and that no two circles of curvature of a logarithmic spiral can meet one another. [See No. CXVIII. below.]

We may take the electric images of these, over and over again, provided the reflecting centre be taken always on the line joining the poles. All such images will be cases satisfying the modified form of the problem.

If we now introduce, as a factor of the right-hand member of (1), a function of θ which is changed into its own reciprocal (without change of sign) when θ increases by $\pi/2$, we may obtain an infinite number of additional classes of solutions of the original question; and from these, by taking their electric images as above, we derive corresponding solutions of the modified form. We may thus obtain an infinite number of classes of solutions where the equations are expressible in ordinary algebraic, not transcendental, forms.

Thus we may take, as a factor in (1), $\tan^2(\theta+\alpha)$. The general integral is complicated, so take the very particular case of $m=1$, $\alpha=\pi/4$. This gives the curves

$$r = a\,\frac{\tan\theta\sec\theta}{(1+\tan\theta)^2}\,\epsilon^{2/(1+\tan\theta)}.$$

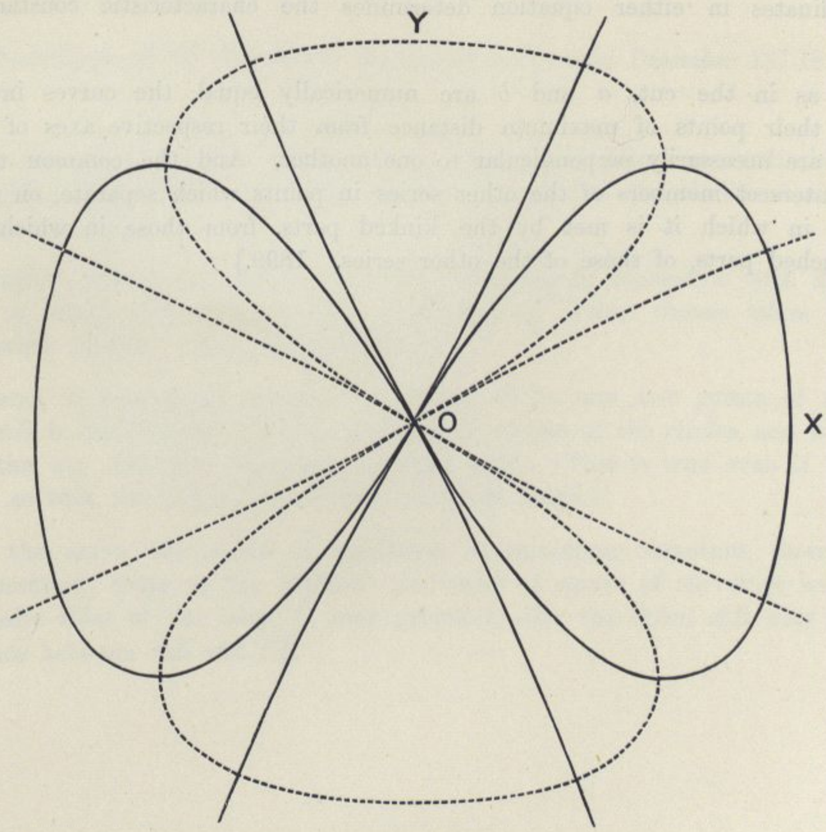
Again, let the factor be $\tan(\theta-\alpha)\tan(\theta+\alpha)$. With $m=1$, and $\tan\alpha = 1/\sqrt{3}$, we get the remarkably simple form

$$\frac{x}{a} = 1 - \frac{y^2}{3x^2}.$$

But such examples may be multiplied indefinitely.

[As the last example given above, though a specially simple one, is curious from several points of view, I append a tracing of the four curves

$$\frac{x}{a} = 1 - \frac{1}{3}\frac{y^2}{x^2}, \text{ and } \frac{y}{b} = 1 - \frac{1}{3}\frac{x^2}{y^2},$$



for the particular cases of *numerical* equality between a and b. The $\pm a$ curves are full, the others dotted.

Whatever be the values of a and b, we have at an intersection of these curves

$$\frac{dy}{dx} = \frac{3}{2}\frac{y^2 - x^2}{xy} = -3\cot 2\theta, \text{ and } \frac{dy}{dx} = \frac{2}{3}\frac{xy}{x^2 - y^2} = \tfrac{1}{3}\tan 2\theta,$$

respectively, so that their orthogonality is obvious.

Each of them consists of a single symmetrical kink, without contrary flexure; having its double point at the origin, where its (infinite) branches cross its axis at angles of $\pm 60°$.

Their form is, of course, *unique*, the constant determining merely the *scale* of each figure; except when it changes sign, and then the figure is simply reversed. But, even in that case, two curves of the same series cannot intersect, except of course at the origin; as, at either side of the origin the parts of the two lie respectively between, and outside, the common tangents to the series. Also it is obvious that *one* member of each series can be made to pass through *any* other assigned point in their plane, provided it be not taken on one of the tangents at the origin. For then the substitution of its coordinates in either equation determines the characteristic constant without ambiguity.

When, as in the cut, a and b are numerically equal, the curves intersect one another at their points of maximum distance from their respective axes of symmetry, where they are necessarily perpendicular to one another. And the common tangents to one series intersect members of the other series in points which separate, on each curve, the regions in which it is met by the kinked parts, from those in which it is met by the branched parts, of those of the other series. 1899.]

CXVIII.

NOTE ON THE CIRCLES OF CURVATURE OF A PLANE CURVE.

[*Proceedings of the Edinburgh Mathematical Society, December* 13, 1895.]

WHEN the curvature of a plane curve continuously increases or diminishes (as is the case with a logarithmic spiral for instance) no two of the circles of curvature can intersect one another.

This curious remark occurred to me some time ago in connection with an accidental feature of a totally different question. (*Systems of Plane Curves whose Orthogonals form a similar System. Anté,* No. CXVII.)

The proof is excessively simple. For if A, B, be any two points of the evolute, the chord AB is the distance between the centres of two of the circles, and is necessarily less than the arc AB, the difference of their radii. (This is true even if the evolute be sinuous, so that the original curve has ramphoid cusps.)

When the curve has points of maximum or minimum curvature, there are corresponding [keratoid] cusps on the evolute; and pairs of circles of curvature whose centres lie on opposite sides of the cusp, C, may intersect:—for the chord AB may now exceed the difference between CA and CB.

CXIX.

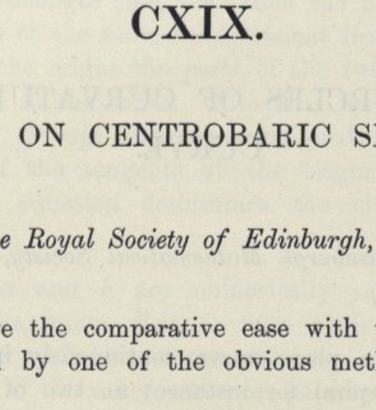
NOTE ON CENTROBARIC SHELLS.

[*Proceedings of the Royal Society of Edinburgh, February* 3, 1896.]

It is singular to observe the comparative ease with which elementary propositions in attraction can be proved by one of the obvious methods, while the proof by the other is tedious.

Thus nothing can be simpler than Newton's proof that a uniform spherical shell exerts no gravitating force on an internal particle. But, so far as I know, there is no such simple proof (of a *direct* character) that the potential is constant throughout the interior.

On the other hand the direct proof that a spherical shell, whose surface-density



is inversely as the cube of the distance from an internal point, is centrobaric is neither short nor simple. (See, for instance, Thomson and Tait's *Elements of Natural Philosophy*,

§ 491.) But we may prove at once that its *potential* at external points is the same as if its mass were condensed at the internal point.

For if an elementary double cone, with its vertex at S, cut out areas K and E, we have

$$\frac{E}{SE^2} = \frac{K}{SK^2}.$$

Let P be any external point, and take T on PS (produced) so that

$$PS \, . \, ST = KS \, . \, SE = b^2.$$

Then we have obviously, from similar triangles,

$$SK \, . \, EP = SP \, . \, KT.$$

Thus
$$\frac{\left(\dfrac{E}{SE^3}\right)}{EP} = \frac{K}{SK \, . \, SE \quad SK \, . \, EP} = \frac{1}{b^2} \cdot \frac{1}{SP} \cdot \frac{K}{KT}.$$

But the sum of the values of $\dfrac{K}{KT}$ is the (constant) potential at T for unit surface-density; so that the sum of the values of the first side of the equation is inversely as SP; and the proposition is proved.

Although no mention has been made of Electric Images, in the above investigation, it is obvious that nearly all their chief elementary properties have been proved, almost intuitively, in the course of these three or four lines. The others are obtained at once by applying the same method to the case in which P is inside the spherical shell, and T outside:—remembering that the potential at T is now inversely as the distance of T from the centre, O, of the sphere; and referring the potential of E to a point S' on OS produced till $OS . OS'$ is the square of the radius of the shell.

[This investigation has been at once further simplified and extended, in § 52 of my little book *Newton's Laws of Motion.* 1899.]

CXX.

ON THE LINEAR AND VECTOR FUNCTION.

[*Proceedings of the Royal Society of Edinburgh, May* 18 *and June* 1, 1896.]

IN the following Abstract I refer to such Linear and Vector Functions, only, as correspond to homogeneous strains which a piece of actual matter can undergo. There is no difficulty:—though caution is often called for:—in extending the propositions to cases which are not realizable in physics*.

The inquiry arose from a desire to ascertain the exact nature of the strain when, though it is not pure, the roots of its cubic are all real:—*i.e.*, when *three* lines of particles, not originally at right angles to one another, are left by it unchanged in direction.

1. The sum, and the product (or the quotient), of two linear and vector functions are also linear and vector functions. But, while the sum is always self-conjugate if the separate functions are so (or if they be conjugate to one another), the product (or quotient) is in general not self-conjugate:—though the determining cubic has, in this case, real roots. The proof can be given in many simple forms.

If ϖ and ω represent any two pure strains, there are three real values of g, each with its corresponding value of ρ, such that

$$\varpi\rho = g\omega\rho \qquad (1).$$

* [Thus the transformations, given below, are presumed to involve *real* quantities only. Dr Muir, in making some valuable comments on one of the results (*Phil. Mag.* 1897, I. 220), appears to have overlooked this important preliminary condition. 1899.]

Assume $\omega^{\frac{1}{2}}\rho = \sigma$; and the equation becomes

$$\omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}}\sigma = g\sigma.$$

But $\omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}}$ is obviously self-conjugate. Hence the three values of g are real, and the vectors σ form a rectangular system. Thus (1) is satisfied by three expressions of the form

$$\rho = \omega^{\frac{1}{2}-}\sigma = g^{\frac{1}{2}}\varpi^{-\frac{1}{2}}\sigma \text{ ....(2)};$$

i.e., there is one rectangular set of vectors which have their directions altered in the same way by the square roots of the inverses of each of the given strains.

But (1) may be written in the form

$$\omega^{-1}\varpi\rho = g\rho,$$

where $\omega^{-1}\varpi$ is in general not a self-conjugate function. Thus

Two pure strains in succession give a strain which is generally rotational, but whose cubic has three real roots.

Conversely, when a strain is such as to leave unchanged three directions in a body, it may be regarded as the resultant of two successive pure strains.

These are to be found from (2), in which the values of g and ρ are now regarded as *given*, so that the problem is reduced to finding ω (a *pure* strain), and the (rectangular) values of σ from three equations of the form

$$\omega^{\frac{1}{2}}\rho = \sigma.$$

When ω is thus found, the value of ϖ is given by (1). The solution is easily seen to express the fact that ω and ϖ, alike, convert the system ρ_1, ρ_2, ρ_3 into vectors parallel to $V\rho_2\rho_3$, $V\rho_3\rho_1$, $V\rho_1\rho_2$, respectively.

2. Other modes of solution of (1) are detailed, of which we need here mention only that which depends upon the formation of the cubic in

$$\phi = \varpi - g\omega,$$

the calculation of the coefficients in M_g, and the comparison of these forms with their equals found from

$$\phi = \varpi\omega^{-1} - g,$$

and from

$$\phi = \omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}} - g;$$

a process which gives interesting quaternion transformations.

3. Some curious consequences can be deduced from these formulæ, which have useful bearing upon the usual matrix mode of treating the problem algebraically.

For, if we take

$$\varpi = \left(\begin{array}{ccc} A & c & b \\ \hline c & B & a \\ b & a & C \end{array}\right) \text{ and } \omega = \left(\begin{array}{ccc} p & 0 & 0 \\ \hline 0 & q & 0 \\ 0 & 0 & r \end{array}\right)$$

which involve complete generality since i, j, k are undefined, we have for the cubic (1) in g

$$\begin{vmatrix} A-pg & c & b \\ c & B-qg & a \\ b & a & C-rg \end{vmatrix} = 0.$$

The transformation of (1) given above is equivalent to dividing the successive rows, and also the columns, of this determinant by $\sqrt{p}$, $\sqrt{q}$, $\sqrt{r}$ respectively. It thus becomes

$$\begin{vmatrix} A/p-g & c/\sqrt{pq} & b/\sqrt{pr} \\ c/\sqrt{pq} & B/q-g & a/\sqrt{qr} \\ b/\sqrt{pr} & a/\sqrt{qr} & C/r-g \end{vmatrix} = 0,$$

from the form of which the reality of the roots is obvious.

A somewhat similar process* shows that the roots of

$$\begin{vmatrix} A-x & b & c \\ d & E-x & f \\ g & h & I-x \end{vmatrix} = 0$$

are always all real, provided the single condition,

$$cdh = bfg,$$

be satisfied.

* [Multiply the rows, and divide the columns, respectively, by p, q, r. It becomes

$$\begin{vmatrix} A-x & bp/q & cp/r \\ dq/p & E-x & fq/r \\ gr/p & hr/q & I-x \end{vmatrix}$$

so that, to make it axi-symmetrical, we must have

$$(p/q)^2 = d/b,$$
$$(q/r)^2 = h/f,$$
$$(r/p)^2 = c/g.$$

Thus finally it becomes

$$\begin{vmatrix} A-x & \sqrt{bd} & \sqrt{cg} \\ \sqrt{db} & E-x & \sqrt{fh} \\ \sqrt{gc} & \sqrt{hf} & I-x \end{vmatrix}$$

if the condition in the text above is satisfied. 1896.]

It is easy to see that this statement may be put in the form:—The roots of $M_g = 0$ are real, provided a rectangular system can be found such that

$$Si\phi jSj\phi kSk\phi i = Sk\phi jSj\phi iSi\phi k.$$

The quaternion form, of which this is an exceedingly particular case, expresses simply that the roots of the cubic in ϕ are all real, if a self-conjugate function ω can be found, such that $\omega\phi$ is self-conjugate. This is merely another way of stating the chief result of § 1 above. But it may be interesting to illustrate it from this point of view. We may write, in consequence of what has just been said,

$$S\rho_1\rho_2\rho_3 \,.\, \phi\rho = g_1 V\rho_2\rho_3 S\rho_1\rho + g_2 V\rho_3\rho_1 S\rho_2\rho + g_3 V\rho_1\rho_2 S\rho_3\rho,$$

and
$$\omega\sigma = p_1\rho_1 S\rho_1\sigma + p_2\rho_2 S\rho_2\sigma + p_3\rho_3 S\rho_3\sigma.$$

These give at once

$$\omega\phi\rho = p_1 g_1 \rho_1 S\rho_1\rho + p_2 g_2 \rho_2 S\rho_2\rho + p_3 g_3 \rho_3 S\rho_3\rho,$$

which is obviously self-conjugate.

4. The results above have immediate application to fluid motion. For, when there is a velocity-potential, the motion is "differentially irrotational":—*i.e.*, the instantaneous change of form of any fluid element is a pure strain; a particular cubical element at each point becoming brick-shaped without change of direction of its edges. But if we think of the result of two successive instantaneous changes of this character, we see that there is in general at every point a definite elementary parallelepiped, the lengths, only, of whose edges are changed by this complex strain. In special cases, only, is a similar result produced by three successive pure strains.

[The remainder of this *Abstract* referred to the genesis and history of No. CXV. above.]

CXXI.

ON THE LINEAR AND VECTOR FUNCTION.

[*Proceedings of the Royal Society of Edinburgh, March* 1, 1897.]

In a paper read to the Society in May last, I treated specially the case in which the Hamiltonian cubic has all its roots real. In that paper I employed little beyond the well-known methods of Hamilton, but some of the results obtained seemed to indicate a novel and useful classification of the various forms of the Linear and Vector Function. This is the main object of the present communication.

1. It is known that we may always write

$$\phi\rho = \Sigma\,(\alpha S\alpha_1\rho)$$

and that three terms of the sum on the right are sufficient, and in general more than is required, to express any linear and vector function. In fact, all necessary generality is secured by fixing, once for all, the values of α, β, γ, or of α_1, β_1, γ_1, leaving the others arbitrary:—subject only to the condition that neither set is coplanar. Thus as a particular case we may write either

$$\phi\rho = \Sigma\alpha Si\rho,$$

or

$$\phi\rho = \Sigma i S\alpha_1\rho.$$

In either case we secure the nine independent scalar coefficients which are required for the expression of the most general homogeneous strain. But forms like these are relics of the early stage of quaternion development, and (as Hamilton expressly urged) they ought to be dispensed with as soon as possible.

2. A linear and vector function is completely determined if we know its effects on each of any system of three non-coplanar unit-vectors, say α, β, γ. If its cubic have

three real roots, these vectors may, if we choose, be taken as the directions which it leaves unaltered; if but one, we may take a corresponding system in the form

$$\alpha, \quad \beta \cos a \pm \iota\gamma \sin a,$$

where ι is $\sqrt{-1}$. But it is preferable to keep the simpler form α, β, γ, with the understanding that β and γ may be bi-vectors, of the form just written.

3. In terms of the three roots thus designed, we may form, with the help of three arbitrary scalars (two of them bi-scalars of the form $y \pm \iota z$, if necessary), three very simple but distinct varieties of linear and vector function:—viz.

(*a*) Strains leaving three directions, α, β, γ or $V\beta\gamma, V\gamma\alpha, V\alpha\beta$, unaltered, so that their reciprocals have the same form.

$$S\alpha\beta\gamma . \phi\rho = x\alpha S\beta\gamma\rho + y\beta S\gamma\alpha\rho + z\gamma S\alpha\beta\rho,$$

with

$$S\alpha\beta\gamma . \phi_1\rho = xV\beta\gamma S\alpha\rho + yV\gamma\alpha S\beta\rho + zV\alpha\beta S\gamma\rho.$$

In this case, if x, y, z are the same in each, ϕ_1 is the conjugate of ϕ.

(When $x = y = z$, these strains leave the form and position of a body unaltered; but each linear dimension is increased x fold.)

(*b*) Pure strains:—

$$\varpi\rho = x\alpha S\alpha\rho + y\beta S\beta\rho + z\gamma S\gamma\rho,$$

with

$$\varpi_1\rho = xV\beta\gamma S\beta\gamma\rho + yV\gamma\alpha S\gamma\alpha\rho + zV\alpha\beta S\alpha\beta\rho.$$

The second of these changes the system α, β, γ, into $V\beta\gamma, V\gamma\alpha, V\alpha\beta$; while the first effects the reverse operation.

(*c*) Combinations of two or more, from (*a*) or (*b*), or from (*a*) and (*b*):—

Either form of (*a*) repeated (with altered scalar constants), simply perpetuates the form. In $\phi\phi_1$ and $\phi_1\phi$ we have new forms, which are pure when $x : y : z$ are the same in each of the factors.

The two forms (*b*), in succession, give one or other of the forms (*a*); and, conversely, either form of (*a*) may be regarded as the resultant of the two forms (*b*) taken in the proper order. This is the main result of my former paper:—for it is obvious that, having between them twelve disposable constants, ϖ and ϖ_1 may be made to represent *any* two pure strains.

But, while $\phi\varpi$ and $\varpi\phi_1$ merely repeat the type ϖ; and $\varpi_1\phi$, and $\phi_1\varpi_1$ the type ϖ_1; we have novel forms in the combinations

$$\varpi\phi, \quad \phi_1\varpi, \quad \phi\varpi_1, \text{ and } \varpi_1\phi_1.$$

Many of these are useful in the solution of equations among forms; such as, for instance,

$$\chi^2 = \psi, \quad \psi\chi' = \chi\psi', \text{ or } \psi\chi = \chi\psi, \text{ \&c.}$$

where χ is to be found when ψ is given. One simple result of the above discussion, which is often of great use in such matters, is the obvious condition that two such forms shall be commutative in their successive application.

4. When two roots are imaginary, all the forms above are still real; since, when β and γ take the forms $\beta \pm \iota\gamma$, y and z must be written $y \pm \iota z$. In the forms (b), the imaginary terms cancel one another; in (a) the real terms do so, and the whole is divisible by ι.

5. Of course, with α, β, γ (as in 2, above) and three scalar constants, we can produce any form of linear and vector function. And the paper concludes with forms in which these constants are merged in a new arbitrary vector.

CXXII.

NOTE ON THE SOLUTION OF EQUATIONS IN LINEAR AND VECTOR FUNCTIONS.

[*Proceedings of the Royal Society of Edinburgh, June* 7, 1897.]

In a paper read to the Society on March 1 (*ante*, No. CXXI.) I spoke of the application of some of its results to the solution of equations involving an unknown Linear and Vector Function. These results depended chiefly upon the expression of the function in terms of its roots, scalar and directional; and I now give a few instances of their utility, keeping in view rather variety of treatment than complexity of subject. The matter admits of practically infinite development, even when we keep to very simple forms of equation, and is thus specially qualified to show the richness in resources which is so characteristic of quaternions. But it will be seen also to be strongly suggestive of the extreme caution required even in the most elementary parts of this field of inquiry.

In what follows, I employ χ to denote the unknown function; ϕ, ψ, etc., known functions. ϖ is specially reserved for a self-conjugate function, and ω for a pure rotation.

1. Given $$\phi\chi = \chi\phi \quad\quad (1);$$

i.e., to find the condition that two functions shall be commutative in their successive application. Let α be a root of ϕ, real or imaginary, so that

$$\phi\alpha = g\alpha.$$

We have at once, by applying the members of the proposed equation to α,

$$\phi\chi\alpha = \chi\phi\alpha = g\chi\alpha.$$

Thus, except in the case of equal roots of ϕ,

$$\chi\alpha = h\alpha\,;$$

so that the required condition is merely that χ has the same directional roots as ϕ. When two values of g are equal, two of the directional roots of χ are limited only to lie in a definite plane:—when all three are equal, ϕ becomes a mere magnification, and χ is, of course, wholly undetermined.

[When the roots of ϕ are all real, we have

$$S\alpha\beta\gamma\,.\,\chi\rho = h_1\alpha S\beta\gamma\rho + h_2\beta S\gamma\alpha\rho + h_3\gamma S\alpha\beta\rho.$$

When two are imaginary we may preserve this form; or, if we wish to express it in terms of real quantities only, we may write it as

$$S\alpha\beta\gamma\,.\,\chi\rho = h_1\alpha S\beta\gamma\rho + (h_2\beta - h_3\gamma)\,S\gamma\alpha\rho + (h_2\gamma + h_3\beta)\,S\alpha\beta\rho,$$

where the meanings of h_2, h_3, β, γ, are entirely changed.

It is well to notice that the squares of these functions preserve the form, so that in the first

$$S\alpha\beta\gamma\,.\,\chi^2\rho = h_1^2\alpha S\beta\gamma\rho + h_2^2\beta S\gamma\alpha\rho + h_3^2\gamma S\gamma\alpha\rho\,;$$

and in the second we have the value

$$h_1^2\alpha S\beta\gamma\rho + \{(h_2^2 - h_3^2)\,\beta - 2h_2h_3\gamma\}\,S\gamma\alpha\rho + \{(h_2^2 - h_3^2)\,\gamma + 2h_2h_3\beta\}\,S\alpha\beta\rho.$$

Thus the square roots of such expressions may be obtained by inspection.]

2. Had the known factors been different in the two members, *i.e.*, had the equation been

$$\phi\chi = \chi\psi \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1'),$$

the same process would still have been applicable, though the result would have been very different. For α being a root of ψ, we have

$$\phi\chi\alpha = g\chi\alpha$$

as before. But we can no longer conclude from this anything further than that the scalar roots of ψ must be the same as those of ϕ, in order that the given equation may not be self-contradictory. Thus, if ψ have three real roots, so must ϕ, and conversely. If this necessary condition be fulfilled, χ is any function which changes the *directional* roots of ψ into those of ϕ. Its own scalar roots remain indefinite.

3. Let the equation be

$$\phi\chi' = \chi\phi' \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2).$$

The members, besides being equal, are conjugates; so that they represent *any* pure strain whatever.

Thus $\chi' = \phi^{-1}\varpi$, and $\chi = \varpi\phi'^{-1}$, which are of course consistent with one another. Remark that, as a particular case, ϖ may be a mere number. If ϖ be taken $= \phi\phi'$, we have the obvious solution $\chi = \phi$.

4. If we alter the order of the factors on one side of (2) we have an altogether new form:—

$$\phi\chi' = \phi'\chi \quad\text{...(3).}$$

Since ϕ is given, this may be written

$$\chi' = \psi\chi,$$

where ψ is known. An immediate transformation by taking the conjugate gives

$$\chi = \psi\chi\psi',$$

a type which is obviously a particular case of (1'); and, besides, will be treated later, with the sole difference that χ will then be the *given* function, and ψ that to be found. But when a solution has thus been obtained, it must be tested in the original equation; for selective eliminations, such as that just given, often introduce irrelevant solutions. (See § 8, below.)

5. A curious modification of (3) is produced by making in it ϕ and χ identical, so that it becomes

$$\chi\chi' = \chi'\chi \quad\text{...(4).}$$

Though no longer linear, this equation is in some respects analogous to (1). It thus imposes the condition that χ and its conjugate shall have the same directional roots. If all three be real they must therefore form a rectangular system. If two be imaginary, the vectors of their real and imaginary parts form a rectangular system with the third. Thus χ may be any pure strain, or a rotation associated with a pure strain symmetrical about the axis of the rotation.

A simpler mode of dealing with (4) is suggested by the last remark. For we may always assume

$$\chi = \varpi\omega,$$

and (4) becomes $\varpi\omega\omega^{-1}\varpi = \varpi^2 = \omega^{-1}\varpi^2\omega,$

from which (coupled with the results of (1)) the above conclusions are obvious.

6. The form $\chi\phi\chi' = \phi' \quad\text{...(5)}$

also admits of simple treatment. Its conjugate is

$$\chi\phi'\chi' = \phi'.$$

Now we can always write $\phi = \varpi + V\epsilon,$

with $\phi' = \varpi - V\epsilon,$

and the equations above become, by addition and subtraction,

$$\chi\varpi\chi' = \varpi, \quad \chi V\epsilon\chi' = V\epsilon.$$

Put the first of these in the form

$$\chi\varpi^{\frac{1}{2}}\omega_1 \,.\, \omega_1^{-1}\varpi^{\frac{1}{2}}\chi' = \varpi^{\frac{1}{2}}\omega_2 \,.\, \omega_2^{-1}\varpi^{\frac{1}{2}},$$

where ω_1 and ω_2 are, so far, arbitrary. As each side is the product of a strain and its conjugate (because the conjugate of a pure rotation is its reciprocal), we may at once write

$$\chi\varpi^{\frac{1}{2}}\omega_1 = \varpi^{\frac{1}{2}}\omega_2,$$

or

$$\chi = \varpi^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}},$$

where $\omega = \omega_2\omega_1^{-1}$ is still arbitrary. To determine it, the second equation above, viz.

$$\chi V\epsilon\chi' = V\epsilon,$$

gives

$$m\epsilon = \chi'\epsilon,$$

where m is the product of the numerical (scalar) roots of χ; obviously unit in this case, as there is no change of volume. This gives

$$\omega\varpi^{\frac{1}{2}}\epsilon = \varpi^{\frac{1}{2}}\epsilon,$$

so that the axis of ω is $\varpi^{\frac{1}{2}}\epsilon$, but the angle of rotation remains undetermined.

The direct algebraic verification of this solution is troublesome, unless we refer the strain to the axes of its pure part ϖ, when it becomes fairly simple. For ϕ can then be written as

$$\left(\begin{array}{ccc} A^2 & -\nu & \mu \\ \nu & B^2 & -\lambda \\ -\mu & \lambda & C^2 \end{array}\right),$$

whence it is easy to see that

$$\chi = \left(\begin{array}{ccc} e+(1-e)\,l^2 & \frac{A}{B}\{-nf+(1-e)\,lm\} & \frac{A}{C}\{mf+(1-e)\,ln\} \\ \frac{B}{A}\{nf+(1-e)\,lm\} & e+(1-e)\,m^2 & \frac{B}{C}\{-lf+(1-e)\,mn\} \\ \frac{C}{A}\{-mf+(1-e)\,ln\} & \frac{C}{B}\{lf+(1-e)\,mn\} & e+(1-e)\,n^2 \end{array}\right),$$

where

$$l = A\lambda/\sqrt{A^2\lambda^2+B^2\mu^2+C^2\nu^2}, \text{ etc.,}$$

and

$$e^2+f^2=1.$$

7. A similar mode of treatment can, of course, be applied to the more general form

$$\chi\phi\chi' = \psi \qquad\qquad (6).$$

After what has just been said, it is easy to see that if $\psi = \varpi_1 + V\epsilon_1$, we shall have

$$\chi = \varpi_1^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}},$$

with the condition for ω (and for the possibility of a solution)

$$m\omega\varpi^{\frac{1}{2}}\epsilon = \varpi_1^{\frac{1}{2}}\epsilon_1,$$

where m is the product of the numerical roots of χ.

[In connection with the results above it may be interesting to find the relations among the various constituents of the two different modes of breaking up a linear vector function into pure and rotational parts:—*i.e.*,

$$\phi = \varpi + V\epsilon = \varpi_1\omega.$$

(See No. XXI. above, for another solution.)

The general form of a pure rotation is

$$\omega = \alpha^{A/\pi}(\quad)\,\alpha^{-A/\pi} = \cos A + \sin A\, V.\alpha - (1-\cos A)\,\alpha S.\alpha,$$

where α is the unit vector axis and A the angle of rotation.

Thus, writing for shortness $\bar{c} = \cos A$ and $\bar{s} = \sin A$,

$$\varpi\rho + V\epsilon\rho = \bar{c}\varpi_1\rho + \bar{s}\varpi_1 V\alpha\rho - (1-\bar{c})\,\varpi_1\alpha S\alpha\rho,$$

$$\varpi\rho - V\epsilon\rho = \bar{c}\varpi_1\rho - \bar{s}\,V\alpha\varpi_1\rho - (1-\bar{c})\,\alpha S\alpha\varpi_1\rho,$$

so that $$2V\epsilon\rho = \bar{s}\,(\varpi_1 V\alpha\rho + V\alpha\varpi_1\rho) + (1-\bar{c})\;V.(V\alpha\varpi_1\alpha)\,\rho.$$

Now Hamilton (in giving his cubic) showed that

$$(m_2 - \varpi_1)\;V\alpha\rho = V\varpi_1\alpha\rho + V\alpha\varpi_1\rho,$$

so we have $$2V\epsilon\rho = \bar{s}\,(m_2 V\alpha\rho - V\varpi_1\alpha\rho) + (1-\bar{c})\;V.(V\alpha\varpi_1\alpha)\,\rho\,;$$

and, as this is true for all values of ρ,

$$2\epsilon = \bar{s}\,(m_2\alpha - \varpi_1\alpha) + (1-\bar{c})\;V\alpha\varpi_1\alpha,$$

the second term disappearing when the rotation is about one of the axes of the pure part of the strain. Again

$$2\varpi\rho = 2\bar{c}\varpi_1\rho + \bar{s}\,(\varpi_1 V\alpha\rho - V\alpha\varpi_1\rho) - (1-\bar{c})\,\{\varpi_1\alpha S\alpha\rho + \alpha S\alpha\varpi_1\rho\}$$

is obviously self-conjugate.]

8. An instantaneous, and (at first sight) apparently quite different, solution of (5) is obtained by multiplying each side into the reciprocal of its conjugate. For we thus have a case of (1) in the form

$$\chi\phi\phi'^{-1} = \phi\phi'^{-1}\chi.$$

But this equation, which would assign to χ *any* value commutative with $\phi\phi'^{-1}$, is very much more general than (5) from which it is derived. [This is an excellent example of the necessity for caution already pointed out.]

To analyse this solution, with the view of restricting it, note that by Hamilton's method we have at once

$$m\,(\phi'^{-1} - \phi^{-1}) = 2V.\varpi\epsilon = 2eV.\varpi^{\frac{1}{2}}\alpha, \text{ suppose,}$$

where m is the product of the scalar roots of ϕ; α a unit vector, and e a scalar constant, both definite.

Thus $$\phi\phi'^{-1}\rho = \rho + \frac{2e}{m}\phi V\varpi^{\frac{1}{2}}\alpha\rho$$

$$= \rho + \frac{2e}{m}(\varpi + eV.\varpi^{-\frac{1}{2}}\alpha)V\varpi^{\frac{1}{2}}\alpha\rho$$

$$= \left(1 - \frac{2e^2}{m}\right)\rho + \frac{2e\overline{m}^{\frac{1}{2}}}{m}\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}\rho - \frac{2e^2}{m}\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}\rho,$$

where $\overline{m}^{\frac{1}{2}}$ is the product of the scalar roots of $\varpi^{\frac{1}{2}}$, and therefore

$$m = \overline{m} - Se\varpi e = \overline{m} + e^2.$$

[The former solution, giving

$$\chi\rho = \varpi^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}}\rho$$
$$= \rho\cos A + \sin A\,\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}\rho - (1 - \cos A)\,\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}\rho,$$

contains this as a particular case, for it is easy to see that the two expressions agree if we are entitled to assume simultaneously

$$\cos A = 1 - \frac{2e^2}{m},\quad \sin A = \frac{2e\overline{m}^{\frac{1}{2}}}{m},\quad 1 - \cos A = \frac{2e^2}{m}.$$

The first and last are identical; and the first and second require merely that we shall have

$$1 = \left(1 - \frac{2e^2}{m}\right)^2 + \frac{4e^2\overline{m}}{m^2};$$

which is satisfied in consequence of the expression for $\overline{m}$ above.]

That the complete admissible value of χ is what we have already found, and contains only the *one* scalar indeterminate A, is easily verified by expressing χ as a linear combination of the operators 1, $\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}$, $\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}$, which are suggested by its relation to $\phi\phi'^{-1}$, and are obviously commutative with one another; and independent, in the sense of not producing any new operator by their combinations. Then the required relations among the coefficients are determined by comparing term by term the expressions for $\phi\chi'$ and $\chi^{-1}\phi$.

9. Finally, we may treat (5) by a method similar to that adopted for (1). Let α *now* be a directional root of χ', so that $\chi'\alpha = g\alpha$. Then we have

$$\chi\phi\alpha = \frac{1}{g}\phi\alpha.$$

But the cubics of χ and χ' are necessarily identical, and thus their common numerical roots can be no others than 1, g, $1/g$. Also, since ϕ is assumed to be real, g is imaginary, for ϕ changes the g directional root of χ' to the $1/g$ root of χ, and conversely.

But, if we operate by the conjugate of (5) upon α, we get

$$\chi\phi'\alpha = \frac{1}{g}\phi'\alpha.$$

Thus the directional roots of χ' are treated alike by ϕ' and by ϕ, and must therefore belong to $\phi^{-1}\phi'$. So those of χ belong to $\phi\phi'^{-1}$. Thus we are again conducted to the previous result; but this third method gives us great additional information as to the intrinsic nature of the strains involved, and the relations which exist among them.

10. It is, of course, only in special cases that simple methods like these can be applied to linear vector-function equations of a little greater complexity. But when they are applicable they often give singularly elegant solutions. As an instance take the equation

$$\phi_1\chi + \chi\phi_2 = \psi \qquad\qquad (7),$$

or, as it may obviously be written,

$$\chi^{-1}\phi_1 + \phi_2\chi^{-1} = \chi^{-1}\psi\chi^{-1}.$$

Let α be a directional root of ϕ_2, then at once

$$\phi_1\chi\alpha + g\chi\alpha = \psi\alpha,$$

or

$$\chi\alpha = (\phi_1 + g)^{-1}\psi\alpha.$$

If the roots of ϕ_2 be unequal, the three equations of this form completely determine χ.

11. Again, let

$$\phi_1\chi + \chi\phi_2 = \phi_3\chi\phi_4 + \psi \qquad\qquad (8).$$

If g_1, α_1, etc., are roots of ϕ_2, this gives three equations of the form

$$(\phi_1 + g_1)\chi\alpha_1 = \phi_3\chi(\phi_4\alpha_1) + \psi\alpha_1.$$

If the values of α be unequal, we can of course find the coefficients in

$$\phi_4\alpha_1 = a_1\alpha_1 + b_1\alpha_2 + c_1\alpha_3$$
$$\phi_4\alpha_2 = a_2\alpha_1 + b_2\alpha_2 + \dots$$
$$\phi_4\alpha_3 = \dots\dots\dots$$

Then, putting λ_1 for $\chi\alpha_1$, etc., we have finally

$$\phi_3^{-1}(\phi_1 + g_1)\lambda_1 = a_1\lambda_1 + b_1\lambda_2 + c_1\lambda_3 + \phi_3^{-1}\psi\alpha_1.$$

The three equations of this form give λ_1, etc., that is, $\chi\alpha_1$, etc., and thus χ is found in terms of its effects on three known vectors.

12. The most general linear equation in χ and χ' may be written as

$$\Sigma\phi\chi\phi_1 + \Sigma\psi\chi'\psi_1 = \xi.$$

Take α, β, γ, three non-coplanar vectors, and let

$$\left.\begin{aligned}\phi_1\alpha &= p\alpha + q\beta + r\gamma\\ \phi_1\beta &= p'\alpha + q'\beta + r'\gamma\\ \phi_1\gamma &= p''\alpha + q''\beta + r''\gamma\end{aligned}\right\}\text{ etc.}$$

$$\left.\begin{aligned}\psi'\alpha &= s\alpha + t\beta + u\gamma\\ \psi'\beta &= s'\alpha + t'\beta + \ldots\\ \psi'\gamma &= s''\alpha + \ldots\ldots\ldots\end{aligned}\right\}\text{ etc.}$$

Apply the members of the given equation to α, β, γ separately; and operate on each of the results with $S.\alpha$, $S.\beta$, $S.\gamma$. We obtain nine scalar equations in $\chi\alpha = \lambda$, $\chi\beta = \mu$, $\chi\gamma = \nu$, of which two are

$$\Sigma\{S\alpha\phi(p\lambda + q\mu + r\nu) + S\psi_1\alpha(s\lambda + t\mu + u\nu)\} = S\alpha\xi\alpha,$$
$$\Sigma\{S\beta\phi(p\lambda + q\mu + r\nu) + S\psi_1\alpha(s'\lambda + t'\mu + u'\nu)\} = S\beta\xi\alpha.$$

These are necessary, and sufficient, to determine λ, μ, ν; and thence χ.

CXXIII.

ON THE DIRECTIONS WHICH ARE MOST ALTERED BY A HOMOGENEOUS STRAIN.

[*Proceedings of the Royal Society of Edinburgh, December* 7, 1897.]

THE cosine of the angle through which a unit vector ρ is turned by the homogeneous strain ϕ is

$$u = -\frac{S\rho\phi\rho}{T\rho \, . \, T\phi\rho}.$$

This is to be a maximum, with the sole condition

$$T\rho = 1.$$

Differentiating, &c., as usual we have

$$x\rho = -2\bar{\phi}\rho S\rho\phi'\phi\rho + \phi'\phi\rho S\rho\phi\rho,$$

where $2\bar{\phi} = \phi + \phi'$.

Operate by $S \, . \, \rho$ and we have

$$-x = -S\rho\phi\rho S\rho\phi'\phi\rho;$$

so that
$$\rho = -2\frac{\bar{\phi}\rho}{S\rho\phi\rho} + \frac{\phi'\phi\rho}{S\rho\phi'\phi\rho}.$$

Hence the required vector, and its positions after the strains $\bar{\phi}$ and $\phi'\phi$, lie in one plane; and the tangent of the angle between ρ and $\bar{\phi}\rho$ is half of the tangent of the angle between ρ and $\phi'\phi\rho$. [In the original, ϕ was (by an oversight) written for $\bar{\phi}$, so that the last statement has been modified. 1899.]

When the strain is pure, the required values of ρ are easily found. Let the chief unit vectors of ϕ be α, β, γ, and its scalars g_1, g_2, g_3. Then the equation above gives at once three of the form

$$S\alpha\rho\,.\left(1+\frac{2g_1}{S\rho\phi\rho}-\frac{g_1^2}{S\rho\phi^2\rho}\right)=0.$$

There are two kinds of solutions of these equations.

First. Let the first factor vanish in two of them, *e.g.*,

$$S\beta\rho=0,\quad S\gamma\rho=0,\quad \text{or}\quad \rho=\alpha.$$

Then the remaining equation is satisfied identically, because its second factor becomes

$$1-\frac{2g_1}{g_1}+\frac{g_1^2}{g_1^2};$$

whence $$u^2=1.$$

Thus, as we might have seen at once, the lines of zero alteration (minima) are the axes of the strain.

Second. Let the second factor vanish in two of the equations, *e.g.*,

$$1+\frac{2g_2}{S\rho\phi\rho}-\frac{g_2^2}{S\rho\phi^2\rho}=0,\quad 1+\frac{2g_3}{S\rho\phi\rho}-\frac{g_3^2}{S\rho\phi^2\rho}=0.$$

These give at once $$S\rho\phi\rho=-\frac{2g_2g_3}{g_2+g_3},\quad S\rho\phi^2\rho=-g_2g_3;$$

so that $$u^2=\frac{4g_2g_3}{(g_2+g_3)^2}.$$

In this case it is evident that we have also

$$S\alpha\rho=0.$$

[In fact, neither the first factors, nor the second factors, in the three equations, can simultaneously vanish:—except in the special case when two of g_1, g_2, g_3 are equal.]

Of the three values of u^2 just found, the least, which depends upon the greatest and least of the three values of g, gives the single vector of maximum displacement:—the other two are minimaxes, corresponding to *cols* where a contour line intersects itself.

(*Read February* 21, 1898.)

The self-intersecting contour-lines, corresponding to 3, 2, 1 as the values of the g's, were exhibited on a globe; whose surface was thus divided into regions in each of which the amount of displacement lies between definite limits. The contour $u^2=\frac{8}{9}$ encloses the regions in which the maximum $\left(u^2=\frac{3}{4}\right)$ is contained:—and (where its separate areas are superposed) one of the minima. This minimum is surrounded by a detached

Fig 1.

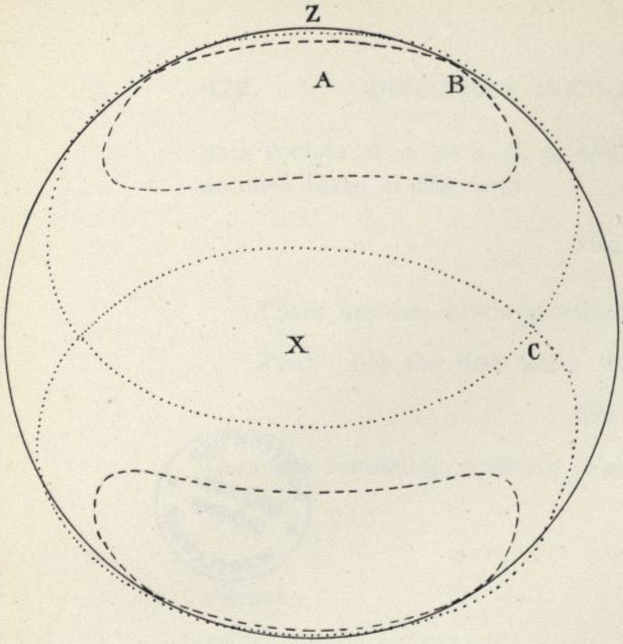
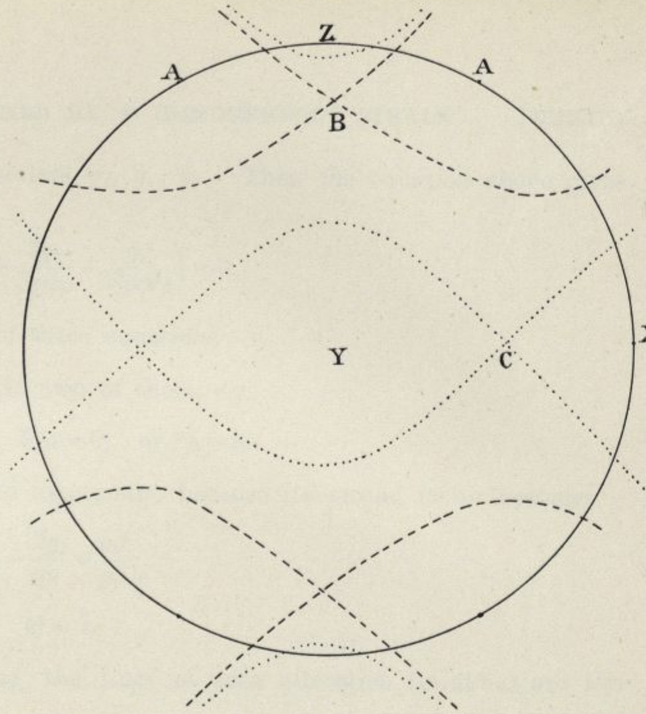


Fig 2



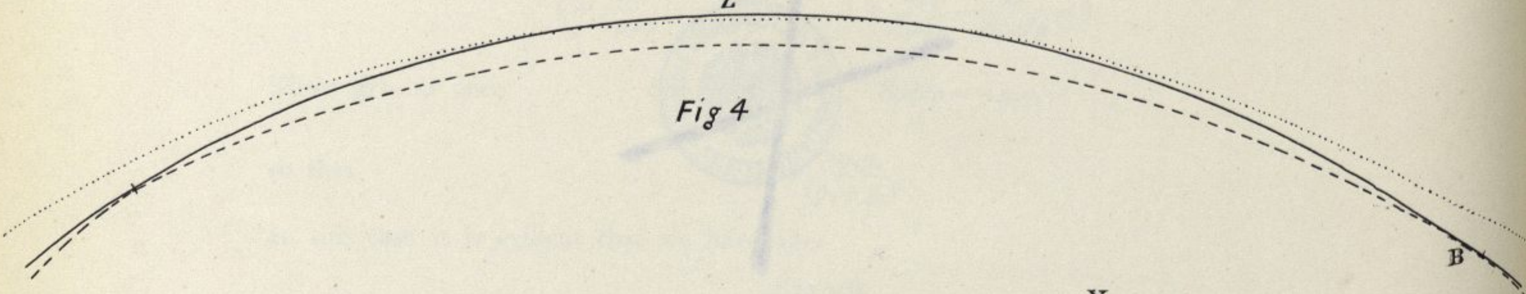


Fig 4

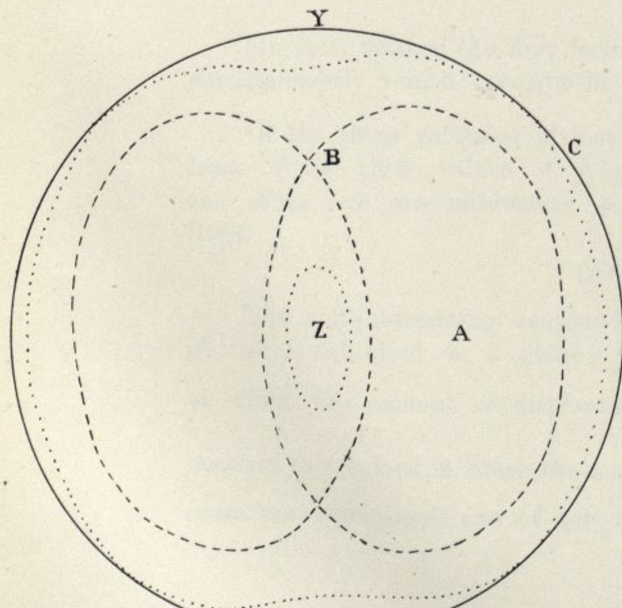


Fig 3.

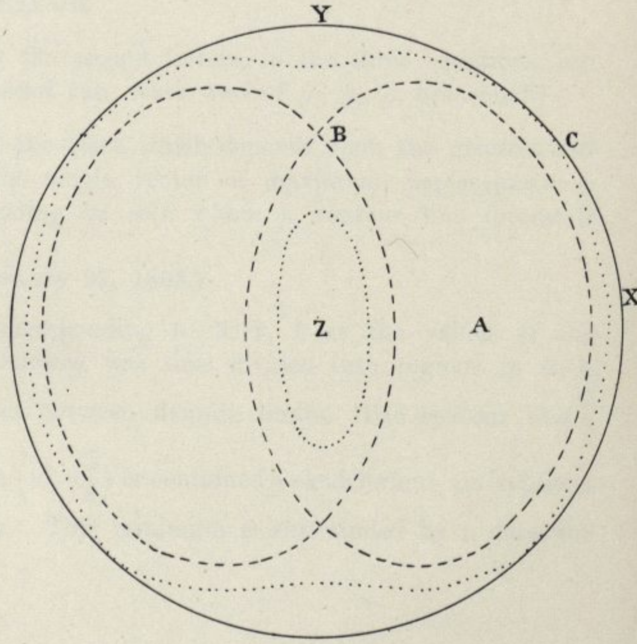


Fig 5

part of $u^2 = \frac{24}{25}$, while the rest surrounds the other two minima ($u^2 = 1$); and the double points of these contours are the minimaxes.

A general idea of their forms may be gathered from their orthogonal projections on the principal planes, as shown in Figs. 1, 2, 3 of Plate VIII. These projections are curves of the 4th order:—but $u^2 = \frac{8}{9}$ (dashed) splits into two equal ellipses on the xy plane, and hyperbolas on that of xz; while $u^2 = \frac{24}{25}$ (dotted) gives ellipses on yz and hyperbolas on xz. Fig. 4 gives, on a fourfold scale, the region near the z pole of the projection on yz, of which the details cannot be shown on the smaller figure.

The curves were traced from their equations. One example must suffice. Thus

$$u^2 = \frac{8}{9} = \frac{(3x^2 + 2y^2 + z^2)^2}{9x^2 + 4y^2 + z^2}$$

gives, eliminating z by the condition $x^2 + y^2 + z^2 = 1$,

$$(2x^2 + y^2 + 1)^2 = \frac{8}{9}(8x^2 + 3y^2 + 1),$$

or

$$\left(2x^2 + y^2 - \frac{1}{3}\right)^2 = \frac{16}{9}x^2,$$

i.e.,

$$2\left(x \pm \frac{1}{3}\right)^2 + y^2 = \frac{5}{9}.$$

The forms of these curves depend only on the ratios $g_1 : g_2 : g_3$, so that I have appended Fig. 5, in which we have $5 : 4 : 3$, for comparison with Fig. 3 where we have $3 : 2 : 1$.

CXXIV.

ON THE LINEAR AND VECTOR FUNCTION.

[*Proceedings of the Royal Society of Edinburgh, May* 1, 1899.]

THREE years ago I called the attention of the Society to the following theorem:—

The resultant of two pure strains is a homogeneous strain which leaves three directions unchanged; and conversely.

[It will be shown below that any strain which has three real roots can also be looked on (in an infinite number of ways) as the resultant of two others which have the same property.]

As I was anxious to introduce this proposition in my advanced class, where I was not justified in employing the extremely simple quaternion proof, I gave a number of different modes of demonstration; of which the most elementary was geometrical, and was based upon the almost obvious fact that

If there be two concentric ellipsoids, determinate in form and position, one of which remains of constant magnitude, while the other may swell or contract without limit; there are three stages at which they touch one another.

[These are, of course, (1) and (2), when one is just wholly inside or just wholly outside the other (that is when their closed curves of intersection shrink into points), and (3) when their curves of intersection intersect one another. The whole matter may obviously be simplified by *first* inflicting a pure strain on the two ellipsoids, such as to make *one* of them into a sphere, *next* considering their conditions of touching, and *finally* inflicting the reciprocal strain.]

But the normal at any point of an ellipsoid is the direction into which the radius-vector of that point is turned by a pure strain; so that *for any two pure strains* *there are three directions which they alter alike.* (These form, of course, the system of conjugate diameters common to the two ellipsoids.) This is the fundamental proposition of the paper referred to, and the theorem follows from it directly.

In the course of some recent investigations I noticed that if ϕ have real roots, so also has

$$\psi\phi\psi^{-1}$$

whatever real strain ψ may be. This is, of course, obvious, for they are $\psi\alpha$, $\psi\beta$, $\psi\gamma$, if α, β, γ be the roots of ϕ. At first sight this appeared to me to be a generalisation of the theorem above, of a nature inconsistent with some of the steps of the proof. But it is easy to see that it is not so. For all expressions of the form

$$\psi\omega\psi'$$

correspond to pure strains if ω is pure. Hence

$$\psi\phi\psi^{-1} = \psi\omega\varpi\psi^{-1} = \psi\omega\psi' \,.\, \psi'^{-1}\varpi\psi^{-1}$$

and is thus, as required by the theorem, the product of two pure strains.

Of course we might have decomposed it into other pairs of factors, thus

$$\psi\omega\psi^{-1} \,.\, \psi\varpi\psi^{-1}, \quad \psi\omega\chi^{-1} \,.\, \chi\varpi\psi^{-1}, \text{ etc.}$$

In the former case the factors have each three real roots, in the latter they have not *generally* more than one.

A great number of curious developments at once suggest themselves, of which I mention one or two.

Thus, let there be three successive pure strains (which may obviously represent any strain). We may alter them individually, as below, in an infinite number of ways without altering the whole.

$$\omega\omega_1\omega_2 = \omega_1^{-1} \,.\, \omega_1\omega\omega_1 \,.\, \omega_2 = \omega \,.\, \omega_1\omega_2\omega_1 \,.\, \omega_1^{-1}$$

$$= \omega_1^{-1}\omega^{-1}\omega_1^{-1} \,.\, \omega_1\omega\omega_1\omega_1^{-1}\,\omega_1\omega\omega_1 \,.\, \omega_2$$

$$= \omega_1^{-1}\omega^{-1}\omega_1^{-1} \,.\, \omega_1\omega\omega_1\omega\omega_1 \,.\, \omega_2 = \text{etc.}$$

The expression $\omega\varpi$ itself, when its three roots are given, *i.e.*, α, β, γ with g_1, g_2, g_3, gives ω and ϖ separately, with three scalars left arbitrary. For we may take

$$\omega\rho = x_1\alpha S\alpha\rho + x_2\beta S\beta\rho + \ldots,$$

$$\varpi\rho = y_1 V\beta\gamma S\beta\gamma\rho + y_2 V\gamma\alpha S\gamma\alpha\rho + \ldots,$$

and then obviously there are three conditions only, viz.

$$\frac{g_1}{x_1y_1} = \frac{g_2}{x_2y_2} = \frac{g_3}{x_3y_3} = S\alpha\beta\gamma.$$

Another portion of the paper deals with a sort of converse of the above problem:—The relation between two strains (whether with three real roots or with one) when their successive application gives a pure strain; and various questions of a similar kind.

In these inquiries we constantly meet with a somewhat puzzling form, which repeats itself in a remarkable manner under the usual modes of treatment, viz.:—

$$\omega V\epsilon\rho + V\epsilon\omega\rho.$$

A little consideration, however, shows that it can be put into the form

$$V(m_2\epsilon - \omega\epsilon)\rho,$$

which is thoroughly tractable.

CXXV.

NOTE ON CLERK-MAXWELL'S LAW OF DISTRIBUTION OF VELOCITY IN A GROUP OF EQUAL COLLIDING SPHERES.

[*Proceedings of the Royal Society of Edinburgh, June* 15, 1896.]

THE sarcastic criticism which M. Bertrand (*Comptes Rendus,* May 4 and 18, 1896) again bestows on Clerk-Maxwell's earliest solution of the fundamental problem in the *Kinetic Theory of Gases,* together with Prof. Boltzmann's very different, but thoroughly depreciatory, remarks (*ib.*, May 26), have led me to reconsider this question, already discussed by me at some length before the Society. Both of these authorities declare Maxwell's investigation to be erroneous:—but, while Prof. Boltzmann allows his *result* to be correct, M. Bertrand goes further, and bluntly calls it absurd. He had, in his *Calcul des Probabilités* (1888), already given Maxwell's proof as an example of illusory methods. I have the misfortune to agree with Maxwell, and to hold that his reasoning, though not by any means complete, is (like his result) correct. (*Trans. R.S.E.*, vol. XXXIII. pp. 66 and 252.)

I have not found anything in these communications of mine (so far at least as the present question is concerned) which I should desire to retract; but they can be considerably improved; and I think that, by the introduction of the *Döppler*-(properly the *Römer*-) principle, the true nature of a part of the argument can be made somewhat more immediately obvious. Also I will venture to express the hope that Prof. Boltzmann may at last recognise that I have, in this matter at least, *not* deserved the reproach of having reasoned in a circle*.

1. The following quotation from my first paper† (in which I have italicized the greater part of one sentence) shows the general ground of my reasoning, which was expressly limited to a very numerous group of equal, perfectly hard, spherical particles.

* *Phil. Mag.*, XXV. (1888), pp. 89, 177.

† [*Anté*, No. LXXVII. pp. 126, 129. 1899.]

"Very slight consideration is required to convince us that, unless we suppose the spheres to collide with one another, it would be impossible to apply any species of finite reasoning to the ascertaining of their distribution at each instant, or the distribution of velocity among those of them which are for the time in any particular region of the containing vessel. But, when the idea of mutual collisions is introduced, *we have at once, in place of the hopelessly complex question of the behaviour of innumerable absolutely isolated individuals, the comparatively simple statistical question of the average behaviour of the various groups of a community.* This distinction is forcibly impressed, even on the non-mathematical, by the extraordinary steadiness with which the numbers of such totally unpredictable, though not uncommon, phenomena as suicides, twin or triple births, dead letters, &c., in any populous country, are maintained year after year.

"On those who are acquainted with the higher developments of the mathematical *Theory of Probabilities* the impression is still more forcible. Every one, therefore, who considers the subject from either of these points of view, must come to the conclusion that continued collisions among our set of elastic spheres will, *provided they are all equal*, produce a state of things in which the percentage of the whole which have, at each moment, any distinctive property must (after *many* collisions) tend towards a definite numerical value; from which it will never afterwards markedly depart."

"When [the final result, in which the distribution of velocity-components is the same for all directions] is arrived at, collisions will not, in the long run, tend to alter it. For then the uniformity of distribution of the spheres in space, and the symmetry of distribution of velocity among them, enable us (by the principle of averages) to dispense with the only limitation above imposed; viz., the parallelism of the lines of centres in the collisions considered."

2. Now, considering the $3 . 10^{20}$ absolutely equal particles in each cubic inch of a gas, where could we hope to find a more perfect example of such a community? Where a more apt subject for the application of the higher parts of the *Theory of Probabilities*? If we are ever to find an approach to statistical regularity, it is surely here, where all the most exacting demands of the mathematician are fully conceded.

Is it not obvious, at once, that such a group must present *at all times, and from all sides*, precisely the same features? In other words:—that the solution of the problem is UNIQUE. (This word practically contains the whole point of the question.) If not, the higher part of the Theory of Probabilities (in which M. Bertrand himself is one of the prominent authorities) is a mere useless outcome of analytical dexterity; and even common-sense, with consistent experience to guide it, is of no value whatever.

A first consequence of this perfect community of interests is that (on the average, of course) the fraction of the whole particles, whose component speeds *in any assigned direction* lie between x and $x + \delta x$ is expressed by

$$f(x)\,\delta x$$

where f is a perfectly definite (and obviously *even*) function.

It is clear from this that the density of ends in the velocity space-diagram *depends on r only*; but we require further information before we can find *how*. (M. Bertrand seems to admit the first statement; but he insists that, otherwise, the solution is *wholly arbitrary*.)

3. [But, before seeking this, we may take another mode of viewing the situation:—as follows. It is, of course, nothing more than an *illustration* of the argument just given.

Suppose, merely for the purpose of examining the condition of the gas, and therefore without any inquiry into other physical possibilities, which have nothing to do with the argument:—

That (a) each particle of the group is self-luminous, and all give out, with equal intensity, light of one definite period. (To illustrate the remark just made, note that this luminosity is *not* attributed to collisions, nor to any assigned physical causes.)

(b) The wave-length of light reaching the eye from a moving source is altered by an amount proportional to the speed with which its distance from the eye alters.

(c) The displacement of light by a grating on which it falls normally is proportional to the wave-length.

(d) An ideal grating may be assumed, of any requisite regularity and fineness; and, again for the sake of argument only, it may be supposed to act, however fine it be, in the same manner as do ordinary gratings.

These premised, the spectrum of the gas will be a band, whose visible breadth depends only on the fineness of the grating and the luminosity of a particle. But this band will present, *at all times and from all sides* of the group, exactly the same appearance.

Its brightness, therefore, at any given distance from its central line, will be constant. But this means that the fraction of the whole number of particles which have any given speed in the line of sight, *depends on that speed alone.* The utmost speed of a gaseous particle is exceedingly small compared with that of light, and the alteration of wave-length is not affected by the part of the motion of the luminous particle which is transverse to the line of sight.]

4. We have not yet exhausted the consequences of absolutely perfect (average) community. For *every* particle, in virtue of citizenship, has a right to, and obtains, its due quota of whatever is shared among the group. Its tenure of any one value of x ceases (usually in a most abrupt way) some 10^{10} times per second, but leaves it *absolutely free* to have, during each of these brief periods, any values of y and z which may fall to it. There are, in fact, definite specifications of x, y, z speeds; but they are distributed among the particles with absolute independence of one another, in a manner which is perpetually changing at an exceptionally rapid rate. And the entire independence of x, y, and z speeds is shown by the fact that, in a

collision, there is a mere interchange of speeds along the line of centres at impact:—*whatever* be the speeds of the impinging particles in other directions.

Thus the assumption, which Maxwell allowed "might appear precarious" (it is carefully to be observed that he did *not* say it appeared so to himself) is fully justified. In any element of volume of the space-diagram of velocities, the density is proportional to

$$f(x) f(y) f(z)$$

whatever rectangular axes be employed. This, of course, gives at once Maxwell's result, viz.:—

$$\left(\frac{h}{\pi}\right) \epsilon^{-h(x^2+y^2+z^2)}.$$

To any one who is doubtful about the accuracy, or the cogency, of the reasoning just sketched, we may put the matter in another form. The solution, we saw, is *unique*. But this is obviously *a* solution, for it is easy to see that *collisions do not alter it**. Therefore it is *the* solution.

5. M. Bertrand treats the above result of Clerk-Maxwell's to the following sweeping condemnation:—

"Il y aurait indulgence à reprocher à cette formule trop peu de rigueur: les habitudes de la Géométrie autorisent à la déclarer tout simplement absurde."

Comment on this would be superfluous.

But it is easy to see how M. Bertrand has been led into this position. The following is, according to his information, the problem as proposed, and solved, by Maxwell:—

"Les molécules d'une masse gazeuse, étant en nombre immense et considéré comme infini, sont animées de vitesses inconnues. On ne sait rien sur les conditions initiales et sur les actions perturbatrices qui s'exercent entre elles et sur elles.

"Déterminer le rapport du nombre total des molécules au nombre de celles dont la vitesse est comprise entre des limites données. On n'admet rien de plus, sinon que, par l'absence de toute ordonnance régulière, tout est pareil dans toutes les directions."

No wonder M. Bertrand says that this reminds one of the question of finding the age of the captain from the size of his vessel!

The real cause for wonder is that M. Bertrand, who must be perfectly aware that strong common-sense was as prominent a characteristic of Maxwell's intellect as was brilliant, and often daring, originality, could believe him capable of propounding such manifest nonsense.

* With this particular form of $f(x)$ not only is $f(x)f(y)f(z)$ an *Invariant* in the usual sense of being independent of the rectangular system of axes employed; but *its separate factors are unaltered* by a collision if one of these axes be taken parallel to the line of centres at impact.

6. What Maxwell *did* propose, and solve, was a very different problem indeed. Here are his words (*Phil. Mag.* XIX. (1860), p. 22):—

"Prop. IV. To find the average number of particles whose velocities lie between given limits, *after a great number of collisions among a great number of equal particles.*"

He had already pointed out that the particles are regarded as spherical and perfectly elastic; and that, though collisions are perpetually altering the velocity of each, the tendency is to some regular law of distribution of *vis viva* among the group. I am far from asserting that his paper (which, epoch-making as it was, is evidently a somewhat hasty and unmatured effort) is free from even large errors: but it certainly does not contain such palpable absurdities as those now laid to its charge.

M. Bertrand entirely ignores the fact that Maxwell was dealing with a "community." And his comment on Maxwell might justly be retorted on himself in a slightly altered form. For he asserts that the x, y, z speeds are not independent, which is virtually the equivalent of the statement that when the latitude of a ship at sea has been anyhow determined, its longitude is no longer wholly indeterminate!

[July 6, 1896. Prof. Boltzmann, to whom I sent a proof of the above, requests me to add, on his part, as follows:—

"I have given expression to my high respect for Maxwell in the Prefaces to the two Parts of my *Lectures on Maxwell's Theory of Electricity and Light*, and specially in the Motto to Part II. And, besides, I regard Maxwell's discovery of the Law of Distribution of Velocity as so important a service that, in comparison, the trifling mistakes which appear to me to occur in his first proof are not worthy of consideration. The letters which I wrote to M. Bertrand, who was good enough to communicate them to the French Academy, had thus by no means the object of expressing my concurrence in M. Bertrand's dissentient (*abfällig*) judgment of Maxwell's work on the Velocity-distribution-law. I wished rather to say that M. Bertrand was so much the less justified in this opinion because the one objection he was able to make had already been made by others, who agree in all essentials with Maxwell."]

CXXVI.

ON THE GENERALIZATION OF JOSEPHUS' PROBLEM.

[*Proceedings of the Royal Society of Edinburgh, July* 18, 1898.]

IN the third Book of *The Wars of the Jews,* Chap. VIII. § 7, we are told that Josephus managed to save himself and a companion out of a total of 41 men, the majority of whom had resolved on self-extermination (to avoid falling into the hands of Vespasian) provided their leader died with them. The passage is very obscure, and in a sense self-contradictory, but it obviously suggests deliberate fraud of some kind on Josephus' part.

"And now," said he, "since it is resolved among you that you will die, come on, let us commit our mutual deaths to determination by lot. He whom the lot falls to first, let him be killed by him that hath the second lot, and thus fortune shall make its progress through us all; nor shall any of us perish by his own right hand, for it would be unfair if, when the rest are gone, somebody should repent and save himself." Whiston, *Works of Flavius Josephus,* IV. 39.

Bachet, in No. XXIII. of his *Problèmes plaisants et délectables,* makes a definite hypothesis as to the possible nature of the lot here spoken of; so that the problem, as we have it, is really his.

"Supposons qu'il ordonna que comptant de 3 en 3 on tuerait toujours le troisième,... il faut que Josèphe se mît le trente-unième après celui par lequel on commençait à compter, au cas qu'il visât à demeurer en vie lui tout seul. Mais s'il voulut sauver un de ses compagnons, il le mit en la seizième place, et s'il en voulut sauver encore un autre, il le mit en la trente-cinquième place."

Thus stated, the problem can be solved in a moment by the graphical process of striking out every third, in succession, of a set of 41 dots placed round a closed curve. When three only are left, they will be found to be the 35th, 16th, and 31st; and, if the process were continued, they would be exterminated in the order given. And any similar question, involving only moderate numbers, would probably be most easily solved in a similar fashion. But, suppose the number of companions of Josephus to have been of the order even of hundreds of thousands only, vastly more if of billions, this graphic method would involve immense risk of error, besides being toilsome in the extreme; and the *whole* process would have to be gone over again if we wished the solution for the case in which the total number of men is altered even by a single unit.

It is easy, however, to see that the following general statement gives the solution of all such problems:—

Let n men be arranged in a ring which closes up its ranks as individuals are picked out. Beginning anywhere, go continuously round, picking out each mth man until r only are left. Let one of these be the man who originally occupied the pth place. Then, if we had begun with $n+1$ men, one of the r left would have been the originally $(p+m)$th, or (if $p+m>n+1$) the $(p+m-n-1)$th.

In other words, provided there are always to be r left, their original positions are each shifted forwards along the closed ring by m places for each addition of a single man to the original group.

A third, but even more simple and suggestive, mode of statement may obviously be based on the illustrations which follow. In these the *original* number of each man is given in black type, the *order* in which he is struck off, if the process be carried out to the bitter end, in ordinary type.

By threes:—for groups of 8, and of 9, men respectively:—

3	5	1	7	4	2	8	6	0
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	
9	7	1	4	6	2	8	5	3
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9.**

Increase by unit every number in the first line (to which a 0 has been appended) and write it over the corresponding number in the third. We have the scheme

4	6	2	8	5	3	9	7	1,
9	7	1	4	6	2	8	5	3.

Here the numbers, and their order, are the same, but those in the lower rank are three places in advance.

By fives:—

12	10	3	5	1	11	8	7	4	2	9	6	0
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	
5	3	10	7	1	13	11	4	6	2	12	9	8
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13.**

The numbers of the first line, increased by units, and those of the third, are

13	11	4	6	2	12	9	8	5	3	10	7	1
5	3	10	7	1	13	11	4	6	2	12	9	8,

again the same order, but now shifted forwards by five places.

It is easy to see that the two rows thus formed are *identical* when $m = n + 1$. Thus

By tens:—

1	4	2	8	6	3	7	9	5	0
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	
2	5	3	9	7	4	8	10	6	1
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**,

and the statement above is obviously verified.

To show how rapidly the results of this process can be extended to higher numbers, I confine myself to the Josephus question, as regards himself alone, the last man. For the others, the mode of procedure is exactly the same.

Given that the final survivor in 41, told off by threes, is the 31st, we have

n	last man
41	31

The rule just given shows that succeeding numbers in these columns are formed as follows:—taking only those which commence, as it were, a new cycle:—

$$41 + x, \qquad 31 + 3x - (41 + x) = 2x - 10.$$

The value of x which makes the right-hand side one or other of 1, and 2, is therefore to be chosen, so we must put $x = 6$, and the result is

47	2

Successive applications of this process give, in order

70	1	13,655	2
105	1	20,482	1
158	2	30,723	1
237	2	46,085	2
355	1	69,127	1
533	2	103,691	2
799	1	155,536	1
1,199	2	233,304	1
1,798	1	349,956	1
2,697	1	524,934	1
4,046	2	787,401	1
6,069	2	1,181,102	2
9,103	1	1,771,653	2

provided the (merely arithmetical) work is correct. And, of course, we can at once interpolate for any intermediate value of n.

Thus, in 799 men, or in 30,723, the first is safe:—in 1000 the 604th; in 100,000 the 92,620th, and in 1,000,000 the 637,798th.

The earlier steps of this process, which lead at once to Bachet's number for 41 (assumed above), are

1	1	9	1
2	2	14	2
3	2	21	2
4	1	31	1
6	1		

so that the method practically deals with millions, when we reach them, more easily than it did with tens.

Unfortunately the cycles become shorter as the radix, and with it the choice of remainders, increases; so that a further improvement of process must, if possible, be introduced when every hundredth man (say) is to be knocked out.

From the data above given, it appears that up to two millions the number of cases in which the first man is safe is 19, while that in which the second is safe is only 16. (The case of one man, only, is excluded.) As these cases should, in the long run, be equally probable, I extended the calculation to

13,059,835,455,001 1

with the result of adding 20 and 19 to these numbers respectively. But the next 15 steps appear to give only 2 cases in favour of the first man!

CXXVII.

KIRCHHOFF.

[*Nature*, Vol. XXXVI. October 27, 1887.]

Geheimrath Gustav Robert Kirchhoff was born at Königsberg on the 12th of March, 1824. He commenced his professorial career at Berlin University as Privat Docent; became Extra-ordinary Professor in Breslau from 1850 to 1854, thereafter till 1874 Professor of Physics in Heidelberg, whence he was finally transferred (in a somewhat similar capacity) to Berlin. His health was seriously and permanently affected by an accident which befell him in Heidelberg many years ago, and he had been unable to lecture for some time before his death.

It is not easy, in a brief notice, to give an adequate idea of Kirchhoff's numerous and important contributions to physical science. Fortunately all his writings are easily accessible. Five years ago his collected papers (*Gesammelte Abhandlungen* von G. Kirchhoff, Leipzig, 1882) were published in a single volume. His lectures on Dynamics (*Vorlesungen über Mathematische Physik*, Leipzig, 1876) have reached at least a third edition; and his greatest work (*Untersuchungen über das Sonnenspectrum*, Berlin, 1862) was, almost immediately after its appearance, republished in an English translation (London, Macmillan). To these he has added, so far as we can discover, only three or four more recent papers; among which are, however, the following, published in the *Berlin Abhandlungen*:—

Über die Formänderung die ein fester elastischer Körper erfährt, wenn er magnetisch oder diëlectrisch polarisirt wird. (1884.)

A subsequent paper gives applications of the results (1884).

Additions to his paper (presently to be mentioned) on the Distribution of Electricity on Two Influencing Spheres. (1885.)

While there are nowadays hundreds of men thoroughly qualified to work out, to its details, a problem already couched in symbols, there are but few who have the gift of putting an entirely new physical question into such a form. The names of Stokes, Thomson, and Clerk-Maxwell will at once occur to British readers as instances of men possessing such power in a marked degree. Kirchhoff had in this respect no superior in Germany, except his life-long friend and colleague v. Helmholtz.

His first published paper, *On electric conduction in a thin plate, and specially in a circular one* (Pogg. Ann. 1845), gives an instance. The extremely elegant results he obtained are now well known, and have of course (once the start was given, or the key-note struck) been widely extended from the point of view of the pure mathematicians. The simpler results of this investigation, it must be mentioned, were fully verified by the author's experimental tracing of the equipotential lines, and by his measurements of their differences of potential. A remark appended to this paper contains two simple but important theorems which enable us to solve, by a perfectly definite process, any problem concerning the distribution of currents in a network of wires. This application forms the subject of a paper of date 1847.

Kirchhoff published subsequently several very valuable papers on electrical questions, among which may be noted those on conduction in curved sheets, on Ohm's Law, on the distribution of electricity on two influencing spheres, on the discharge of the Leyden Jar, on the motion of electricity in submarine cables, &c. Among these is a short, but important, paper on the *Determination of the constant on which depends the Intensity of induced currents* (Pogg. Ann. 1849). This involves the absolute measurement of electric resistance in a definite wire. Kirchhoff was also the inventor of a valuable addition to the Wheatstone Bridge. To the above class of papers may be added two elaborate memoirs on Induced Magnetism (*Crelle*, 1853; *Pogg. Ergänzungsband*, 1870).

Another series of valuable investigations deals with the equilibrium and motion of elastic solids, especially in the form of plates, and of rods. The British reader will find part of the substance of these papers reproduced in Thomson and Tait's *Natural Philosophy*. There are among them careful experimental determinations of the value of Poisson's Ratio (that of the lateral contraction to the axial extension of a rod under traction) for different substances. These results fully bear out the conclusions of Stokes, who was the first to point out the fallacy involved in the statement that the ratio in question is *necessarily* 1/4.

Kirchhoff's *Lectures on Dynamics* are pretty well known in this country, so that we need not describe them in detail. Like the majority of his separate papers they are somewhat tough reading, but the labour of following them is certainly recompensed. They form rather a collection of short treatises on special branches of the subject, than a systematic digest of it. One of the most noteworthy features of the earlier chapters is the mode in which dynamical principles (*e.g.* the *Laws of Motion*) are introduced. While recognizing the great simplification in processes and in verbal expression which is made possible by the use of the term Force, Kirchhoff altogether objects to the introduction of the notion of *Cause*, as a step leading only to confusion and obscurity in many fundamental questions. In fact he roundly asserts that the introduction of systems of Forces renders it impossible to give a complete definition of Force. And this, he says, depends on the result of experience that in natural motions the separate forces are always more easily specified than is their resultant. He prefers to speak of the motions which are observed to take place, and by the help of these (with the fundamental conceptions of Time, Space, and Matter) to form the general dynamical equations. Once these are obtained, their application may be much facilitated by the introduction of the *Name* Force; and we may thus express in simple terms what it would otherwise be difficult to formulate in words. So long as the motion of a single particle of matter only is concerned we can, from proper data, investigate its velocity and its acceleration, as directed quantities of definite magnitude. Thus we proceed from Kepler's Laws to find the acceleration of a planet's motion. This is discovered to be directed towards the sun, and to be in magnitude inversely as the square of the distance. We may call it by the name Force if we please, but we are not to imagine it as an active agent. Something quite analogous appears in the equations of motion when we introduce the idea of Constraint. The mode in which the idea of Mass is introduced by Kirchhoff is peculiar. It is really equivalent to a proof (ultimately based on experiments) of Newton's *Third Law*. Once, however, it is introduced, the same species of reasoning (which differs but slightly from what we should call Kinematical) leads to the establishment of D'Alembert's and Hamilton's *Principles*, with the definition of the Potential Function, the establishment of Lagrange's Generalized Equations, and the proof of Conservation of Energy, &c. The observational and experimental warrant for this mode of treatment is, according to Kirchhoff, the fact that the components of acceleration are in general found to be functions of *position*. [Kirchhoff's view of Force has some resemblance to, but is not identical with either of, the views previously published by Peirce and by the writer.] This is the chief *peculiarity* of the book, and very different opinions may naturally be held as to its value, especially as regards the strange admixture of Kinematics and Dynamics.

Of the rest, however, all who have read it must speak in the highest terms. A great deal of very valuable and original matter, sometimes dealing with extremely recondite subjects, is to be found in almost every chapter. Among these we may specially mention the investigation of surface conditions in the distortion of an elastic solid, with the treatment of capillarity, of vortex-motion, and of discontinuous fluid motion (*Flüssigkeitsstrahlen*).

Besides these definite classes of papers, there is a number of noteworthy memoirs of a more miscellaneous character:—on important propositions in the Thermodynamics of solution and vaporization, on crystalline reflection and refraction, on the influence of heat conduction in a special case of propagation of sound, on the optical constants of Aragonite, and on the Thermal Conductivity of Iron.

Finally we have the series of papers on Radiation, partly mathematical, partly experimental, which, in 1859 and 1860, produced such a profound impression in the world of science, and which culminated in the great work on the solar spectrum whose title is given above. The history of Spectrum Analysis has, from that date, been one of unbroken progress. Light from the most distant of visible bodies has been ascertained to convey a species of telegraphic message which, when we have learned to interpret it, gives us information alike of a chemical and of a purely physical character. We can analyze the atmosphere of a star, comet, or nebula, and tell (approximately at least) the temperature and pressure of the glowing gas. But, at the present time, the fact that such information is attainable is matter of common knowledge.

This is not an occasion on which we can speak of questions of priority, even though we might be specially attracted to them by finding v. Helmholtz and Sir W. Thomson publicly taking (in full knowledge of *all* the facts) almost absolutely antagonistic views. However these points may ultimately be settled, it is certain that Kirchhoff was (in 1859) entirely unaware of what Stokes and Balfour Stewart had previously done, and that he, with the powerful assistance of Bunsen, MADE what is now called Spectrum Analysis: Kirchhoff, by his elaborate comparison of the solar spectrum with the spectra of various elements, and by his artificial production of a new line whose *relative* darkness or brightness he could vary at pleasure; Bunsen by his success in discovering by the aid of the prism two new metallic elements.

CXXVIII.

HAMILTON.

[*From Encyclopædia Britannica,* 1880.]

Hamilton, Sir William Rowan, one of the really great mathematicians of the present century, was born in Dublin, August 4, 1805. His father, who was a solicitor, and his uncle (curate of Trim), migrated from Scotland in youth. A branch of the Scottish family to which they belonged had settled in the north of Ireland in the time of James I., and this fact seems to have given rise to the common impression that Hamilton was an Irishman.

His genius displayed itself, even in his infancy, at first in the form of a wonderful power of acquiring languages. At the age of seven he had already made very considerable progress in Hebrew, and before he was thirteen he had acquired, under the care of his uncle, who was an extraordinary linguist, almost as many languages as he had years of age. Among these, besides the classical and the modern European languages, were included Persian, Arabic, Hindustani, Sanskrit, and even Malay. But though to the very end of his life he retained much of the singular learning of his childhood and youth, often reading Persian and Arabic in the intervals of sterner pursuits, he had long abandoned them as a study, and employed them merely as a relaxation.

His mathematical studies seem to have been undertaken and carried to their full development without any assistance whatever, and the result is that his writings belong to no particular "school," unless indeed we consider them to form, as they are well entitled to do, a school by themselves. As an arithmetical calculator he was not only wonderfully expert, but he seems to have occasionally found a positive delight in working out to an enormous number of places of decimals the result of some irksome calculation. At the age of twelve he engaged Colburn, the American "calculating boy," who was then being exhibited as a curiosity in Dublin, and he had not always the worst of the encounter. But, two years before, he had accidentally fallen in with a Latin copy of *Euclid*, which he eagerly devoured; and at twelve he attacked Newton's *Arithmetica Universalis*. This was his introduction to modern analysis. He soon commenced to read the *Principia*, and at sixteen he had mastered a great part of that work, besides some more modern works on analytical geometry and the differential calculus.

About this period he was also engaged in preparation for entrance at Trinity College, Dublin, and had therefore to devote a portion of his time to classics. In the summer of 1822, in his seventeenth year, he began a systematic study of Laplace's *Mécanique Céleste*. Nothing could be better fitted to call forth such mathematical powers as those of Hamilton; for Laplace's great work, rich to profusion in analytical processes alike novel and powerful, demands from the most gifted student careful and often laborious study. It was in the successful effort to open this treasure-house that Hamilton's mind received its final temper. "Dès lors il commença à marcher seul," to use the words of the biographer of another great mathematician. From that time he appears to have devoted himself almost wholly to original investigation (so far at least as regards mathematics), though he ever kept himself well acquainted with the progress of science both in Britain and abroad.

Having detected an important defect in one of Laplace's demonstrations, he was induced by a friend to write out his remarks, that they might be shown to Dr Brinkley, afterwards bishop of Cloyne, who was then royal astronomer for Ireland and an accomplished mathematician. Brinkley seems at once to have perceived the vast talents of young Hamilton, and to have encouraged him in the kindest manner. He is said to have remarked in 1823 of this lad of eighteen,—"This young man, I do not say *will be*, but *is*, the first mathematician of his age."

Hamilton's career at college was perhaps unexampled. Amongst a number of competitors of more than ordinary merit, he was first in every subject, and at every examination. His is said to be the only recent case in which a student obtained the honour of an *optime* in more than one subject. This distinction had then become very rare, not being given unless the candidate displayed a thorough mastery over his subject. Hamilton received it for Greek and for physics. How many more such honours he might have attained it is impossible to say; but he was expected to win both the gold medals at the degree examination, had his career as a student not been cut short by an unprecedented event. This was his appointment to the Andrews professorship of astronomy in the university of Dublin, vacated by Dr Brinkley in 1827. The chair was not exactly offered to him, as has been sometimes asserted, but the electors, having met and talked over the subject, authorized one of their number, who was Hamilton's personal friend, to urge him to become a candidate, a step which his modesty had prevented him from taking. Thus, when barely twenty-two, he was established at the Dublin Observatory. He was not specially fitted for the post, for although he had a profound acquaintance with theoretical astronomy, he had paid but little attention to the regular work of the practical astronomer. And it must be said that his time was better employed in grand original investigations than it would have been had he spent it in meridian observations made even with the best of instruments,—infinitely better than if he had spent it on those of the observatory, which, however good originally, were then totally unfit for the delicate requirements of modern astronomy. Indeed there can be little doubt that Hamilton was intended, by the university authorities who elected him to the professorship of astronomy, to spend his time as he best could for the advancement of science, without being tied down to any particular branch. Had he devoted himself to practical astronomy they would assuredly have furnished him with modern instruments and an adequate staff of assistants.

In 1835, being secretary to the meeting of the British Association which was held that year in Dublin, he was knighted by the lord-lieutenant. But far higher honours rapidly succeeded, among which we may merely mention his election in 1837 to the president's chair in the Royal Irish Academy, and the rare and coveted distinction of being made corresponding member of the academy of St Petersburg. These are the few salient points (other, of course, than the epochs of his more important discoveries and inventions presently to be considered) in the uneventful life of this great man. He retained his wonderful faculties unimpaired to the very last, and steadily continued till within a day or two of his death (September 2, 1865) the task (his *Elements of Quaternions*) which had occupied the last six years of his life.

The germ of his first great discovery was contained in one of those early papers which in 1823 he communicated to Dr Brinkley, by whom, under the title of *Caustics*, it was presented in 1824 to the Royal Irish Academy. It was referred as usual to a committee. Their report, while acknowledging the novelty and value of its contents, and the great mathematical skill of its author, recommended that, before being published, it should be still further developed and simplified. During the next three years the paper grew to an immense bulk, principally by the additional details which had been inserted at the desire of the committee. But it also assumed a much more intelligible form, and the grand features of the new method were now easily to be seen. Hamilton himself seems not till this period to have fully understood either the nature or the importance of his discovery, for it is only now that we find him announcing his intention of applying his method to dynamics. The paper was finally entitled "Theory of Systems of Rays," and the first part was printed in 1828 in the *Transactions of the Royal Irish Academy*. The second and third parts have not yet been printed; but it is understood that their more important contents have appeared in the three voluminous supplements (to the first part) which have been published in the same *Transactions*, and in the two papers "On a General Method in Dynamics," which appeared in the *Philosophical Transactions* in 1834–5. The principle of "Varying Action" is the great feature of these papers; and it is strange, indeed, that the one particular result of this theory which, perhaps more than anything else that Hamilton has done, has rendered his name known beyond the little world of true philosophers, should have been easily within the reach of Fresnel and others for many years before, and in no way required Hamilton's new conceptions or methods, although it was by them that he was led to its discovery. This singular result is still known by the name "Conical Refraction," which he proposed for it when he first predicted its existence in the third supplement to his *Systems of Rays*, read in 1832.

The step from optics to dynamics in the application of the method of "Varying Action" was made in 1827, and communicated to the Royal Society of London, in whose *Philosophical Transactions* for 1834 and 1835 there are two papers on the subject. These display, like the "Systems of Rays," a mastery over symbols and a flow of mathematical language almost unequalled. But they contain what is far more valuable still, the greatest addition which dynamical science had received since the grand strides made by Newton and Lagrange. Jacobi and other mathematicians have developed to a great extent, and as a question of pure mathematics only, Hamilton's processes, and have thus made extensive additions to our knowledge of differential equations. But there can be little doubt that we have as yet obtained only a mere glimpse of the vast physical results of which they contain the germ. And though this is of course by far the more valuable aspect in which any such contribution to science can be looked at, the other must not be despised. It is characteristic of most of Hamilton's, as of nearly all great discoveries, that even their indirect consequences are of high value.

The other great contribution made by Hamilton to mathematical science, the Calculus of Quaternions, is fully treated under that heading. [No. CXXIX. below.] It is not necessary to say here more than this, that quaternions form as great an advance relatively to the Cartesian methods as the latter, when first propounded, formed relatively to Euclidian geometry. The following characteristic extract from a letter shows Hamilton's own opinion of his mathematical work, and also gives a hint of the devices which he employed to render written language as expressive as actual speech. His first great work, *Lectures on Quaternions* (Dublin, 1852), is almost painful to read in consequence of the frequent use of italics and capitals.

"I hope that it may not be considered as unpardonable vanity or presumption on my part, if, as my own taste has always led me to feel a greater interest in *methods* than in *results*, so it is by METHODS, rather than by *any* THEOREMS, which *can* be separately *quoted*, that I desire and hope to be remembered. Nevertheless it is only human nature, to derive *some* pleasure from being cited, now and then, even about a "Theorem"; especially where . . . . . the quoter can enrich the subject, by combining it with researches of *his own*."

The discoveries, papers, and treatises we have mentioned might well have formed the whole work of a long and laborious life. But, not to speak of his enormous collection of MS. books, full to overflowing with new and original matter, which have been handed over to Trinity College, Dublin, and of whose contents it is to be hoped a large portion may yet be published, the works we have already called attention to barely form the greater portion of what he has published. His extraordinary investigations connected with the solution of algebraic equations of the fifth degree, and his examination of the results arrived at by Abel, Jerrard, and Badano, in their researches on this subject, form another grand contribution to science. There is next his great paper on *Fluctuating Functions*, a subject which, since the time of Fourier, has been of immense and ever increasing value in physical applications of mathematics. There is also the extremely ingenious invention of the Hodograph. Of his extensive investigations into the solution (especially by numerical approximation) of certain classes of differential equations which constantly occur in the treatment of physical questions, only a few items have been published, at intervals, in the *Philosophical Magazine*. Besides all this, Hamilton was a voluminous correspondent. Often a single letter of his occupied from fifty to a hundred or more closely written pages, all devoted to the minute consideration of every feature of some particular problem: for it was one of the peculiar characteristics of his mind never to be satisfied with a general understanding of a question; he pursued it until he knew it in all its details. He was ever courteous and kind in answering applications for assistance in the study of his works, even when his compliance must have cost him much valuable time. He was excessively precise and hard to please with reference to the final polish of his own works for publication; and it was probably for this reason that he published so little compared with the extent of his investigations.

Like most men of great originality, Hamilton generally matured his ideas before putting pen to paper. "He used to carry on," says his elder son, "long trains of algebraical and arithmetical calculations in his mind, during which he was unconscious of the earthly necessity of eating; we used to bring in a 'snack' and leave it in his study, but a brief nod of recognition of the intrusion of the chop or cutlet was often the only result, and his thoughts went on soaring upwards."

For further details about Hamilton (his poetry and his association with poets, for instance), the reader is referred to the *Dublin University Magazine* (Jan. 1842), the *Gentleman's Magazine* (Jan. 1866), and the *Monthly Notices of the Royal Astronomical Society* (Feb. 1866); and also to an article by the present writer in the *North British Review* (Sept. 1866), from which much of the above sketch has been taken. [See, also, especially in connection with some of the opening statements above, *Life of Sir W. R. Hamilton* by the Rev. R. P. Graves (3 vols.; Dublin 1882–89). And, in particular, *Addendum* to that work (Dublin 1891). This *Addendum* refers particularly to the notice of Hamilton in the *Dictionary of National Biography*. On this I remarked (*Nature*, XLIII. 608), "the patent error of that notice is the confusion of Hamilton's *Varying Action* with his *Quaternions*. The consequence is that Hamilton gets no credit for his absolutely invaluable contribution to *Theoretical Dynamics*!" 1899.]

CXXIX.

QUATERNIONS.

[*From Encyclopædia Britannica*, 1886.]

THE word quaternion properly means "a set of four." In employing such a word to denote a new mathematical method, Sir W. R. Hamilton (No. CXXVIII.) was probably influenced by the recollection of its Greek equivalent, the Pythagorean Tetractys, the mystic source of all things.

Quaternions (as a mathematical *method*) is an extension, or improvement, of Cartesian geometry, in which the artifices of coordinate axes, &c., are got rid of, *all* directions in space being treated on precisely the same terms. It is therefore, except in some of its degraded forms, possessed of the perfect isotropy of Euclidian space.

From the purely geometrical point of view, a quaternion may be regarded as *the quotient of two directed lines in space*—or, what comes to the same thing, as *the factor, or operator, which changes one directed line into another*. Its analytical definition cannot be given for the moment; it will appear in the course of the article.

History of the Method.—The evolution of quaternions belongs in part to each of two weighty branches of mathematical history—the interpretation of the *imaginary* (or *impossible*) quantity of common algebra, and the Cartesian application of algebra to geometry. Sir W. R. Hamilton was led to his great invention by keeping geometrical applications constantly before him while he endeavoured to give a real significance to $\sqrt{-1}$. We will therefore confine ourselves, so far as his predecessors are concerned, to attempts at interpretation which had geometrical applications in view.

One geometrical interpretation of the negative sign of algebra was early seen to be mere *reversal* of direction along a line. Thus, when an image is formed by a plane mirror, the distance of any point in it from the mirror is simply the negative of that of the corresponding point of the object. Or if motion in one direction along a line be treated as positive, motion in the opposite direction along the same line is negative. In the case of time, measured from the Christian era, this distinction is at once given by the letters A.D. or B.C., prefixed to the date. And to find the position, in time, of one event relatively to another, we have only to subtract the date of the second (taking account of its sign) from that of the first. Thus to find the interval between the battles of Marathon (490 B.C.) and Waterloo (1815 A.D.) we have

$$+1815-(-490)=2305 \text{ years.}$$

And it is obvious that the same process applies in all cases in which we deal with quantities which may be regarded as of one directed dimension only, such as distances along a line, rotations about an axis, &c. But it is essential to notice that this is by no means necessarily true of *operators*. To turn a line through a certain angle in a given plane, a certain operator is required; but when we wish to turn it through an equal negative angle we must not, in general, employ the negative of the former operator. For the negative of the operator which turns a line through a given angle in a given plane will in all cases produce the negative of the original result, which is *not* the result of the reverse operator, unless the angle involved be an odd multiple of a right angle. This is, of course, on the usual assumption that the sign of a product is changed when that of *any one* of its factors is changed,—which merely means that -1 is commutative with all other quantities.

The celebrated Wallis seems to have been the first to push this idea further. In his *Treatise of Algebra* (1685) he distinctly proposes to construct the imaginary roots of a quadratic equation by going *out of* the line on which the roots, if real, would have been constructed.

In 1804 the Abbé Buée*, apparently without any knowledge of Wallis's work, developed this idea so far as to make it useful in geometrical applications. He gave, in fact, the theory of what in Hamilton's system is called *Composition of Vectors in one plane*—*i.e.*, the combination, by + and −, of complanar directed lines. His constructions are based on the idea that the imaginaries $\pm\sqrt{-1}$ represent a unit line, and its reverse, *perpendicular to* the line on which the real units ± 1 are measured. In this sense the imaginary expression $a+b\sqrt{-1}$ is constructed by measuring a length a along the fundamental line (for real quantities), and from its extremity a line of length b in some direction perpendicular to the fundamental line. But he did not attack the question of the representation of products or quotients of directed lines. The step he took is really nothing more than the kinematical principle of the composition of linear velocities, but expressed in terms of the algebraic imaginary.

In 1806 (the year of *publication* of Buée's paper) Argand published a pamphlet† in which precisely the same ideas are developed, but to a considerably greater extent. For

* *Phil. Trans.*, 1806.

† *Essai sur une manière de représenter les Quantités Imaginaires dans les Constructions Géométriques.* A second edition was published by Hoüel (Paris, 1874). There is added an important *Appendix*, consisting of the papers from *Gergonne's Annales* which are referred to in the text above. Almost nothing can, it seems, be learned of Argand's private life, except that in all probability he was born at Geneva in 1768.

an interpretation is assigned to the *product* of two directed lines in one plane, when each is expressed as the sum of a real and an imaginary part. This product is interpreted as another directed line, forming the fourth term of a proportion, of which the first term is the real (positive) unit-line, and the other two are the factor-lines. Argand's work remained unnoticed until the question was again raised in *Gergonne's Annales*, 1813, by Français. This writer stated that he had found the germ of his remarks among the papers of his deceased brother, and that they had come from Legendre, who had himself received them from some one unnamed. This led to a letter from Argand, in which he stated his communications with Legendre, and gave a résumé of the contents of his pamphlet. In a further communication to the *Annales*, Argand pushed on the applications of his theory. He has given by means of it a simple proof of the existence of n roots, and no more, in every rational algebraic equation of the nth degree with real coefficients. About 1828 Warren in England, and Mourey in France, independently of one another and of Argand, reinvented these modes of interpretation; and still later, in the writings of Cauchy, Gauss, and others, the properties of the expression $a+b\sqrt{-1}$ were developed into the immense and most important subject now called the *theory of complex numbers*. From the more purely symbolical point of view it was developed by Peacock, De Morgan, &c., as *double algebra*.

Argand's method may be put, for reference, in the following form. The directed line whose length is a, and which makes an angle θ with the real (positive) unit line, is expressed by

$$a(\cos\theta + i\sin\theta),$$

where i is regarded as $+\sqrt{-1}$. The sum of two such lines (formed by adding together the real and the imaginary parts of two such expressions) can, of course, be expressed as a third directed line—the diagonal of the parallelogram of which they are conterminous sides. The product, P, of two such lines is, as we have seen, given by

$$1 : a(\cos\theta + i\sin\theta) :: a'(\cos\theta' + i\sin\theta') : P,$$

or

$$P = aa'\{\cos(\theta+\theta') + i\sin(\theta+\theta')\}.$$

Its length is, therefore, the product of the lengths of the factors, and its inclination to the real unit is the sum of those of the factors. If we write the expressions for the two lines in the form

$$A + Bi, \quad A' + B'i,$$

the product is

$$AA' - BB' + i(AB' + BA');$$

and the fact that the length of the product line is the product of those of the factors is seen in the form

$$(A^2 + B^2)(A'^2 + B'^2) = (AA' - BB')^2 + (AB' + BA')^2.$$

In the modern theory of complex numbers this is expressed by saying that the *Norm* of a product is equal to the product of the norms of the factors.

Argand's attempts to extend his method to space generally were fruitless. The reasons will be obvious later; but we mention them just now because they called forth from Servois (*Gergonne's Annales*, 1813) a very remarkable comment, in which was contained the only yet discovered trace of an anticipation of the method of Hamilton. Argand had been led to deny that such an expression as i^i could be expressed in the form $A+Bi$,—although, as is well known, Euler showed that one of its values is a real quantity, the exponential function of $-\pi/2$. Servois says, with reference to the general representation of a directed line in space:—

"L'analogie semblerait exiger que le trinôme fût de la forme

$$p\cos\alpha + q\cos\beta + r\cos\gamma;$$

α, β, γ étant les angles d'une droite avec trois axes rectangulaires; et qu'on eût

$$(p\cos\alpha + q\cos\beta + r\cos\gamma)(p'\cos\alpha + q'\cos\beta + r'\cos\gamma) = \cos^2\alpha + \cos^2\beta + \cos^2\gamma = 1.$$

Les valeurs de p, q, r, p', q', r' qui satisferaient à cette condition seraient *absurdes*; mais seraient-elles imaginaires, reductibles à la forme générale $A+B\sqrt{-1}$? Voilà une question d'analyse fort singulière que je soumets à vos lumières. La simple proposition que je vous en fais suffit pour vous faire voir que je ne crois point que toute fonction analytique non réelle soit vraiment reductible à la forme $A+B\sqrt{-1}$."

As will be seen later, the fundamental i, j, k of quaternions, with their reciprocals, furnish a set of six quantities which satisfy the conditions imposed by Servois. And it is quite certain that they cannot be represented by ordinary imaginaries.

Something far more closely analogous to quaternions than anything in Argand's work ought to have been suggested by De Moivre's theorem (1730). Instead of regarding, as Buée and Argand had done, the expression $a(\cos\theta + i\sin\theta)$ as a directed line, let us suppose it to represent the *operator* which, when applied to *any* line in the plane in which θ is measured, turns it in that plane through the angle θ, and at the same time increases its length in the ratio $a:1$. From the new point of view we see at once, as it were, *why* it is true that

$$(\cos\theta + i\sin\theta)^m = \cos m\theta + i\sin m\theta.$$

For this equation merely states that m turnings of a line through successive equal angles, in one plane, give the same result as a single turning through m times the common angle. To make this process applicable to *any* plane in space, it is clear that we must have *a special value of* i for each such plane. In other words, a unit line, drawn in any direction whatever, must have -1 for its square. In such a system there will be no line in space specially distinguished as the *real unit line*: all will be alike imaginary, or rather alike real. We may state, in passing, that every quaternion can be represented as $a(\cos\theta + \varpi\sin\theta)$,—where a is a real number, θ a real angle, and ϖ a directed unit line whose square is -1. Hamilton took this grand step, but, as we have already said, without any help from the previous work of De Moivre. The course of his investigations is minutely described in the preface to his first great work* on the subject. Hamilton, like most of the many inquirers who endeavoured to give a real interpretation to the imaginary of common algebra, found that at least two kinds, orders, or

* *Lectures on Quaternions*, Dublin, 1853.

ranks of quantities were necessary for the purpose. But, instead of dealing with points on a line, and then wandering out at right angles to it, as Buée and Argand had done, he chose to look on algebra as the science of *pure time**, and to investigate the properties of "sets" of time-steps. In its essential nature a set is a linear function of any number of *distinct* units of the same species. Hence the simplest form of a set is a *couple*; and it was to the possible laws of combination of couples that Hamilton first directed his attention. It is obvious that the way in which the two separate time-steps are involved in the couple will determine these laws of combination. But Hamilton's special object required that these laws should be such as to lead to certain assumed results; and he therefore commenced by assuming these, and from the assumption determined how the separate time-steps must be involved in the couple. If we use Roman letters for mere numbers, capitals for instants of time, Greek letters for time-steps, and a parenthesis to denote a couple, the laws assumed by Hamilton as the basis of a system were as follows:—

$$(B_1,\ B_2) - (A_1,\ A_2) = (B_1 - A_1,\ B_2 - A_2) = (\alpha,\ \beta);$$

$$(\mathrm{a},\ \mathrm{b})(\alpha,\ \beta) = (\mathrm{a}\alpha - \mathrm{b}\beta,\ \mathrm{b}\alpha + \mathrm{a}\beta)\dagger.$$

To show how we give, by such assumptions, a real interpretation to the ordinary algebraic imaginary, take the simple case $\mathrm{a} = 0$, $\mathrm{b} = 1$, and the second of the above formulæ gives

$$(0,\ 1)(\alpha,\ \beta) = (-\beta,\ \alpha).$$

Multiply once more by the number-couple (0, 1), and we have

$$(0,\ 1)(0,\ 1)(\alpha,\ \beta) = (0,\ 1)(-\beta,\ \alpha) = (-\alpha,\ -\beta)$$
$$= (-1,\ 0)(\alpha,\ \beta) = -(\alpha,\ \beta).$$

Thus the number-couple (0, 1), when twice applied to a step-couple, simply changes its sign. That we have here a perfectly *real* and intelligible interpretation of the ordinary algebraic imaginary is easily seen by an illustration, even if it be a somewhat extravagant one. Some Eastern potentate, possessed of absolute power, covets the vast possessions of his vizier and of his barber. He determines to rob them both (an operation which may be very satisfactorily expressed by -1); but, being a wag, he chooses his own way of doing it. He degrades his vizier to the office of barber, taking all his goods in the process; and makes the barber his vizier. Next day he repeats the operation. Each of the victims has been restored to his former rank, but the operator -1 has been applied to both.

Hamilton, still keeping prominently before him as his great object the invention of a method applicable to space of three dimensions, proceeded to study the properties of *triplets* of the form $x + iy + jz$, by which he proposed to represent the directed line in space whose projections on the coordinate axes are x, y, z. The composition of two such lines by the algebraic addition of their several projections agreed with the

* *Theory of Conjugate Functions, or Algebraic Couples, with a Preliminary and Elementary Essay on Algebra as the Science of Pure Time*, read in 1833 and 1835, and published in *Trans. R. I. A.*, XVII. ii. (1835).

† Compare these with the long-subsequent ideas of Grassmann, presently to be described.

assumption of Buée and Argand for the case of coplanar lines. But, assuming the *distributive* principle, the product of two lines appeared to give the expression

$$xx' - yy' - zz' + i(yx' + xy') + j(xz' + zx') + ij(yz' + zy').$$

For the square of j, like that of i, was assumed to be negative unity. But the interpretation of ij presented a difficulty,—in fact *the main difficulty* of the whole investigation,—and it is specially interesting to see how Hamilton attacked it. He saw that he could get a hint from the simpler case, already thoroughly discussed, provided the two factor lines were in one plane through the real unit line. This requires merely that

$$y : z :: y' : z'; \text{ or } yz' - zy' = 0;$$

but then the product should be of the same form as the separate factors. Thus, in this special case, the term in ij ought to vanish. But the numerical factor appears to be $yz' + zy'$, while it is the quantity $yz' - zy'$ which really vanishes. Hence Hamilton was at first inclined to think that ij must be treated as *nil*. But he soon saw that "a less harsh supposition" would suit the simple case. For his speculations on sets had already familiarized him with the idea that multiplication might in certain cases not be commutative; so that, as the last term in the above product is made up of the two separate terms $ijyz'$ and $jizy'$, the term would vanish of itself when the factor lines are coplanar provided $ij = -ji$, for it would then assume the form $ij(yz' - zy')$. He had now the following expression for the product of any two directed lines

$$xx' - yy' - zz' + i(yx' + xy') + j(xz' + zx') + ij(yz' - zy').$$

But his result had to be submitted to another test, the Law of the Norms. As soon as he found, by trial, that this law was satisfied, he took the final step. "This led me," he says, "to conceive that perhaps, instead of seeking to *confine* ourselves to *triplets*,......we ought to regard these as only *imperfect forms of* Quaternions,......and that thus my old conception of *sets* might receive a new and useful application." In a very short time he settled his fundamental assumptions. He had now three distinct space-units i, j, k; and the following conditions regulated their combination by multiplication:—

$$i^2 = j^2 = k^2 = -1, \quad ij = -ji = k, \quad jk = -kj = i, \quad ki = -ik = j^*.$$

And *now* the product of two quaternions could be at once expressed as a third quaternion, thus—

$$(a + ib + jc + kd)(a' + ib' + jc' + kd') = A + iB + jC + kD,$$

where

$$\begin{aligned} A &= aa' - bb' - cc' - dd', \\ B &= ab' + ba' + cd' - dc', \\ C &= ac' + ca' + db' - bd'. \\ D &= ad' + da' + bc' - cb'. \end{aligned}$$

Hamilton at once found that the Law of the Norms holds,—not being aware that

* It will be easy to see that, instead of the last three of these, we may write the single one $ijk = -1$.

Euler had long before decomposed the product of two sums of four squares into this very set of four squares. And now a directed line in space came to be represented as $ix+jy+kz$, while the product of two lines is the quaternion

$$-(xx'+yy'+zz')+i(yz'-zy')+j(zx'-xz')+k(xy'-yx').$$

To any one acquainted, even to a slight extent, with the elements of Cartesian geometry of three dimensions, a glance at the extremely suggestive constituents of this expression shows how justly Hamilton was entitled to say—"When the conception......had been so far unfolded and fixed in my mind, I felt that the *new instrument for applying calculation to geometry*, for which I had so long sought, was now, at least in part, attained." The date of this memorable discovery is October 16, 1843.

We can devote but a few lines to the consideration of the expression above. Suppose, for simplicity, the factor lines to be each of unit length. Then x, y, z, x', y', z' express their direction-cosines. Also, if θ be the angle between them, and x'', y'', z'' the direction-cosines of a line perpendicular to each of them, we have

$$xx'+yy'+zz'=\cos\theta,\ \ yz'-zy'=x''\sin\theta,\ \ \&c.,$$

so that the product of two unit lines is now expressed as

$$-\cos\theta+(ix''+jy''+kz'')\sin\theta.$$

Thus, when the factors are parallel, or $\theta=0$, the product, which is now the square of any (unit) line, is -1. And when the two factor lines are at right angles to one another, or $\theta=\pi/2$, the product is simply $ix''+jy''+kz''$, the unit line perpendicular to both. Hence, and in this lies the main element of the symmetry and simplicity of the quaternion calculus, *all systems of three mutually rectangular unit lines in space have the same properties as the fundamental system* i, j, k. In other words, if the system (considered as rigid) be made to turn about till the *first* factor coincides with i and the second with j, the product will coincide with k. This fundamental system, therefore, becomes unnecessary; and the quaternion method, in every case, takes its reference lines solely from the problem to which it is applied. It has therefore, as it were, a unique *internal* character of its own.

Hamilton, having gone thus far, proceeded to evolve these results from a train of *a priori* or metaphysical reasoning, which is so interesting in itself, and so characteristic of the man, that we briefly sketch its nature.

Let it be supposed that the product of two directed lines is something which has quantity; *i.e.*, it may be halved, or doubled, for instance. Also let us assume (a) space to have the same properties in all directions, and make the convention (b) that to change the sign of any one factor changes the sign of a product. Then the product of two lines which have the same direction *cannot be*, even in part, *a directed quantity*. For, if the directed part have the same direction as the factors, (b) shows that it will be reversed by reversing either, and therefore will recover its original direction when both are reversed. But this would obviously be inconsistent with (a). If it be perpendicular to the factor lines, (a) shows that it must have simultaneously every such direction. Hence it *must* be a mere number.

Again, the product of two lines at right angles to one another cannot, even in part, be a number. For the reversal of either factor must, by (b), change its sign. But, if we look at the two factors in their new position by the light of (a), we see that the sign must not change. But there is nothing to prevent its being represented by a directed line if, as farther applications of (a) and (b) show we must do, we take it perpendicular to each of the factor lines.

Hamilton seems never to have been quite satisfied with the apparent *heterogeneity* of a quaternion, depending as it does on a numerical and a directed part. He indulged in a great deal of speculation as to the existence of an *extra-spatial unit,* which was to furnish the *raison d'être* of the numerical part, and render the quaternion *homogeneous* as well as linear. But, for this, we must refer to his own works.

Hamilton was not the only worker at the theory of sets. The year after the first publication of the quaternion method, there appeared a work of great originality, by Grassmann*, in which results closely analogous to some of those of Hamilton were given. In particular two species of multiplication ("inner" and "outer") of directed lines in one plane were given. The results of these two kinds of multiplication correspond respectively to the numerical and the directed parts of Hamilton's quaternion product. But Grassmann distinctly states in his preface that he had not had leisure to extend his method to angles in space. Hamilton and Grassmann, while their earlier work had much in common, had very different objects in view. Hamilton, as we have seen, had geometrical application as his main object; when he realized the quaternion system, he felt that his object was gained, and thenceforth confined himself to the development of his method. Grassmann's object seems to have been, all along, of a much more ambitious character, viz., to discover, if possible, a system or systems in which every conceivable mode of dealing with sets should be included. That he made very great advances towards the attainment of this object all will allow; that his method, even as completed in 1862, fully attains it is not so certain. But his claims, however great they may be, can in no way conflict with those of Hamilton, whose mode of multiplying *couples* (in which the "inner" and "outer" multiplication are essentially involved) was produced in 1833, and whose quaternion system was completed and published before Grassmann had elaborated for press even the rudimentary portions of his own system, in which the veritable difficulty of the whole subject, the application to angles in space, had not even been attacked. Grassmann made in 1854 a somewhat savage onslaught on Cauchy and De St Venant, the former of whom had invented, while the latter had exemplified in application, the system of "*clefs algébriques*," which is almost precisely that of Grassmann. [See letter now appended to this article. 1899.] But it is to be observed that Grassmann, though he virtually accused Cauchy of plagiarism, does not appear to have preferred any such charge against Hamilton. He does not allude to Hamilton in the second edition of

* *Die Ausdehnungslehre*, Leipsic, 1844; 2d ed., "*vollständig und in strenger Form bearbeitet*," Berlin, 1862. See also the collected works of Möbius, and those of Clifford, for a general explanation of Grassmann's method.

his work. But in 1877, in the *Mathematische Annalen*, XII., he gave a paper "On the Place of Quaternions in the *Ausdehnungslehre*," in which he condemns, as far as he can, the nomenclature and methods of Hamilton.

There are many other systems, based on various principles, which have been given for application to geometry of directed lines, but those which deal with products of lines are all of such complexity as to be practically useless in application. Others, such as the *Barycentrische Calcül* of Möbius, and the *Méthode des Équipollences* of Bellavitis, give elegant modes of treating space problems, so long as we confine ourselves to projective geometry and matters of that order; but they are limited in their field, and therefore need not be discussed here. More general systems, having close analogies to quaternions, have been given since Hamilton's discovery was published. As instances we may take Goodwin's and O'Brien's papers in the *Cambridge Philosophical Transactions* for 1849.

Relations to other Branches of Science.—Even the above brief narrative shows how close is the connexion between quaternions and the ordinary Cartesian space-geometry. Were this all, the gain by their introduction would consist mainly in a clearer insight into the mechanism of coordinate systems, rectangular or not—a very important addition to theory, but little advance so far as practical application is concerned. But we have now to consider that, as yet, we have not taken advantage of the *perfect symmetry* of the method. When that is done, the full value of Hamilton's grand step becomes evident, and the gain is quite as extensive from the practical as from the theoretical point of view. Hamilton, in fact, remarks*, "I regard it as an inelegance and imperfection in this calculus, or rather in the state to which it has hitherto been unfolded, whenever it becomes, or *seems* to become, necessary to have recourse......to the resources of ordinary algebra, for the *solution of equations in quaternions*." This refers to the use of the x, y, z coordinates,—associated, of course, with i, j, k. But when, instead of the highly artificial expression $ix+jy+kz$, to denote a finite directed line, we employ a single letter, α (Hamilton uses the Greek alphabet for this purpose), and find that we are permitted to deal with it exactly as we should have dealt with the more complex expression, the immense gain is at least in part obvious. Any quaternion may now be expressed in numerous simple forms. Thus we may regard it as the sum of a number and a line, $a+\alpha$, or as the product, $\beta\gamma$, or the quotient, $\delta\epsilon^{-1}$, of two directed lines, &c., while, in many cases, we may represent it, so far as it is required, by a single letter such as q, r, &c.

Perhaps to the student there is no part of elementary mathematics so repulsive as is spherical trigonometry. Also, everything relating to change of systems of axes, as for instance in the kinematics of a rigid system, where we have constantly to consider one set of rotations with regard to axes fixed in space, and another set with regard to axes fixed in the system, is a matter of troublesome complexity by the usual methods. But every quaternion formula is a proposition in spherical (sometimes degrading to plane) trigonometry, and has the full advantage of the symmetry of the method. And one of Hamilton's earliest advances in the study of his system (an advance independently made, only a few months later, by Cayley) was the interpretation of the

* *Lectures on Quaternions*, § 513.

singular operator $q(\ \ )q^{-1}$, where q is a quaternion. Applied to *any* directed line, this operator at once turns it, *conically*, through a definite angle, about a definite axis. Thus rotation is now expressed in symbols at least as simply as it can be exhibited by means of a model. Had quaternions effected nothing more than this, they would still have inaugurated one of the most necessary, and apparently impracticable, of reforms.

The physical properties of a heterogeneous body (provided they vary *continuously* from point to point) are known to depend, in the neighbourhood of any one point of the body, on a quadric function of the coordinates with reference to that point. The same is true of physical quantities such as potential, temperature, &c., throughout small regions in which their variations are continuous; and also, without restriction of dimensions, of moments of inertia, &c. Hence, in addition to its geometrical applications to surfaces of the second order, the theory of quadric functions of position is of fundamental importance in physics. Here the symmetry points at once to the selection of the three principal axes as the directions for i, j, k; and it would appear at first sight as if quaternions could not simplify, though they might improve in elegance, the solution of questions of this kind. But it is not so. Even in Hamilton's earlier work it was shown that all such questions were reducible to the *solution of linear equations in quaternions*; and he proved that this, in turn, depended on the determination of a certain operator, which could be represented for purposes of calculation by a single symbol. The method is essentially the same as that developed, under the name of "matrices" by Cayley in 1858; but it has the peculiar advantage of the simplicity which is the natural consequence of entire freedom from conventional reference lines.

Sufficient has already been said to show the close connexion between quaternions and the theory of numbers. But one most important connexion with modern physics must be pointed out, as it is probably destined to be of great service in the immediate future. In the theory of surfaces, in hydrokinetics, heat-conduction, potentials, &c., we constantly meet with what is called *Laplace's operator*, viz.,

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}.$$

We know that this is an *invariant*; *i.e.*, it is independent of the particular directions chosen for the rectangular coordinate axes. Here, then, is a case specially adapted to the isotropy of the quaternion system; and Hamilton easily saw that the expression

$$i\frac{d}{dx}+j\frac{d}{dy}+k\frac{d}{dz}$$

could be, like $ix+jy+kz$, effectively expressed by a single letter. He chose for this purpose ∇. And we now see that the square of ∇ is the negative of Laplace's operator; while ∇ itself, when applied to any numerical quantity conceived as having a definite value at each point of space, gives *the direction and the rate of most rapid change* of that quantity. Thus, applied to a potential, it gives the direction and magnitude of the force; to a distribution of temperature in a conducting solid, it gives (when multiplied by the conductivity) the flux of heat, &c.

No better testimony to the value of the quaternion method could be desired than the constant use made of its notation by mathematicians like Clifford (in his *Kinematic*) and by physicists like Clerk-Maxwell (in his *Electricity and Magnetism*). Neither of these men professed to employ the calculus itself, but they recognized fully the extraordinary clearness of insight which is gained even by merely translating the unwieldy Cartesian expressions met with in hydrokinetics and in electrodynamics into the pregnant language of quaternions.

Works on the Subject.—Of course the great works on this subject are the two immense treatises by Hamilton himself. Of these the second (*Elements of Quaternions*, London, 1866; 2nd ed. 1899) was posthumous—incomplete in one short part of the original plan only, but that a most important part, the theory and applications of ∇. These two works, along with Hamilton's other papers on quaternions (in the *Dublin Proceedings* and *Transactions*, the *Philosophical Magazine*, &c.), are storehouses of information, of which but a small portion has yet been extracted. A German translation of Hamilton's *Elements* has recently been published by Glan.

Other works on the subject, in order of date, are Allegret, *Essai sur le Calcul des Quaternions* (Paris, 1862); Tait, *An Elementary Treatise on Quaternions* (Oxford, 1867; 2nd ed., Cambridge, 1873; 3rd, 1890; German translation by v. Scherff, 1880, and French by Plarr, 1882—84); Kelland and Tait, *Introduction to Quaternions* (London, 1873; 2nd ed. 1882); Hoüel, *Éléments de la Théorie des Quaternions* (Paris, 1874); Unverzagt, *Theorie der Quaternionen* (Wiesbaden, 1876); Laisant, *Introduction à la Méthode des Quaternions* (Paris, 1881); Graefe, *Vorlesungen über die Theorie der Quaternionen* (Leipsic, 1884). [To these must now be added McAulay, *Utility of Quaternions in Physics*, London, 1893; as well as a number of elementary treatises. 1899.]

An excellent article on the "Principles" of the science, by Dillner, will be found in the *Mathematische Annalen*, vol. XI., 1877. And a very valuable article on the general question, *Linear Associative Algebra*, by the late Prof. Peirce, was ultimately printed in vol. IV. of the *American Journal of Mathematics*. Sylvester and others have recently published extensive contributions to the subject, including quaternions under the general class *matrix*, and have developed much farther than Hamilton lived to do the solution of equations in quaternions. Several of the works named above are little more than compilations, and some of the French ones are painfully disfigured by an attempt to introduce an improvement of Hamilton's notation; but the mere fact that so many have already appeared shows the sure progress which the method is now making.

[In an article by Prof. F. Klein (*Math. Ann.* LI. 1898) a claim is somewhat obscurely made for Gauss to a share, at least, in the invention of Quaternions. Full information on the subject is postponed till the publication of Gauss' *Nachlass*, in Vol. VIII. of his *Gesammelte Werke*. From the article mentioned above, and from a "Digression on Quaternions" in Klein und Sommerfeld *Ueber die Theorie des Kreisels* (p. 58), this claim appears to rest on some singular misapprehension of the nature of a Quaternion:—whereby it is identified with a totally different kind of concept, a certain very restricted form of linear and vector *Operator*. 1899.]

APPENDIX.

(*Reprinted on account of the passage now marked. See* p. 452 *above.*)

QUATERNIONS AND THE AUSDEHNUNGSLEHRE.

[*Nature*, June 4th, 1891.]

Prof. Gibbs' second long letter was evidently written before he could have read my reply to the first. This is unfortunate, as it tends to confuse those third parties who may be interested in the question now raised. Of course that question is naturally confined to the invention of methods, for it would be preposterous to compare Grassmann with Hamilton as an analyst.

I have again read my article "Quaternions" in the *Encyc. Brit.*, and have consulted once more the authorities there referred to. I have not found anything which I should wish to *alter*. There is much, of course, which I should have liked to extend, had the Editor permitted. An article on Quaternions, rigorously limited to four pages, could obviously be no place for a discussion of Grassmann's scientific work, except in its bearings upon Hamilton's calculus. Moreover, had a similar article on the *Ausdehnungslehre* been asked of me, I should certainly have declined to undertake it. Since 1860, when I ceased to be a Professor of Mathematics, I have paid no special attention to general systems of *Sets*, *Matrices*, or *Algebras*; and without much further knowledge I should not attempt to write in any detail about such subjects. I may, however, call attention to the facts which follow: for they appear to be decisive of the question now raised. Cauchy (*Comptes Rendus*, 10/1/53) claimed *quaternia* as a special case of his "clefs algébriques." Grassmann, in turn (*Comptes Rendus*, 17/4/54; and *Crelle*, 49), declared Cauchy's methods to be precisely those of the Ausdehnungslehre. But Hamilton (*Lectures*, Pref. p. 64, foot-note), says of the clefs algébriques (and therefore, *on Grassmann's own showing*, of the methods of the Ausdehnungslehre) that they are "*included* in that theory of SETS in algebra......announced by me in 1835......of which SETS I have always considered the QUATERNIONS......to be merely a *particular* CASE."

But all this has nothing to do with Quaternions, regarded as a calculus "*uniquely* adapted to Euclidian space." Grassmann lived to have his fling at them, but (so far as I know) he ventured on no claim to priority. Hamilton, on the other hand, even after reading the first *Ausdehnungslehre*, did claim priority and was never answered. He quoted, and commented upon, the very passage (of the *Preface* to that work) my allusion to which is censured by Prof. Gibbs. [*Lectures*, Pref. p. 62, footnote.] I still think, and it would seem that Hamilton also thought, that it was *solely because Grassmann had not realized the conception of the quaternion*, whether as $\beta\alpha$ or as $\beta\alpha^{-1}$, that he felt those difficulties (as to angles in space) which he says he had not had leisure to overcome. I have not seen the original work, but I have consulted what professes to be a *verbatim* reprint, produced under the author's supervision. [*Die Ausdehnungslehre von* 1844, *oder die lineale Ausdehnungslehre, &c. Zweite, im Text unveränderte Auflage.* Leipzig, 1878.] Prof. Gibbs' citations from my article give a very incomplete and one-sided representation of the few remarks I felt it necessary and sufficient to make about Grassmann. I need not quote them here, as anyone interested in the matter can readily consult the article.

In regard to Matrices, I do not think I have ever claimed anything for Hamilton beyond the *separable* ϕ, and the symbolic cubic (or biquadratic, as the case may be) with its linear factors; and these I still assert to be exclusively his. My own work in this direction has been confined to Hamilton's ϕ, with its square-root, its applications to stress and strain, &c.

As to the general history, of which (as I have said above) I claim no exact or extensive knowledge, Cayley and Sylvester will, no doubt, defend themselves if they see fit. It would be at once ridiculous and impertinent on my part were I to take up the cudgels in their behalf.

P. G. TAIT.

CXXX.

RADIATION AND CONVECTION.

[*From Encyclopædia Britannica*, 1886.]

1. WHEN a red-hot cannon ball is taken out of a furnace and suspended in the air it is observed to cool, *i.e.*, to part with heat, and it continues to do so at a gradually diminishing rate till it finally reaches the temperature of the room. But the process by which this effect is produced is a very complex one. If the hand be held at a distance of a few inches from the hot ball on either side of it or *below* it, the feeling of warmth experienced is considerable; but it becomes intolerable when the hand is held at the same distance *above* the ball. Even this rude form of experiment is sufficient to show that two processes of cooling are simultaneously at work, —one which apparently leads to the loss of heat in all directions indifferently, another which leads to a special loss in a vertical direction upwards. If the experiment is made in a dark room, into which a ray of sunlight is admitted so as to throw a shadow of the ball on a screen, we see that the column of air above the ball also casts a distinct shadow. It is, in fact, a column of air very irregularly heated by contact with the ball, and rising, in obedience to hydrostatic laws, in the colder and denser air around it. This conveyance of heat by the motion of the heated body itself is called *convection*; the process by which heat is lost indifferently in all directions is called *radiation.* These two processes are entirely different in their nature, laws, and mechanism; but we have to treat of both in the present article.

2. To illustrate how the third method by which heat can be transferred, viz. *conduction,* is involved in this process, let the cannon ball (which for this purpose should be a large one) be again heated and at once immersed in water until it just ceases to be luminous in the dark, and then be immediately hung up in the air. After a short period it again becomes red-hot all over, and the phenomenon then

proceeds precisely as before, except that the surface of the ball does not become so hot as it was before being plunged in the water. This form of experiment, which requires that the interior shall be very considerably cooled before the surface ceases to be self-luminous, does not succeed nearly so well with a copper ball as with an iron one, on account of the comparatively high conductivity of copper. In fact, even when its surface is covered with lamp-black, to make the loss by radiation as great as possible, the difference of temperature between the centre and the surface of a very hot copper ball—which is only an inch or two in diameter—is inconsiderable.

3. In conduction there is passage of heat from hotter to colder parts of the same body; in convection an irregularly heated fluid becomes hydrostatically unstable, and each part carries its heat with it to its new position. In both processes heat is conveyed from place to place. But it is quite otherwise with radiation. That a body cools in consequence of radiation is certain; that other bodies which absorb the radiation are thereby heated is also certain; but it does not at all follow that what passes in the radiant form is heat. To return for a moment to the red-hot cannon ball. If, while the hand is held below it, a thick but dry plate of rock-salt is interposed between the ball and the hand there is no perceptible diminution of warmth, and the temperature of the salt is not perceptibly raised by the radiation which passes through it. When a piece of clear ice is cut into the form of a large burning-glass it can be employed to inflame tinder by concentrating the sun's rays, and the lens does the work nearly as rapidly as if it had been made of glass. It is certainly not what we ordinarily call "heat" which can be transmitted under conditions like these. Radiation is undoubtedly a transference of energy, which was in the form commonly called heat in the radiating body, and becomes heat in a body which absorbs it; but it is transformed as it leaves the first body, and retransformed when it is absorbed by the second. Until the comparatively recent full recognition of the conservation and transformation of energy it was almost impossible to form precise ideas on matters like this; and, consequently, we find in the writings even of men like Prévost and Sir J. Leslie notions of the wildest character as to the mechanism of radiation. Leslie, strangely, regarded it as a species of "pulsation" in the air, in some respects analogous to sound, and propagated with the same speed as sound. Prévost, on the other hand, says, "Le calorique est un fluide discret; chaque élément de calorique suit constamment la même ligne droite, tant qu'aucun obstacle ne l'arrête. Dans un espace chaud, chaque point est traversé sans cesse en tout sens par des filets de calorique."

4. The more intensely the cannon ball is heated the more luminous does it become, and also the more nearly white is the light which it gives out. So well is this known that in almost all forms of civilized speech there are terms corresponding to our "red-hot," "white-hot," &c. As another instance, suppose a powerful electric current is made to pass through a stout iron wire. The wire becomes gradually hotter, up to a certain point, at which the loss by radiation and convection just balances the gain of heat by electric resistance. And as it becomes hotter the amount of its radiation increases, till at a definite temperature it becomes just visible in the dark by red rays of low refrangibility. As it becomes still hotter the whole radiation increases;

the red rays formerly given off become more luminous, and are joined by others of higher refrangibility. This process goes on, the whole amount of radiation still increasing, each kind of visible light becoming more intense, and new rays of light of higher refrangibility coming in, until the whole becomes white, *i.e.*, gives off all the more efficient kinds of visible light in much the same relative proportion as that in which they exist in sunlight. When the circuit is broken, exactly the same phenomena occur in the reverse order, the various kinds of light disappearing later as their refrangibility is less. But the radiation continues, growing weaker every instant, even after the whole is dark. This simple observation irresistibly points to the conclusion that the so-called "radiant heat" is precisely the same phenomenon as "light," only the invisible rays are still less refrangible than the lowest red, and that our sense of sight is confined to rays of a certain definite range of refrangibility, while the sense of touch comes in where sight fails us. Sir W. Herschel in 1798, by placing the bulb of a thermometer in the solar spectrum formed by a flint-glass prism, found that the highest temperature was in the dark region outside the lowest visible red,—a result amply verified at the time by others, though warmly contested by Leslie.

5. This striking conclusion is not without close analogies in connection with the other senses, especially that of hearing. Thus it has long been known that the "range of hearing" differs considerably in different individuals, some, for instance, being painfully affected by the chirp of a cricket, which is inaudible to others whose general hearing is quite as good. Extremely low notes, on the other hand, of whose existence we have ample dynamical evidence, are not heard by any one; when perceived at all they are *felt*.

6. We may now rapidly run over the principal facts characteristic of the behaviour of visible rays, and point out how far each has been found to characterize that of so-called "radiant heat" under similar conditions.

(*a*) Rectilinear propagation: an opaque screen which is placed so as to intercept the sun's light intercepts its heat also, whether it be close to the observer, at a few miles from him (as a cloud or a mountain), or 240,000 miles off (as the moon in a total eclipse). (*b*) Speed of propagation: this must be of the same order of magnitude, at least, for both phenomena, *i.e.*, 186,000 miles or so per second; for the sun's heat ceases to be perceptible the moment an eclipse becomes total, and is perceived again the instant the edge of the sun's disk is visible. (*c*) Reflexion: the law must be exactly the same, for the heat-producing rays from a star are concentrated by Lord Rosse's great reflector along with its light. (*d*) Refraction: when a lens is not achromatic its principal focus for red rays is farther off than that for blue rays; that for dark heat is still farther off. Herschel's determination of the warmest region of the spectrum (§ 4 above) is another case in point. (*e*) Oblique radiation: an illuminated or a self-luminous surface appears equally bright however it is inclined to the line of sight. The radiation of heat from a hot blackened surface (through an aperture which it appears to fill) is sensibly the same however it be inclined (Leslie, Fourier, Melloni). (*f*) Intensity: when there is no absorption by the way the intensity of the light received from a luminous point-source is inversely as the square of the distance. The same is true of dark heat. But this is not a new analogy; it is a mere consequence of (*a*) rectilinear propagation. (*g*) Selective absorption: light which has been sifted by passing through one plate of blue glass passes in much greater percentage through a second plate of the same glass, and in still greater percentage through a third. The same is true of radiant heat, even when the experiment is made with uncoloured glass; for clear glass absorbs certain colours of dark heat more than others (De Laroche, Melloni). (*h*) Interference bands, whether produced by two mirrors or by gratings, characterize dark heat as well as light; only they indicate longer waves (Fizeau and Foucault). (*i*) Polarization and double refraction: with special apparatus, such as plates of mica split by heat into numerous parallel films, the polarization of dark heat is easily established. When two of these bundles are so placed as to intercept the heat an unsplit film of mica interposed between them allows the heat to pass, or arrests it, as it is made to rotate in its own plane (Forbes). (*j*) By proper chemical adjustments photographs of a region of the solar spectrum beyond the visible red have been obtained (Abney). We might mention more, but those given above, when considered together, are conclusive. In fact (*b*) or (*i*) alone would almost settle the question.

7. But there is a superior as well as an inferior limit of visible rays. Light whose period of vibration is too small to produce any impression on the optic nerve can be degraded by fluorescence into visible rays, and can also be detected by its energetic action on various photographic chemicals. In fact photographic portraits can be taken in a room which appears absolutely dark to the keenest eyesight. By one or other of these processes the solar spectrum with its dark lines and the electric arc with its bright lines have been delineated to many times the length of their visible ranges. The electric arc especially gives (in either of these ways) a spectrum of extraordinary length; for we can examine it, as we can *not* examine sunlight, before it has suffered any sensible absorption.

8. Thus radiation is one phenomenon, and (as we shall find) the spectrum of a *black body* (a conception roughly realized in the carbon poles of an electric lamp) is continuous from the longest possible wave-length to the shortest which it is hot enough to emit. These various groups of rays, however, are perceived by us in very different ways, whether by direct impressions of sense or by the different modes in which they effect physical changes or transformations. The only way as yet known to us of treating them all alike is to convert their energy into the heat-form and measure it as such. This we can do in a satisfactory manner by the thermo-electric pile and galvanometer.

9. Of the history of the gradual development of the theory of radiation we can give only the main features. The apparent concentration of cold by a concave mirror, which had been long before observed by Porta, was rediscovered by Pictet, and led to the extremely important enunciation of the Law of Exchanges by Prévost in 1791. As we have already seen, Prévost's idea of the nature of radiation was a corpuscular one, no doubt greatly influenced in this direction by the speculations of Lesage. But the

value of his theory as a concise statement of facts and a mode of co-ordinating them is not thereby materially lessened. We give his own statements in the following close paraphrase, in which the italics are retained, from sect. IX. of his *Du Calorique Rayonnant* (Geneva, 1809).

"1. Free caloric is a radiant fluid. And because caloric becomes free at the surfaces of bodies *every point of the surface of a body is a centre, towards and from which filaments* (filets) *of caloric move in all directions.*

"2. *Heat equilibrium* between two neighbouring free spaces *consists in equality of exchange.*

"3. When equilibrium is interfered with it is re-established by inequalities of exchange. And, in a medium of constant temperature, a hotter or a colder body reaches this temperature according to the law that *difference of temperature diminishes in geometrical progression in successive equal intervals of time.*

"4. If into *a locality at uniform temperature a reflecting* or refracting *surface* is introduced, it *has no effect in the way of changing the temperature* at any point in that locality.

"5. If into a locality otherwise at uniform temperature *there is introduced a warmer or a colder body, and next a reflecting* or refracting *surface, the points on which the rays emanating from the body are thrown by these surfaces will be affected,* in the sense of being warmed if the body is warmer, and cooled if it is colder.

"6. *A reflecting body,* heated or cooled in its interior, *will acquire the surrounding temperature more slowly* than would a non-reflector.

"7. *A reflecting body,* heated or cooled in its interior, *will less affect* (in the way of heating or cooling it) another body placed at a little distance than would a non-reflecting body under the same circumstances.

"All these consequences have been verified by experiment, except that which regards the *refraction* of cold. This experiment remains to be made, and I confidently predict the result, at least if the refraction of cold can be accurately observed. This result is indicated in the fourth and fifth consequences [above], and they might thus be subjected to a new test. It is scarcely necessary to point out here the precautions requisite to guard against illusory results of all kinds in this matter."

10. There the matter rested, so far as theory is concerned, for more than half a century. Leslie and, after him, many others added fact by fact, up to the time of De la Provostaye and Desains, whose experiments pointed to a real improvement of the theory in the form of specialization. But, though such experiments indicated, on the whole, a proportionality between the radiating and absorbing powers of bodies and a diminution of both in the case of highly reflecting surfaces, the anomalies frequently met with (depending on the then unrecognized colour-differences of various radiations) prevented any grand generalization. The first real step of the general theory, in advance of what Prévost had achieved, and it was one of immense import, was made by Balfour Stewart in 1858. Before we take it up, however, we may briefly consider

Prévost's statements, putting aside his erroneous views as to the nature of heat; and we must also introduce some results of the splendid investigations of Sadi Carnot (1824), which cast an entirely new light on the whole subject of heat.

11. Prévost's leading idea was that all bodies, whether cold or hot, are constantly radiating heat. This of itself was a very great step. It is distinctly enunciated in the term "exchange" which he employs. And from the way in which he introduces it it is obvious that he means (though he does not expressly say so) that the radiation from a body depends on its own nature and temperature alone, and is independent altogether of the nature and temperature of any adjacent body. This also was a step in advance, and of the utmost value. It will be seen later that Prévost was altogether wrong in his assumption of the geometrical rate of adjustment of differences of temperature,—a statement commonly and erroneously ascribed to Newton*, but true only approximately, and even so for very small temperature differences alone. Newton in the *Queries* to the third book of his *Optics* distinctly recognizes the propagation of heat from a hot body to a cold one by the vibrations of an intervening medium. But he says nothing as to bodies of the same temperature.

12. To Carnot we owe the proposition that *the thermal motivity of a system cannot be increased by internal actions.* A system in which all the parts are at the same temperature has no thermal motivity, for bodies at different temperatures are required in order to work a heat-engine, so as to convert part of their heat into work. Hence, if the contents of an enclosure which is impervious to heat are at any instant at one and the same temperature, no changes of temperature can take place among them. This is certainly true so far as our modes of measurement are concerned, because the particles of matter (those of a gas, for instance) are excessively small in comparison with the dimensions of any of our forms of apparatus for measuring temperatures. Something akin to this statement has often been assumed as a direct result of experiment: *a number of bodies (of any kinds) within the same impervious enclosure, which contains no source of heat, will ultimately acquire the same temperature.* This form is more general than that above, inasmuch as it involves considerations of dissipation of energy. Either of them, were it strictly true, would suffice for our present purpose. But neither statement can be considered as rigorously true. We may employ them, however, in our reasoning as true in the statistical sense; but we must not be surprised if we should find that the assumption of their rigorous truth may in some special cases lead us to theoretical results which are inconsistent with experimental facts,—*i.e.*, if we should find that deviations from an average, which are on far too minute a scale to be directly detected by any of our most delicate instruments, may be seized upon and converted into observable phenomena by some of the almost incomparably more delicate systems which we call individual particles of matter.

13. The next great advance was made by Balfour Stewart†. The grand novelty which he introduced, and from which all his varied results follow almost intuitively,

* Mitchell, *Trans. R. S. E.*, 1899.

† *Trans. R. S. E.*, 1858; see also *Phil. Mag.*, 1863, I. p. 354.

is the idea of *the absolute uniformity* (*qualitative as well as quantitative*) of the radiation at all points, and in all directions, within an enclosure impervious to heat, when thermal equilibrium has once been arrived at. (So strongly does he insist on this point that he even states that, whatever be the nature of the bodies in the enclosure, the radiation there will, when equilibrium is established, be that of a black body at the same temperature. He does not expressly say that the proposition will still be true even if the bodies can radiate, and therefore absorb, one definite wave-length only; but this is a legitimate deduction from his statements. To this we will recur.) His desire to escape the difficulties of surface-reflexion led him to consider the radiation inside an imperfectly transparent body in the enclosure above spoken of. He thus arrived at an immediate proof of the existence of internal radiation, which recruits the stream of radiant heat in any direction step by step precisely to the amount by which it has been weakened by absorption. Thus the radiation and absorption rigorously compensate one another, not merely in quantity but in quality also, so that a body which is specially absorptive of one particular ray is in the same proportion specially radiative of the same ray, its temperature being the same in both cases. To complete the statement, all that is necessary is to show how one ray may differ from another, viz., in intensity, wave-length, and polarization.

14. The illustrations which Stewart brought forward in support of his theory are of the two following kinds. (1) He experimentally verified the existence of internal radiation, to which his theory had led him. This he did by showing that a thick plate of rock-salt (chosen on account of its comparative transparency to heat-radiations) radiates more than a thin one at the same temperature,—surrounding bodies being in this case of course at a lower temperature, so that the effect should not be masked by transmission. The same was found true of mica and of glass. (2) He showed that each of these bodies is more opaque to radiations from a portion of its own substance than to radiation in general. Then comes his conclusion, based, it will be observed, on his fundamental assumption as to the nature of the equilibrium radiation in an enclosure. It is merely a detailed explanation that, once equilibrium has been arrived at, the consequent uniformity of radiation throughout the interior of a body requires the step-by-step compensation already mentioned. And thus he finally arrives at the statement that at any temperature a body's radiation is exactly the same both as to quality and quantity as that of its absorption from the radiation of a black body at the same temperature. In symbolical language Stewart's proposition (extended in virtue of a principle always assumed) amounts to this:—at any one temperature let R be the radiation of a black body, and eR (where e is never greater than 1) that of any other substance, both for the same definite wave-length; then the substance will, while at that temperature, absorb the fraction e of radiation of that wave-length, whatever be the source from which it comes. The last clause contains the plausible assumption already referred to. Stewart proceeds to show, in a very original and ingenious way, that his result is compatible with the known facts of reflexion, refraction, &c., and arrives at the conclusion that for internal radiation parallel to a plane the amount is (in isotropic bodies) proportional to the refractive index. Of course, when the restriction of parallelism to a plane is removed the internal radiation is found

to be proportional to the square of the refractive index. This obvious completion of the statement was first given by Stewart himself at a somewhat later date.

15. So far Stewart had restricted his work to "dark heat," as it was then called; and he says that he did so expressly in order to confine himself to rays "which were universally acknowledged to produce heat by their absorption." But he soon proceeded to apply himself to luminous radiations. And here he brought forward the extremely important fact that "coloured glasses invariably lose their colour in the fire" when exactly at the temperature of the coals behind them, *i.e.*, they compensate exactly for their absorption by their radiation. But a red glass when colder than the coals behind appears red, while if it be hotter than they are it appears green. He also showed that a piece of china or earthenware with a dark pattern on a light ground appears to have a light pattern on a dark ground when it is taken out of the fire and examined in a dark room. Hence he concluded that his extension of Prévost's theory was true for luminous rays also.

16. In this part of the subject he had been anticipated, for Fraunhofer had long ago shown that the flame of a candle when examined by a prism gives bright lines (*i.e.*, maxima of intensity of radiation) in the position of the constituents of a remarkable double dark line (*i.e.*, minima of radiation) in the solar spectrum, which he called *D*. Hallows Miller had afterwards more rigorously verified the exact coincidence of these bright and dark lines. But Foucault* went very much farther, and proved that the electric arc, which shows these lines bright in its spectrum, not only intensifies their blackness in the spectrum of sunlight transmitted through it, but produces them as dark lines in the otherwise continuous spectrum of the light from one of the carbon points, when that light is made by reflexion to pass through the arc. Stokes about 1850 pointed out the true nature of the connection of these phenomena, and illustrated it by a dynamical analogy drawn from sound. He stated his conclusions to Sir W. Thomson†, who (from 1852 at least) gave them regularly in his public lectures, always pointing out that one constituent of the solar atmosphere is certainly sodium, and that others are to be discovered by the coincidences of solar dark lines with bright lines given by terrestrial substances rendered incandescent in the state of vapour. Stokes's analogy is based on the fact of synchronism (long ago discussed by Hooke and others), viz., that a musical string is set in vibration when the note to which it is tuned is sounded in its neighbourhood. Hence we have only to imagine a space containing a great number of such strings, all tuned to the same note. Such an arrangement would form, as it were, a medium which, when agitated, would give that note, but which would be set in vibration by, and therefore diminish the intensity of, that particular note in any mixed sound which passed through it.

17. Late in 1859 appeared Kirchhoff's first paper on the subject‡. He supplied one important omission in Stewart's development of the theory by showing *why* it is

* *L'Institut*, 7th February, 1849; see *Phil. Mag.*, 1860, I. p. 193.

† *Brit. Assoc.*, President's address, 1871.

‡ *Pogg. Ann.*, or *Phil. Mag.*, 1860.

necessary to use as an absorbing body one colder than the source in order to produce reversal of spectral lines. This we will presently consider. Kirchhoff's proof of the equality of radiating and absorbing powers is an elaborate but unnecessary piece of mathematics, called for in consequence of his mode of attacking the question. He chose to limit his reasoning to special wave-lengths by introducing the complex mechanism of the colours of thin plates and a consequent appeal to Fourier's theorem instead of to the obviously permissible assumption of a substance imperfectly transparent for one special wave-length, but perfectly transparent for all others; and he did not, as Stewart had done, carry his reasoning into the interior of the body. With all its elaboration, his mode of attacking the question leads us no farther than could Stewart's. Both are ultimately based on the final equilibrium of temperature in an enclosure, required by Carnot's principle, and both are, as a consequence, equally inapplicable to exceptional cases, such as the behaviour of fluorescent or phosphorescent substances. In fact (see "Thermodynamics," No. CXXXI. below), Carnot's principle is established only on a statistical basis of averages, and is not necessarily true when we are dealing with portions of space, which, though of essentially finite dimensions, are extremely small in comparison with the sentient part of even the tiniest instrument for measuring temperature.

18. Kirchhoff's addition to Stewart's result may be given as follows. Let radiation r, of the same particular wave-length as that spoken of in § 14, fall on the substance; er of it will be absorbed, and $(1-e)r$ transmitted. This will be recruited by the radiation of the substance itself, so that the whole amount for that particular wave-length becomes $(1-e)r+eR$, or $r-e(r-R)$. Thus the radiation is weakened only when $R<r$, a condition which requires that the source (even if it be a black body) should be at a higher temperature than the absorbing substance (§ 4, above). But the converse is, of course, not necessarily true. This part of the subject, as well as the special work of Kirchhoff and of Bunsen, belongs properly to spectrum analysis.

19. From the extension of Prévost's theory, obtained in either of the ways just explained, we see at once how the constancy of the radiation in an enclosure is maintained. In the neighbourhood of and perpendicular to the surfaces of a black body it is wholly due to radiation, near a transparent body wholly to transmission. A body which reflects must to the same extent be deficient in its radiation and transmission; thus a perfect reflector can neither radiate nor transmit. And a body which polarizes by reflexion must supply by radiation what is requisite to render the whole radiation unpolarized. A body, such as a plate of tourmaline, which polarizes transmitted light, must radiate light polarized in the same plane as that which it absorbs. Kirchhoff and Stewart independently gave this beautiful application.

20. Empirical formulæ representing more or less closely the law of cooling of bodies, whether by radiation alone or by simultaneous radiation and convection, have at least an historic interest. What is called Newton's Law of Cooling (see p. 462 above) was employed by Fourier in his *Théorie Analytique de la Chaleur*. Here the rate of

surface-loss was taken as proportional to the excess of temperature over surrounding bodies. For small differences of temperature it is accurate enough in its applications, such as to the corrections for loss of heat in experimental determinations of specific heat, &c., but it was soon found to give results much below the truth, even when the excess of temperature was only 10° C.

21. Dulong and Petit, by carefully noting the rate of cooling of the bulb of a large thermometer enclosed in a metallic vessel with blackened walls, from which the air had been as far as possible extracted and which was maintained at a constant temperature, were led to propound the exponential formula $A\alpha^t + B$ to represent the radiation from a black surface at temperature t. As this is an exponential formula, we may take t as representing absolute temperature, for the only result will be a definite change of value of the constant A. Hence if t_0 be the temperature of the enclosure, the rate of loss of heat should be $A(\alpha^t - \alpha^{t_0})$, or $A\alpha^{t_0}(\alpha^{t-t_0} - 1)$. The quantity A was found by them to depend on the nature of the radiating surface, but α was found to have the constant value 1·0077. As the approximate accuracy of this expression was verified by the experiments of De la Provostaye and Desains for temperature differences up to 200° C., it may be well to point out two of its consequences. (1) For a given difference of temperatures the radiation is an exponential function of the lower (or of the higher) temperature. (2) For a given temperature of the enclosure the radiation is as $(1{\cdot}0077)^\theta - 1$, or $\theta(1 + 0{\cdot}0038\theta + \ldots)$, where θ is the temperature excess of the cooling body. Thus the (so-called) Newtonian law gives 4 per cent. too little at 10° C. of difference.

22. Dulong and Petit have also given an empirical formula for the rate of loss by simultaneous radiation and convection. This is of a highly artificial character, the part due to radiation being as in the last section, while that due to convection is independent of it, and also of the nature of the surface of the cooling body. It is found to be proportional to a power of the pressure of the surrounding gas (the power depending on the nature of the gas), and also to a definite power of the temperature excess. The reader must be referred to French treatises, especially that of Desains, for further information.

23. Our knowledge of the numerical rate of surface-emission is as yet scanty, but the following data, due to Nicol*, may be useful in approximate calculations. Loss in heat units (1 lb. water raised 1° C. in temperature) per square foot per minute, from

Bright copper	1·09	0·51	0·42
Blackened copper	2·03	1·46	1·35.

The temperatures of body and enclosure were 58° C. and 8° C., and the pressure of contained air in the three columns was about 30, 4, and 0·4 inches of mercury respectively. The enclosure was blackened.

* *Proc. R. S. E.*, VII. 1870, p. 206.

24. Scanty as is our knowledge of radiation, it is not at all surprising that that of convection should be almost *nil*, except as regards some of its practical applications. Here we have to deal with a problem of hydrokinetics of a character, even in common cases, of far higher difficulty than many hydrokinetic problems of which not even approximate solutions have been obtained.

25. What is called Döppler's Principle has more recently* led Stewart to some curious speculations, which a simple example will easily explain. Suppose two parallel plates of the same substance, perfectly transparent except to one definite wave-length, to be moving towards or from one another. Each, we presume, will radiate as before, and on that account cool; but the radiation which reaches either is no longer of the kind which alone it can absorb, whether it come directly from the other, or is part of its own or of the other's radiation reflected from the enclosure. Hence it would appear that relative motion is incompatible with temperature equilibrium in an enclosure, and thus that there must be some effect analogous to resistance to the motion. We may get over this difficulty if we adopt the former speculation of Stewart, referred to in brackets in § 13 above. For this would lead to the result that, as soon as either of the bodies has cooled, ever so slightly, the radiation in the enclosure should become that belonging to a black body of a slightly higher temperature than before, and thus the plates would be furnished with radiation which they could at once absorb, and be gradually heated to their former temperature.

26. A very recent speculation, founded by Boltzmann† upon some ideas due to Bartoli, is closely connected in principle with that just mentioned. This speculation is highly interesting, because it leads to an expression for the amount of the whole radiation from a black body in terms of its absolute temperature. Boltzmann's investigation may be put, as follows, in an exceedingly simple form. It was pointed out by Clerk-Maxwell, as a result of his electro-magnetic theory of light, that radiation falling on the surface of a body must produce a certain pressure. It is easy to see (most simply by the analogy of the virial equation), that the measure of the pressure per square unit on the surface of an impervious enclosure, in which there is thermal equilibrium, must be one-third of the whole energy of radiation per cubic unit of the enclosed space. We may now consider a reversible engine conveying heat from one black body to another at a different temperature, by operations alternately of the isothermal and the adiabatic character, which consist in altering the volume of the enclosure, with or without one of the bodies present in it. For one of the fundamental equations (p. 478 below) gives

$$\frac{dE}{dv} = t\frac{dp}{dt} - p,$$

where t is the absolute temperature. If f be the pressure on unit surface, $3f$ is the energy per unit of volume, and this equation becomes

$$t\frac{df}{dt} - f = 3f.$$

* *Brit. Assoc. Report*, 1871.

† *Wiedemann's Ann.*, 1884, XXII.

Hence it follows at once that, if the fundamental assumptions be granted, the energy of radiation of a black body per unit volume of the enclosure is proportional to the fourth power of the absolute temperature. It is not a little remarkable that Stefan* had some years previously shown that this very expression agrees more closely with the experimental determinations of Dulong and Petit than does their own empirical formula.

27. It would appear from this expression that, if an impervious enclosure containing only one black body in thermal equilibrium is separated into two parts by an impervious partition, any alteration of volume of the part not containing the black body will produce a corresponding alteration of the radiation in its interior. It will now correspond to that of a second black body, whose temperature is to that of the first in the inverse ratio of the fourth roots of the volumes of the detached part of the enclosure.

28. Lecher† has endeavoured to show that the distribution of energy among the constituents of the radiation from a black body does not alter with temperature. Such a result, though apparently inconsistent with many well-known facts, appears to be consistent with and to harmonize many others. It accords perfectly with the notion of the absolute uniformity (statistical) of the energy in an enclosure, and its being exactly that of a black body, even if the contents (as in § 25) consist of a body which can radiate one particular quality of light alone. And if this be the case it will also follow that the intensity of radiation of any one wave-length by any one body in a given state depends on the temperature in exactly the same way as does the whole radiation from a black body. Unfortunately this last deduction does not accord with Melloni's results; at least the discrepance from them would appear to be somewhat beyond what could fairly be set down to error of experiment. But it is in thorough accordance with the common assumption (§ 14) that the percentage absorption of any particular radiation does not depend on the temperature of the source. The facts of fluorescence and phosphorescence, involving the radiation of visible rays at temperatures where even a black body is invisible, have not yet been dealt with under any general theory of radiation; though Stokes has pointed out a dynamical explanation of a thoroughly satisfactory character, they remain outside the domain of Carnot's principle.

* *Sitzungsber. d. k. Ak. in Wien*, 1879.

† *Wiedemann's Ann.*, 1882, XVII.

CXXXI.

THERMODYNAMICS.

[*From Encyclopædia Britannica,* 1888.]

In a strict interpretation, this branch of science, sometimes called the Dynamical Theory of Heat, deals with the relations between heat and work, though it is often extended so as to include all transformations of energy. Either term is an infelicitous one, for there is no direct reference to force in the majority of questions dealt with in the subject. Even the title of Carnot's work, presently to be described, is much better chosen than is the more modern designation. On the other hand, such a German phrase as *die bewegende Kraft der Wärme* is in all respects intolerable.

It has been shown * * * that Newton's enunciation of the conservation of energy as a general principle of nature was defective in respect of the connection between work and heat, and that, about the beginning of the present century, this *lacuna* was completely filled up by the researches of Rumford and Davy. Joule's experimental demonstration of the principle, and his determination of the work-equivalent of heat by various totally independent processes, have been discussed.

But the conservation of energy, alone, gives us an altogether inadequate basis for reasoning on the work of a heat-engine. It enables us to calculate how much work is equivalent to an assigned amount of heat, and *vice versâ,* provided the transformation can be effected; but it tells us nothing with respect to the percentage of either which can, under given circumstances, be converted into the other. For this purpose we require a special case of the law of transformation of energy. This was first given in Carnot's extraordinary work entitled *Réflexions sur la Puissance Motrice du Feu,* Paris, 1824*.

* The author, N. L. Sadi Carnot (1796—1832), was the second son of Napoleon's celebrated minister of war, himself a mathematician of real note even among the wonderful galaxy of which France could then boast.

The chief novelties of Carnot's work are the introduction of the idea of a cycle of operations, and the invaluable discovery of the special property of a *reversible cycle*. It is not too much to say that, without these wonderful novelties, thermodynamics as a theoretical science could not have been developed.

Carnot's work seems to have excited no attention at the time of its publication. Ten years later (1834) Clapeyron gave some of its main features in an analytical form, and he also employed Watt's diagram for the exhibition of others. Even this, however, failed to call attention properly to the extremely novel processes of Carnot, and it was reserved for Sir W. Thomson (in 1848, and more at length in 1849) to point out to scientific men their full value. His papers on Carnot's treatise, following closely after the splendid experimental researches of Colding and Joule, secured for the dynamical theory of heat its position as a recognized branch of science. James Thomson, by Carnot's methods, predicted in 1849 the lowering of the freezing point of water by pressure, which was verified experimentally in the same year by his brother. Von Helmholtz had published, two years before, a strikingly original and comprehensive pamphlet on the conservation of energy. The start once given, Rankine, Clausius, and W. Thomson rapidly developed, though from very different standpoints, the theory of thermodynamics. The methods adopted by Thomson differed in one special

The delicate constitution of Sadi was attributed to the agitated circumstances of the time of his birth, which led to the proscription and temporary exile of his parents. He was admitted in 1812 to the École Polytechnique, where he was a fellow-student of the famous Chasles. Late in 1814 he left the school with a commission in the Engineers, and with prospects of rapid advancement in his profession. But Waterloo and the Restoration led to a second and final proscription of his father; and, though Sadi was not himself cashiered, he was purposely told off for the merest drudgeries of his service; "il fut envoyé successivement dans plusieurs places fortes pour y faire son métier d'ingénieur, compter des briques, réparer des pans de murailles, et lever des plans destinés à s'enfouir dans les cartons," as we learn from a biographical notice written by his younger brother. Disgusted with an employment which afforded him neither leisure for original work nor opportunities for acquiring scientific instruction, he presented himself in 1819 at the examination for admission to the staff-corps (état-major), and obtained a lieutenancy. He now devoted himself with astonishing ardour to mathematics, chemistry, natural history, technology, and even political economy. He was an enthusiast in music and other fine arts; and he habitually practised as an amusement, while deeply studying in theory, all sorts of athletic sports, including swimming and fencing. He became captain in the engineers in 1827, but left the service altogether in the following year. His naturally feeble constitution, farther weakened by excessive devotion to study, broke down finally in 1832. A relapse of scarlatina led to brain fever, from which he had but partially recovered when he fell a victim to cholera. Thus died, at the early age of thirty-six, one of the most profound and original thinkers who have ever devoted themselves to science. The work named above was the only one he published. Though of itself sufficient to put him in the very foremost rank, it contains only a fragment of Sadi Carnot's discoveries. Fortunately his manuscripts have been preserved, and extracts from them have been appended by his brother to a reprint (1878) of the *Puissance Motrice*. These show that he had not only realized for himself the true nature of heat, but had noted down for trial many of the best modern methods of finding its mechanical equivalent, such as those of Joule with the perforated piston and with the internal friction of water and mercury. W. Thomson's experiment with a current of gas forced through a porous plug is also given. One sentence of extract, however, must suffice, and it is astonishing to think that it was written over sixty years ago. "On peut donc poser en thèse générale que la puissance motrice est en quantité invariable dans la nature, qu'elle n'est jamais, à proprement parler, ni produite, ni détruite. À la vérité, elle change de forme, c'est-à-dire qu'elle produit tantôt un genre de mouvement, tantôt un autre; mais elle n'est jamais anéantie."

characteristic from those of his concurrents,—they were based entirely on the experimental facts and on necessary principles; and, when hypothesis was absolutely required, attention was carefully directed to its nature and to the reasons which appeared to justify it.

Three specially important additions to pure science followed almost directly from Carnot's methods:—(1) the *absolute* definition of temperature; (2) the thermodynamic function or entropy; (3) the dissipation of energy. The first (in 1848) and the third (in 1852) were given by W. Thomson. The second, though introduced by Rankine, was also specially treated by Clausius.

In giving a brief sketch of the science, we will not adhere strictly to any of the separate paths pursued by its founders, but will employ for each step what appears to be most easily intelligible to the general reader. And we will arrange the steps in such an order that the necessity for each may be distinctly visible before we take it.

1. *General Notions.*—The conversion of mechanical work into heat can always be effected completely. In fact, friction, without which even statical results would be all but unrealizable in practical life, interferes to a marked extent in almost every problem of kinetics,—and work done against friction is (as a rule) converted into heat. But the conversion of heat into work can be effected only in part, usually in very small part. Thus heat is regarded as the lower or less useful of these forms of energy, and when part of it is elevated in rank by conversion into work the remainder sinks still lower in the scale of usefulness than before.

There are but two processes known to us for the conversion of heat into work, viz., that adopted in heat-engines, where the changes of volume of the "working substance" are employed, and that of electromagnetic engines driven by thermoelectric currents. To the latter we will not again refer. And for simplicity we will suppose the working substance to be fluid, so as to have the same pressure throughout, or, if it be solid, to be isotropic, and to be subject only to hydrostatic pressure, or to tension uniform in all directions and the same from point to point.

The state of unit mass of such a substance is known by experiment to be fully determined when its volume and pressure are given, even if (as in the case of ice in presence of water, or of water in presence of steam) part of it is in one molecular state and part in another. But, the state being determinate, so must be the temperature, and also the amount of energy which the substance contains. This consideration is insisted on by Carnot as the foundation of his investigations. In other words, before we are entitled to reason upon the relation between the heat supplied to and the work done by the working substance, Carnot says we must bring that substance, by means of a *cycle* of operations, back to precisely its primitive state as regards volume, temperature, and molecular condition.

2. *Watt's Diagram.*—Watt's indicator-diagram enables us to represent our operations graphically. For if OM (fig. 1) represent the volume, at any instant, of the unit mass of working substance, MP its pressure, the point P is determinate and corresponds to a definite temperature, definite energy, &c. If the points of any curve, as PP', in the diagram represent the successive states through which the working substance is made to pass, the work done is represented by the area $MPP'M'$. Hence, a cycle of operations, whose essential nature is to bring the working substance back to its primitive state, is necessarily represented by a *closed* boundary, such as $PP'Q'Q$, in the diagram. The area enclosed is the excess of the work done by the working substance over that spent on it during the cycle. [This is positive if the closed path be described clockwise, as indicated by the arrow-heads.]

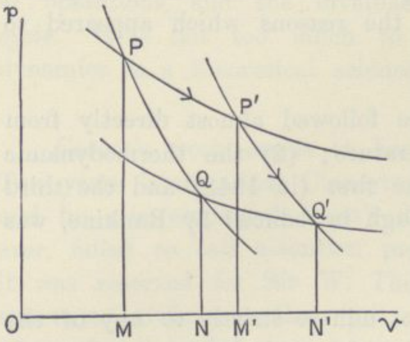


FIG. 1.

3. *Carnot's Cycle.*—For a reason which will immediately appear, Carnot limited the operations in his cycle to two kinds, employed alternately during the expansion and during the compression of the working substance. The first of these involves change of volume *at constant temperature*; the second, change of volume *without direct loss or gain of heat.* [In his hypothetical engine the substance was supposed to be in contact with a body kept at constant temperature, or to be entirely surrounded by non-conducting materials.] The corresponding curves in the diagram are called *isothermals,* or lines of equal temperature, and *adiabatic* lines respectively. We may consider these as having been found, for any particular working substance, by the direct use of Watt's indicator. It is easy to see that one, and only one, of each of these kinds of lines can be found for an assigned initial state of the working substance; also that, because in expansion at constant temperature heat must be constantly supplied, the pressure will fall off less rapidly than it does in adiabatic expansion. Thus in the diagram the adiabatic lines PQ, $P'Q'$ cut the lines of equal temperature PP', QQ' downwards and to the right. Thus the boundary of the area $PP'Q'Q$ does not cross itself. To determine the behaviour of the engine we have therefore only to find how much heat is taken in along PP' and how much is given out in $Q'Q$. Their difference is *equivalent* to the work expressed by the area $PP'Q'Q$.

4. *Carnot's Principle of Reversibility.*—It will be observed that each operation of this cycle is strictly *reversible*; for instance, to take the working substance along the path $P'P$ we should have to spend on it step by step as much work as it gave out in passing along PP', and we should thus restore to the source of heat exactly the amount of heat which the working substance took from it during the expansion. In the case of the adiabatics the work spent during compression is the same as that done during the corresponding expansion, and there is no question of loss or gain of heat directly.

If, however, a transfer of heat between the working substance and its surroundings have taken place on account of a finite difference of temperature, it is clear that such an operation is not reversible. Strictly speaking, isothermal expansion or contraction is unattainable in practice, but it is (without limit) more closely approximated to as the operation is more slowly performed. The adiabatic condition, on the other hand, is more closely approximated to in practice the more swiftly the operation is performed. We have an excellent instance of this in the compression and dilatation of air caused by the propagation of a sound-wave.

And now we have Carnot's invaluable proposition, *a reversible heat-engine is a perfect engine*,—perfect, that is, in the sense that no other heat-engine can be superior to it. Before giving the proof, let us see the immense consequences of this proposition. Reversibility is the sole test of perfection; so that all heat-engines, *whatever be the working substance*, provided only they be reversible, convert into work (under given circumstances) the same fraction of the heat supplied to them. The only circumstances involved are the temperatures of the source and condenser. Thus we are furnished with a general principle on which to reason about transformation of heat, altogether independently of the properties of any particular substance.

The proof, as Carnot gave it on the hypothesis of the materiality of heat, is *ex absurdo*. It is as follows. Suppose a heat-engine A to be capable of giving more work from a given amount of heat than is a reversible engine B, the temperatures of source and condenser being the same for each. Use the two as a compound engine, A working direct and B reversed. By hypothesis B requires to be furnished with part only of the work given by A to be able to restore to the source the heat abstracted by A, and thus at every complete stroke of the compound engine the source has its heat restored to it, while a certain amount of external work has been done. This would be the PERPETUAL MOTION.

5. *The Basis of the Second Law of Thermodynamics.*—Carnot's reasoning, just given, is based on the hypothesis that heat (or caloric) is indestructible, and that (under certain conditions) it does work in being let down from a higher to a lower temperature, just as does water when falling to a lower level. It is clear from several expressions in his work that Carnot was not at all satisfied with this view, even in 1824, and we have seen that he soon afterwards reached the true theory. But it is also clear that such an assumption somewhat simplifies the reasoning, for in his hypothetical heat-engine all the heat which leaves the boiler goes to the condenser, and *vice versâ* in the reversed working. The precise point of Carnot's investigation where the supposed indestructibility of heat introduces error is when, after virtually saying compress from Q' to a state Q determined by the condition that the heat given out shall be exactly equal to that taken in during the expansion from P to P', he assumes that, on farther compressing adiabatically to the original volume, the point P will be reached and the cycle completed. J. Thomson, in 1849, rectified this by putting it in the true form:—compress from Q' to a state Q, such that subsequent adiabatic compression will ultimately lead to the state P.

We have now to consider that, if an engine (whether simple or compound) does work at all by means of heat, *less* heat necessarily reaches the condenser than left the boiler. Hence, if there be two engines A and B as before, and the joint system be worked in such a way that B constantly restores to the source the heat taken from it by A, we can account for the excess of work done by A over that spent on B solely by supposing that *B takes more heat from the condenser than A gives to it.* Such a compound engine would transform into work heat taken solely from the condenser. And the work so obtained might be employed on B, so as to make it convey heat to the source while farther cooling the condenser.

Clausius, in 1850, sought to complete the proof by the simple statement that "this contradicts the usual behaviour of heat, which always tends to pass from warmer bodies to colder." Some years later he employed the axiom, "it is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature." W. Thomson, in 1851, employed the axiom, "it is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects." But he was careful to supplement this by further statements of an extremely guarded character. And rightly so, for Clerk-Maxwell has pointed out that such axioms are, as it were, only accidentally correct, and that the true basis of the second law of thermodynamics lies in the extreme smallness and enormous number of the particles of matter, and in consequence the *steadiness* of their average behaviour. Had we the means of dealing with the particles individually, we could develop on the large scale what takes place continually on a very minute scale in every mass of gas,—the occasional, but ephemeral, aggregation of warmer particles in one small region and of colder in another.

6. *The Laws of Thermodynamics.*—I. When equal quantities of mechanical effect are produced by any means whatever from purely thermal sources, or lost in purely thermal effects, equal quantities of heat are put out of existence, or are generated. [To this we may add, after Joule, that in the latitude of Manchester 772 foot-pounds of work are capable of raising the temperature of a pound of water from 50° F. to 51° F. This corresponds to 1390 foot-pounds per centigrade degree, and in metrical units to 425 kilogramme-metres per calorie.]

II. If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed, it produces as much mechanical effect as can be produced by any thermodynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat.

7. *Absolute Temperature.*—We have seen that the fraction of the heat supplied to it which a reversible engine can convert into work depends *only* on the temperatures of the boiler and of the condenser. On this result of Carnot's Sir W. Thomson based his absolute definition of temperature. It is clear that a certain freedom of choice is left, and Thomson endeavoured to preserve as close an agreement as possible between the new scale and that of the air thermometer. Thus the definition ultimately fixed

on, after exhaustive experiments, runs:—"The temperatures of two bodies are proportional to the quantities of heat respectively taken in and given out in localities at one temperature and at the other respectively, by a material system subjected to a complete cycle of perfectly reversible thermodynamic operations, and not allowed to part with or take in heat at any other temperature; or, the absolute values of two temperatures are to one another in the proportion of the heat taken in to the heat rejected in a perfect thermodynamic engine, working with a source and refrigerator at the higher and lower of the temperatures respectively*." If we now refer again to fig. 1, we see that, t and t' being the absolute temperatures corresponding to PP' and QQ', and H, H' the amounts of heat taken in during the operation PP' and given out during the operation $Q'Q$ respectively, we have

$$H/t = H'/t',$$

whatever be the values of t and t'. Also, if heat be measured in terms of work, we have

$$H - H' = \text{area } PP'Q'Q.$$

Thus with a reversible engine working between temperatures t and t' the fraction of the heat supplied which is converted into work is $(t - t')/t$.

It is now evident that we can construct Watt's diagram in such a way that the lines of equal temperature and the adiabatics may together intercept a series of equal areas. Thus let PP' (fig. 2) be the isothermal t, and on it so take points P', P'', P''', &c., that, as the working substance passes from P to P', P' to P'', &c., t units of heat (the unit being of any assigned value) shall in each case be taken in. Let QQ', RR', &c., be other isothermals, so drawn that the successive areas PQ', QR', &c., between any two selected adiabatics, may be equal. Then, as it is clear that all the successive areas between each one pair of isothermals are equal (each representing the area $t - t'$), it follows that all the quadrilateral areas in the figure are equal.

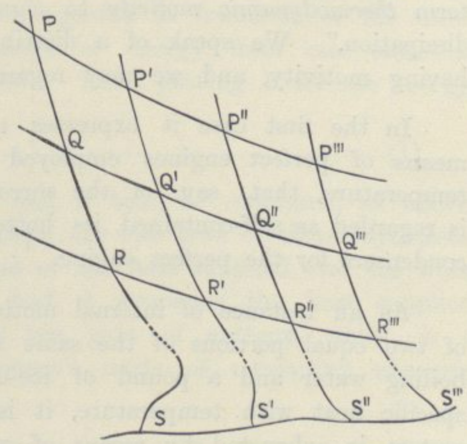


FIG. 2.

It is now clear that the area included between PP' and the two adiabatics PQR, $P'Q'R'$ is essentially *finite*, being numerically equal to t. Thus the temperature for each isothermal is represented by the corresponding area. This is indicated in the cut by the introduction of an arbitrary line SS', supposed to be the isothermal of absolute zero. The lower parts of the adiabatics also are unknown, so that we may draw them as we please, subject to the condition that the entire areas PS', $P'S''$, $P''S'''$, &c., shall all be equal. To find, on the absolute scale, the numerical values of two definite temperatures, such as the usually employed freezing and boiling points of water, we

* *Trans. R. S. E.*, May 1854.

must therefore find their *ratio* (that of the heat taken and the heat rejected by a reversible engine working between these temperatures), and *assign* the number of degrees in the interval.

Thomson and Joule experimentally showed that this ratio is about 1·365. Hence, if we assume (as in the centigrade scale) 100 degrees as the range, the temperatures in question are 274 and 374 nearly.

8. *Entropy.*—Just as the lines PP', QQ', &c., are characterized by constant temperature along each, so we figure to ourselves a quantity which is characteristic of each adiabatic line,—being constant along it. The equation of last section at once points out such a quantity. If we write ϕ for its value along PQ, ϕ' for $P'Q'$, we may define thus

$$\phi' - \phi = H/t.$$

From the statements as to the equality of the areas in fig. 2 the reader will see at once that the area bounded by t, t', ϕ, ϕ' is $(t-t')(\phi'-\phi)$. We are concerned only with the *changes* of ϕ, not with its actual magnitude, so that any one adiabatic may be chosen as that for which $\phi = 0$.

9. *The Dissipation of Energy.*—Sir William Thomson has recently introduced the term *thermodynamic motivity* to signify "the possession the waste of which is called dissipation." We speak of a distribution of heat in a body or system of bodies as having motivity, and we may regard it from without or from within the system.

In the first case it expresses the amount of work which can be obtained by means of perfect engines employed to reduce the whole system to some definite temperature, that, say, of the surrounding medium. In the second case the system is regarded as self-contained, its hotter parts acting as sources, and its colder parts as condensers for the perfect engine.

As an instance of internal motivity we may take the case of a system consisting of two equal portions of the same substance at different temperatures, say a pound of boiling water and a pound of ice-cold water. If we neglect the (small) change of specific heat with temperature, it is found that, when the internal motivity of the system is exhausted by means of perfect engines, the temperature is about 46° C., being the centigrade temperature corresponding to the geometrical mean of the original absolute temperatures of the parts. Had the parts been simply mixed so as to dissipate the internal motivity, the resulting temperature would have been 50° C. Thus the work gained (*i.e.*, the original internal motivity) is the equivalent of the heat which would raise two pounds of water from 46° C. to 50° C.

As an instance of motivity regarded from without we may take the simple case of the working substance in § 2, on the hypothesis that there is an assigned lower temperature limit. As there is no supply of heat, it is clear that the maximum of work will be obtained by allowing the substance to expand adiabatically till its temperature sinks to the assigned limit.

Thus if P (fig. 3) be its given position on Watt's diagram, PQ the adiabatic through P, and $P'Q$ the isothermal of the lower temperature limit, Q is determinate, and the motivity is the area $PQNM$. If, again, we wish to find the motivity when the initial and final states P and P' are given, with the condition that the temperature is not to fall below that of the state P', the problem is reduced to finding the course PP' for which the area $PP'M'M$ is greatest. As no heat is supplied, the course cannot rise above the adiabatic PQ, and by hypothesis it cannot fall below the isothermal $P'Q$,—hence it must be the broken line PQP'. Thus, under the circumstances stated, the motivity is represented by the area $MPQP'M'$. If any other lawful course, such as PP', be taken, there is an unnecessary waste of motivity represented by the area PQP'.

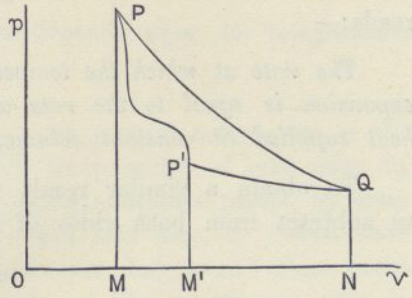


FIG. 3.

10. *Elementary Thermodynamic Relations.*—From what precedes it is clear that, when the state of unit mass of the working substance is given by a point in the diagram, an isothermal and an adiabatic can be drawn through that point, and thus ϕ and t are determinate for each particular substance when p and v are given. Thus any two of the four quantities p, v, t, ϕ may be regarded as functions of the other two, chosen as independent variables. The change of energy from one state to another can, of course, be expressed as in § 9, above. Thus, putting E for the energy, we have at once

$$dE = td\phi - pdv \quad \text{...(1)}$$

if ϕ and v be chosen as independent variables, and if heat be measured, as above, in units of work. This equation expresses, in symbols, the two laws of thermodynamics. For it states that the gain of energy is the excess of the heat supplied over the work done, which is an expression of the first law. And it expresses the heat supplied as the product of the absolute temperature by the gain of entropy, which is a statement of the second law in terms of Thomson's mode of measuring absolute temperature.

But we now have two equations in partial differential coefficients:—

$$\left(\frac{dE}{d\phi}\right) = t, \quad \left(\frac{dE}{dv}\right) = -p.$$

From these we have two expressions for the value of $\left(\dfrac{d^2E}{dvd\phi}\right)$.

Equating them, we are led to the thermodynamic relation

$$\left(\frac{dt}{dv}\right) = -\left(\frac{dp}{d\phi}\right),$$

the differential coefficients being again partial.

This expresses a property of all "working substances," defined as in § 1. To state it in words, let us multiply and divide the right-hand side by t, and it then reads:—

The rate at which the temperature falls off per unit increase of volume in adiabatic expansion is equal to the rate at which the pressure increases per dynamical unit of heat supplied at constant volume, multiplied by the absolute temperature.

To obtain a similar result with v and t as independent variables, we have only to subtract from both sides of (1) the complete differential $d(t\phi)$, so that

$$d(E - t\phi) = -\phi dt - pdv.$$

Proceeding exactly as before, we find

$$\left(\frac{d\phi}{dv}\right) = \left(\frac{dp}{dt}\right).$$

In words this result runs (when both sides are multiplied by t):—

The rate of increase of pressure with temperature at constant volume, multiplied by the absolute temperature, is equal to the rate at which heat must be supplied per unit increase of volume to keep the temperature constant.

Very slight variations of the process just given obtain the following varieties of expression:—

$$\left(\frac{dv}{d\phi}\right) = \left(\frac{dt}{dp}\right) \text{ and } \left(\frac{dv}{dt}\right) = -\left(\frac{d\phi}{dp}\right),$$

which are to be interpreted as above.

11. *Increase of Total Energy under various Conditions.*—The expression (1) of § 10 may be put in various forms, each convenient for some special purpose. We give one example, as sufficiently showing the processes employed. Thus, suppose we wish to find how the energy of the working substance varies with its volume when the temperature is kept constant, we must express dE in terms of dv and dt. Thus

$$dE = t\left(\frac{d\phi}{dt}\right) dt + t\left(\frac{d\phi}{dv}\right) dv - pdv.$$

But we have, by § 10, under present conditions

$$\left(\frac{d\phi}{dv}\right) = \left(\frac{dp}{dt}\right).$$

Hence

$$\left(\frac{dE}{dv}\right) = t\left(\frac{dp}{dt}\right) - p,$$

a result assumed in a previous article (RADIATION, No. CXXX. above).

If the working substance have the property (that of the so-called "ideal" perfect gas)

$$pv = Rt,$$

we see that, for it, $$\left(\frac{dE}{dv}\right) = 0.$$

The energy of (unit mass of) such a substance thus depends upon its temperature alone.

12. *Specific Heat of a Fluid.*—Specific heat in its most general acceptation is the heat required, under some given condition, to raise the temperature of unit mass by one degree. Thus it is the heat taken in while the working substance passes, by some assigned path, from one isothermal t to another $t+1$; and this may, of course, have as many values as there are possible paths. Usually, however, but two of these paths are spoken of, and these are taken parallel respectively to the coordinate axes in Watt's diagram, so that we speak of the specific heat at constant volume or at constant pressure. In what follows these will be denoted by c and k respectively.

Take v and p for the independent variables, as in the diagram, and let κ be the specific heat corresponding to the condition

$$f(v, p) = \text{const.}$$

Then $$\kappa dt = td\phi = t\left(\frac{d\phi}{dv}dv + \frac{d\phi}{dp}dp\right);$$

while $$0 = \frac{df}{dv}dv + \frac{df}{dp}dp,$$

and $$dt = \frac{dt}{dv}dv + \frac{dt}{dp}dp.$$

Thus $$\kappa = t\,\frac{\dfrac{d\phi}{dv}\dfrac{df}{dp} - \dfrac{d\phi}{dp}\dfrac{df}{dv}}{\dfrac{dt}{dv}\dfrac{df}{dp} - \dfrac{dt}{dp}\dfrac{df}{dv}}.$$

This expression vanishes if f and ϕ vary together, *i.e.*, in adiabatic expansion, and becomes infinite if f and t vary together, *i.e.*, in isothermal expansion; as might easily have been foreseen. Otherwise it has a finite value. It is usual, however, to choose v and t as independent variables, while we deal analytically (as distinguished from diagrammatically) with the subject. From this point of view we have

$$\kappa dt = t\left(\frac{d\phi}{dv}dv + \frac{d\phi}{dt}dt\right).$$

But the last term on the right is, by definition, cdt; so that

$$(\kappa - c)\,dt = t\frac{d\phi}{dv}dv,$$

with the condition $$\frac{df}{dt}dt + \frac{df}{dv}dv = 0.$$

Thus
$$\kappa - c = -t\frac{d\phi}{dv}\frac{df}{dt}\Big/\frac{df}{dv},$$

which is a perfectly general expression. As the most important case, let f represent the pressure, then we see, by § 10, that

$$\frac{d\phi}{dv} = \frac{dp}{dt},$$

and the formula becomes
$$k - c = -t\left(\frac{dp}{dt}\right)^2\Big/\frac{dp}{dv}.$$

13. *Properties of an Ideal Substance which follows the Laws of Boyle and Charles.*—Closely approximate ideas of the thermal behaviour of a gas such as air, at ordinary temperatures and pressures, may be obtained by assuming the relation

$$pv = Rt,$$

which expresses the laws of Boyle and Charles. Thus, by the formula of last section, we have at once

$$k - c = t\frac{R^2}{v^2}\Big/\frac{p}{v} = R,$$

a relation given originally by Carnot.

Hence, in such a substance,

$$d\phi = c\frac{dt}{t} + (k - c)\frac{dv}{v},$$

or
$$\phi - \phi_0 = c\log t + (k - c)\log v.$$

In terms of volume and pressure, this is

$$\phi - \phi_0 = c\log p/R + k\log v,$$

or
$$pv^{k/c} = R\epsilon^{(\phi-\phi_0)/c},$$

the equation of the adiabatics on Watt's diagram.

This is (for ϕ constant) the relation between p and v in the propagation of sound. It follows from the theory of wave-motion that the speed of sound is

$$\sqrt{\frac{k}{c}Rt},$$

where t is the temperature of the undisturbed air. This expression gives, by comparison with the observed speed of sound, a very accurate determination of the ratio k/c in terms of R. The value of R is easily obtained by experiment, and we have just seen that it is equal to $k - c$; so that k and c can be found for air with great accuracy by this process,—a most remarkable instance of the indirect measurement of a quantity (c) whose direct determination presents very formidable difficulties.

14. *Effect of Pressure on the Melting or Boiling Point of a Substance.*—By the second of the thermodynamic relations in § 10, above, we have

$$\left(\frac{dp}{dt}\right) = \left(\frac{d\phi}{dv}\right),$$

so that

$$\delta p = \left(\frac{dp}{dt}\right)\delta t + \left(\frac{dp}{dv}\right)\delta v = \left(\frac{d\phi}{dv}\right)\delta t + \left(\frac{dp}{dv}\right)\delta v.$$

But, if the fraction e of the working substance be in one molecular state (say liquid) in which V_0 is the volume of unit mass, while the remainder $1 - e$ is in a state (solid) where V_1 is the volume of unit mass, we have obviously

$$v = eV_0 + (1-e)V_1.$$

Let L be the latent heat of the liquid, then

$$\left(\frac{d\phi}{dv}\right) = \frac{td\phi}{t(V_0 - V_1)de} = \frac{L}{t(V_0 - V_1)}.$$

Also, as in a mixture of the same substance in two different states, the pressure remains the same while the volume changes at constant temperature, we have $dp/dv = 0$, so that finally

$$\delta t = \frac{t(V_0 - V_1)}{L}\delta p,$$

which shows how the temperature is altered by a small change of pressure.

In the case of ice and water, V_1 is greater than V_0, so the temperature of the freezing-point is lowered by increase of pressure. When the proper numerical values of V_0, V_1, and L are introduced, it is found that the freezing-point is lowered by about $0^\circ{\cdot}0074$ C. for each additional atmosphere.

When water and steam are in equilibrium, we have V_0 much greater than V_1, so that the boiling-point (as is well known) is raised by pressure. The same happens, and for the same reason, with the melting point, in the case of bodies which *expand* in the act of melting, such as beeswax, paraffin, cast-iron, and lava. Such bodies may therefore be kept solid by sufficient pressure, even at temperatures far above their ordinary melting points.

This is, in a slightly altered form, the reasoning of James Thomson, alluded to above as one of the first striking applications of Carnot's methods made after his work was recalled to notice.

15. *Effect of Pressure on Maximum Density Point of Water.*—One of the most singular properties of water at atmospheric pressure is that it has its maximum density at 4° C. Another, first pointed out by Canton in 1764, is that its compressibility (per atmosphere) is greater at low than at ordinary temperatures—being, according to his measurements, 0·000,049 at 34° F., and only 0·000,044 at 64° F. It is easy to see (though it appears to have been first pointed out by Puschl in 1875)

that the second of these properties involves the *lowering* of the maximum density point by increase of pressure. To calculate the numerical amount of this effect, note that the expansibility, like all other thermal properties, may be expressed as a function of any two of the quantities p, v, t, ϕ; say in the present case p and t. Then we have for the expansibility

$$e = \frac{1}{v}\left(\frac{dv}{dt}\right) = \left(\frac{d}{dt}\right)\log v = f(p, t).$$

Also the compressibility may be expressed as

$$\mathfrak{c} = -\frac{1}{v}\left(\frac{dv}{dp}\right) = -\left(\frac{d}{dp}\right)\log v.$$

The relation between small simultaneous increments of pressure and temperature, which are such as to leave the expansibility unchanged, is thus

$$\left(\frac{de}{dt}\right)\delta t + \left(\frac{de}{dp}\right)\delta p = 0.$$

Now the expansibility is zero at the maximum density point, for which therefore this equation holds. But the equations above give

$$\left(\frac{de}{dp}\right) = \left(\frac{d^2}{dpdt}\right)\log v = -\left(\frac{d\mathfrak{c}}{dt}\right);$$

so that

$$\left(\frac{de}{dt}\right)\delta t - \left(\frac{d\mathfrak{c}}{dt}\right)\delta p = 0.$$

The volume of water at low temperatures under atmospheric pressure varies approximately as

$$1 + \frac{(t-4)^2}{144{,}000}.$$

Thus we have $\left(\frac{de}{dt}\right) = \frac{1}{72{,}000}$ nearly; and from Canton's experimental result above stated we gather that (roughly at least)

$$\left(\frac{d\mathfrak{c}}{dt}\right) = -0{\cdot}000{,}005\,\frac{1{\cdot}8}{30} = -0{\cdot}000{,}000{,}3;$$

from which the formula gives $-0^\circ{\cdot}02$ C. nearly for the change of the maximum density point due to one additional atmosphere.

Recent investigations, carried out by direct as well as by indirect methods, seem to agree in showing that the true value is somewhat less than this, viz., about $-0^\circ{\cdot}018$ C.; so that water has its maximum density at 0° C. when subjected to about 223 atmospheres. Thus, taking account of the result of § 14 above, we find that the maximum density point coincides with the freezing-point at $-2^\circ{\cdot}8$ C. under an additional pressure of about 377 atmospheres, or (say) 2·5 tons weight per square inch.

16. *Motivity and Entropy, Dissipation of Energy.*—The motivity of the quantity H of heat, in a body at temperature t, is

$$H(t-t_0)/t,$$

where t_0 is the lowest available temperature.

The entropy is expressed simply as

$$H/t,$$

being independent of any limit of temperature.

If the heat pass, by conduction, to a body of temperature t' (lower than t, but higher than t_0), the change of motivity (*i.e.*, the dissipation of energy) is

$$Ht_0\left(\frac{1}{t}-\frac{1}{t'}\right),$$

which is, of course, *loss*; while the corresponding change of entropy is the *gain*

$$H\left(\frac{1}{t'}-\frac{1}{t}\right).$$

The numerical values of these quantities differ by the factor t_0, so that, if we could have a condenser at absolute zero, there could be no dissipation of energy. But we see that Clausius's statement that the entropy of the universe tends to a maximum is practically merely another way of expressing Thomson's earlier theory of the dissipation of energy.

The whole *point* of the matter may be summarised as follows. When heat is exchanged among a number of bodies, part of it being transformed by heat-engines into work, the work *obtainable* (*i.e.*, the motivity) is

$$\Sigma(H)-t_0\Sigma(H/t).$$

The work *obtained*, however, is simply

$$\Sigma(H).$$

Thus the waste, or amount needlessly dissipated, is

$$-t_0\Sigma(H/t).$$

This must be essentially a positive quantity, except in the case when perfect engines have been employed in *all* the operations. In that case (unless indeed the unattainable condition $t_0=0$ were fulfilled)

$$\Sigma(H/t)=0,$$

which is the general expression of reversibility.

17. *Works on the Subject.*—Carnot's work has, as we have seen, been reprinted. The scattered papers of Rankine, Thomson, and Clausius have also been issued in collected forms. So have the experimental papers of Joule. The special treatises on *Thermodynamics* are very numerous; but that of Clerk-Maxwell (*Theory of Heat*), though in some respects rather formidable to a beginner, is as yet far superior to any of its rivals.

CXXXII.

MACQUORN RANKINE.

[From a Memoir prefixed to *Rankine's Scientific Papers,* 1881.]

THE life of a genuine scientific man is, from the common point of view, almost always uneventful. Engrossed with the paramount claims of inquiries raised high above the domain of mere human passions, he is with difficulty tempted to come forward in political discussions, even when they are of national importance; and he regards with surprise, if not with contempt, the petty municipal squabbles in which local notoriety is so eagerly sought. To him the discovery of a new law of nature, or even a new experimental fact, or the invention of a novel mathematical method, no matter who has been the first to reach it, is an event of an order altogether different from, and higher than, those which are so profusely chronicled in the newspapers. It is something true and good for ever, not a mere temporary outcome of craft or expediency. With few exceptions, such men pass through life unnoticed by, almost unknown to, the mass of even their educated countrymen. Yet it is they who, far more than any autocrats or statesmen, are really moulding the history of the times to come. Man has been left entirely to himself in the struggle for creature comforts, as well as for the higher appliances which advance civilization; and it is to science, and not to so-called statecraft, that he must look for such things. Science can and does provide the means, statecraft can but more or less judiciously promote, regulate, or forbid, their use or abuse. One is the lavish and utterly unselfish furnisher of material good, the other the too often churlish and ignorant dispenser of it. In the moral world their analogues are charity and the relieving officer! So much it is necessary to say for the sake of the general reader; to the world of science no apology need be made. In *it* Rankine's was and is a well-known name.

It is high eulogy, but strictly correct, to say that Rankine holds a prominent place among the chief scientific men of the last half century. He was one of the little group of thinkers to whom, after the wondrous Sadi Carnot, the world is indebted for the pure science of modern thermodynamics. Were this all, it would be undoubtedly much. But his services to applied science were relatively even greater. By his admirable teaching, his excellent text-books, and his original memoirs, he has done more than any other man of recent times for the advancement of British Scientific Engineering. He did not, indeed, himself design or construct gigantic structures; but he taught, or was the means of teaching, that invaluable class of men to whom the projectors of such works entrust the calculations on which their safety, as well as their efficiency, mainly depend. For, behind the great architect or engineer, and concealed by his portentous form, there is the real worker, without whom failure would be certain. The public knows but little of such men. Not every von Moltke has his services publicly acknowledged and rewarded by his Imperial employer! But he who makes possible the existence of such men confers lasting benefit on his country. And it is quite certain that Rankine accomplished the task.

* * *

In concluding the scientific part of this brief notice of a true man, we need scarcely point out to the reader how much of Rankine's usefulness was due to steady and honest work. The unscientific are prone to imagine that talent (especially when, as in Rankine's case, it rises to the level of genius) is necessarily rapid and off-hand in producing its fruits. No greater mistake could be made. The most powerful intellects work slowly and patiently at a new subject. Such was the case with Newton, and so it is still. Rapid they may be, and in general are, in new applications of principles long since mastered; but it is only your pseudo-scientific man who forms his opinions at once on a new subject. This truth was preeminently realized in Rankine, who was prompt to reply when his knowledge was sufficient, but patient and reticent when he felt that more knowledge was necessary. With him thought was never divorced from work:—both were good of their kind:—the thought profound and thorough, the work a workman-like expression of the thought. Few, if any, practical engineers have contributed so much to abstract science, and in no case has scientific study been applied with more effect to practical engineering. Rankine's name will ever hold a high place in the history of science, and will worthily be associated with those of the great men we have recently lost. And, when we think of who these were, how strangely does such a list:—including the names of Babbage, Boole, Brewster, Leslie Ellis, Forbes, Herschel, Rowan Hamilton, Clerk-Maxwell, Rankine, and others; though confined to physical or mathematical science alone:—contrast with the astonishing utterance of the Prime Minister of Great Britain and Ireland, to the effect that the present is by no means an age abounding in minds of the first order! Ten such men lost by this little country within the last dozen years or so—any one of whom would have made himself an enduring name had he lived in any preceding age, be it that of Hooke and Newton, or that of Cavendish and Watt! Nay more, even such losses as these have not extinguished the hopes of science amongst us. Every one of these great men has, by some mysterious influence of his genius, kindled the sacred thirst for new knowledge in younger but kindred spirits, many of whom will certainly rival, some even may excel, their teachers!

CXXXIII.

ON THE TEACHING OF NATURAL PHILOSOPHY*.

[*Contemporary Review. January,* 1878.]

At the very outset of our work two questions of great importance come prominently forward. One of these, I have reason to conclude from long experience, is probably a puzzling one to a great many of you: the other is of paramount consequence to us all. And both are of consequence not to us alone but to the whole country, in its present feverish state of longing for what it but vaguely understands and calls *science-teaching.* These questions are, *What is Natural Philosophy?* and, *How is it to be taught?*

A few words only, on the first question, must suffice for the present. The term *Natural Philosophy* was employed by Newton to describe the study of the powers of nature: the investigation of forces from the motions they produce, and the application of the results to the explanation of other phenomena. It is thus a subject to whose proper discussion mathematical methods are indispensable. The *Principia* commences with a clear and simple statement of the fundamental laws of motion, proceeds to develop their more immediate consequences by a powerful mathematical method of the author's own creation, and extends them to the whole of what is now called *Physical Astronomy.* And in the Preface, Newton obviously hints his belief that in time a similar mode of explanation would be extended to the other phenomena of external nature.

In many departments this has been done to a remarkable extent during the two centuries which have elapsed since the publication of the *Principia.* In others, scarcely a single step of any considerable magnitude has been taken; and in consequence, the boundary between that which is properly the subject of the natural

* Extended from Notes of the Introductory Lecture to the ordinary course of Natural Philosophy in Edinburgh University, October 31st, 1877.

philosopher's inquiries and that which is altogether beyond his province is at present entirely indefinite. There can be no doubt that, in many important respects, even life itself is dependent upon purely physical conditions. The physiologists have quite recently seized, for their own inquiries, a great part of the natural philosopher's apparatus, and with it his methods of experimenting. But to say that even the very lowest form of life, not to speak of its higher forms, still less of volition and consciousness, can be fully *explained* on physical principles alone—*i.e.*, by the mere relative motions and interactions of portions of inanimate matter, however refined and sublimated—is simply unscientific. There is absolutely nothing known in physical science which can lend the slightest support to such an idea. In fact, it follows at once from the *Laws of Motion* that a material system, left to itself, has a perfectly determined future, *i.e.*, that upon its configuration and motion at any instant depend all its subsequent changes; so that its whole history, past and to come, is to be gathered from one almost instantaneous, if sufficiently comprehensive glance. In a purely material system there is thus *necessarily* nothing of the nature of a free agent. To suppose that life, even in its lowest form, is wholly material, involves therefore either a denial of the truth of Newton's laws of motion, or an erroneous use of the term "matter." Both are alike unscientific.

Though the sphere of our inquiries extends wherever matter is to be found, and is therefore coextensive with the physical universe itself, there are other things, not only without but within that universe, with which our science has absolutely no power to deal. In this room we simply recognize them, and pass on.

Modern extensions of a very general statement made by Newton enable us now to specify much more definitely than was possible in his time the range of physical science. We may now call it the *Science of Matter and Energy*. These are, as the whole work of the session will be designed to prove to you, *the two real things* in the physical universe; both unchangeable in amount, but the one consisting of parts which preserve their identity; while the other is manifested only in the act of transformation, and though measurable cannot be identified. I do not at present enter on an exposition of the nature or laws of either; that exposition will come at the proper time; but the fact that so short and simple a definition is possible is extremely instructive, showing, as it unquestionably does, what very great advances physical science has made in recent times. The definition, in fact, is but little inferior in simplicity to two of those with which most of you are no doubt already to a certain extent familiar—that of Geometry as the *Science of Pure Space*, and of Algebra as the *Science of Pure Time*.

But, for to-day at least, our second question, viz., *How is Natural Philosophy to be taught?* is of more immediate importance. The answer, in an elementary class like this, must of course be—"popularly." But this word has many senses, even in the present connection—one alone good, the others of variously graduated amounts of badness.

Let us begin with one or two of the bad ones. The subject is a very serious one for you, and therefore must be considered carefully, in spite of the celebrated dictum of

Terence, *Obsequium amicos, veritas odium parit.* (In other words, Flatter your audience and tickle their ears, if you seek to ingratiate yourself with them; tell them the truth, if you wish to raise enemies.) But science is one form of truth. When the surgeon is convinced that the knife is required, it becomes his *duty* to operate. And Shakspeare gives us the proper answer to the time-serving caution of Terence and Cicero in the well-known words, "Let the *galled* jade wince."

One of these wholly bad methods was recently very well put by a *Saturday* critic, as follows:—

"The name of 'Popular Science' is, in itself, a doubtful and somewhat invidious one, being commonly taken to mean the superficial exposition of results by a speaker or writer who himself understands them imperfectly, to the intent that his hearers or readers may be able to talk about them without understanding them at all."

Clerk-Maxwell had previously put it in a somewhat different form:—

"The forcible language and striking illustrations by which those who are past hope of even being beginners may be prevented from becoming conscious of intellectual exhaustion before the hour has elapsed."

This, I need hardly say, is not in any sense science-teaching. It appears, however, that there is a great demand for it, more especially with audiences which seek amusement rather than instruction; and this demand of course is satisfied. Such an audience gets what it seeks, and, I may add, exactly what it deserves.

Not quite so monstrous as that just alluded to, yet far too common, is the essentially vague and highly ornamented style of so-called science-teaching. The objections to this method are of three kinds at least—each independently fatal.

First. It gives the hearer, if he have no previous acquaintance with Physics, an altogether erroneous impression of the intrinsic difficulty of the subject. He is exhorted, in grandiloquent flights of laboured earnestness, to exert his utmost stretch of intellect, that he may comprehend the great step in explanation which is next to be given; and when, after this effort, the impression on his mind is seemingly quite inadequate, he begins to fancy that he has not understood at all—that there must be some profound mystery in the words he has heard which has entirely escaped his utmost penetration. After a very few attempts he gives up in despair. How many a man has been driven away altogether, whose intellect might have largely contributed to the advance of Physics, merely by finding that he can make nothing of the pompous dicta of his teacher or text-book, except something so simple that he fancies it cannot possibly be what was meant!

Second. It altogether spoils the student's taste for the simple facts of true science. And it does so just as certainly as an undiluted course of negro melodies or music-hall comic songs is destructive of all relish for the true art of Mozart or Haydn, or as sensation novels render Scott's highest fancies tame by contrast. And,

"...... as if increase of appetite had grown
By what it fed on,......"

the action on the listener is made to react on the teacher, and he is called upon for further and further outrages on the simplicity of science. Sauces and spices not only impair the digestion, they create a craving for other stimulants of ever-increasing pungency and deleteriousness.

But, *thirdly*. No one having a true appreciation of the admirable simplicity of science could be guilty of these outrages. To attempt to introduce into *science* the meretricious adjuncts of "word-painting," &c., can only be the work of dabblers—not of scientific men, just as

> "To gild refinèd gold, to paint the lily,
> To throw a perfume on the violet,
> To smooth the ice, or add another hue
> Unto the rainbow; or with taper light
> To seek the beauteous eye of heaven to garnish,
> Is *wasteful and ridiculous excess.*"

None could attempt such a work who had the smallest knowledge of the true beauty of nature. Did he know it, he would feel how utterly inadequate, as well as uncalled-for, were all his greatest efforts. For, again in Shakspeare's words, such a course

> "Makes sound opinion sick, and truth suspected,
> For putting on so new a fashioned robe."

"In the great majority of 'popular' scientific works the author, as a rule, has not an exact knowledge of his subject, and does his best to avoid committing himself, among difficulties which he must at least try to *appear* to explain. On such occasions he usually has recourse to a flood of vague generalities, than which nothing can be conceived more pernicious to the really intelligent student. In science 'fine language' is entirely out of place; the stern truth, which is its only basis, requires not merely that we should never disguise a difficulty, but, on the contrary, that we should call special attention to it, as a probable source of vàluable information. If you meet with an author who, like the cuttle-fish, endeavours to escape from a difficult position by darkening all around him with an inky cloud of verbiage, close the book at once and seek information elsewhere."

But I must come back to the really important point, which is this:—

True science is in itself simple, and should be explained in as simple and definite language as possible.

Word-painting finds some of its most appropriate subjects when employed to deal with human snobbery or human vice—where the depraved tastes and wills of mortals are concerned—not the simple and immutable truths of science. Battles, murders, executions; political, legal, and sectarian squabbles; gossip, ostentation, toadyism, and such like, are of its proper subjects. Not that the word-painter need be himself necessarily snobbish or vicious—far from it. But it is here, as our best poets and satirists have shown, that his truest field is to be found. Science sits enthroned, like the gods of Epicurus, far above the influence of mere human passions, be they virtuous or evil, and must be treated by an entirely different code of rules. And a great deal of the very shallowest of the pseudo-science of the present day probably owes its origin to the habitual use, with reference to physical phenomena, of terms or synonyms whose derivation shows them to have reasonable application to human beings and their actions alone—not at all to matter and energy. In dealing with such pseudo-science it is, of course, permissible to me, even after what I have said, to use word-painting as far as may be thought necessary.

The Pygmalions of modern days do not require to beseech Aphroditè to animate the ivory for them. Like the savage with his *Totem*, they have themselves already attributed life to it. "It comes," as v. Helmholtz says, "to the same thing as Schopenhauer's metaphysics. The stars are to love and hate one another, feel pleasure and displeasure, and to try to move in a way corresponding to these feelings." The latest phase of this peculiar non-science tells us that all matter is ALIVE; or at least that it contains the "promise and potency" (whatever these may be) "of all terrestrial life." All this probably originated in the very simple manner already hinted at; viz., in the confusion of terms constructed for application to thinking beings only, with others applicable only to brute matter, and a blind following of this confusion to its necessarily preposterous consequences. So much for the attempts to introduce into science an element altogether incompatible with the fundamental conditions of its existence.

When simple and definite language cannot be employed, it is solely on account of our ignorance. Ignorance may of course be either *unavoidable* or *inexcusable*.

It is unavoidable only when knowledge is not to be had. But that of which there is no knowledge is not yet part of science. All we can do with it is simply to confess our ignorance and seek for information.

As an excellent illustration of this we may take two very common phenomena—a *rainbow* and an *aurora*—the one, to a certain extent at least, thoroughly understood; the other scarcely understood in almost any particular. Yet it is possible that, in our latitudes at least, we see the one nearly as often as the other. For, though there are probably fewer auroras to be seen than rainbows, the one phenomenon is in general much more widely seen than the other. A rainbow is usually a mere local phenomenon, depending on a rain-cloud of moderate extent; while an aurora, when it occurs, may extend over a whole terrestrial hemisphere. Just like total eclipses, lunar and solar. Wherever the moon can be seen, the lunar eclipse is visible, and to all alike. But a total solar eclipse is usually visible from a mere strip of the earth—some fifty miles or so in breadth.

The branch of natural philosophy which is called *Geometrical Optics* is based upon three experimental facts or laws, which are assumed as exactly true, and as representing the whole truth—the rectilinear propagation of light in any one uniform medium, and the laws of its reflexion and refraction at the common surface of two such media; and as a science it is nothing more than the developed mathematical consequences of these three postulates.

Hence, if these laws were rigorously true, and represented *all* the truth, nothing but mathematical investigation based on them would be required for the complete development of the phenomena of the rainbow—except the additional postulate, also derived from experiment, that falling drops of water assume an exact spherical form —and, as data for numerical calculation, the experimentally-determined refractive index for each ray of light at the common surface of air and water.

Thus for instance we can tell why the rainbow has the form of a portion of a circle surrounding the point opposite to the sun; why it is red on the outer edge; what is the order of the other colours, and why they are much less pure than the red; why the whole of the background enclosed within it is brighter than that just outside; and so on. Also why there is a second (also circular) rainbow; why it is concentric with the first; and why its colours are arranged in the reverse order, &c.

But, so long at least as we keep to *Geometrical* optics, we cannot explain the spurious bows which are usually seen, like ripples, within the primary and outside the second rainbow; nor why the light of both bows is polarized, and so forth. We must apply to a higher branch of our science; and we find that *Physical Optics*, which gives the results to which those of geometrical optics are only approximations, enables us to supply the explanation of these phenomena also.

When we turn to the aurora we find nothing so definite to explain. This may, to some extent at least, account for our present ignorance. We remark, no doubt, a general relation between the direction of the earth's magnetic force and that of the streamers: but their appearance is capricious and variable in the extreme. Usually they have a pale green colour, which the spectroscope shows to be due to homogeneous light; but in very fine displays they are sometimes blood-red, sometimes blue. Auroral arches give sometimes a sensibly continuous spectrum; sometimes a single bright line. We can *imitate* many of the phenomena by passing electric discharges through rarefied gases; and we find that the streamers so produced are influenced by magnetic force. But we do not yet know for certain the source of the discharges which produce the aurora, nor do we even know what substance it is to whose incandescence its light is due. We find by a statistical method that auroras, like cyclones, are most numerous when there are most spots on the sun; but the connection between these phenomena is not yet known. Here, in fact, we are only *beginning* to understand, and can but confess our ignorance.

But do not imagine that there is nothing about the rainbow which we cannot explain, even of that which is seen at once by untrained observers. All the phenomena connected with it which we can explain are mathematical deductions from observed facts which are assumed in the investigation. But these facts are, in the main, themselves not yet explained. Just as there are many exceedingly expert calculators who habitually and usefully employ logarithmic tables without having the least idea of what a logarithm really is, or of the manner in which the tables themselves were originally calculated; so the natural philosopher uses the observed facts of refraction and reflection without having as yet anything better than guesses as to *their* possible proximate cause. And it is so throughout our whole subject: assuming one result, we can prove that the others *must* follow. In this direction great advances have been made, and every extension of mathematics renders more of such deductions possible. But when we try to reverse the process, and thus to explain our hitherto assumed results, we are met by difficulties of a very different order.

The subject of *Physical Astronomy*, to which I have already alluded, gives at once one of the most striking and one of the most easily intelligible illustrations of this point. Given the law of gravitation, the masses of the sun and planets, and their relative positions and motions *at any one instant*,—the investigation of their future motions, until new disturbing causes come in, is entirely within the power of the mathematician. But how shall we account for gravitation? This is a question of an entirely different nature from the other, and but one even plausible attempt to answer it has yet been made.

But to resume. The digression I have just made had for its object to show you how closely full knowledge and absolute ignorance may be and are associated in many parts of our subject—absolute command of the necessary consequences of a phenomenon, entire ignorance of its actual nature or cause.

And in every branch of physics the student ought to be most carefully instructed about matters of this kind. A comparatively small amount of mathematical training will often be found sufficient to enable him to trace the consequences of a known truth to a considerable distance; and no such training is necessary to enable him to see (provided it be properly presented to him) the boundary between our knowledge and our ignorance—at least when that ignorance is not directly dependent upon the inadequacy of our deductive powers.

The work of Lucretius is perhaps the only really successful attempt at scientific poetry. And it is so because it was written before there was any true physical science. The methods throughout employed are entirely those of *à priori* reasoning, and therefore worse than worthless, altogether misleading. Scientific poetry, using both words in their highest sense, is now impossible. The two things are in their very nature antagonistic. A scientific man *may* occasionally be a poet also; but he has then two distinct and almost mutually incompatible natures; and, when he writes poetry, he puts science aside. But, on the other hand, when he writes science, he puts poetry and all its devices aside. Mark this well! A poet may, possibly with great effect on the unthinking multitude, write of

> "...... the huger orbs which wheel
> In circuits vast throughout the wide abyss
> Of unimagined Chaos—till they reach
> Æthereal splendour......."

(The word "unimagined" may puzzle the reader, but it probably alludes to Ovid's expression "*sine imagine*." For this sort of thing is nothing if not *classical!* The contempt in which "scholars" even now hold mere "physicists" is proverbial. And they claim the right of using at will new words of this kind, in whose company even the "tremendous empyrean" would, perhaps, not be quite out of place.)

But, whether this sort of thing be poetry or not, it is in no sense science. "Huge," and "vast," and such-like (for which, if the rhythm permit, you may substitute their similars, "Titanic," "gigantic," &c.), good honest English though they be, are utterly unscientific words. In science we restrict ourselves to *small* and *great*, and these amply suffice for all our wants. But even these terms are limited with us to a mere relative sense; and it can only be through ignorance or forgetfulness of this that more sonorous terms are employed. The size of every finite object depends entirely upon the unit in terms of which you measure it. There is nothing absolutely great but the Infinite.

A few moments' reflection will convince you of the truth of what I have just said. Let us only go by easily comprehensible stages from one (so-called) extreme to the other. Begin with the smallest thing you can see, and compare it with the greatest. I suppose you have all seen a good barometer. The vernier attached to such an instrument is usually read to thousandths of an inch, but it sometimes leaves you in doubt which of two such divisions to choose. This gives the limit of vision with the unaided eye. Let us therefore begin with an object whose size is about 1-2000th of an inch. Let us choose as our scale of relative magnitude 1 to 250,000 or thereabouts. It is nearly the proportion in which each of you individually stands to the whole population of Edinburgh. (I am not attempting anything beyond the rudest illustration, because that will amply suffice for my present purpose.) Well: 250,000 times the diameter of our *minimum visibile* gives us a length of ten feet or so—three or four paces. Increased again in about the same ratio, it becomes more than 400 miles, somewhere about the distance from Edinburgh to London. Perform the operation again, and you get (approximately enough for our purpose) the sun's distance from the earth. Operate once more, and you have got beyond the nearest fixed star. Another such operation would give a distance far beyond that of anything we can ever hope to see. Yet you have reached it by repeating, at most *five* times, upon the smallest thing you can see, an operation in itself not very difficult to imagine. Now as there is absolutely nothing known to science which can preclude us from carrying this process farther, so there is absolutely no reason why we may not in thought *reverse* it, and thus go back from the smallest visible thing to various successive orders of smallness. And the *first* of these that we thus reach has already been pointed to by science as at least a rough approximation to that coarse-grainedness which we *know* to exist (though we shall never be able to see it) even in the most homogeneous substances, such as glass and water. For several trains of reasoning, entirely independent of one another, but based upon experimental facts, enable us to say with certainty that all matter becomes heterogeneous (in some as yet quite unknown way) when we consider portions of it whose dimensions are somewhere about 1-500,000,000th of an inch. We have, as yet, absolutely no information beyond this, save that, if there be ultimate atoms, they are at least considerably more minute still.

Next comes the very important question—*How far is experimental illustration necessary and useful?* Here we find excessively wide divergence, alike in theory and in practice.

In some lecture-theatres, experiment is everything; in others, the exhibition of gorgeous displays illustrative of nothing in particular is said occasionally to alternate with real or imagined (but equally sensational) danger to the audience, from which they are preserved (or supposed to be preserved) only by the extraordinary presence of mind of the presiding performers—a modern resuscitation of the ancient after-dinner amusement of tight-rope dancing, high above the heads of the banqueters, where each had thus a very genuine, if selfish, interest in the nerve and steadiness of the artists.

Contrasted in the most direct manner with these, is the dictum not long ago laid down:—

> "It may be said that the fact makes a stronger impression on the boy through the medium of his sight—that he believes it the more confidently. I say that this ought not to be the case. If he does not believe the statements of his tutor—probably a clergyman of mature knowledge, recognized ability, and blameless character—his suspicion is irrational, and manifests a want of the power of appreciating evidence—a want fatal to his success in that branch of science which he is supposed to be cultivating."

Between such extremes many courses may be traced. But it is better to dismiss the consideration of both, simply on the ground that they *are* extremes, and therefore alike absurd.

Many facts cannot be made thoroughly intelligible without experiment; many others require no illustration whatever, except what can be best given by a few chalk-lines on a blackboard. To teach an essentially experimental science without illustrative experiments may conceivably be possible in the abstract, but certainly not with professors and students such as are to be found on this little planet.

And, on the other hand, you must all remember that we meet here to discuss science, and science alone. A University class-room is not a place of public amusement, with its pantomime displays of red and blue fire, its tricks whether of prestigiation or of prestidigitation, or its stump-oratory. The best and greatest experimenter who ever lived used none of these poor devices to win cheap applause. His language (except perhaps when non-experimenting pundits pressed upon him their fearful Greek names for his splendid discoveries) was ever the very simplest that could be used: his experiments, whether brilliant or commonplace in the eyes of the mere sight-seer, were chosen solely with the object of thoroughly explaining his subject; and his whole bearing was impressed with the one paramount and solemn feeling of duty, alike to his audience and to science. Long ages may pass before his equal, or even his rival, can appear; but the great example he has left should be imitated by us all as closely as possible.

Nothing is easier in extempore speaking, as I dare say many of you know by trial, than what is happily called "piling up the agony." For, as has been well said,

> "...... men there be that make
> Parade of fluency, and deftly play
> With points of speech as jugglers toss their balls;
> A tinkling crew, from whose light-squandered wit
> No seed of virtue grows."

Every one who has a little self-confidence and a little readiness can manage it without trouble. But it is so because in such speaking there is no necessity for precision in the use of words, and no objection to any epithet whatever, even if it be altogether misplaced. But the essence of all such discourse is necessarily *fancy*, and not *fact*. Here, during the serious work of the session, we are tied down almost exclusively to facts. Fancies *must* appear occasionally; but we admit them only in the carefully-guarded form of a reference to old opinions, or to a "good working hypothesis." Still, facts are not necessarily dry: not even if they be mere statistics. All depends on the way in which they are put. One of the most amusing of the many clever songs, written and sung by the late Professor Rankine in his moments of relaxation, was an almost literal transcript of a prosaic statistical description of a little Irish town, taken from a gazetteer! He was a truly original man of science, and therefore exact in his statements; but he could be at once both exact and interesting. And I believe that the intrinsic beauty of science is such that it cannot suffer in the minds of a really intelligent audience, however poor be the oratorical powers of its expounder, provided only he can state its facts with clearness. Oratory is essentially *art*, and therefore essentially *not science*.

There is nothing false in the *theory*, at least, of what are called Chinese copies. If it could be *fully* carried out, the results would be as good as the original—in fact, undistinguishable from it. But it is solely because we cannot have the theory carried out in perfection that true artists are forced to slur over details, and to give "broad effects" as they call them. The members of the Pre-Raphaelite school are thoroughly right in one part at least of their system: unfortunately it is completely unrealizable in practice. But the "broad effects" of which I have spoken are true *art*, though perhaps in a somewhat modified sense of the word (which, not being a scientific one, has many shades of meaning). To introduce these "broad effects" into science may be artful, but it is certainly unscientific. In so-called "popular science," if anywhere, *Ars est celare inscientiam*. The "artful dodge" is to conceal want of knowledge. Vague explanations, however artful, no more resemble true science than do even the highest flights of the imagination, whether in *Ivanhoe* or *Quentin Durward*, Knickerbocker's *New York* or Macaulay's *England*, resemble history. And when the explanation is bombastic as well as vague, its type is the same as that of the well-known speech of Sergeant Buzfuz.

One ludicrous feature of the "high-falutin" style is that if you adopt it you throw away all your most formidable ammunition on the smaller game, and have nothing proportionate left for the larger. It is as if you used a solid shot from an 81-ton gun upon a single skirmisher! As I have already said, you waste your grandest terms, such as huge, vast, enormous, tremendous, &c., on your mere millions or billions; and then what is left for the poor trillions? The true lesson to be learned from this is, that such terms are altogether inadmissible in science.

But even if we could suppose a speaker to use these magnificent words as a genuine description of the impression made on himself by certain phenomena, you must remember that he is describing *not* what is known of the objective fact (which, except occasionally from a biographic point of view, is what the listener really wants), but the more or less inadequate subjective impression which it has produced, or which he desires you to think it has produced, on "what he is pleased to call his mind." Whether it be his own mind, or that of some imaginary individual, matters not. To do this, except perhaps when lecturing on psychology, is to be unscientific. True scientific teaching, I cannot too often repeat, requires that the facts and their *necessary* consequences alone should be stated (and illustrated if required) as simply as possible. The impression they are to produce on the mind of the reader, or hearer, is then to be left entirely to himself. No one has any *right* to suppose, much less to take for granted, that his own notions, whether they be "so-called poetic instincts" (to use the lowest term of contempt) or half-comprehended and imperfectly expressed feelings of wonder, admiration, or awe, are either more true to fact or more sound in foundation than those of the least scientific among his readers or his audience. When he does so he resembles a mere leader of a *claque*. * * * If your minds cannot relish simple food, they are not in that healthy state which is required for the study of science. Healthy mental appetite needs only hunger-sauce. *That* it always has in plenty, and repletion is impossible.

But you must remember that language cannot be simple unless it be definite; though sometimes, from the very nature of the case, it may be very difficult to understand, even when none but the simplest terms are used. Multiple meanings for technical words are totally foreign to the spirit of true science. When an altogether new idea has to be expressed, a new word must be coined for it. None but a blockhead could object to a new word for a new idea. And the habitual use of non-scientific words in the teaching of science betrays ignorance, or (at the very least) wilful indefiniteness.

Do not fancy, however, that you will have very many new words to learn. A month of *Botany* or of *Entomology*, as these are too often taught, will introduce you to a hundredfold as many new and strange terms as you will require in the whole course of natural philosophy; and, among them, to many words of a far more "difficult complexion" than any with which, solely for the sake of definiteness, we find ourselves constrained to deal.

But you will easily reconcile yourselves to the necessity for new terms if you bear in mind that these not only secure to us that definiteness without which science is impossible, but at the same time enable us to get rid of an enormous number of wholly absurd stock-phrases which you find in almost every journal you take up, wherever at least common physical phenomena are referred to. When we are told that a building was "struck by the *electric fluid*" we may have some difficulty in understanding the process; but we cannot be at all surprised to learn that it was immediately thereafter "seized upon by the *devouring element*, which raged unchecked till the whole was reduced to ashes." I have no fault to find with the penny-a-liner who writes such things as these: it is all directly in the way of his business, and he has been trained to it. Perhaps his graphic descriptions may occasionally rise even to poetry. But when I meet with anything like this,—and there are but too many works, professedly on natural philosophy, which are full of such things,—I *know* that I am not dealing with science.

A wild and plaintive wail for definiteness often comes from those writers and lecturers who are habitually the most vague. A few crocodile tears are shed, appearances are preserved, and they plunge at once into greater mistiness of verbosity than before.

Considering the actual state of the great majority at least of our schools and our elementary text-books, I should prefer that you came here completely untaught in physical science. You would then have nothing to *unlearn.* This is an absolutely incalculable gain. Unlearning is by far the hardest task that was ever imposed on a student, or on any one else. And it is also one of those altogether *avoidable* tasks which, when we have allowed them to become necessary, irritate us as much as does a perfectly unprofitable one—such as the prison crank or shot-drill. And in this lies by far the greatest responsibility of all writers and teachers. Merely to fail in giving instruction is bad enough, but to give false information can be the work only of utter ignorance or of carelessness, amounting, so far as its effects go, almost to diabolical wickedness.

Every one of you who has habitually made use of his opportunities of observation must have already seen a greal deal which it will be my duty to help him to understand. But I should prefer, if possible, to have the entire guidance of him in helping him to understand it. And I should commence by warning him in the most formal manner against the study of books of an essentially unscientific character. By all means let him read fiction and romance as a relaxation from severer studies; but let the fiction be devoted to its legitimate object, human will and human action; don't let it tamper with the truths of science. From the *Arabian Nights,* through *Don Quixote,* to Scott, the student has an ample field of really profitable reading of this kind; but when he wishes to *study,* let him carefully eschew the unprofitable, or rather pernicious, species of literary fiction which is commonly called "popular science."

As I have already said, in this elementary class you will require very little mathematical knowledge, but such knowledge is in itself one of those wholly good things of which no one can ever have too much. And, moreover, it is one of the few things which it is not very easy to teach badly. A really good student will learn mathematics almost *in spite of* the badness of his teaching. No pompous generalities can gloss over an incorrect demonstration; at least in the eyes of any one competent to understand a correct one. Can it be on this account that there are so many more aspirants to the teaching of physics than to that of the higher mathematics? If so, it is a very serious matter for the progress of science in this country; as bad, at least, as was the case in those old days when it was supposed that a man who had notoriously failed in everything else must have been designed by nature for the vocation of schoolmaster; a truly wonderful application of teleology.

But even this queer kind of Dominie was not so strange a monstrosity as the modern manikins of *paper science,* who are always thrusting their crude notions on the world; the anatomists who have never dissected, the astronomers who have never used a telescope, or the geologists who have never carried a hammer! The old metaphysical pretenders to science had at least some small excuse for their conduct in the fact that true science was all but unknown in the days when *they* chiefly flourished, and when their *à priori* dogmatism was too generally looked upon as science. But that singular race is now well-nigh extinct, and in their place have come the paper-scientists (the barbarous word suits them exactly)—those who, with a strange mixture of half-apprehended fact and thoroughly appretiated nonsense, pour out continuous floods of information of the most self-contradictory character. Such writers loudly claim the honours of *discovery* for any little chance remark of theirs which research may happen ultimately to substantiate, but keep quietly in the background the mass of unreason in which it was originally enveloped. This species may be compared to midges, perhaps occasionally to mosquitos, continually pestering men of science to an extent altogether disproportionate to its own importance in the scale of being. Now and then it buzzes shrilly enough to attract the attention of the great sound-hearted, but unreasoning because non-scientific, public which, when it *does* interfere with scientific matters, can hardly fail to make a mess of them.

Think, for a moment, of the late *vivisection crusade* or of the *anti-vaccinators*. What absolute fiends in human form were not the whole race of really scientific medical men made out to be, at least in the less cautious of these heated denunciations? How many camels are unconsciously swallowed while these gnats are being so carefully strained out, is obvious to all who can take a calm, and therefore a not necessarily unreasonable, view of the matter.

But the victims of such people are not in scientific ranks alone. Every man who occupies a prominent position of any kind is considered as a fit subject for their attacks. By private letters and public appeals, gratuitous advice and remonstrance are perpetually intruded upon him. If he succeed in anything, it is of course because these unsought hints were taken: if he fail, it is because they were contemptuously left unheeded!

Enough of this necessary but unpleasant digression. I *know* that it is at least quite as easy to understand the most recondite mathematics as to follow the highest of genuine physical reasoning; and therefore, when I find apparently profound physical speculation associated with incapacity for the higher mathematics, I feel convinced that the profundity cannot be real. One very necessary remark, however, must be made here: not in qualification, but in explanation, of this statement. One of the greatest of physical reasoners, Faraday, professed, as most of you are aware, to know very little of mathematics. But in fact he was merely unacquainted with the technical use of symbols. His modes of regarding physical problems were of the highest order of mathematics. Many of the very best things in the recent great works on *Electricity* by Clerk-Maxwell and Sir William Thomson are (as the authors cheerfully acknowledge) little more than well-executed *translations* of Faraday's conceptions into the conventional language of the higher analysis.

I hope that the time is not far off when no one who is not (at least in the same sense as Faraday) a genuine mathematician, however he may be otherwise qualified, will be looked upon as even a *possible* candidate for a chair of Natural Philosophy in any of our Universities. Of course such a danger would be out of the question if we were to constantly bear in mind the sense in which Newton understood the term Natural Philosophy. There is nothing so well fitted as mathematics "to take the nonsense out of a man," as it is popularly phrased. No doubt a man may be an excellent mathematician, and yet have absolutely no knowledge of physics; but he cannot possibly *know* physics as it is unless he be a mathematician. Much of the most vaunted laboratory work is not nearly of so high an order of skilled labour as the every-day duty of a good telegraph clerk, especially if he be in charge of a syphon-recorder. And many an elaborate memoir which fills half a volume of the transactions of some learned society is essentially as unsightly and inconvenient an object as the mounds of valueless dross which encumber the access to a mine, and destroy what otherwise might have been an expanse of fruitful soil.

There are many ways in which these mounds may grow. The miner may be totally ignorant of geology, and may thus have bored and excavated in a locality which he ought to have known would furnish nothing. Or he may have, by chance or by the advice of knowing friends, hit upon a really good locality. Even *then* there are many modes of failure, two of which are very common. He may fail to recognize the ore when he has got it; and so it goes at once to the refuse-heap, possibly to be worked up again long after by somebody who has a little more mineralogical knowledge—as in the recent case of the mines of Laurium. Here he *may* be useful—at second-hand. Or, if it be fossils or crystals, for instance, for which he is seeking, his procedure may be so rough as to smash them irreparably in the act of mining. This is dog in the manger with a vengeance. But, anyhow, he generally manages to disgust every other digger with the particular locality which he has turned upside down; and thus exercises a *real*, though essentially *negative*, influence on the progress of mining.

The parallel here hinted at is a very apt one, and can be traced much farther. For there are other peculiarities in the modes of working adopted by some miners, which have their exact counterparts in many so-called scientific inquiries; but, for the present, we must leave them unnoticed.

There is but one way of being scientific: but the number of ways of being unscientific is infinite, and the temptations alluring us to them are numerous and strong. Indolence is the most innocent in appearance, but in fact probably the most insidious and dangerous of all. By this I mean of course not mere idleness, but that easily acquired and fatal habit of stopping just short of the final necessary step in each explanation. Faraday long ago pointed this out in his discourse on *Mental Inertia*. Many things which are excessively simple when thoroughly understood are by no means easy to acquire; and the student too often contents himself with that *half* learning which, though it costs considerable pains, leaves no permanent impression on the mind, while "one struggle more" would have made the subject his own for ever after.

Science, like all other learning, can be reached only by continued exertion. And, even when we have done our utmost, we always find that the best we have managed to achieve has been merely to avoid straying very far from the one true path.

For, though science is in itself essentially simple, and is ever best expressed in the simplest terms, it is my duty to warn you in the most formal manner that the study of it is beset with difficulties, many of which cannot but constitute real obstacles in the way even of the mere beginner. And this forms another of the fatal objections to the school-teaching of physical science. For there is as yet absolutely no known road to science except through or over these obstacles, and a certain amount of maturity of mind is required to overcome them.

If any one should deny this, you may at once conclude either that his mental powers are of a considerably higher order than those of Newton (who attributed all his success to close and patient study) or, what is intrinsically at least somewhat more probable, that he has not yet traversed the true path himself. But it would be a mere exercise of unprofitable casuistry to inquire which is the less untrustworthy guide, he who affirms that the whole road is easy, or he who is continually pointing out fancied difficulties.

Here, as in everything to which the human mind or hand can be applied, nothing of value is to be gained without effort; and all that your teacher can possibly do for you is to endeavour, so far as in him lies, to make sure that your individual efforts shall be properly directed, and that as little energy as possible shall be wasted by any of you in a necessarily unprofitable direction.

END OF VOLUME II.

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